Practical Issues

Department of Government
London School of Economics and Political Science

Administrative Stuff

1. Summative Essay Deadline
   Tuesday LT Week 1
2. Topics for Weeks 6–11

Increasing/Decreasing Power

Increases Power

- Bigger sample
- Precise measures
- Covariates?

Decreases Power

- Attrition
- Noncompliance
- Clustering

Covariates in Experiments

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Identification of a causal effect only requires randomization

We don't need to include covariates in analysis!

$$Y = \beta_0 + \beta_1 X + \epsilon \tag{1}$$

$$Y = \beta_0 + \beta_1 X + \beta_{2\_J} Z + \epsilon \tag{2}$$

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Independence of potential outcomes from treatment assignment is an *asymptotic* property of randomization!

Block Randomization I

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Basic idea: randomization occurs within strata defined *before* treatment assignment

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CATE is estimate for each stratum; aggregated to SATE

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Why?

- Eliminate chance imbalances
- Optimized for estimating CATEs
- More precise SATE estimate

Exp.	Control				Treatment			
1	M	M	M	M	F	F	F	F
2	M	M	M	F	M	F	F	F
3	M	M	F	F	M	M	F	F
4	M	F	F	F	M	M	M	F
5	F	F	F	F	M	M	M	M

Obs.	X_{1i}	X_{2i}	D_i
1	Male	Old	0
2	Male	Old	1
3	Male	Young	1
4	Male	Young	0
5	Female	Old	1
6	Female	Old	0
7	Female	Young	0
8	Female	Young	1

Block Randomization II

Blocking ensures ignorability of all covariates used to construct the blocks

Incorporates covariates explicitly into the *design*

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- Most valuable in small samples

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Blocking ensures ignorability of all covariates used to construct the blocks

Incorporates covariates explicitly into the *design*

When is blocking *statistically* useful?

- If those covariates affect values of potential outcomes, blocking reduces the variance of the SATE
- Most valuable in small samples
- Not valuable if all blocks have similar potential outcomes

Statistical Properties I

Complete randomization:

$$SATE = \frac{1}{n_1} \sum Y_{1i} - \frac{1}{n_0} \sum Y_{0i}$$

Block randomization:

$$SATE_{blocked} = \sum_{1}^{J} \left(\frac{n_j}{n} \right) (\widehat{CATE}_j)$$

Obs.	X_{1i}	X_{2i}	D_i	Y_i	CATE
1	Male	Old	0	5	
2	Male	Old	1	10	
3	Male	Young	1	4	
4	Male	Young	0	1	
5	Female	Old	1	6	
6	Female	Old	0	2	
7	Female	Young	0	6	
8	Female	Young	1	9	

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SATE Estimation

$$
\begin{aligned}
SATE &= \left(\frac{2}{8} * 5\right) + \left(\frac{2}{8} * 3\right) + \left(\frac{2}{8} * 4\right) + \left(\frac{2}{8} * 3\right) \\
&= 3.75
\end{aligned}
$$

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\end{aligned}
$$

The blocked and unblocked estimates are the same here because $Pr(Treatment)$ is constant across blocks and blocks are all the same size.

SATE Estimation

We can use weighted regression to estimate this in an OLS framework

Weights are the inverse prob. of being treated w/in block

Pr(Treated) by block: $p_{ij} = Pr(D_i = 1|J = j)$

Weight (Treated): $w_{ij} = \frac{1}{p_{ij}}$

Weight (Control): $w_{ij} = \frac{1}{1 - p_{ij}}$

Statistical Properties II

Complete randomization:

$$\widehat{SE}_{SATE} = \sqrt{\frac{\widehat{Var}(Y_0)}{n_0} + \frac{\widehat{Var}(Y_1)}{n_1}}$$

Block randomization:

$$\widehat{SE}_{SATE_{blocked}} = \sqrt{\sum_{j=1}^{J} \left(\frac{n_j}{n}\right)^2 \widehat{Var}(CATE_j)}$$

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When is the blocked design more efficient?

Questions?

Baseline Outcome Measure

Recall our key definition:

The observation of units after, and possibly before, a randomly assigned intervention in a controlled setting, which tests one or more precise causal expectations

