# Session IV
# Practical Issues

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# Beyond One-shot Designs

- Surveys can be used as a measurement instrument for a field treatment or a manipulation applied in a different survey panel wave
  1. Measure effect duration in two-wave panel
  2. Solicit pre-treatment outcome measures in a two-wave panel
  3. Measure effects of field treatment in post-test only design
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  4. Randomly encourage field treatment in pre-test and measure effects in post-test
- Problems? Compliance & nonresponse

# I. Effect Duration

- Use a two- (or more-) wave panel to measure duration of effects
  - T1: Treatment and outcome measurement
  - T2+: Outcome measurement
- Two main concerns
  - Attrition
  - Panel conditioning

# II. Within-Subjects Designs

- Estimate treatment effects as a difference-in-differences
- Instead of using the post-treatment mean-difference in $Y$ to estimate the causal effect, use the difference in pre-post differences for the two groups:

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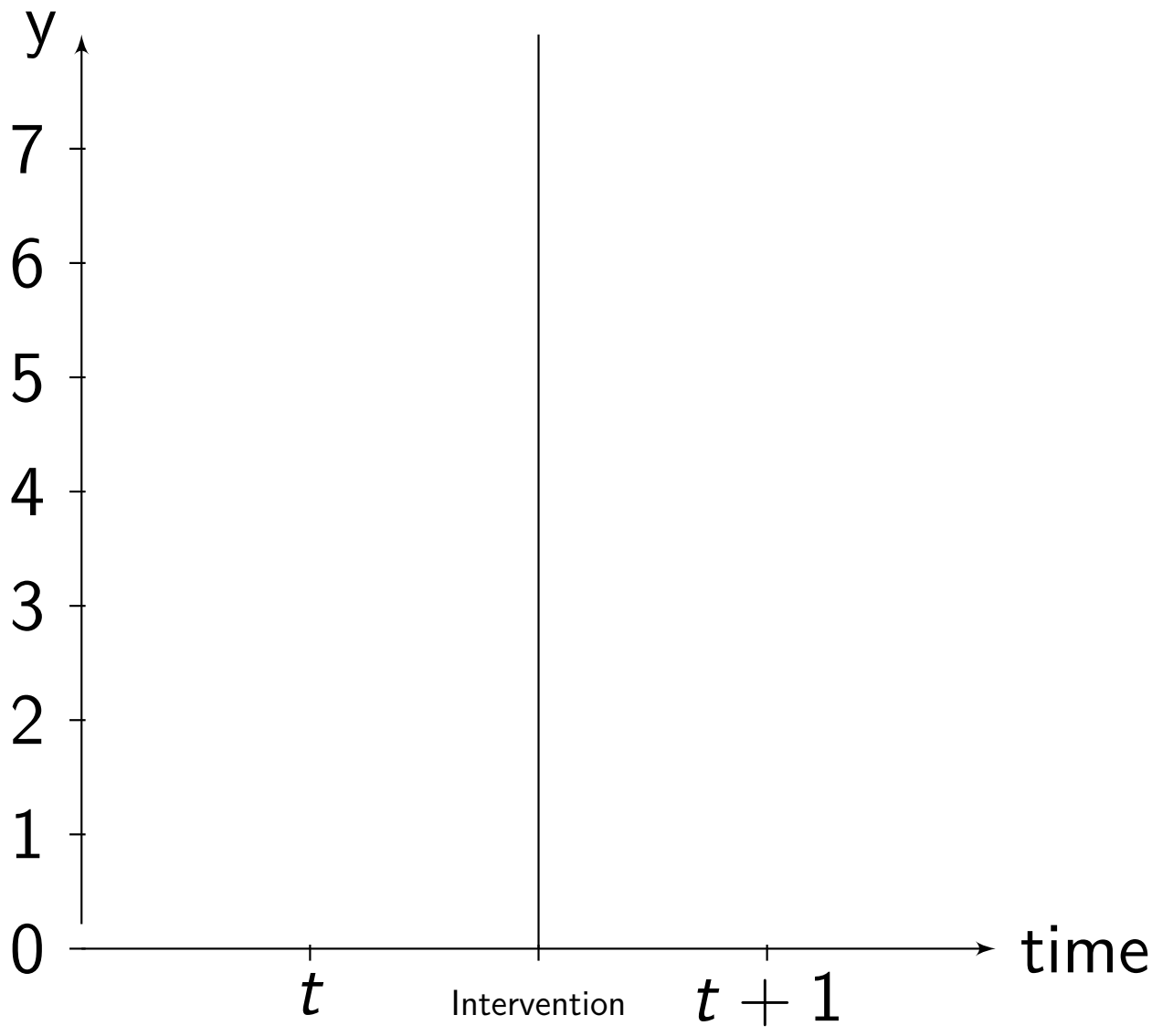
- Advantageous because variance for paired samples decreases as correlation between $t_0$ and $t_1$ observations