RDD: Regression Discontinuity Analysis

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Quasi-Experimental Designs to Discuss

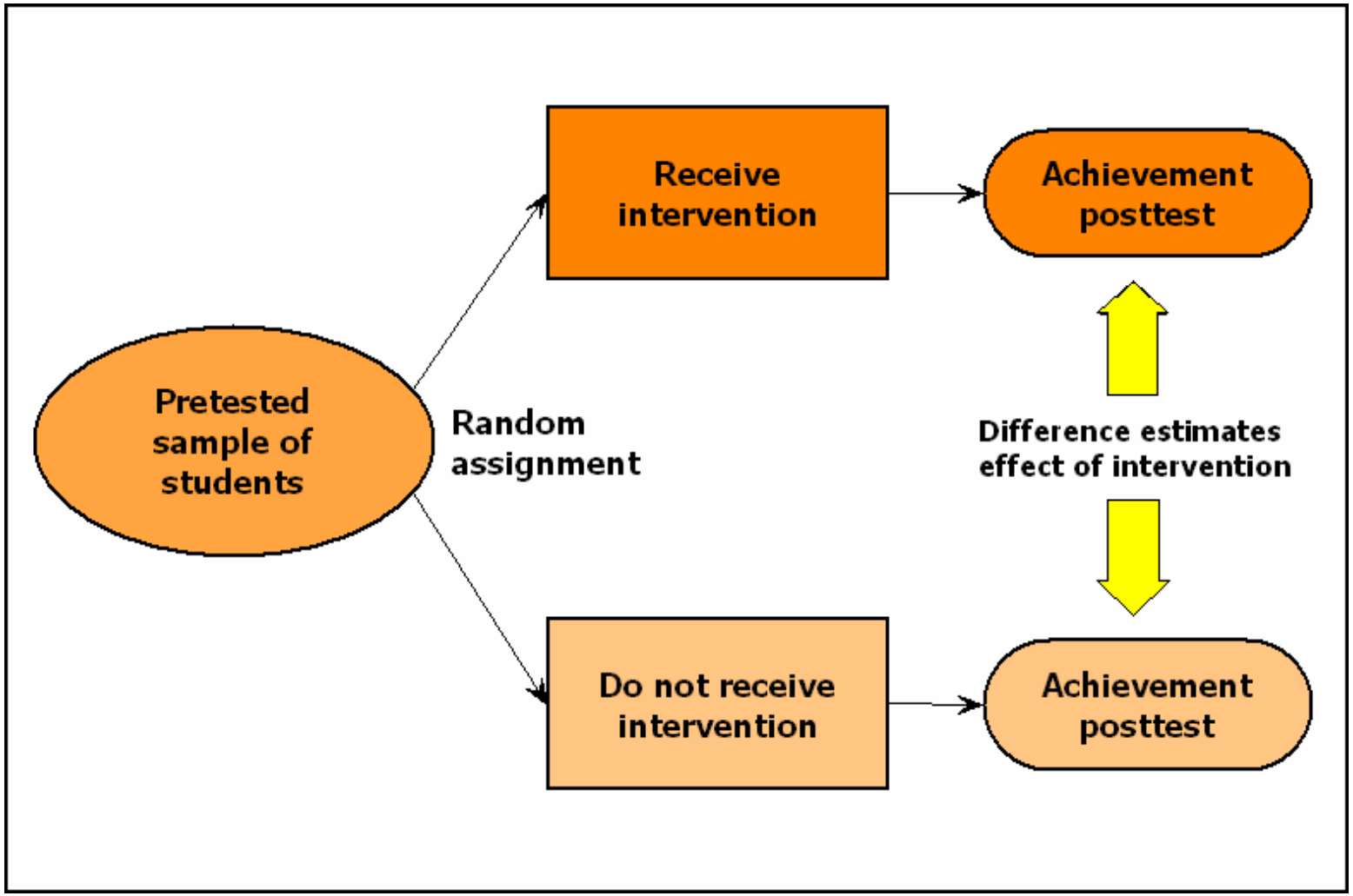
- Regression discontinuity
- Nonrandomized comparison groups with statistical controls
  - Analysis of covariance
  - Matching
  - Propensity scores

The Regression-Discontinuity (R-D) Design

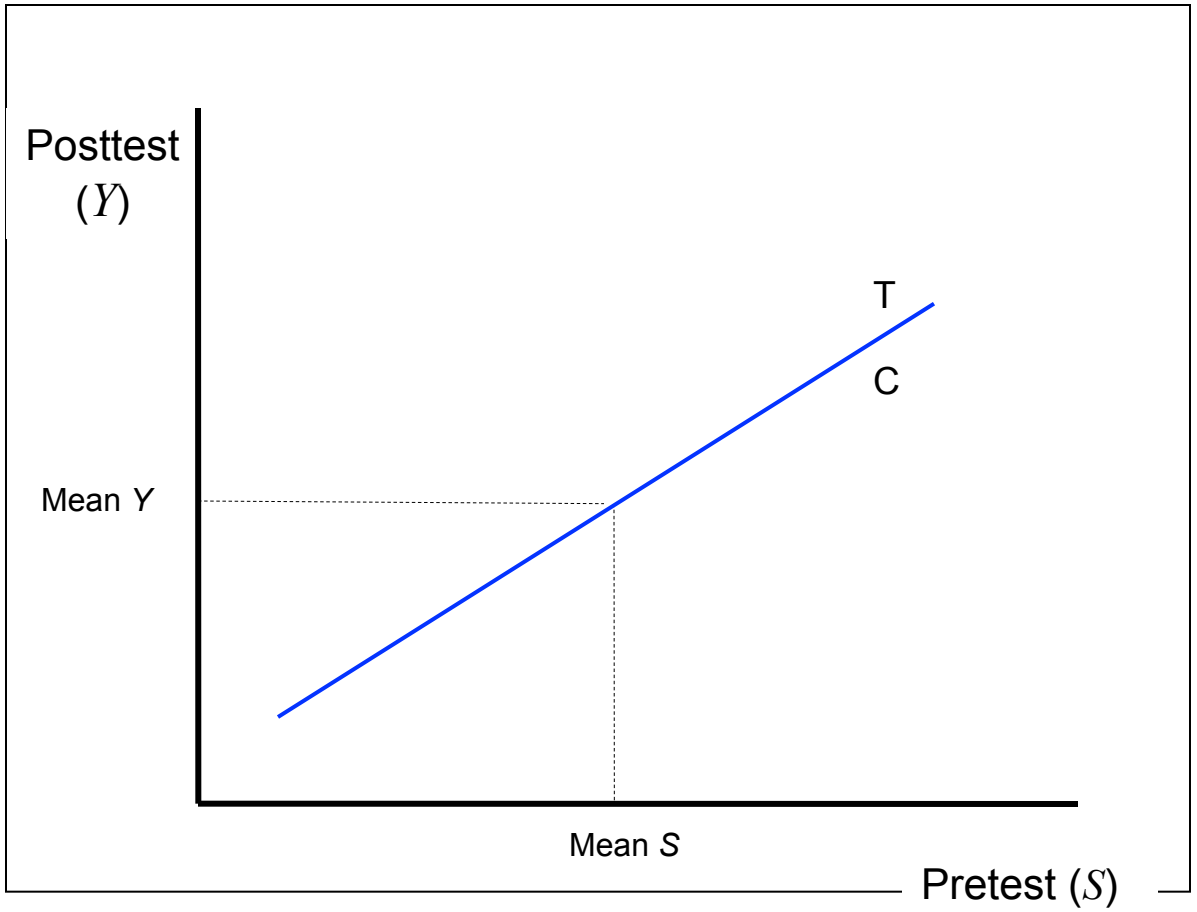
Advantages of the R-D design?

- When well-executed, its internal validity is strong– comparable to a randomized experiment.
- It is adaptable to many circumstances where it may be difficult to apply a randomized design.

Think first of a pretest-posttest randomized field experiment



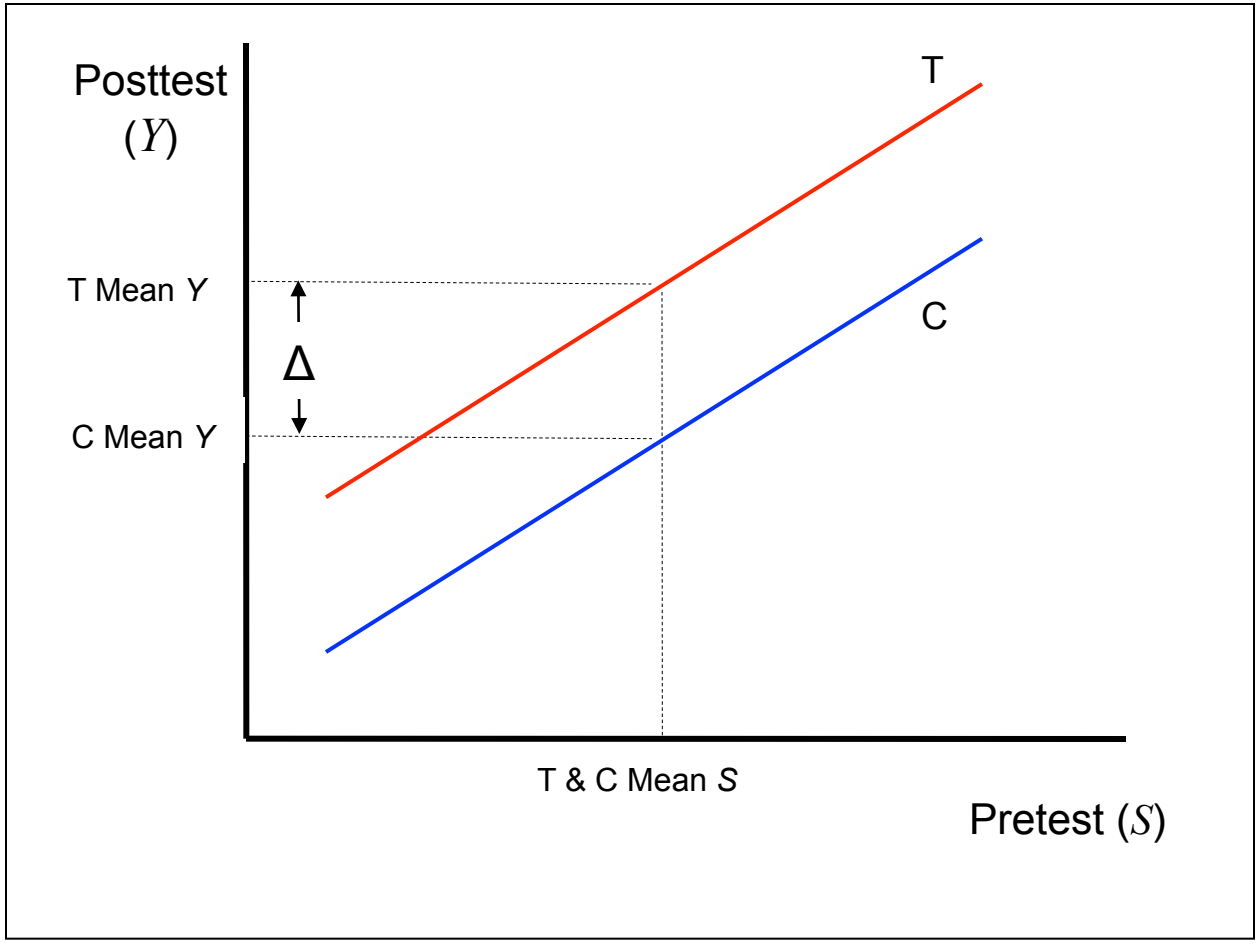
Posttest on pretest regression for randomized experiment (with no treatment effect)



Corresponding regression equation (*T*: 1=treatment, 0=control)

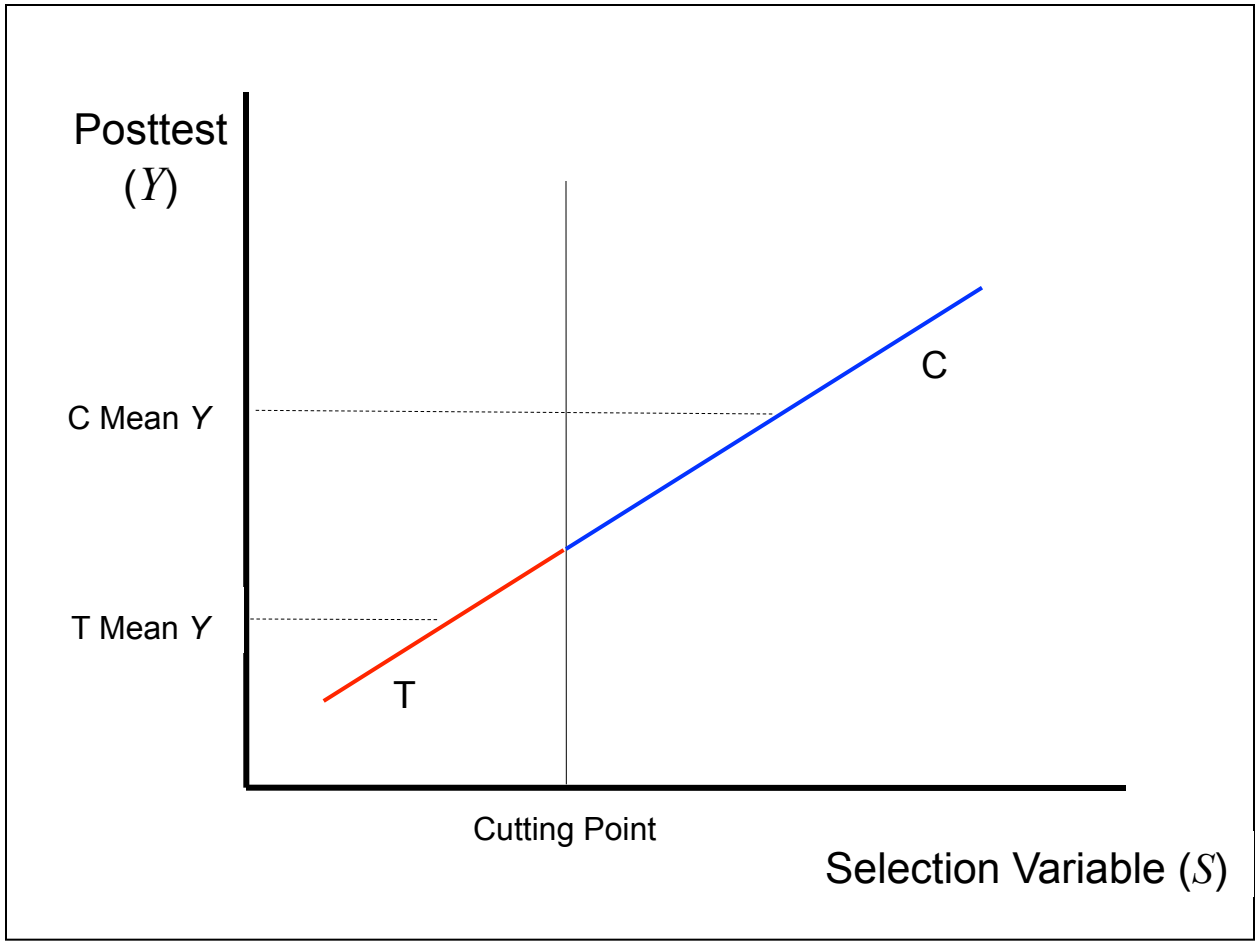
$$Y_i = B_0 + B_S S_i + B_T T_i + e_i$$

Pretest-posttest randomized experiment (with treatment effect)