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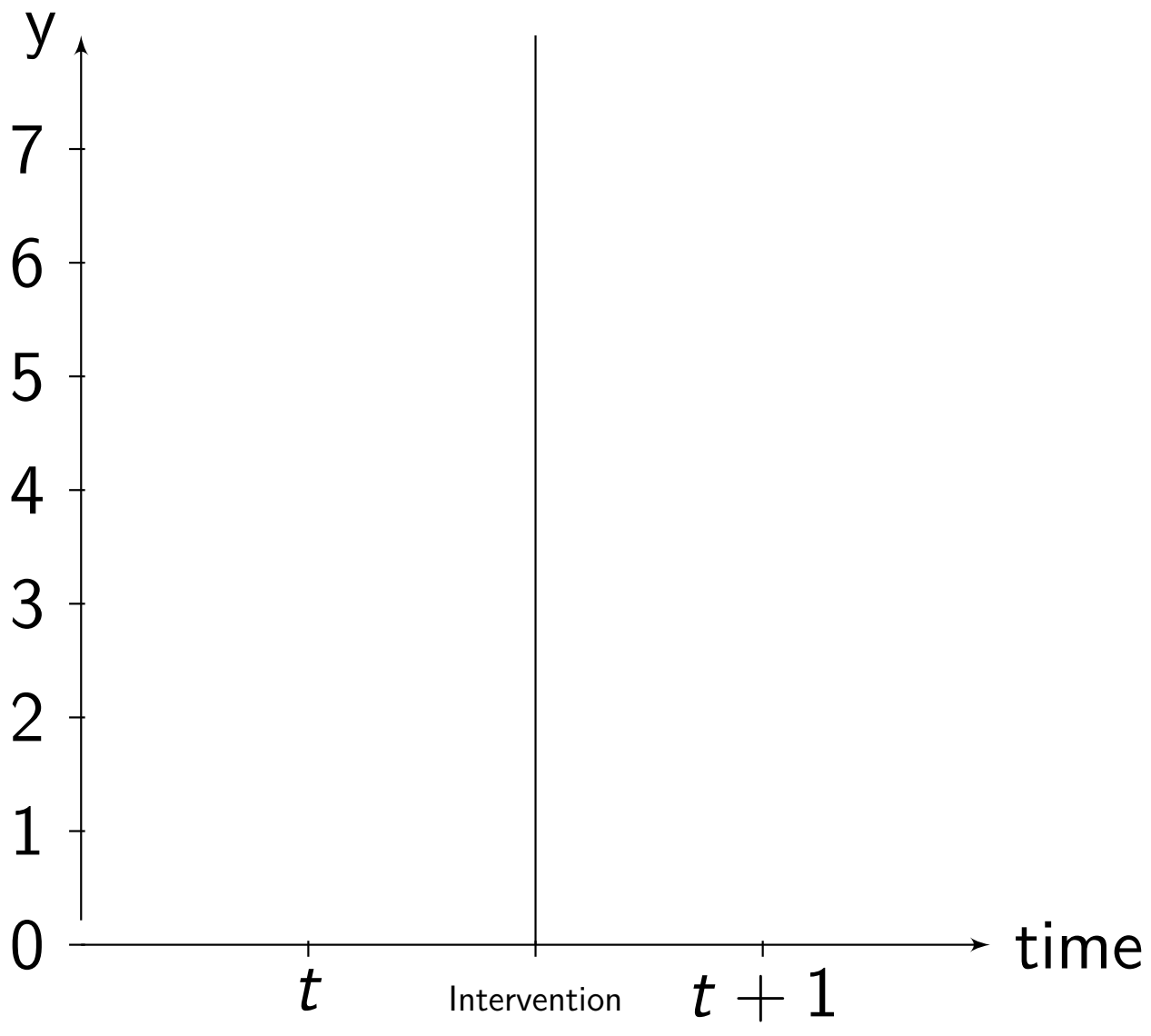
Pretreatment measures of the outcome can be particularly helpful!