increases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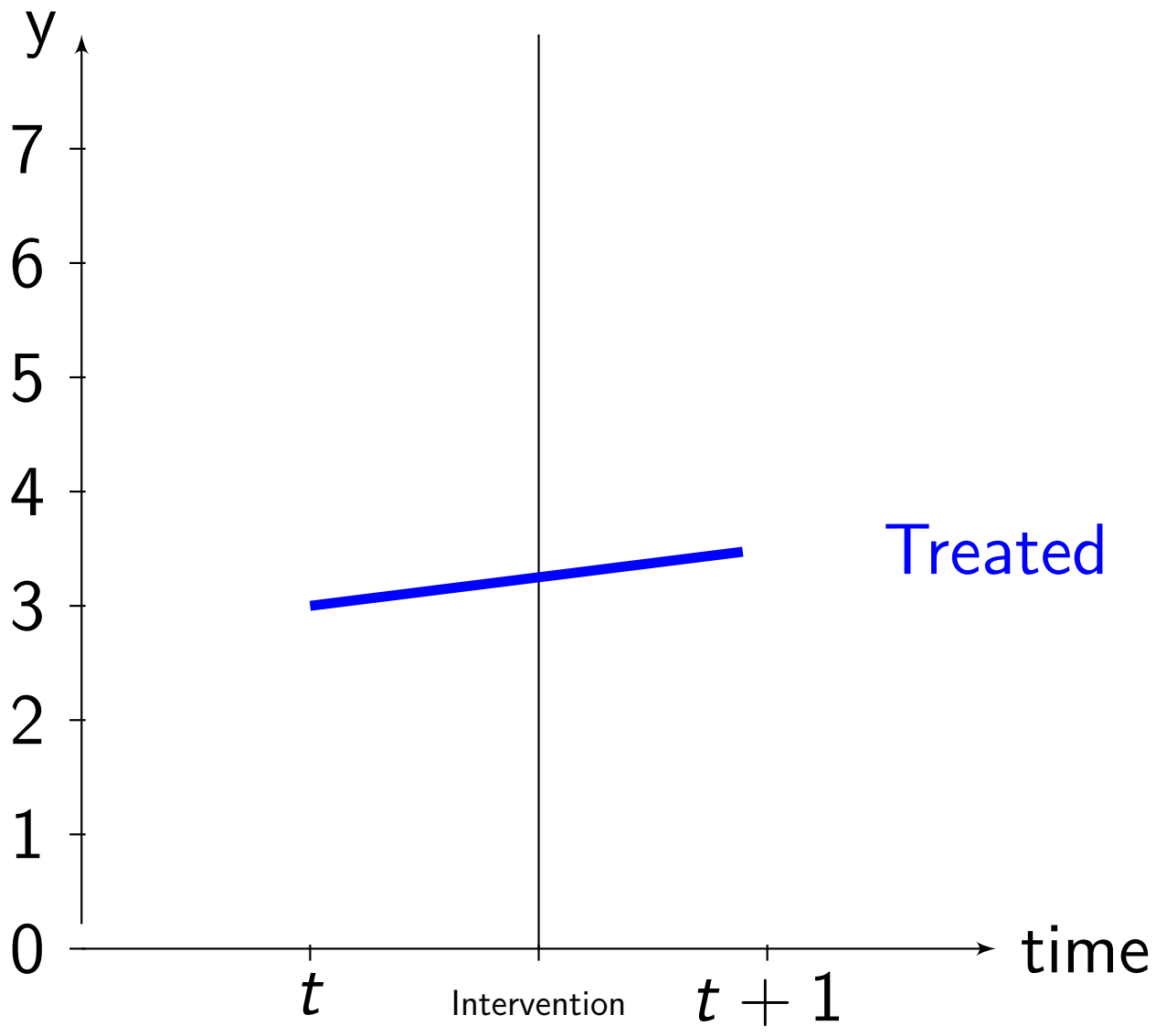
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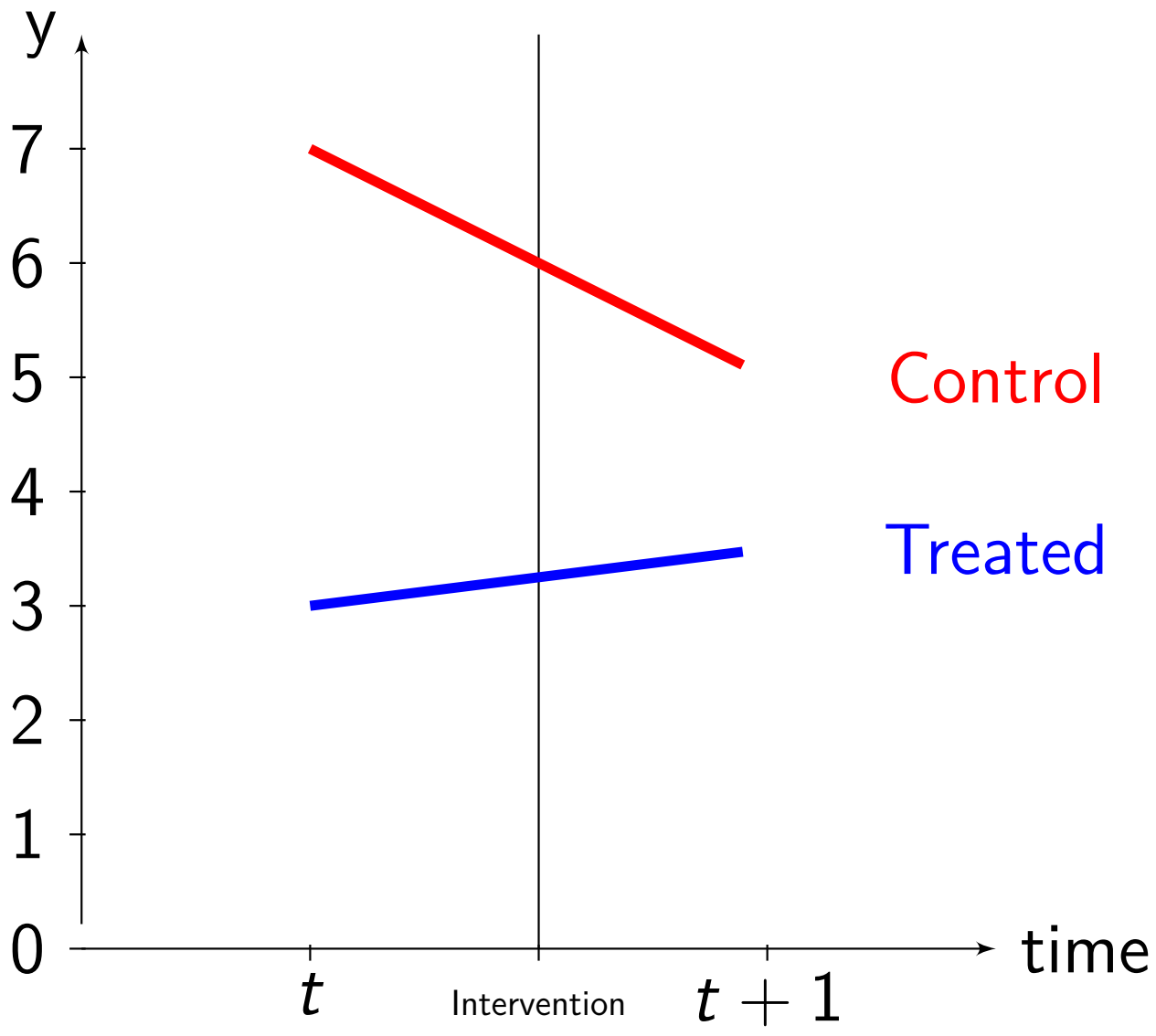
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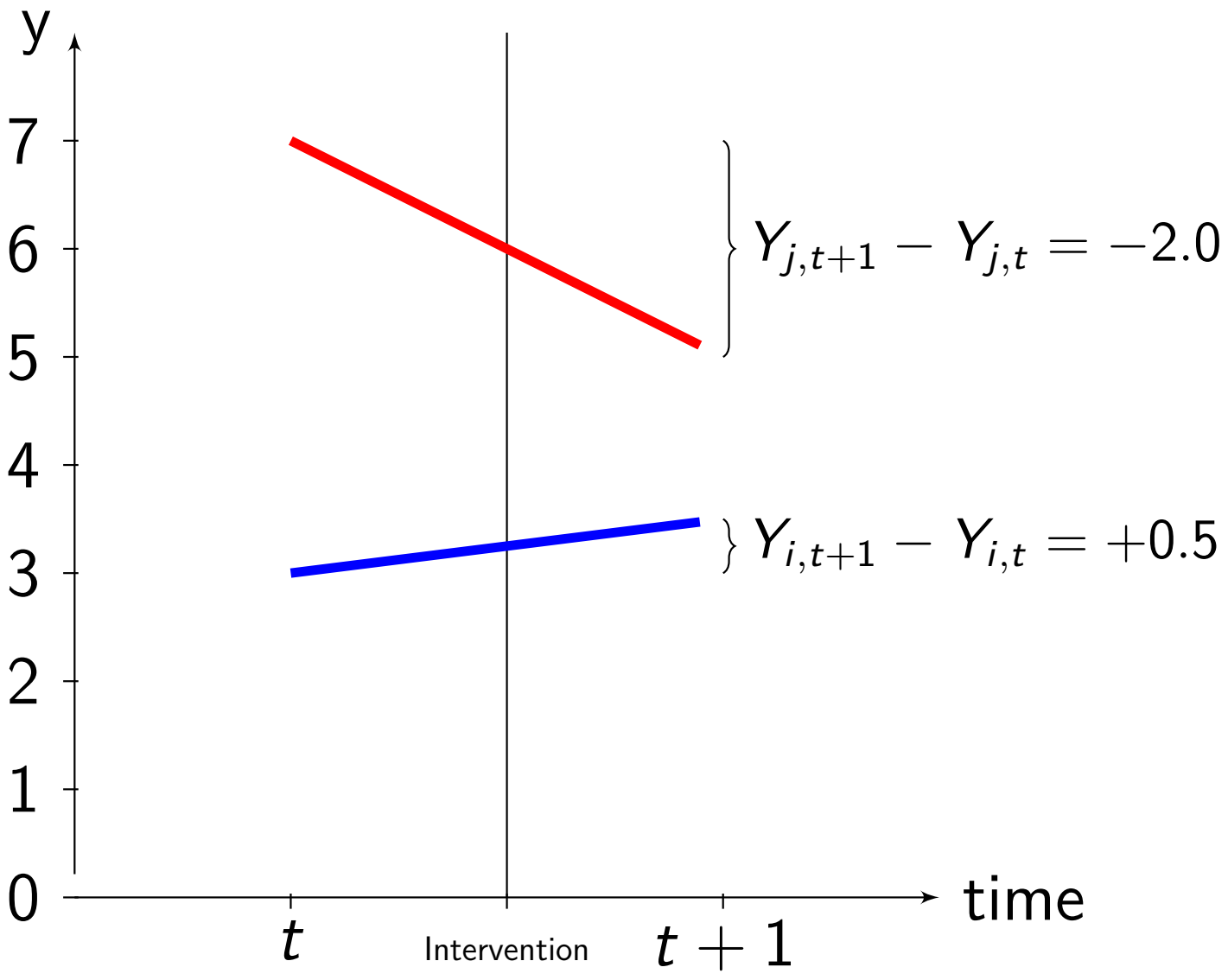
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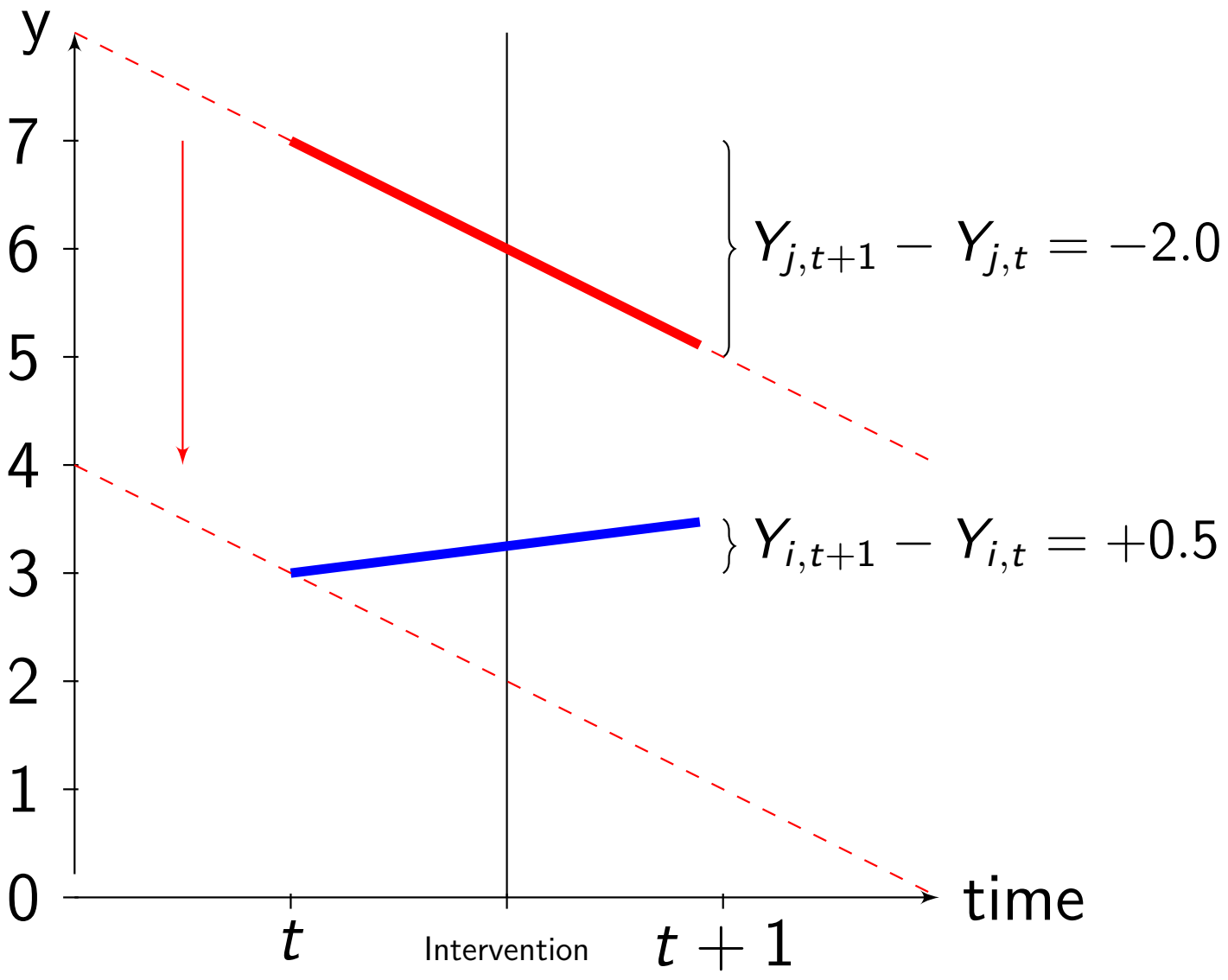