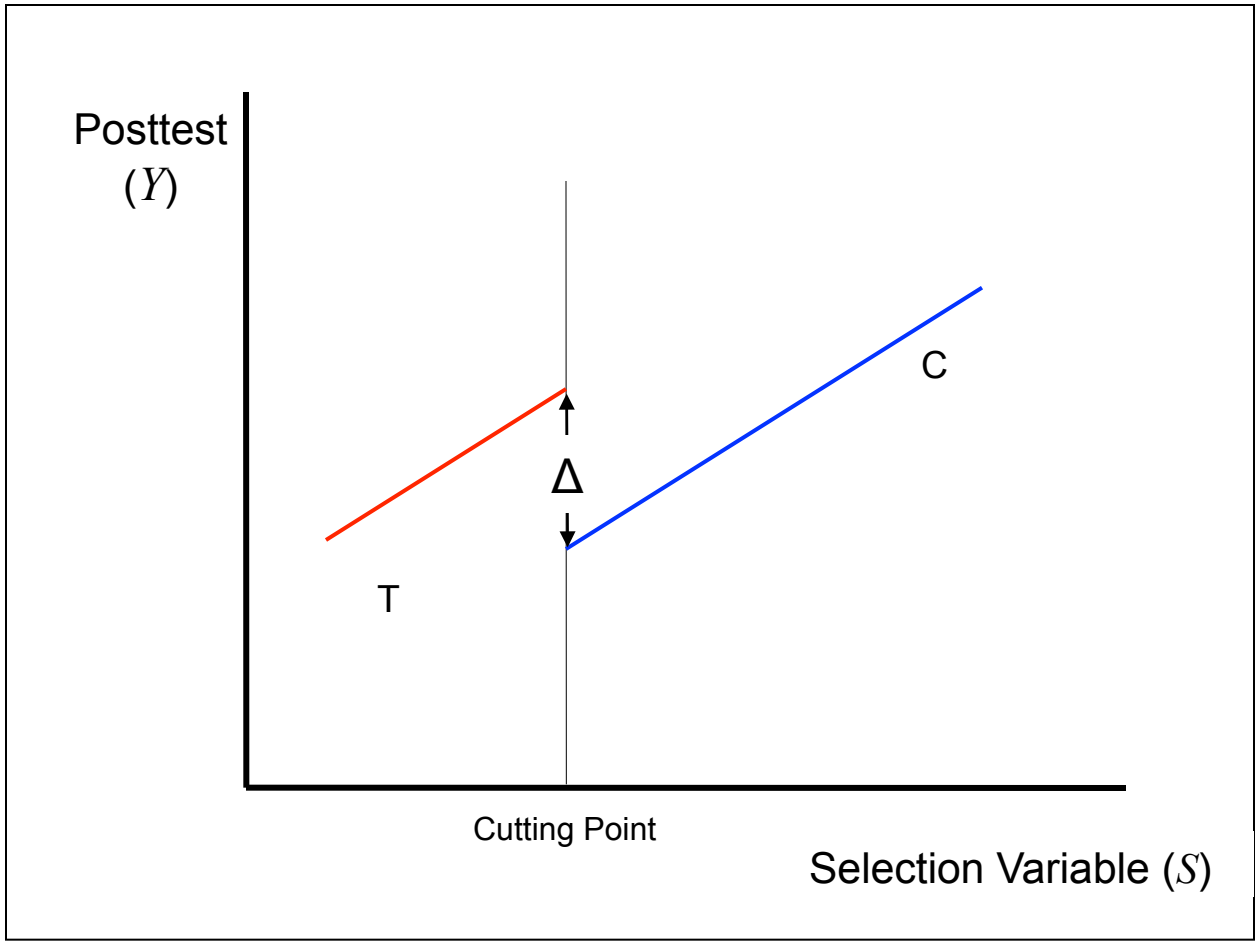
$$Y_i = B_0 + B_S S_i + B_T T_i + e_i$$

Regression discontinuity (with no treatment effect)



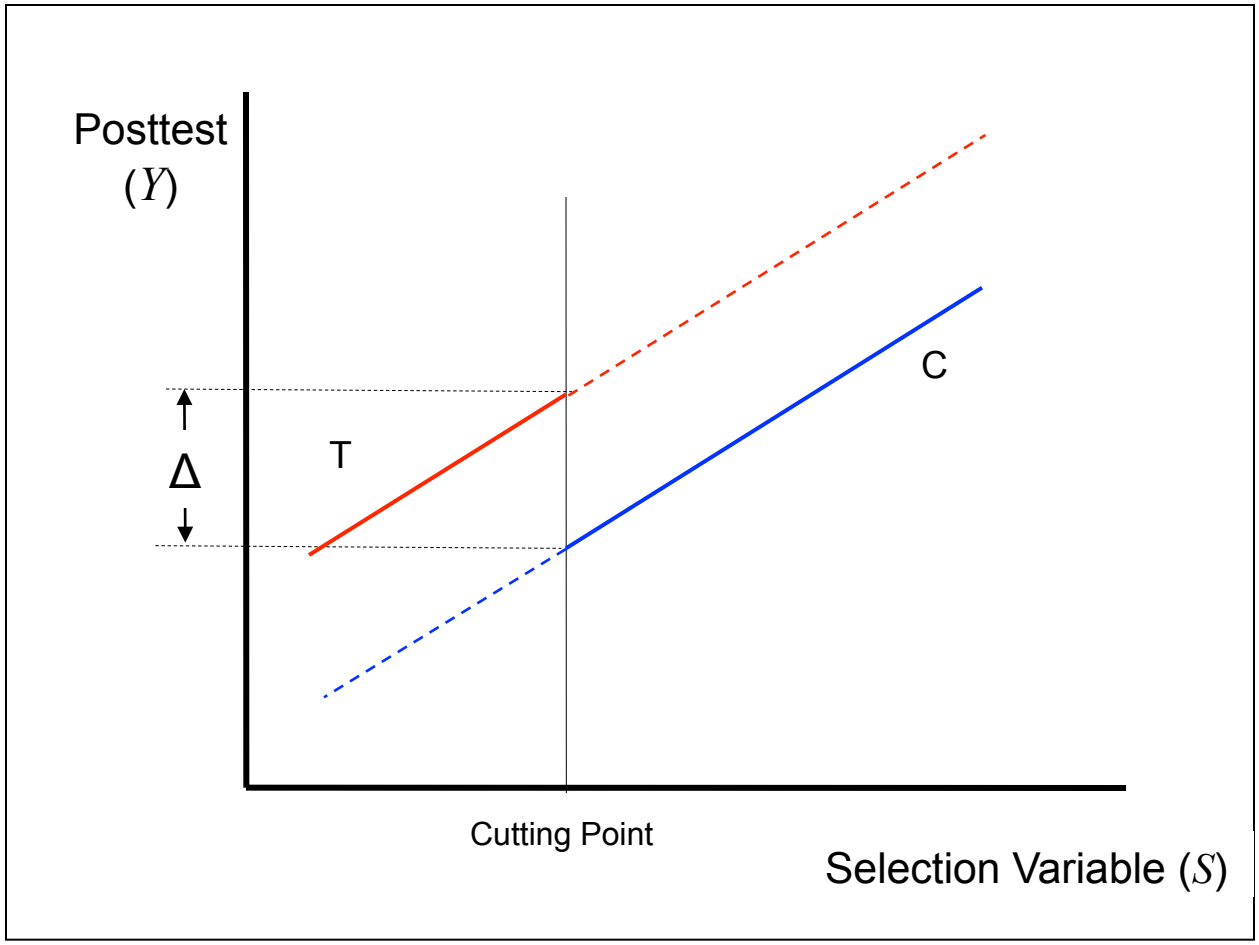
$$Y_i = B_0 + B_S S_i + B_T T_i + e_i$$

Regression discontinuity (with treatment effect)