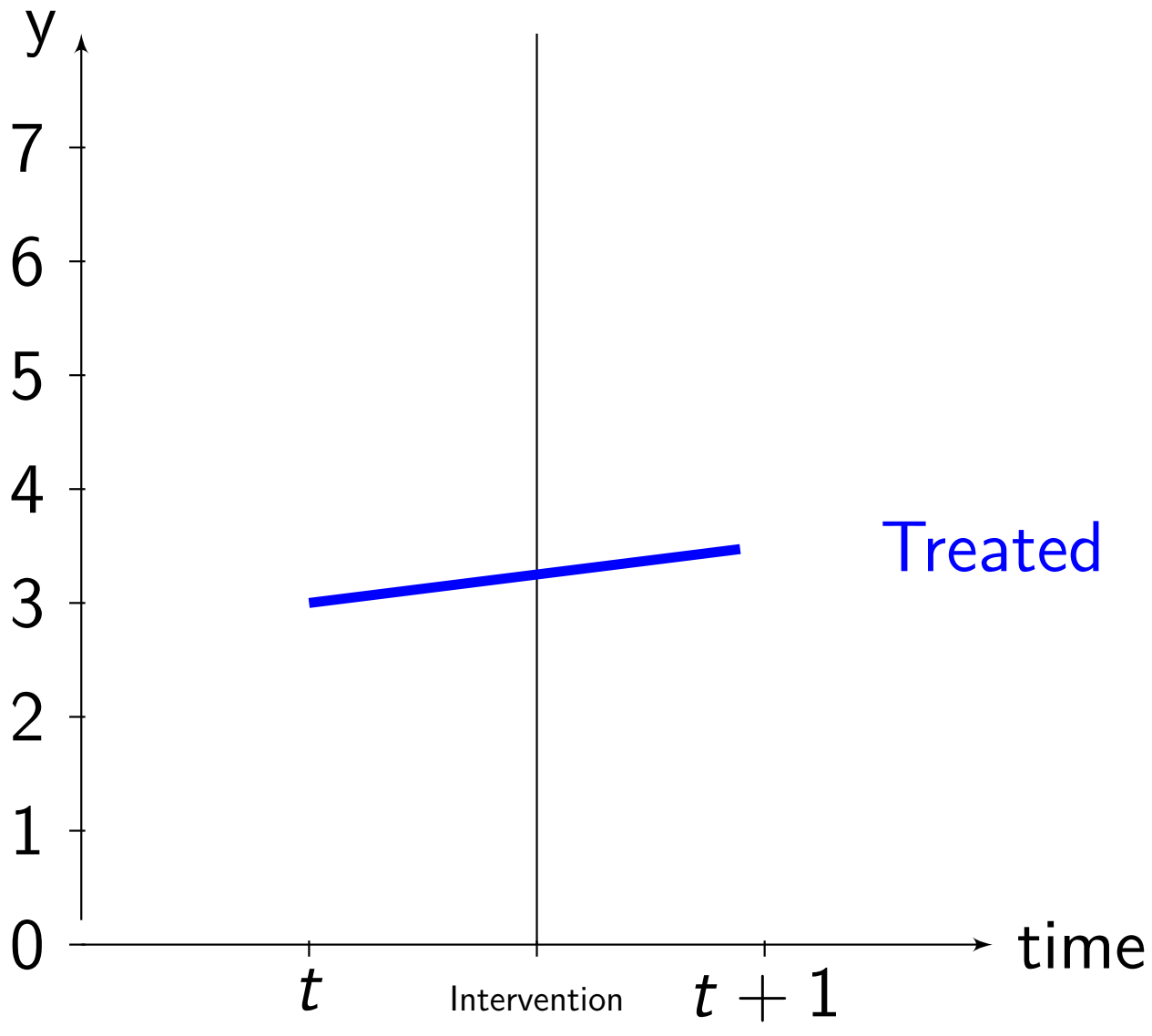
Baseline Outcome Measure

This changes our estimator of *ATE* from simple *mean-difference* to *difference-in-differences* (DID)

$$(\hat{Y}_{0,t+1} - \hat{Y}_{0,t}) - (\hat{Y}_{j,t+1} - \hat{Y}_{j,t})$$

Advantageous because variance for paired samples decreases as correlation between Y_0 and Y_1 increases