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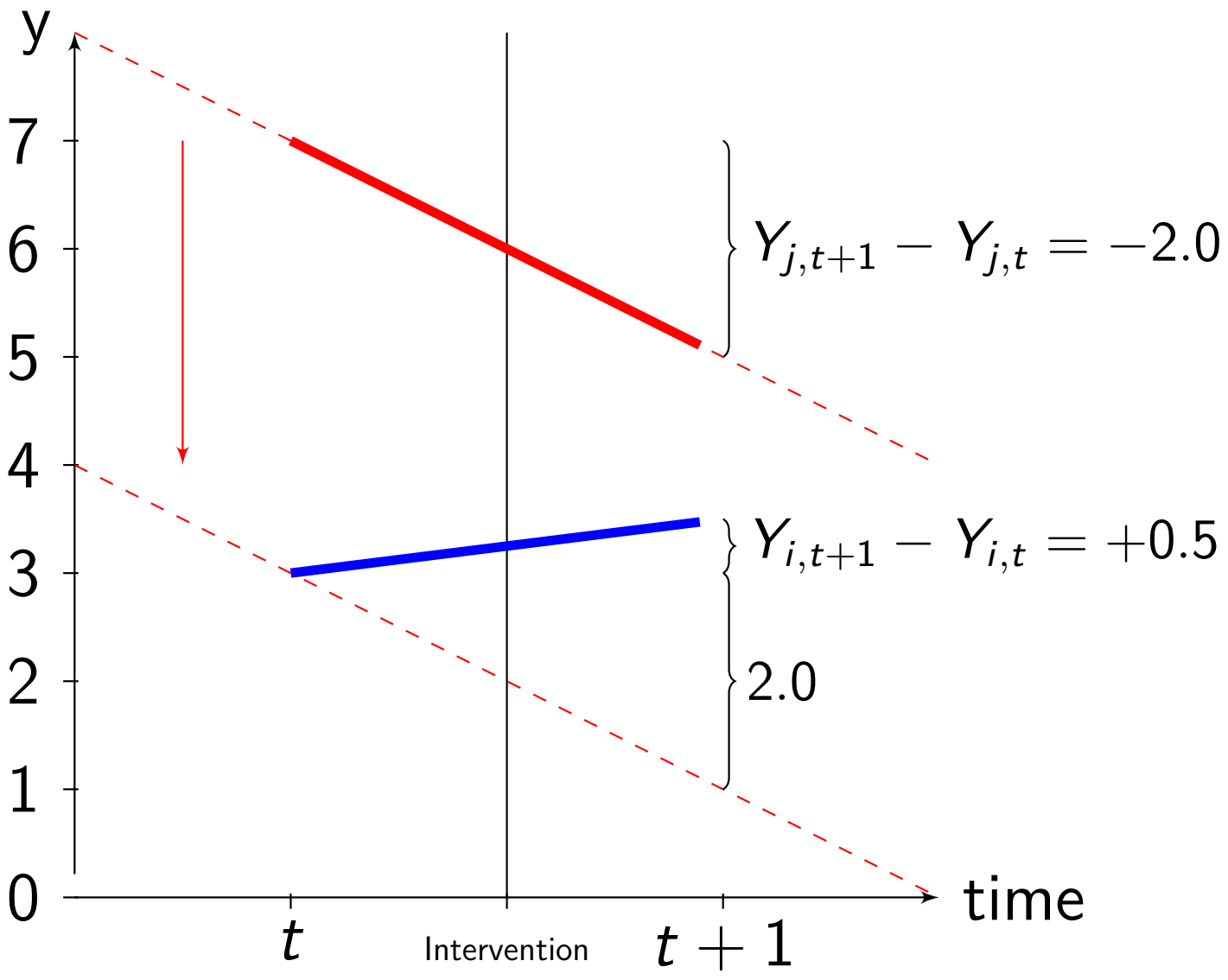
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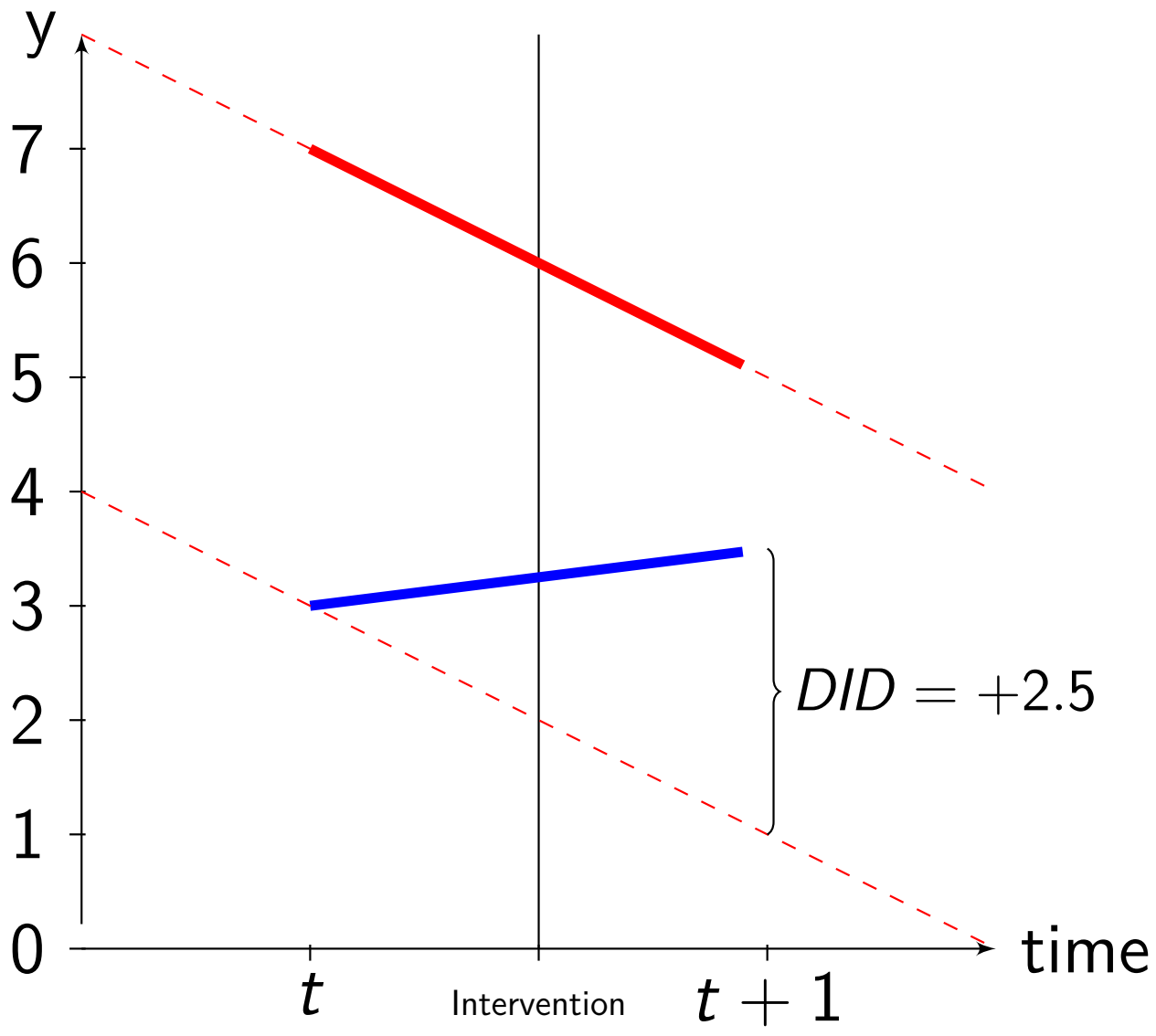
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- Issues
  - Nonresponse
  - Noncompliance