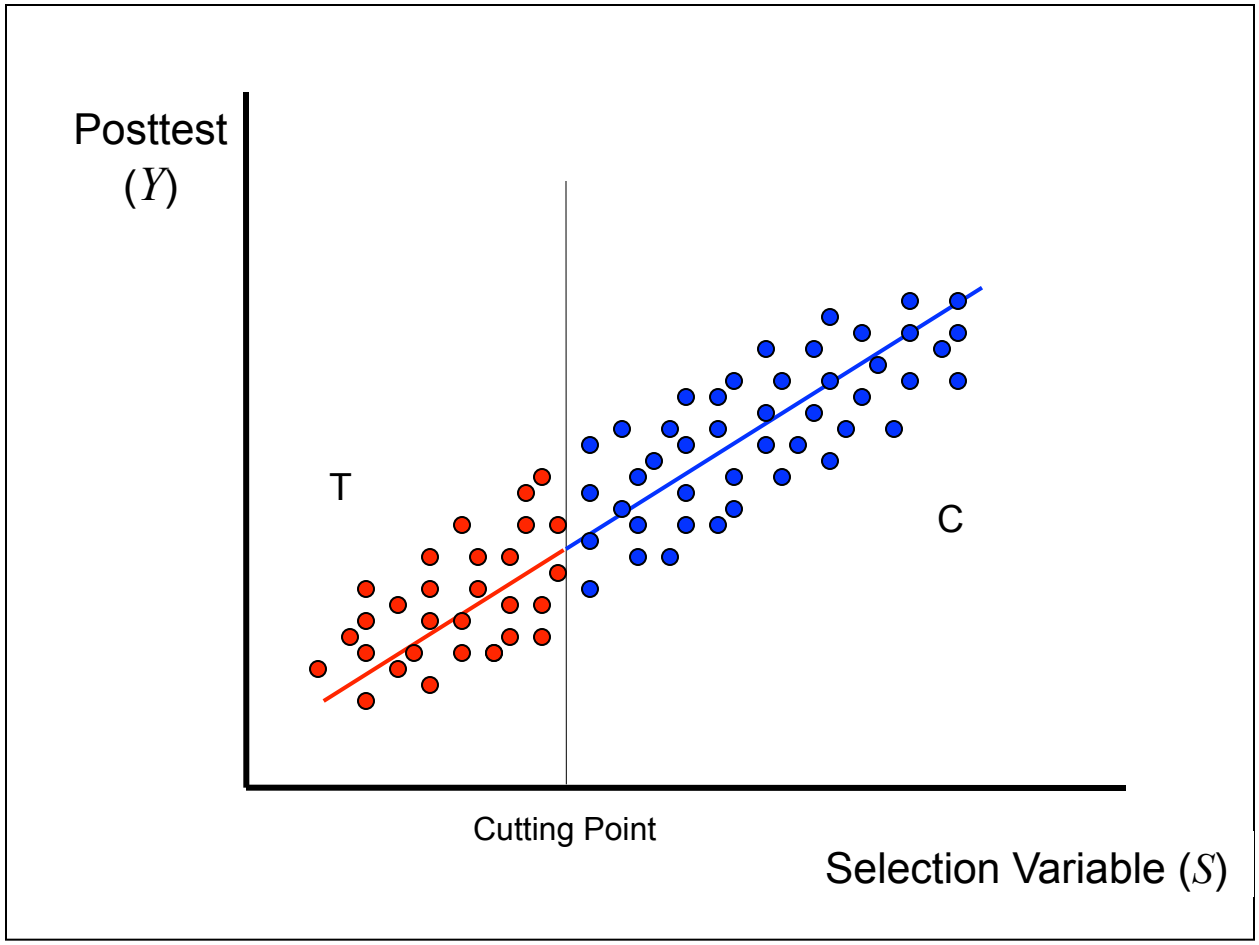
$$Y_i = B_0 + B_S S_i + B_T T_i + e_i$$

Regression discontinuity effect estimate compared with RCT estimate



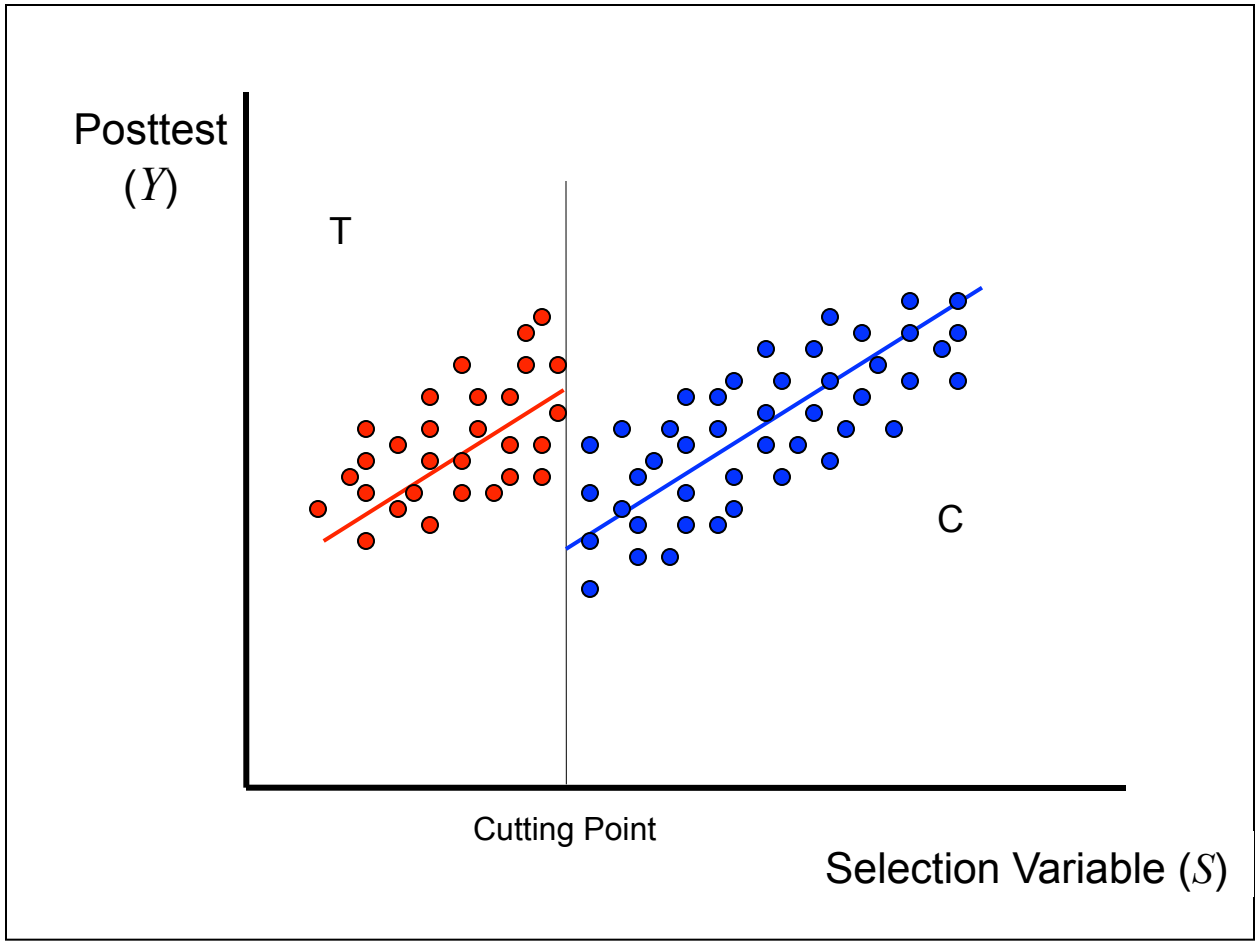
$$Y_i = B_0 + B_S S_i + B_T T_i + e_i$$

Regression discontinuity scatterplot (null case)



$$Y_i = B_0 + B_S S_i + B_T T_i + e_i$$

Regression discontinuity scatterplot (Tx effect)



$$Y_i = B_0 + B_S S_i + B_T T_i + e_i$$

The selection variable for R-D

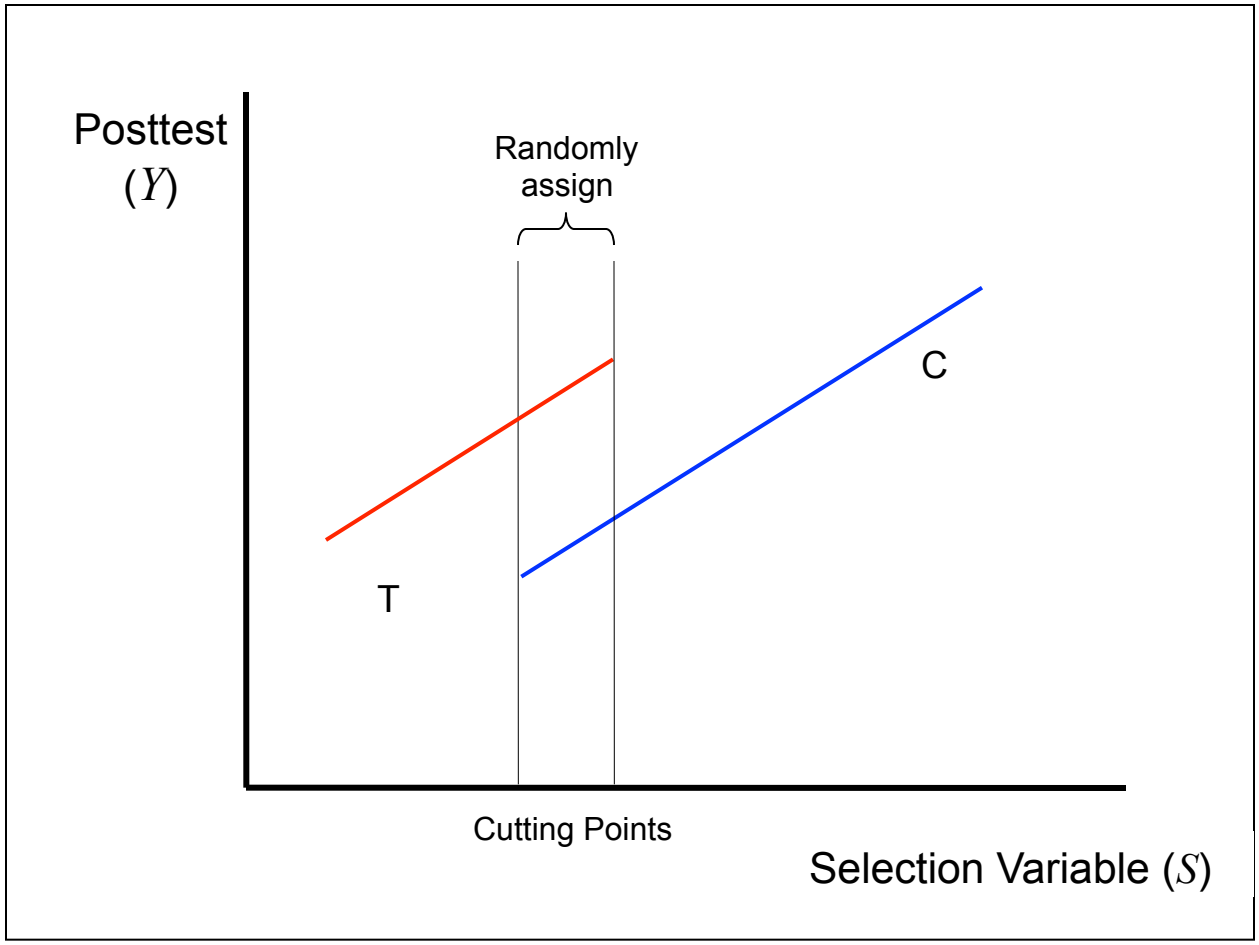
- A continuous quantitative variable measured on every candidate for assignment to T or C who will participate in the study
- Assignment to T or C strictly on the basis of the score obtained and a predetermined cutting point
- Does not have to correlate highly with the outcome variable (more power if it does)
- Can be tailored to represent an appropriate basis for the assignment decision in the setting

Why does it work?

- There is selection bias and nonequivalence between the T and C groups but …

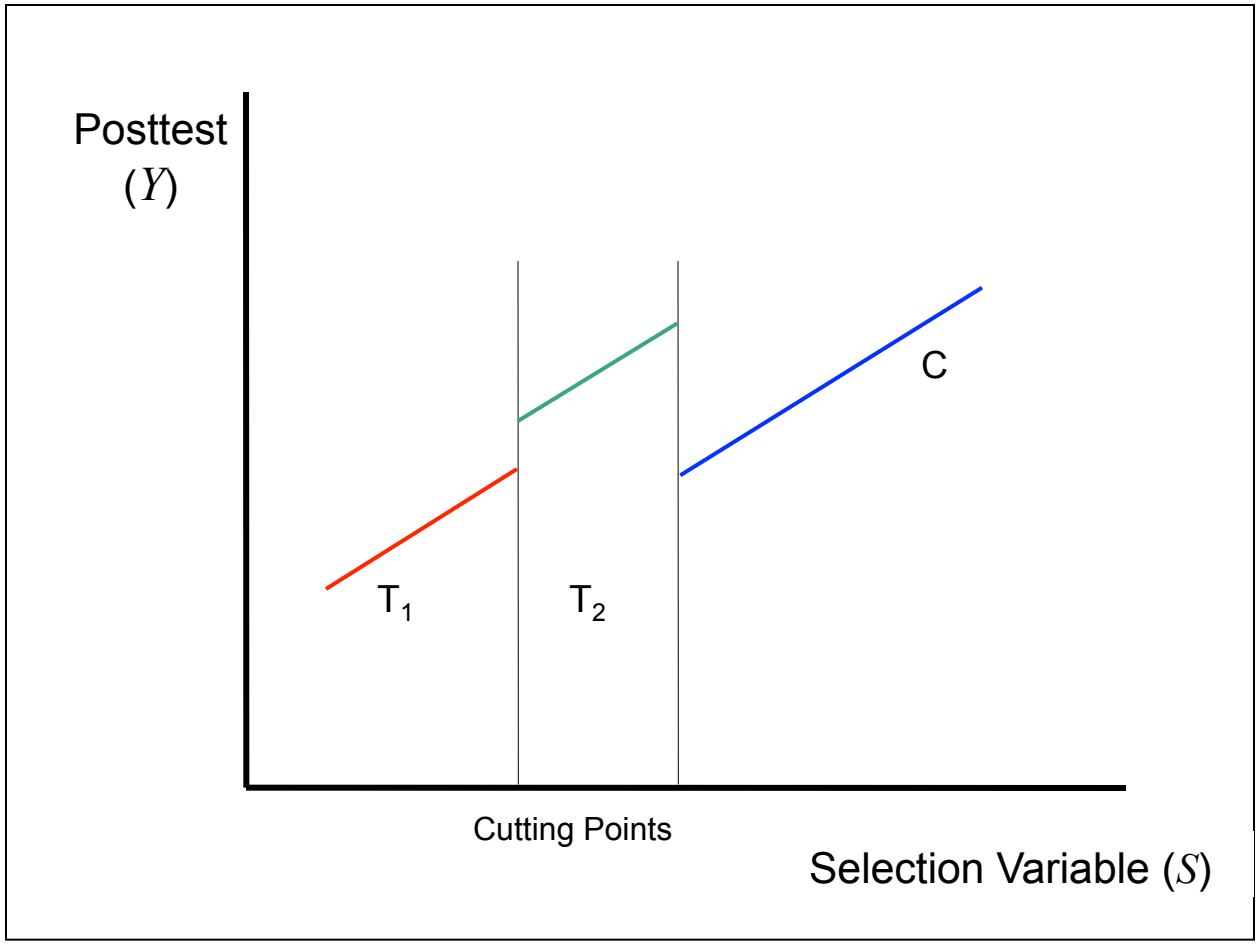
  its source is perfectly specified by the cutting point variable and can be statistically modeled (think perfect propensity score)
- Any difference between the T and C groups that might affect the outcome, whether known or unknown, has to be correlated with the cutting point variable and be "controlled" by it to the extent that it is related to the outcome (think perfect covariate)