y
7
6
5
4
3
2
1
0
time
t
Intervention
$t+1$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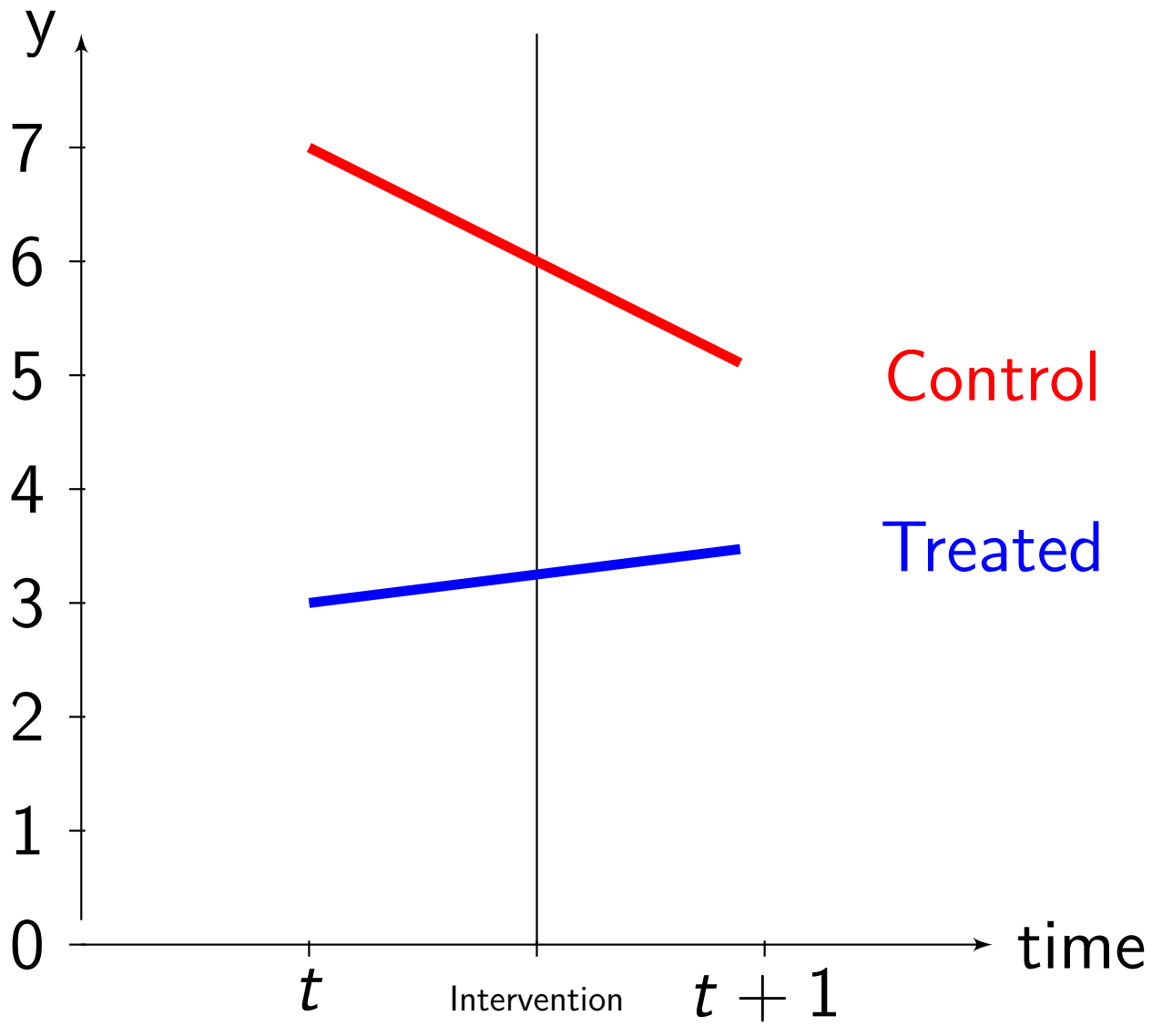
y
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