# 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" [2]
- This causes problems for our analysis because factors other than randomization explain why individuals receive their treatment
- Lots of methods for dealing with this, but the consequence is generally reduced power

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

# Asymmetric Noncompliance

- Noncompliance *asymmetric* if only in one group
- We can ignore non-compliance and analyze the "intention to treat" effect, which will underestimate our effects because some people were not treated as assigned
  $ITT = \overline{Y}_1 - \overline{Y}_0$
- We can use "instrumental variables" to estimate the "local average treatment effect" (LATE) for those that complied with treatment:
  $LATE = \frac{ITT}{PercentCompliant}$
- We can ignore randomization and analyze data "as-treated", but this makes our study no longer an experiment

# 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!

# Two-Sided Noncompliance

- Two-sided noncompliance is more complex analytically
- Stronger assumptions are required to analyze it and we won't discus them here
- Best to try to develop a better design to avoid this rather than try to deal with the complexities of analyzing a broken design

# IV. Treatment Encouragement

- Design:
  - T1: Encourage treatment
  - T2: Measure effects
- Examples:
  1. Albertson and Lawrence[3]

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# Treatment Noncompliance

# Treatment Noncompliance

- Several strategies
  - "As treated" analysis
  - "Intention to treat" analysis
  - Estimate a LATE

# Questions?

# Quiz time!

# Compliance

1. What is compliance?

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2. How can we analyze experimental data when there is noncompliance?