R-D variants: Tie-breaker randomization



$$Y_i = B_0 + B_S S_i + B_T T_i + e_i$$

R-D variants: Double cutting points

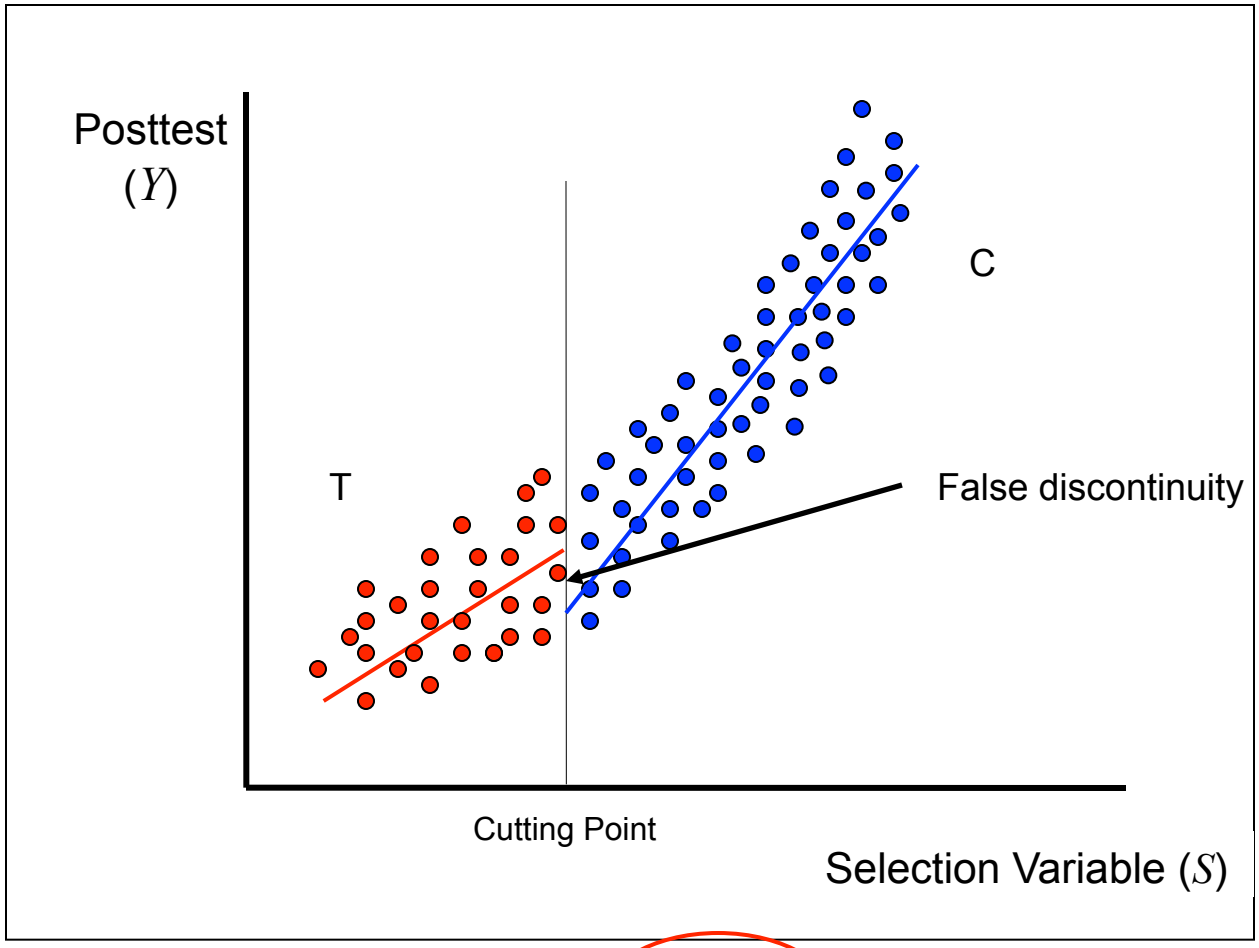


$$Y_i = B_0 + B_S S_i + B_{T1} T_{1i} + B_{T2} T_{2i} + e_i$$

Special issues with the R-D design

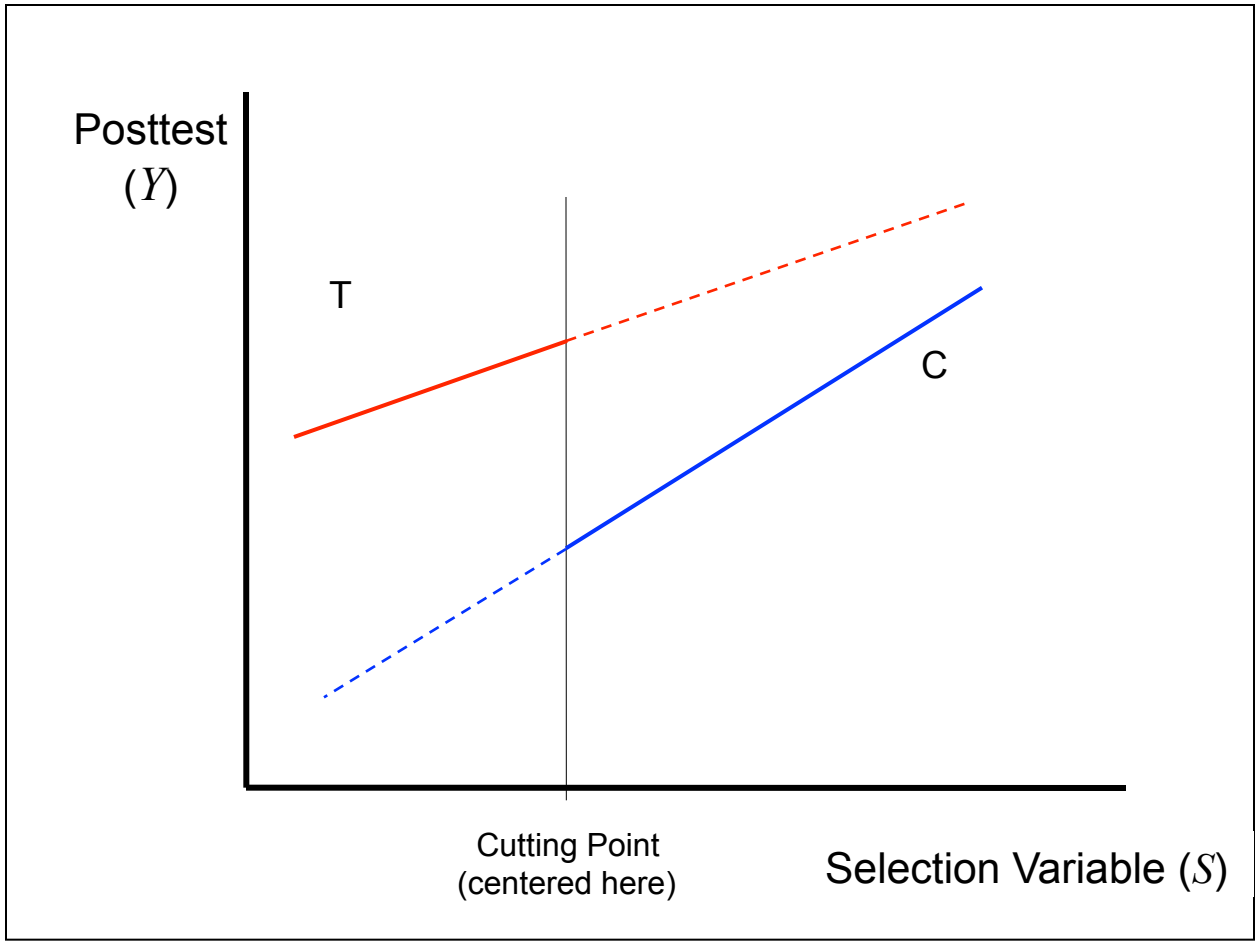
- Correctly fitting the functional form– possibility that it is not linear
  - curvilinear functions
  - interaction with the cutting point
  - consider short, dense regression lines
- Statistical power
  - sample size requirements relative to RCT
  - when covariates are helpful

Lines fit to curvilinear function



$$Y_i = B_0 + B_S S_i + B_{SQ} S_{SQ}^2 + B_T T_i + e_i$$

R-D effect estimate with an interaction compared with RCT estimate

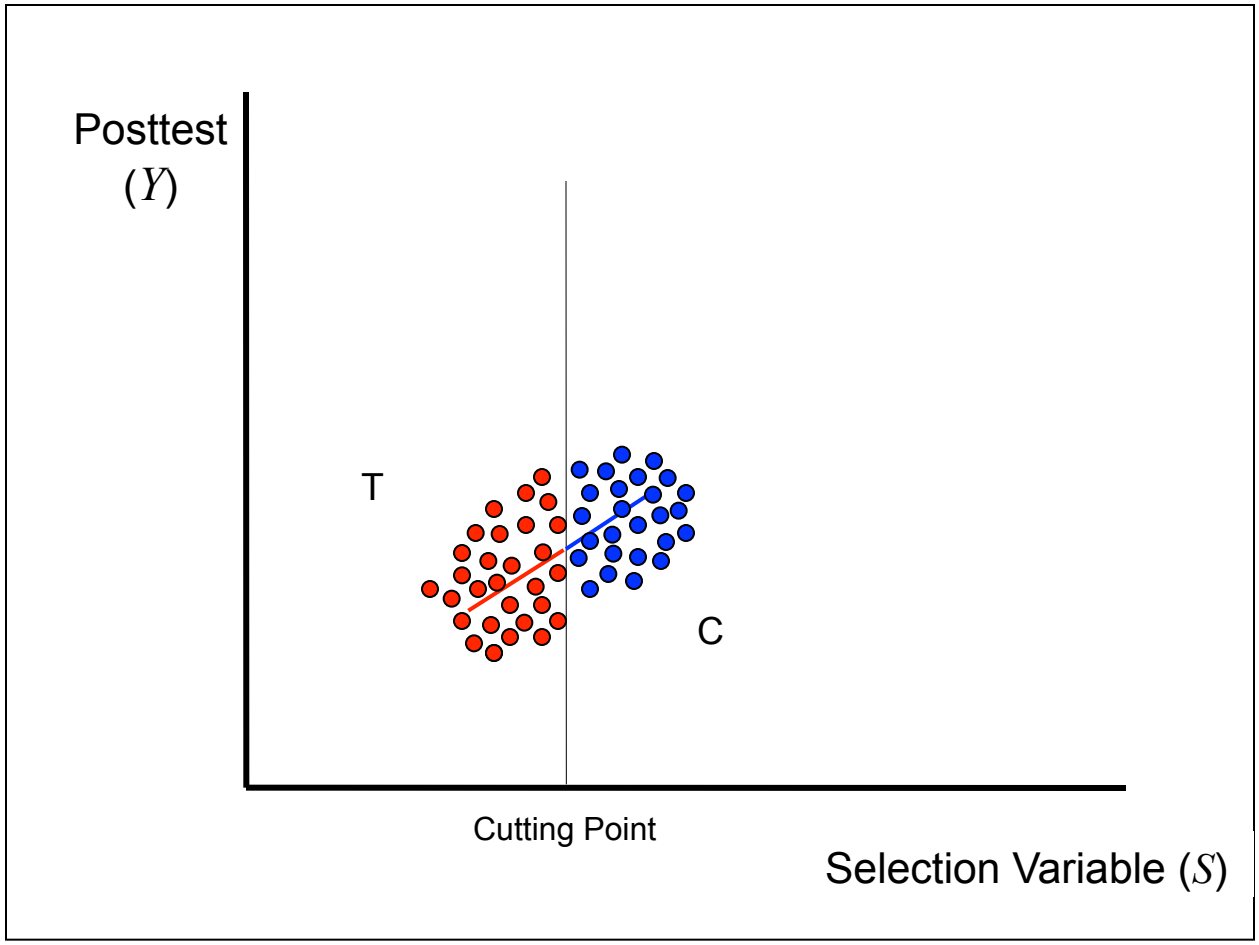


$$Y_i = B_0 + B_S S_i + B_T T_i + B_I (S_i \times T_i) + e_i$$

Modeling the functional form

- Visual inspection of scatterplots with candidate functions superimposed is important
- If possible, establish the functional form on data observed prior to implementation of treatment, e.g., pretest and posttest archival data for a prior school year
- Reverse stepwise modeling– fit higher order functions and successively drop those that are not needed
- Use regression diagnostics– R^2 and goodness of fit indicators, distribution of residuals

Short dense regressions for R-D



$$Y_i = B_0 + B_S S_i + B_T T_i + e_i$$

Statistical power

- Typically requires about 3 times as many participants as a comparable RCT
- Lower when the correlation between the cutting point continuum and treatment variable is large
- Higher when the correlation between the cutting point continuum and the outcome variable is large
- Improved by adding covariates correlated with outcome but not the cutting point continuum

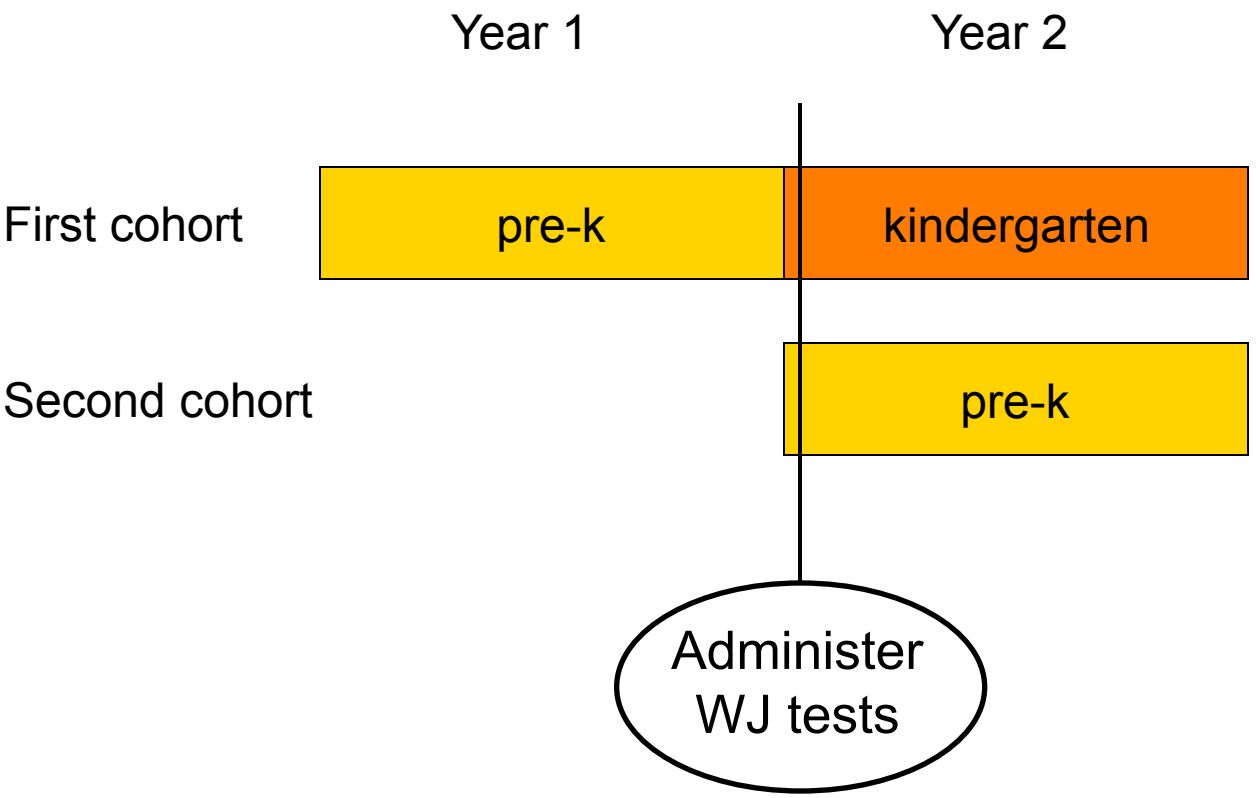
Example: Effects of pre-k

W. T. Gormley, T. Gayer, D. Phillips, & B. Dawson (2005). The effects of universal pre-k on cognitive development. *Developmental Psychology*, 41(6), 872-884.