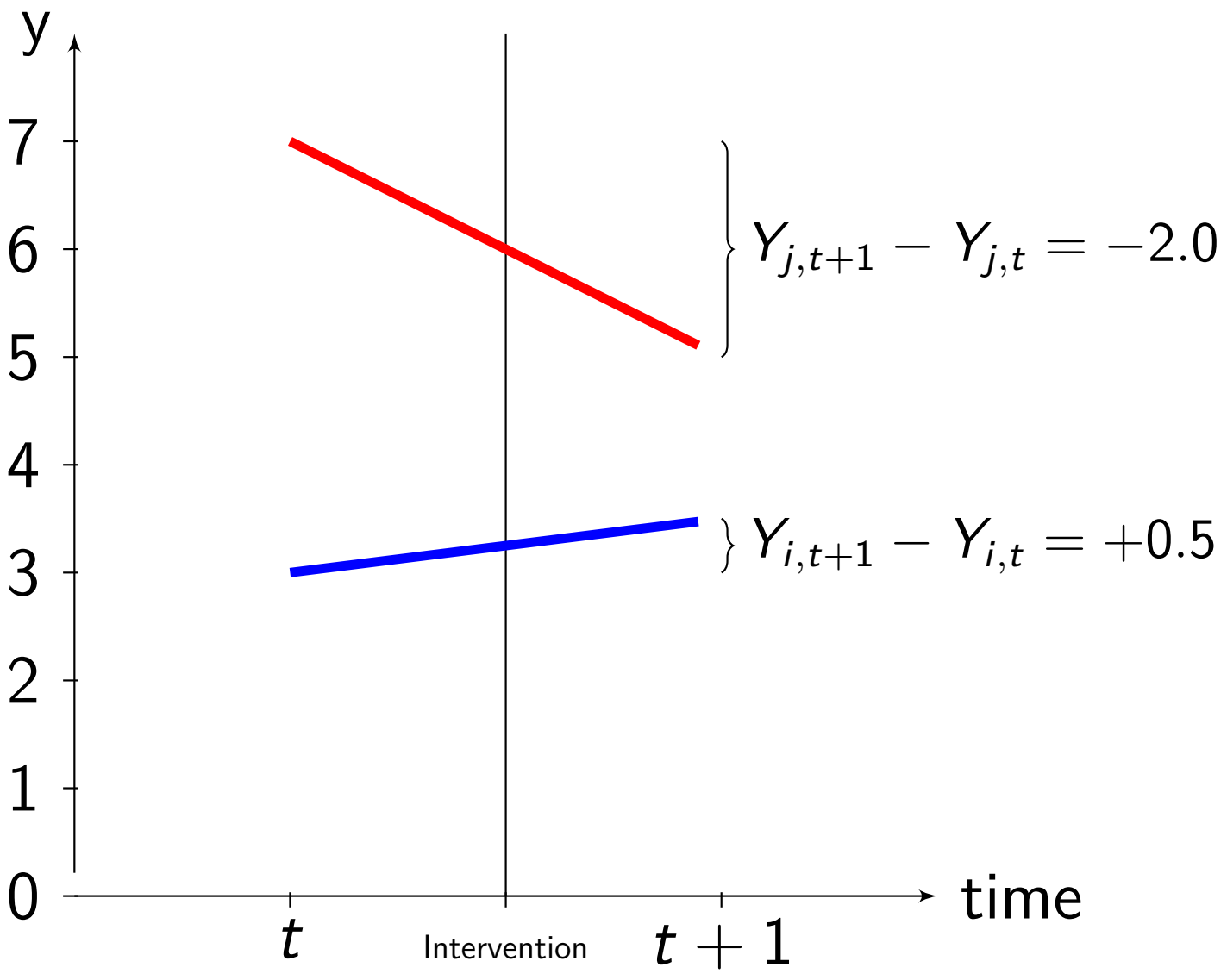
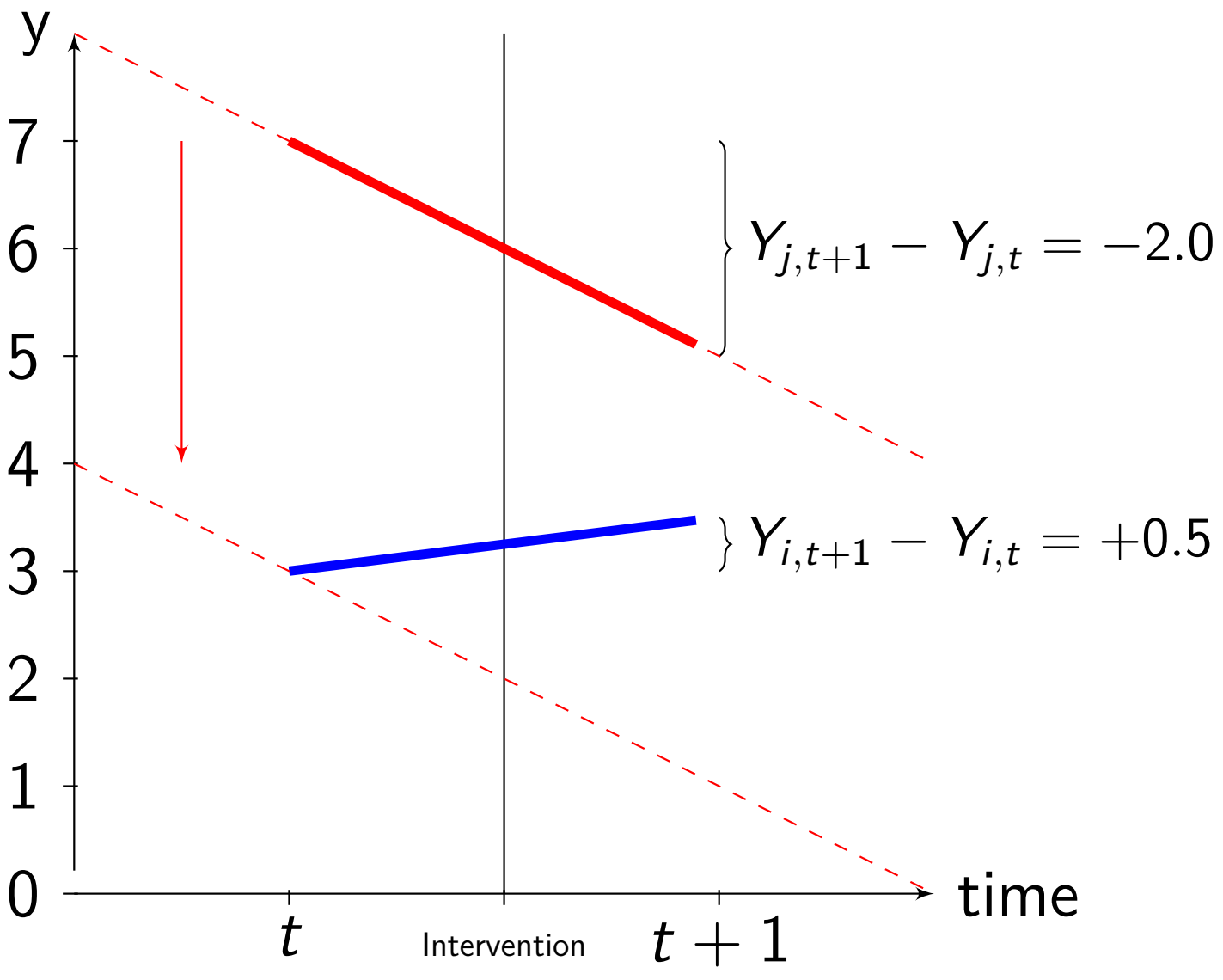
6
5
4
3
2
1
0
Control
Treated
time
t
Intervention
$t+1$