# Balance testing

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2. What can we do if we find a covariate imbalance between groups?
3. How can we avoid this problem entirely?

# Nonresponse and Attrition

1. Do we care about outcome nonresponse in experiments?

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2. How can we analyze experimental data when there is outcome nonresponse or post-treatment attrition?

# Manipulation checks

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2. What do we do if some respondents "fail" a manipulation check?

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2. What does it mean for an experiment to be *underpowered*?
3. What can we do to reduce the probability of obtaining an (unwanted) "null effect"?

# Effect heterogeneity

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2. What can we do pre-implementation to address possible heterogeneity?

# Representativeness

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2. What kind of causal inferences can we draw from an experiment on a descriptively unrepresentative sample?

# Peer Review

1. What should we do if a peer reviewer asks us to "control" for covariates in the analysis?

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2. What should we do if a peer reviewer asks us to include or exclude particular respondents from the analysis?

# Questions?

# History: Key Moments

1. Tuskegee (1932-1972) and Guatemala (1946-1948) Studies
2. Nuremberg Code (1947)
3. Helsinki Declaration (1964)
4. U.S. 45 CFR 46 (1974) and "Common Rule" (1991)
5. The Belmont Report (1979)
6. EU Data Protection Directive (1995; 2012)
    - UK Data Protection Act (1998)

# Helsinki Declaration

- Adopted by the World Medical Association in 1964[4]
- Narrowly focused on medical research
- Expanded the Nuremberg Code
  - Relaxed consent requirements
  - Risks should not exceed benefits
  - Institutionalization of ethics oversight

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- Do these rules apply to non-medical research?

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# The Belmont Report

- Commissioned by the U.S. Government in 1979[5]
- Three overarching principles:
  1. Respect for persons
  2. Beneficence
  3. Justice
- Three policy implications:
  - Informed consent
  - Assessment of risks/benefits
  - Care for vulnerable populations

[5] http://www.hhs.gov/ohrp/humansubjects/guidance/belmont.html

# Benefits and Harm

- What is a “benefit”?
- What is a “harm”?
- How do we balance the two?

# Ethical Considerations

- Most ethical issues are not unique to *experimental* social science
- Some especially important issues:
  1. Randomization
  2. Informed consent
  3. Privacy
  4. Deception
  5. Publication bias

# I. Randomization

- Is it ethical to randomize?

# II. Informed Consent

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  - What is consent?
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  - What exactly do they have to consent to?
- Cross-national variations
  - Consent forms required in U.S.
  - Not required in UK

# III. Privacy

- Under EU Data Protection Directive (1995), data can be processed when:
  - Consent is given
  - Data are used for a "legitimate" purpose
  - Anonymous or confidential
- Data cannot leave the EU except under conditions

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- Experimental might be additionally sensitive
- Answers reflect "manipulated" attitudes, behaviors, perceptions, etc. that respondents may not have given in another setting

# IV. Deception

- Major distinction between psychology tradition and economics tradition[6]
  - Purpose of the study
  - Purpose of specific items or tasks
  - Order or length of questionnaire

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  - Omission: In a multi-round trust game, an additional round is added
  - Commission: Telling respondents it is a dictator game, but it is actually a trust game

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- If studies are meant to policy or practical implications, then we care about PATE or a set of CATEs, including whether their effects are positive, negative, or zero.
- Publication bias (toward "significant" results) invites wasting resources on treatments that actually don't work

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1. Explain how to analyze experiments quantitatively.
2. Explain how to design experiments that speak to relevant research questions and theories.
3. Evaluate the uses and limitations of several common survey experimental paradigms.
4. Identify practical issues that arise in the implementation of experiments and evaluate how to anticipate and respond to them.

# Wrap-up

- Thanks to all of you!
- Stay in touch (t.leeper@lse.ac.uk)
- Good luck with your research!