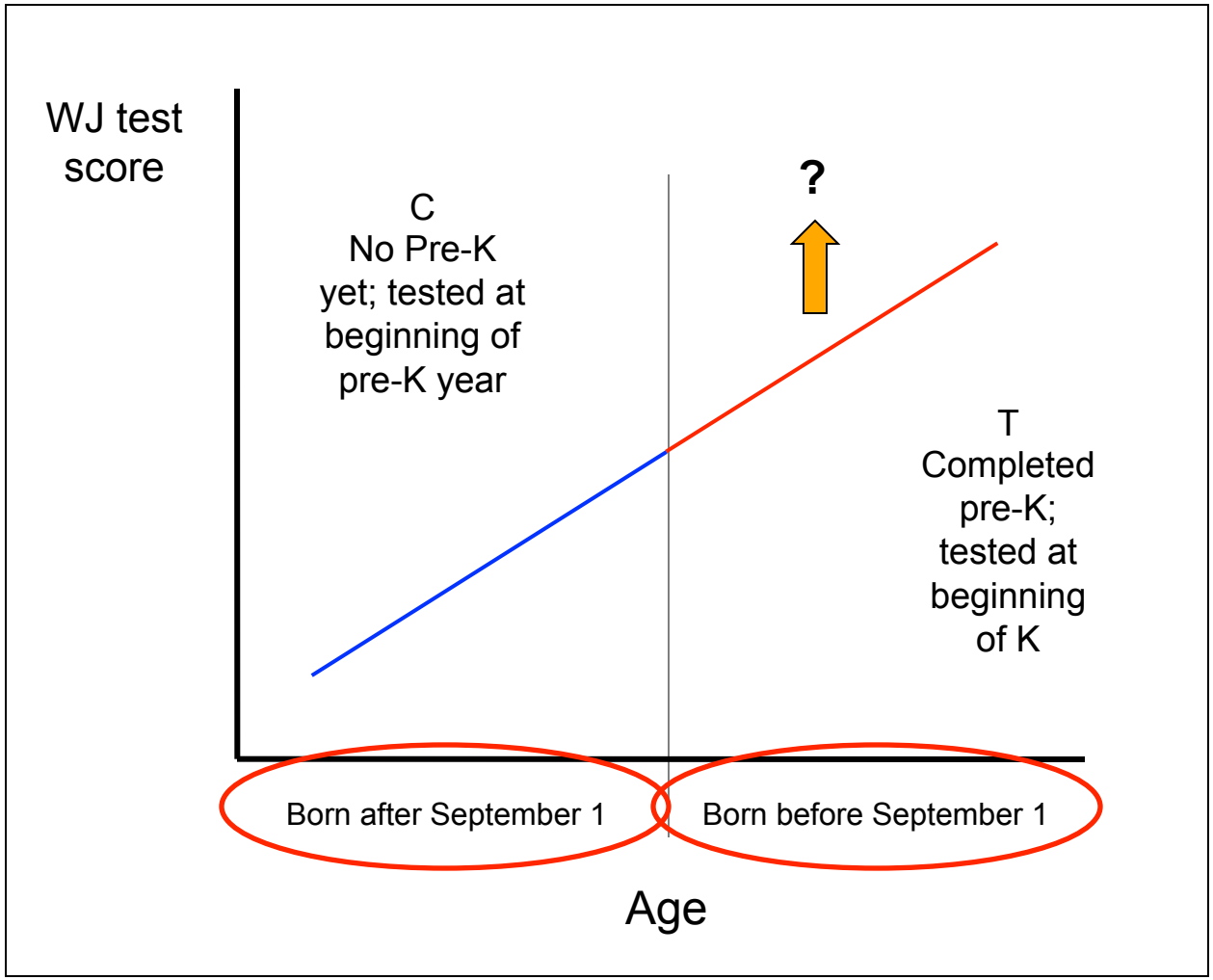
Study overview

- Universal pre-k for four year old children in Oklahoma
- Eligibility for pre-k determined strictly on the basis of age– cutoff by birthday
- Overall sample of 1,567 children just beginning pre-k plus 1,461 children just beginning kindergarten who had been in pre-k the previous year
- WJ Letter-Word, Spelling, and Applied Problems as outcome variables

Samples and testing



Entry into Pre-K Selected by Birthday



Excerpts from Regression Analysis

Variable	Letter-Word *B coeff*	Spelling *B coeff*	Applied Probs *B coeff*
Treatment (T)	3.00*	1.86*	1.94*
Age: Days ± from Sept 1	.01	.01*	.02*
$Days^2$	.00	.00	.00
Days x T	.00	-.01	-.01
$Days^2$ x T	.00	.00	.00
Free lunch	-1.28*	-.89*	-1.38*
Black	.04	-.44*	-2.34*
Hispanic	-1.70*	-.48*	-3.66*
Female	.92*	1.05*	.76*
Mother's educ: HS	.59*	.57*	1.25*

* $p<.05$

Selected references on R-D

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Jacob, B. A. & Lefgren, L. (2002). Remedial education and student achievement: A regression-discontinuity analysis. Working Paper 8919, National Bureau of Economic Research (www.nber.org/papers/w8919)

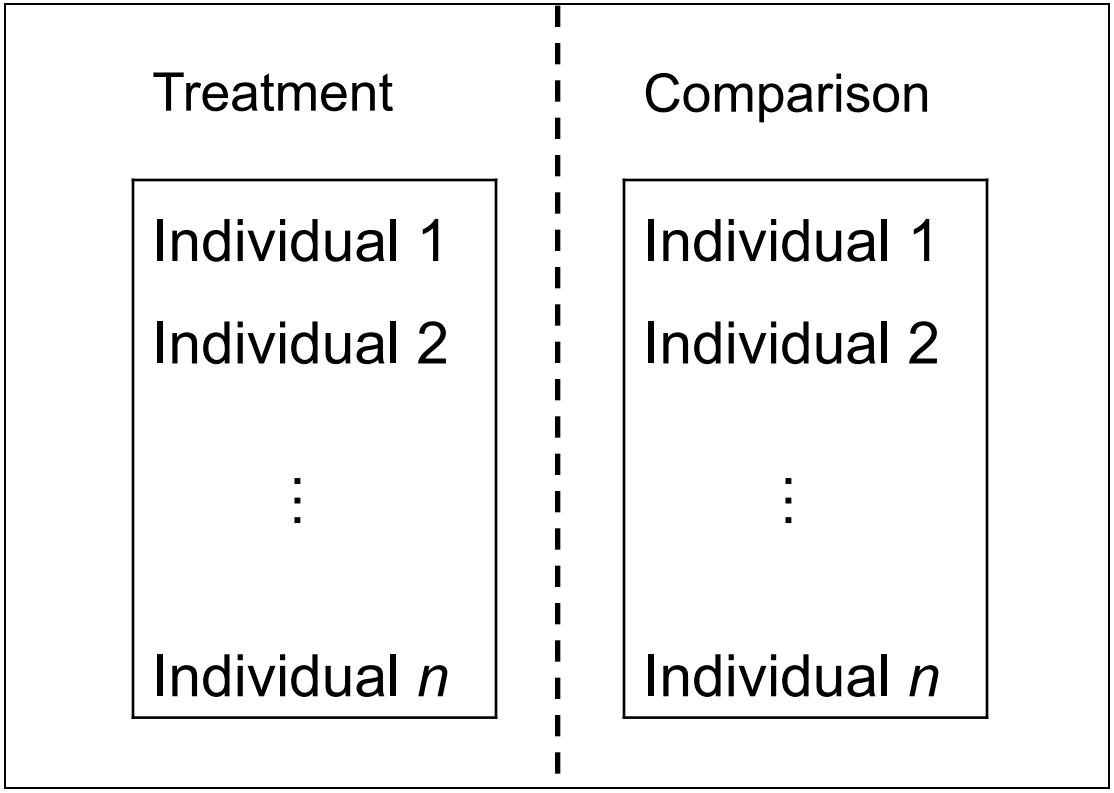
Kane, T. J. (2003). A quasi-experimental estimate of the impact of financial aid on college-going. Working Paper 9703, National Bureau of Economic Research (www.nber.org/papers/w9703)

Nonrandomized Comparison Groups with Statistical Controls

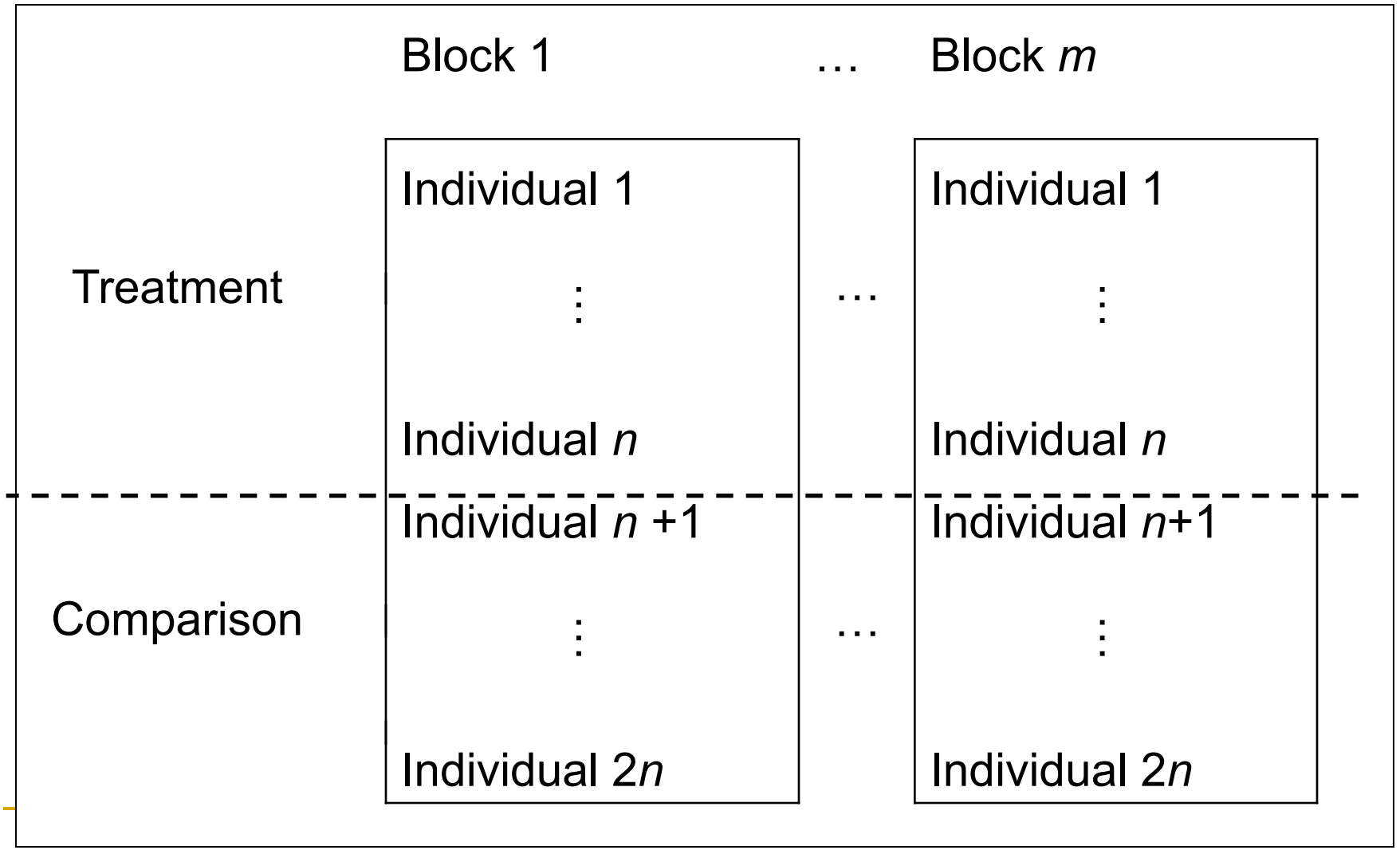
- ANCOVA/OLS statistical controls
- Matching
- Propensity scores

Nonequivalent comparison analog to the completely randomized design

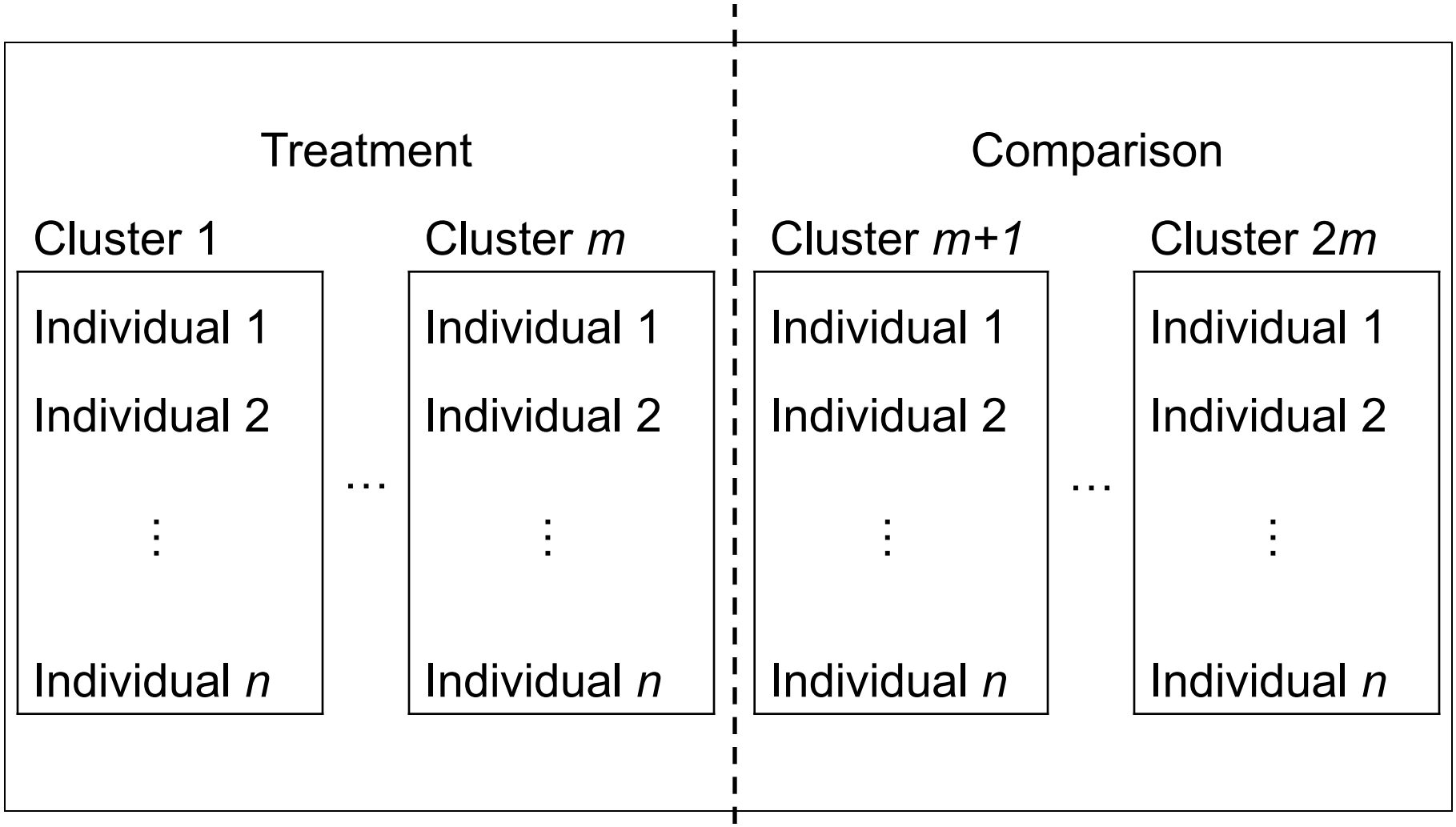
Individuals are selected into treatment and control conditions through some nonrandom more-or-less natural process