y
7
6
5
4
3
2
1
0
$Y_{j,t+1} - Y_{j,t} = -2.0$
$Y_{i,t+1} - Y_{i,t} = +0.5$
time
t
Intervention
$t + 1$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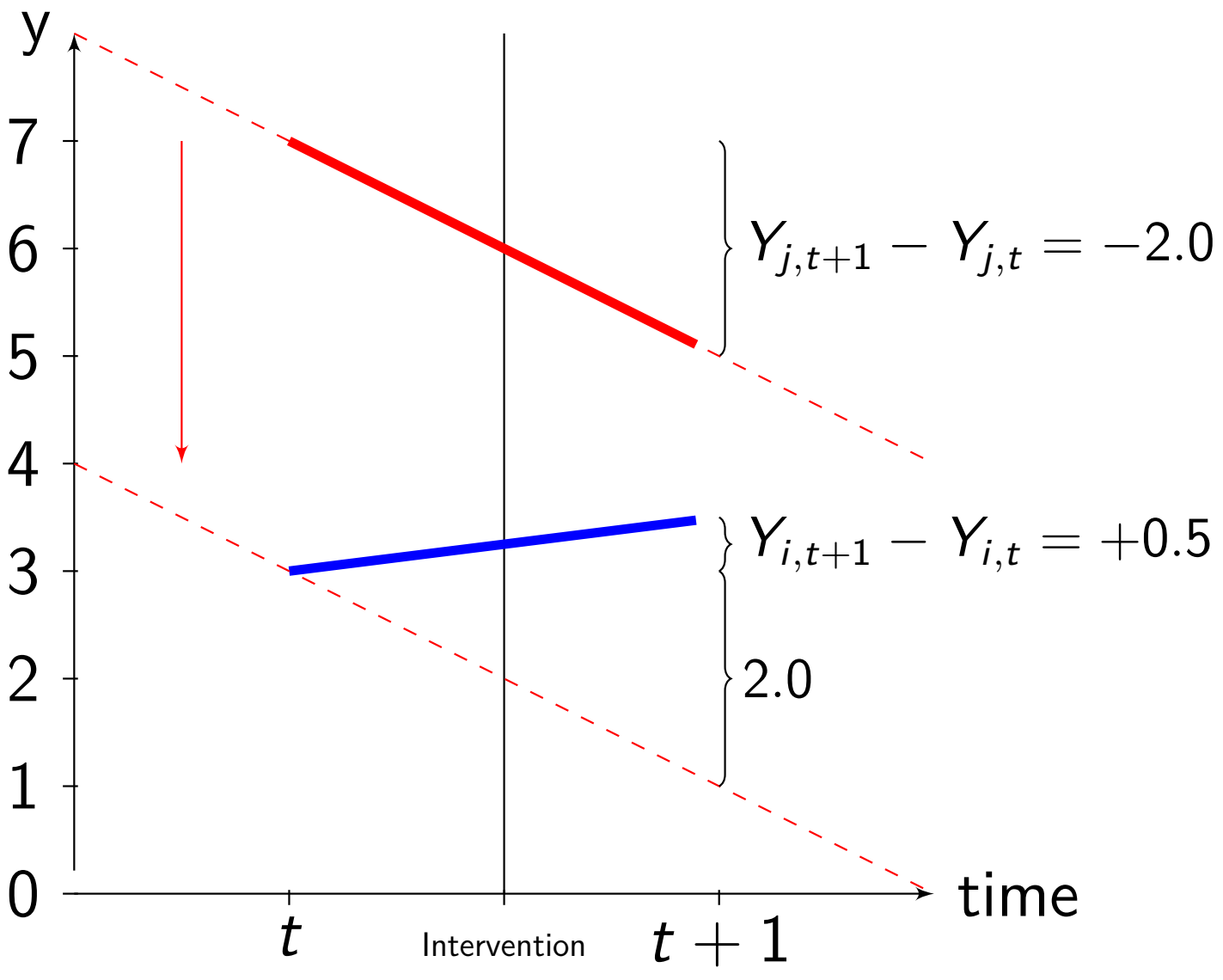
time
t
Intervention
$t + 1$


y
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6
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$Y_{j,t+1} - Y_{j,t} = -2.0$
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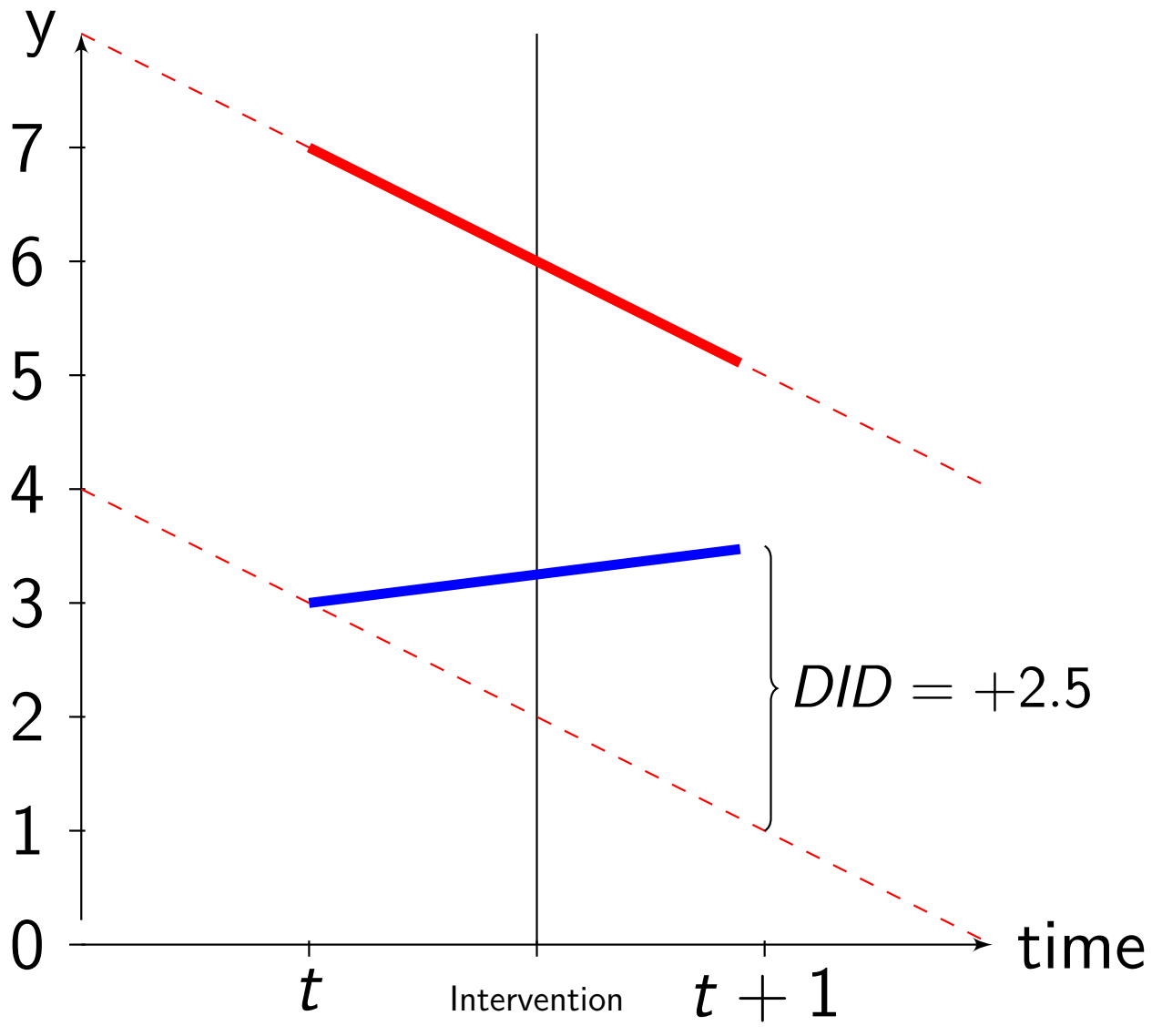
2.0
time
t
Intervention
$t + 1$