Nonequivalent comparison analog to the randomized block design



The nonequivalent comparison analog to the hierarchical design



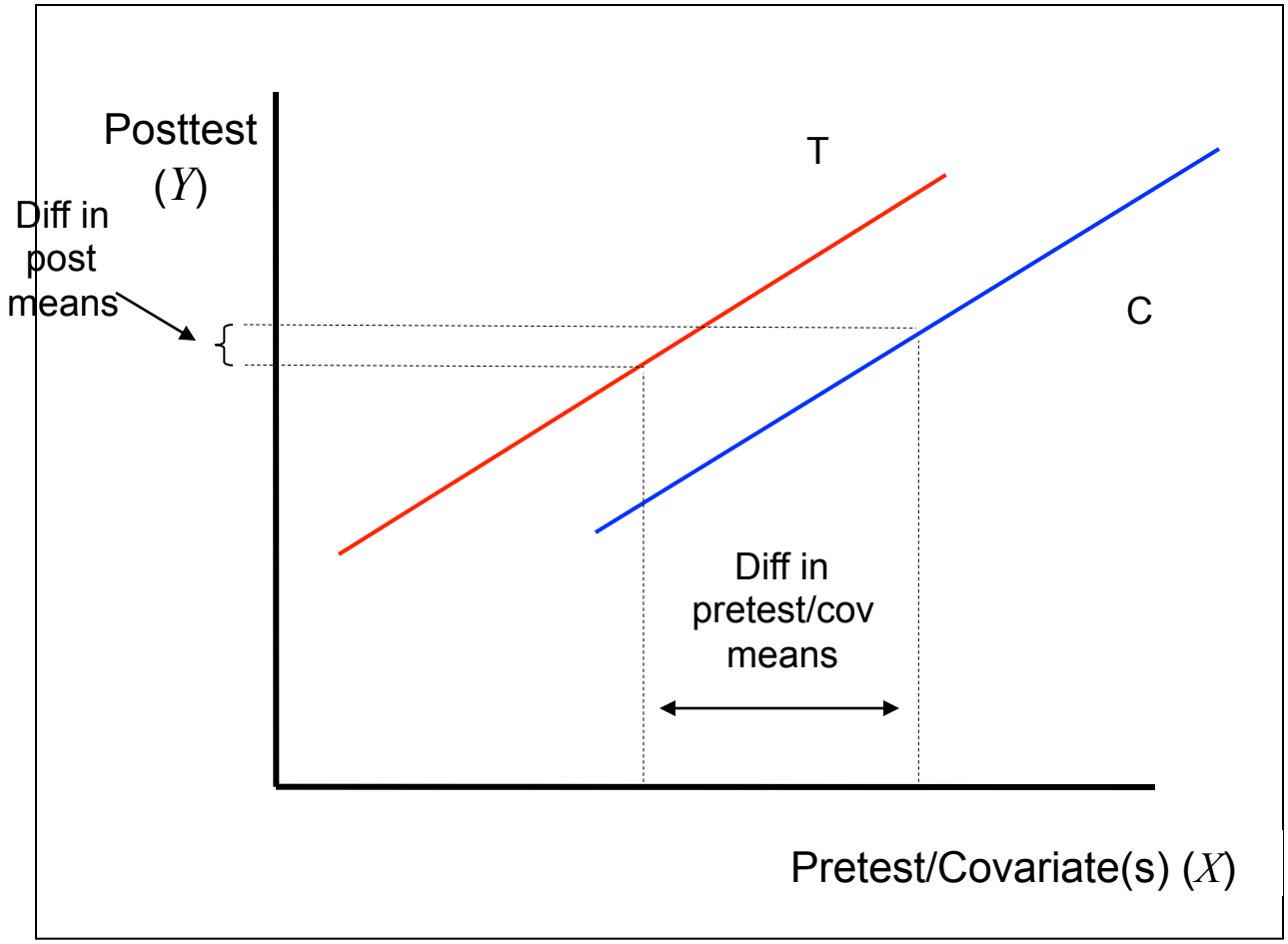
Issues for obtaining good Tx effect estimates from nonrandomized comparison groups

- The fundamental problem: selection bias
- Knowing/measuring the variables necessary and sufficient to statistically control the selection bias
  - characteristics on which the groups differ that are related to the outcome
  - relevant characteristics not highly correlated with other characteristics already accounted for
- Using an analysis model that properly adjusts for the selection bias, given appropriate control variables

Nonrandomized comparisons of possible interest

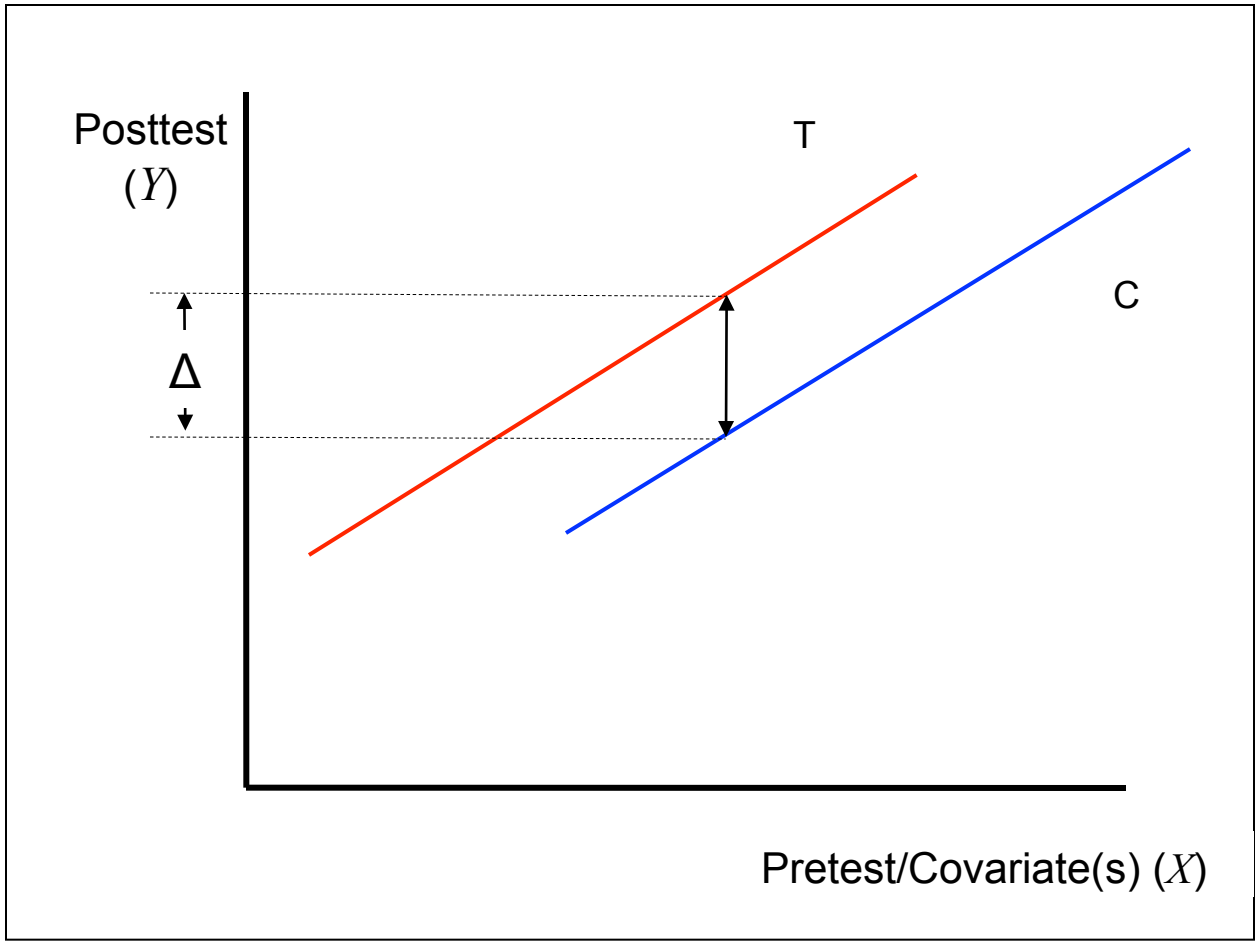
- Nonequivalent comparison/control group for estimating treatment effects
- Attrition analysis– comparing leavers and stayers, adjusting for differential attrition
- Treatment on the treated analysis (TOT)– estimating treatment effects on those who actually received treatment.