y
7
6
5
4
3
2
1
0
$DID = +2.5$
time
t
Intervention
$t + 1$

Statistical Advantages I

In post-treatment-only designs:

$$\widehat{ATE}_{Diff} = \frac{\sum_{i=1}^{n_1}(x_{i,1,t+1})}{n_1} - \frac{\sum_{i=1}^{n_0}(x_{i,0,t+1})}{n_0}$$

The variance of this estimate is:

$$Var(\widehat{ATE}_{Diff}) = Var(\bar{Y}_{1,t+1}) + Var(\bar{Y}_{0,t+1})$$

Statistical Advantages II

In pre/post-treatment designs:

$$\widehat{ATE}_{DID} = \frac{\sum_{i=1}^{n_1}(x_{i,1,t+1} - x_{i,1,t})}{n_1} - \frac{\sum_{i=1}^{n_0}(x_{i,0,t+1} - x_{i,0,t})}{n_0}$$

The variance of this estimate is:

$$\begin{aligned} Var(\widehat{ATE}_{DID}) &= Var(\bar{Y}_{1,t+1} - \bar{Y}_{1,t}) + Var(\bar{Y}_{0,t+1} - \bar{Y}_{0,t}) \\ &= \Big(Var(\bar{Y}_{1,t+1}) + Var(\bar{Y}_{1,t}) - Cov(\bar{Y}_{1,t+1}, \bar{Y}_{1,t})\Big) \\ &+ \Big(Var(\bar{Y}_{0,t+1}) + Var(\bar{Y}_{0,t}) - Cov(\bar{Y}_{0,t+1}, \bar{Y}_{0,t})\Big) \end{aligned}$$

```
# create some fake data
set.seed(54321)
n <- 400L
y0 <- rnorm(n)
x <- rbinom(n, 1L, 0.5)

# high Cor(y0, y1)
y1a <- y0 + 0.25*x + rnorm(n, sd = 0.25)
summary(lm(y1a ~ x))
summary(lm(I(y1a-y0) ~ x))

# low Cor(y0, y1)
y1b <- y0 + 0.25*x + rnorm(n, sd = 2)
summary(lm(y1b ~ x))
summary(lm(I(y1b-y0) ~ x))
```

Practicalities of blocking

Blocked randomization and use of pre-treatment measures only works in some circumstances

Need to observe covariates pre-treatment in order to block on them

- Challenging in a cross-sectional design

The cost of gathering pre-treatment data might also outweigh the gain in precision

- May introduce other biases

Discussion Questions

1. How do we know if an experiment worked (well)? What criteria should we apply for evaluating this?
2. What is a failure of randomization? How do we avoid it? When is randomization simple versus difficult?
3. What is noncompliance? Why does it occur? What consequences does it have for experimental inference?
4. What can we do about noncompliance? Is it a problem that can be solved?
5. Is experimentation ethical? Why or why not?

Randomization Failure

Randomization Failure

The process of physical randomization was distorted such that assignment was not random.

Imbalance

Physical randomization was used but the treatment groups were not comparable on covariates.

Noncompliance

Compliance is when individuals receive and accept the treatment to which they are assigned

Noncompliance:

"when subjects who were assigned to receive the treatment go untreated or when subjects assigned to the control group are treated" [1]

[1] Gerber & Green. 2012. *Field Experiments*, p.132.

Why do we care?

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Factors other than randomization explain why individuals receive their treatment

Why do we care?

Factors other than randomization explain why individuals receive their treatment

Reduced power

Why do we care?

Factors other than randomization explain why individuals receive their treatment

Reduced power

Typically a downwardly biased estimate of SATE

Types of noncompliance

Asymmetric

- Some assignment to treatment A take treatment B instead

Symmetric

- Some assignment to treatment A take treatment B instead, and vice versa

What can we do?

Intention-to-treatment (ITT) estimation

As-treated analysis

Something else

Intention-to-treat

Ignore the treatment actually received

Compared units assigned to each treatment condition:
$ITT = \overline{Y}_1 - \overline{Y}_0$

Tends to underestimate effect of treatment

As-Treated Analysis

Ignore treatment assignment and analyse as an observational study

Moves us out of experimental territory

Comparison has no causal interpretation

Something else

Noncompliance *asymmetric* if only in one group

We can use a "Wald estimator" to estimate the "local average treatment effect" (LATE):

$$LATE = \frac{ITT}{\%Compliant}$$

Effect is *local* to those whose treatment status could be manipulated by treatment assignment

Two-Sided Noncompliance

Two-sided noncompliance is more complex analytically

Stronger assumptions are required to analyse it

Best to try to develop a better design to avoid this rather than try to deal with the complexities of analyzing a broken design

Local Average Treatment Effect

IV estimate is *local* to the variation in X that is due to variation in D

LATE is effect for those who *comply*

Four subpopulations:

Compliers: $X = 1$ only if $D = 1$
Always-takers: $X = 1$ regardless of D
Never-takers: $X = 0$ regardless of D
Defiers: $X = 1$ only if $D = 0$

Exclusion restriction! Monotonicity!

Attrition and Nonresponse

Attrition: Loss of units from the study sample

Nonresponse: Missing outcome data for one or more units

Spillover/Contamination

Some units assigned to Treatment A are exposed (directly or indirectly) to Treatment B

Common examples

- Within-household spillover
- Geographical randomization

Really difficult to correct for analytically