Nonequivalent comparison groups: Pretest/covariate and posttest means



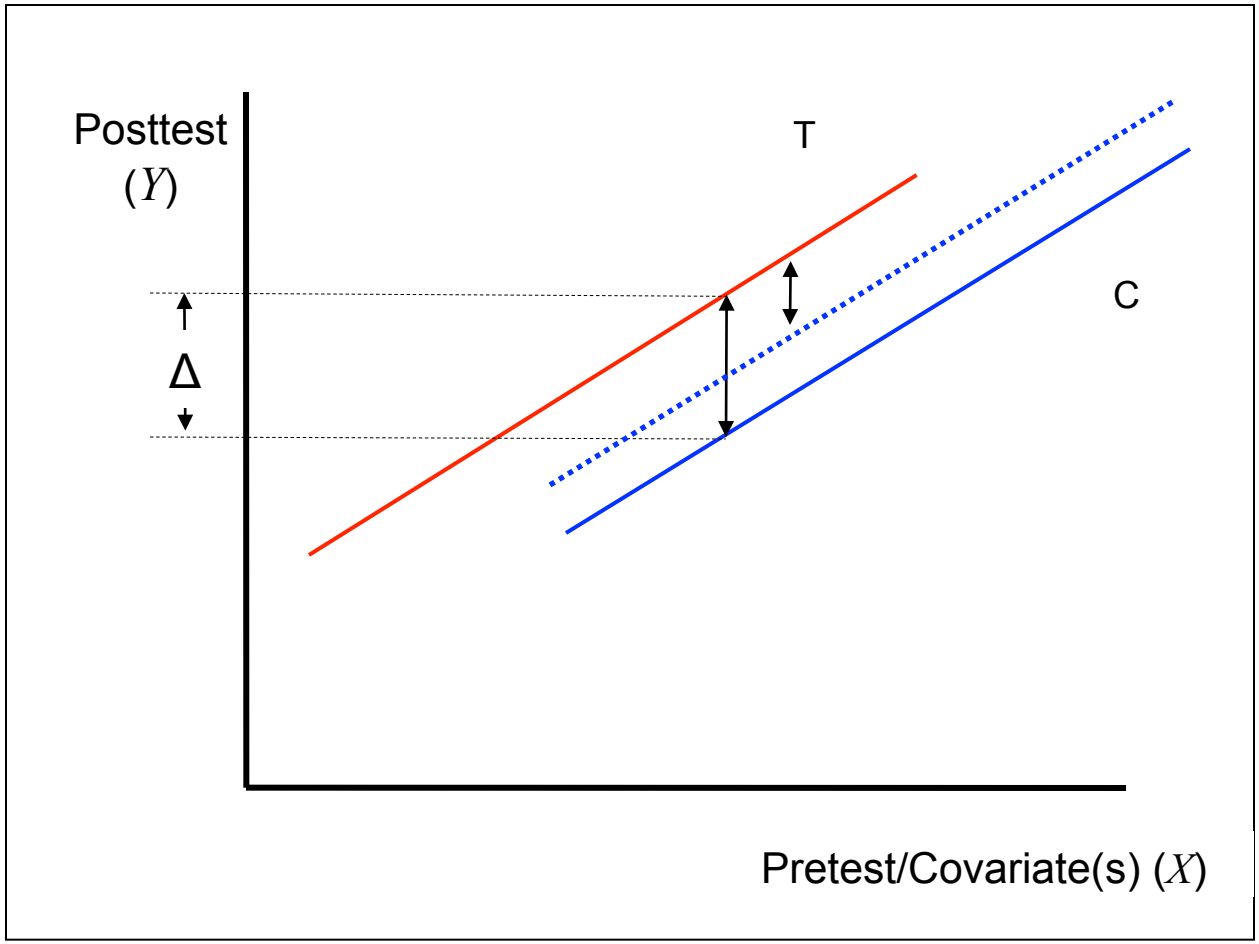
$$Y_i = B_0 + B_X X_i + B_T T_i + e_i$$

Nonequivalent comparison groups: Covariate-adjusted treatment effect estimate



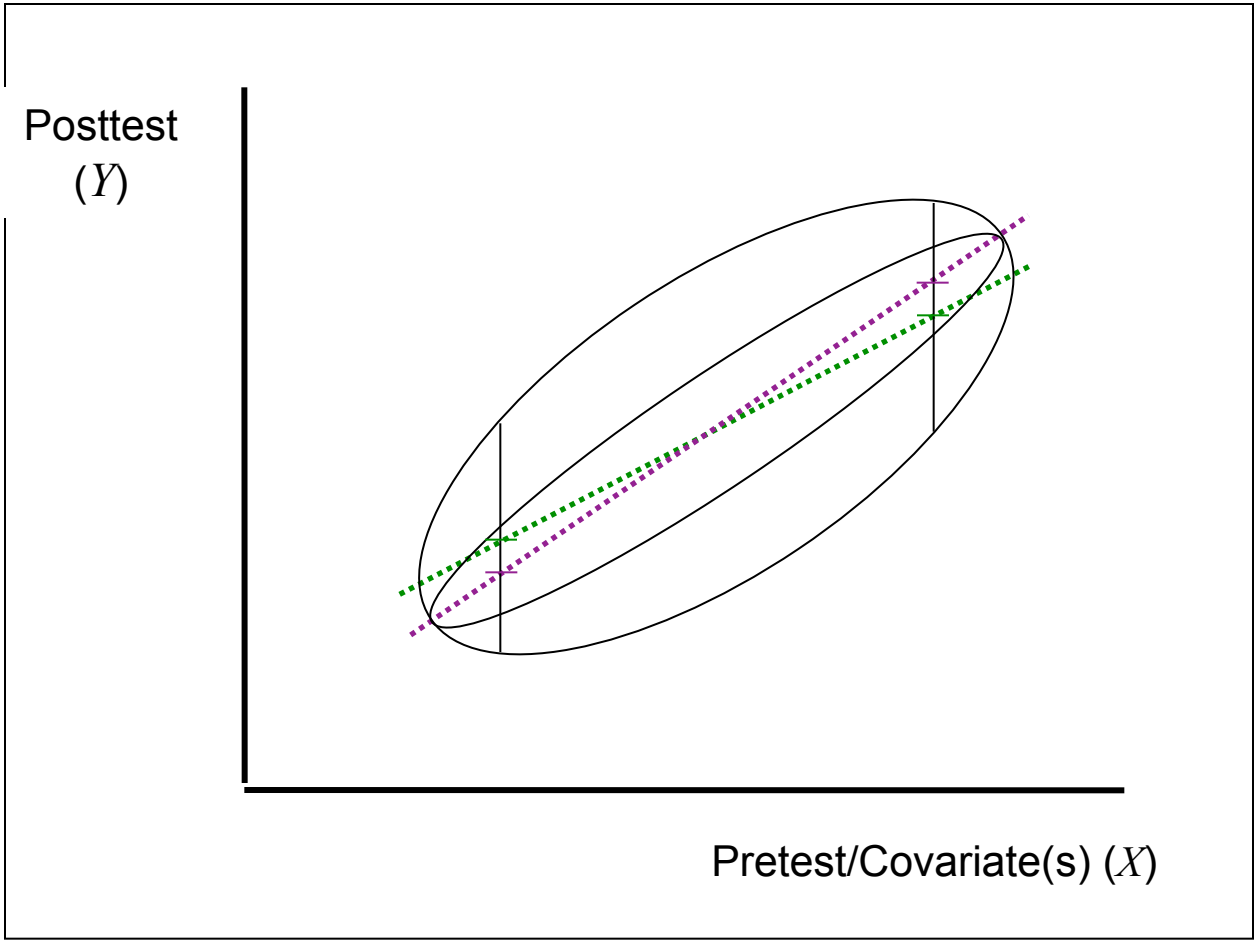
$$Y_i = B_0 + B_X X_i + B_T T_i + e_i$$

Covariate-adjusted treatment effect estimate with a relevant covariate left out

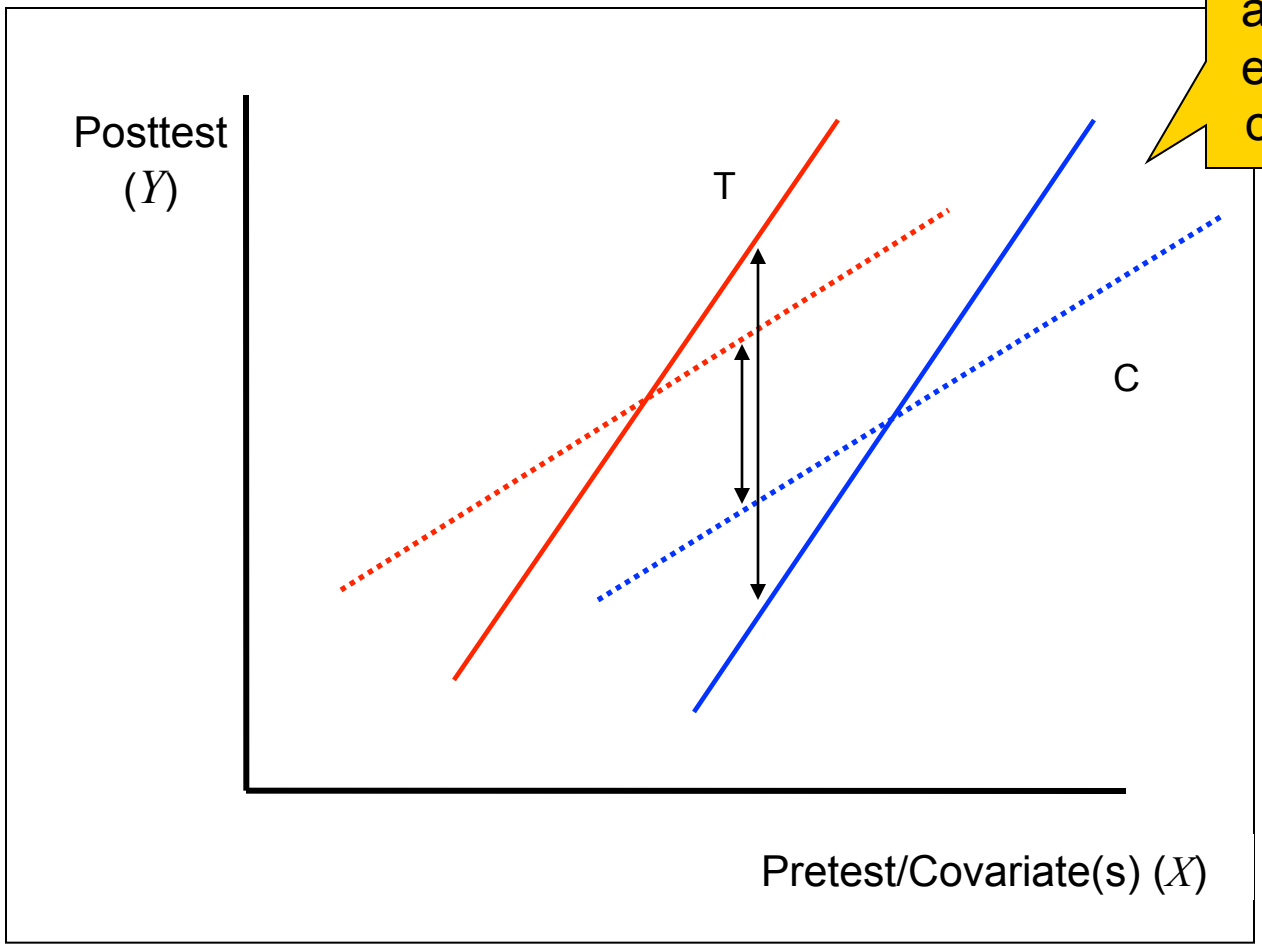


$$Y_i = B_0 + B_{X1}X_{1i} + B_{X2}X_{2i} + B_T T_i + e_i$$

Nonequivalent comparison groups: Unreliability in the covariate



Nonequivalent comparison groups: Unreliability in the covariate



Note: Will not always under-estimate, may over-estimate

$$Y_i = B_0 + B_X X_i + B_T T_i + e_i$$

Using control variables via matching

- Groupwise matching: select control comparison to be groupwise similar to treatment group, e.g., schools with similar demographics, geography, etc.

  Generally a good idea.

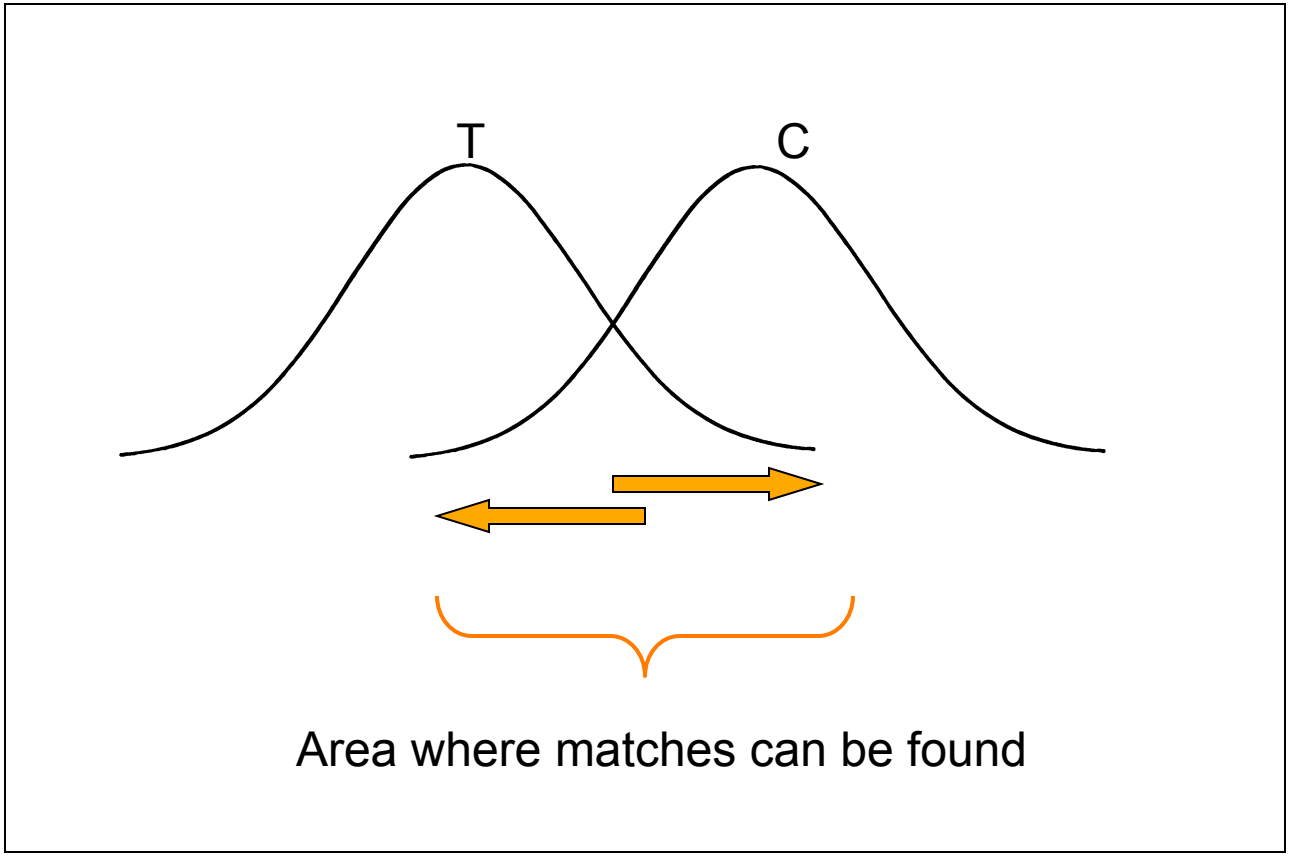
- Individual matching: select individuals from the potential control pool that match treatment individuals on one or more observed characteristics.

  May not be a good idea.

Potential problems with individual level matching

- Basic problem with nonequivalent designs– need to match on all relevant variables to obtain a good treatment effect estimate.
- If match on too few variables, may omit some that are important to control.
- If try to match on too many variables, the sample will be restricted to the cases that can be matched; may be overly narrow.
- If must select disproportionately from one tail of the treatment distribution and the other tail of the control distribution, may have regression to the mean artifact.

Regression to the mean: Matching on the pretest



Propensity scores

What is a propensity score (Rosenbaum & Rubin, 1983)?

- The probability (or propensity) of being in the treatment group instead of the comparison group (or stayers vs. leavers, treated vs untreated)
- Estimated ("predicted") from data on the characteristics of individuals in the sample
- As a probability, it ranges from 0 to 1
- Is calculated for each member of a sample

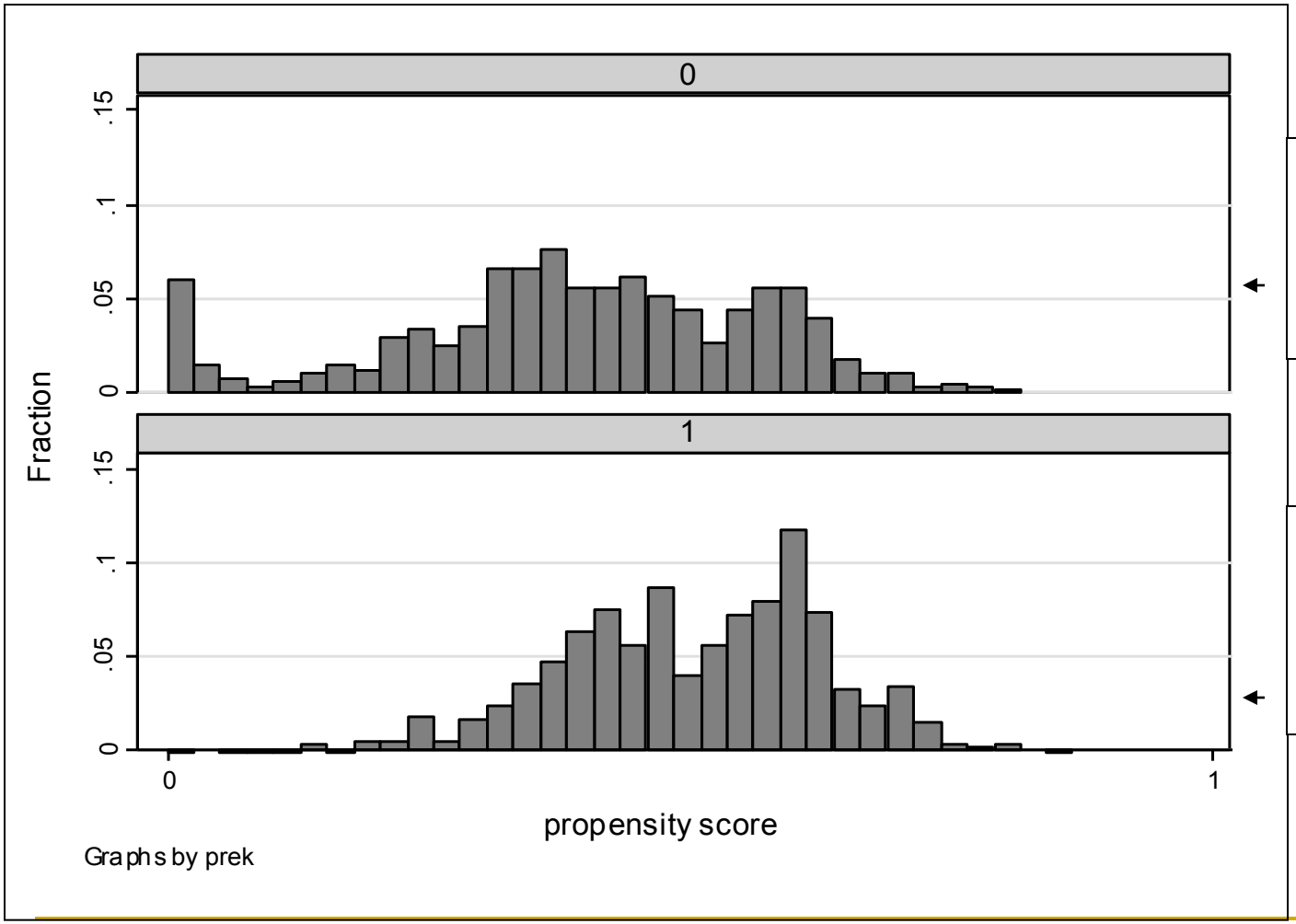
Computing the propensity score

- First estimate $T_i = f(S_{1i}, S_{2i}, S_{3i} \ldots S_{ki})$ for all T and C members in the sample
  - Logistic regression typically used; other methods include probit regression, classification trees
  - All relevant characteristics assumed to be included among the predictor variables (covariates)
- E.g., fit logistic regression $T_i = B_1S_{1i} + B_2S_{2i} \ldots + e_i$
- Compute $propensity_i = B_1S_{1i} + B_2S_{2i} \ldots S_{ki}$
- The propensity score thus combines all the information from the covariates into a single variable optimized to distinguish the characteristics of the T sample from those of the C sample

What to do with the propensity score

- Determine how similar the treatment and control group are on the composite of variables in the propensity score; decide if one or both need to be trimmed to create a better overall match.
- Use the propensity score to select T and C cases that match.
- Use the propensity score as a statistical control variable, e.g., in an ANCOVA.

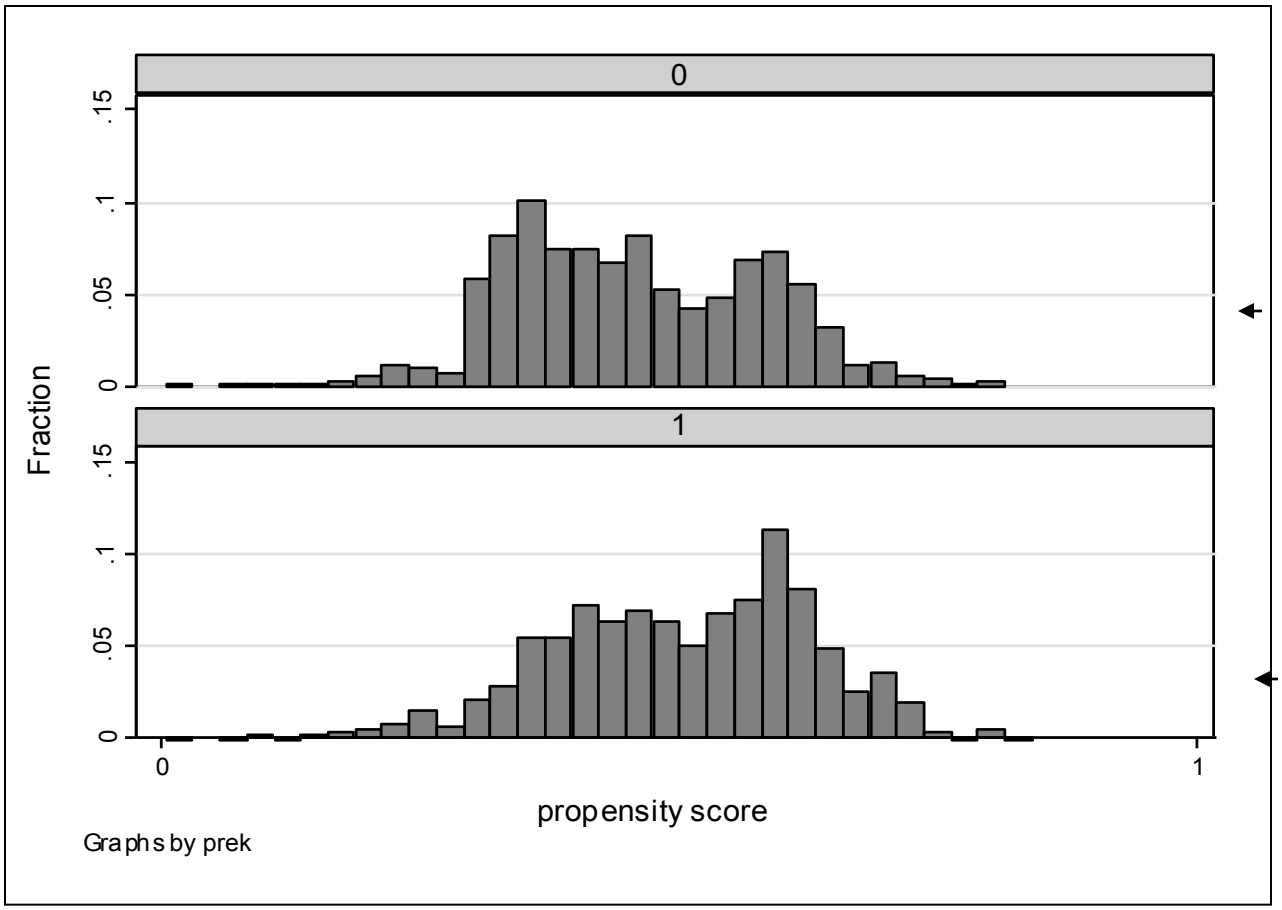
Propensity score distribution *before* trimming
(example from Hill pre-K Study)



Comparison Group (n=1,144):
25th percentile: 0.30
50th percentile: 0.40
75th percentile: 0.53
Mean = 0.39

Treatment Group (n=908):
25th percentile: 0.42
50th percentile: 0.52
75th percentile: 0.60
Mean = 0.50

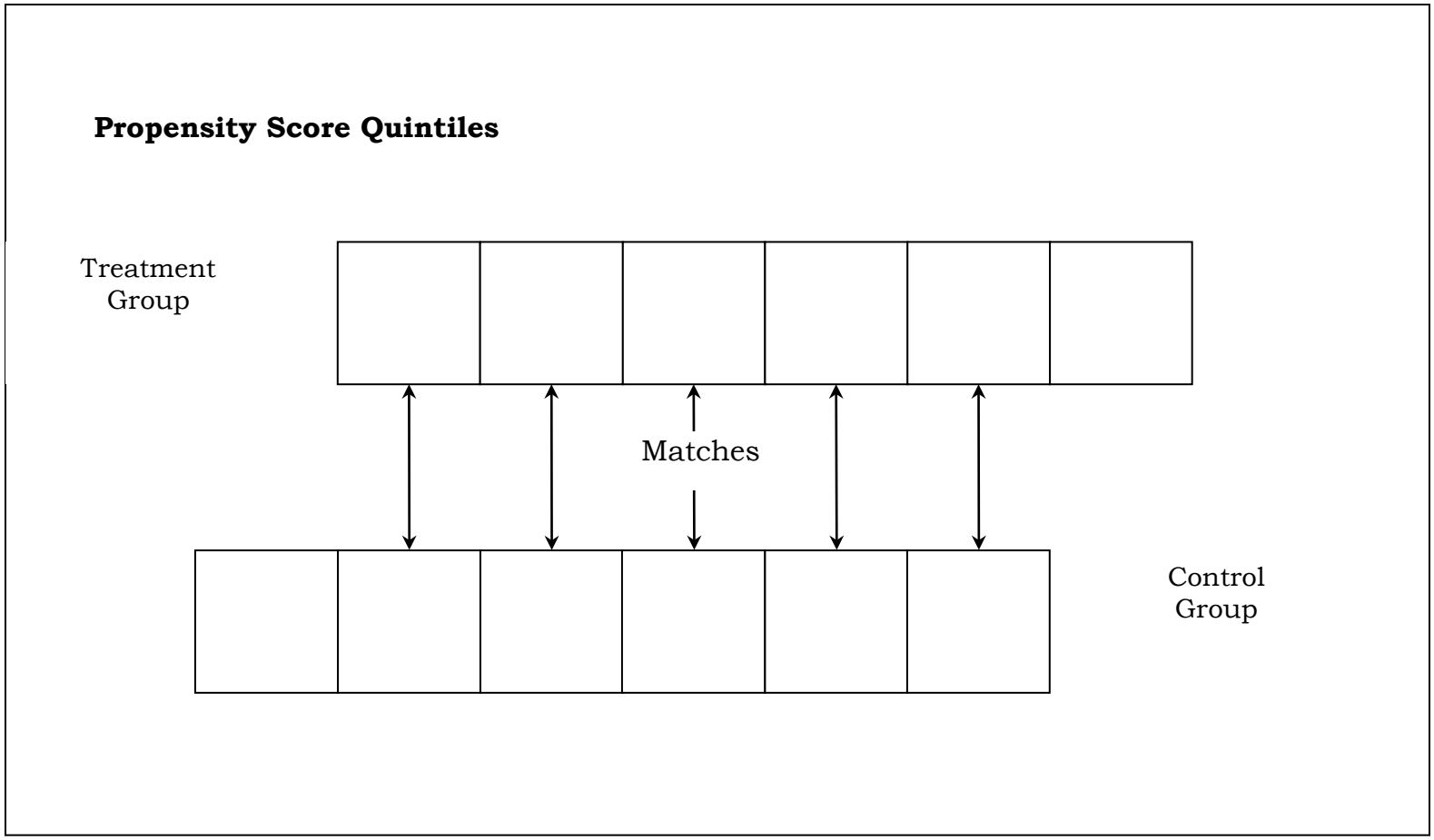
Propensity score distribution *after* trimming
(example from Hill pre-K Study)



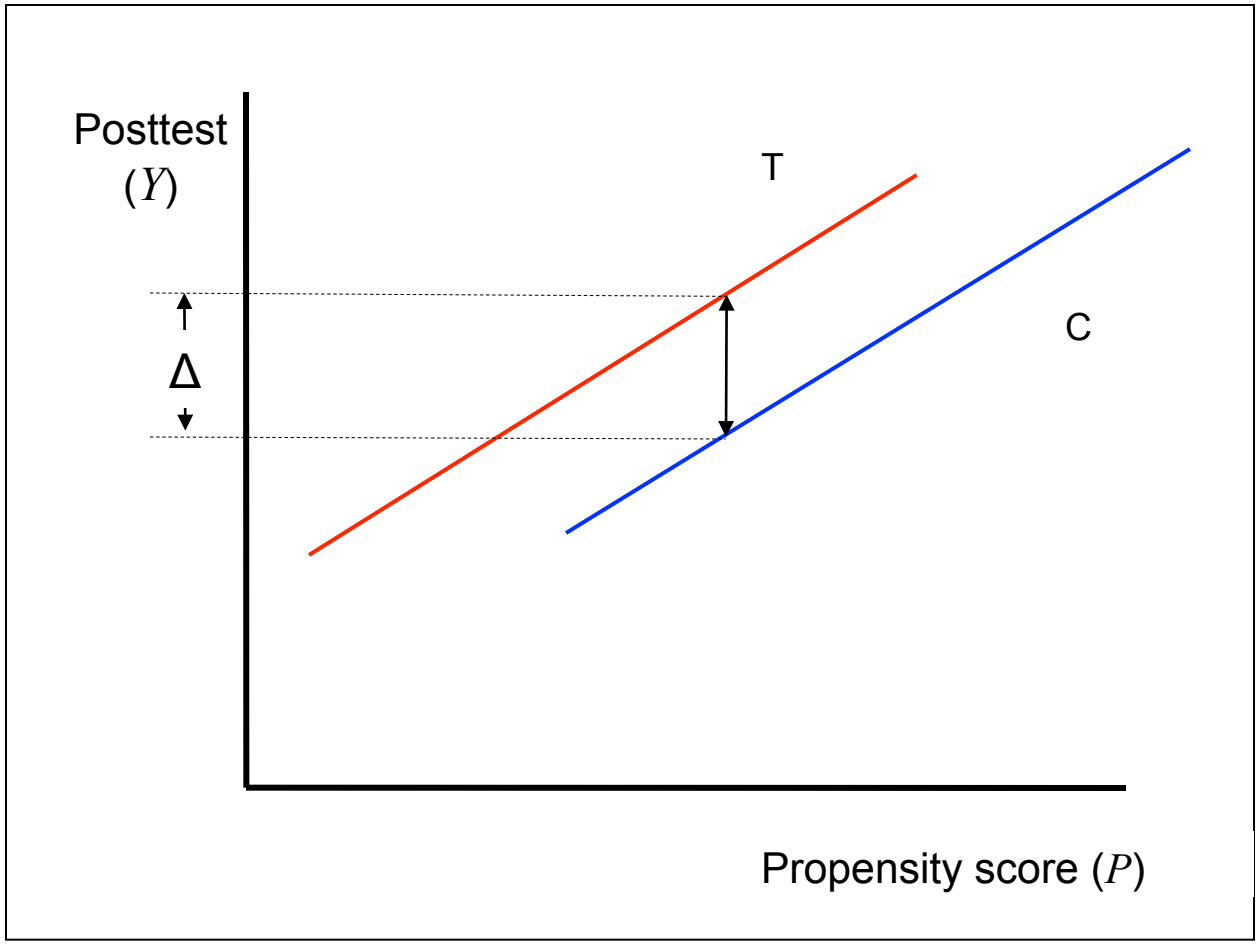
Comparison Group (n=908):
25th percentile: 0.36
50th percentile: 0.45
75th percentile: 0.56
Mean = 0.46

Treatment Group (n=908):
25th percentile: 0.42
50th percentile: 0.52
75th percentile: 0.60
Mean = 0.50

Estimate the treatment effect, e.g., by differences between matched strata



Estimate the treatment effect, e.g., by using the propensity score as a covariate



$$Y_i = B_0 + B_P P_i + B_T T_i + e_i$$

Discouraging evidence about the validity of treatment effect estimates

- The relatively few studies with head-to-head comparisons of nonequivalent comparison groups with statistical controls and randomized controls show very uneven results– some cases where treatment effects are comparable, many where they are not.
- Failure to include all the relevant covariates appears to be the main problem.

Selected references on nonequivalent comparison designs

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