TESTING THE VALIDITY OF THE COUNTERFACTUAL

Jesse Lecy

Core Concepts

The Selection Problem

Tests for Group Equivalence

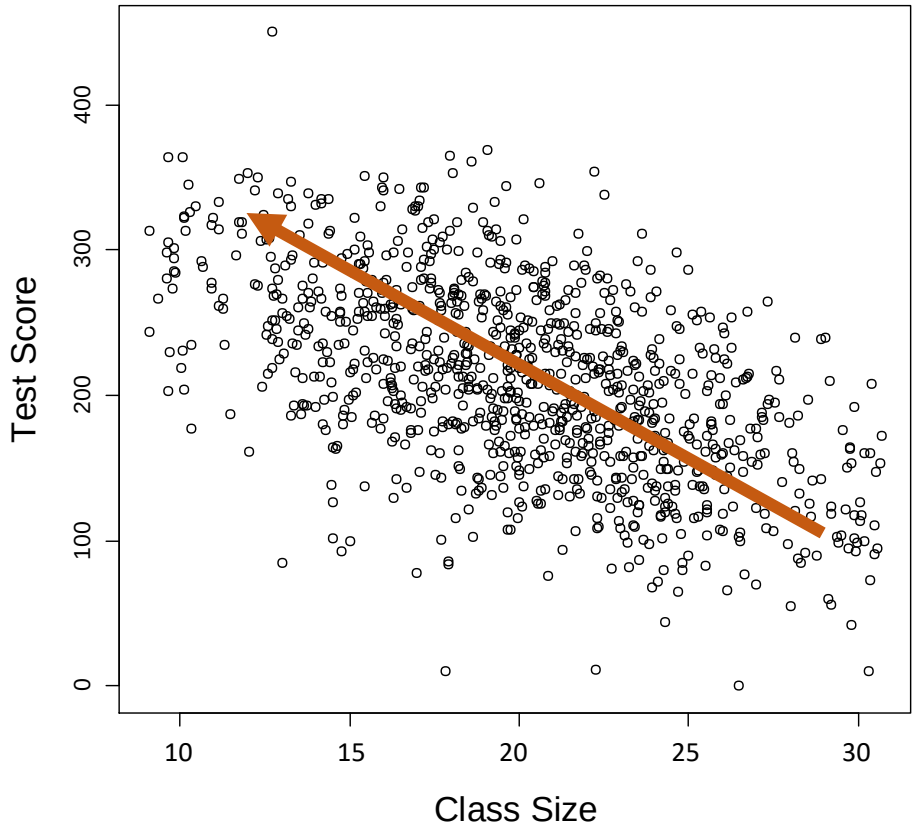
Omnibus Test

Bonferroni Correction

Tests for Nonrandom Attrition

NATURE GIVES US CORRELATIONS: THE SELECTION PROBLEM IN EVALUATION RESEARCH

If we reduce classroom size we should see student performance improve. Right?



THE MATH OF CITIES

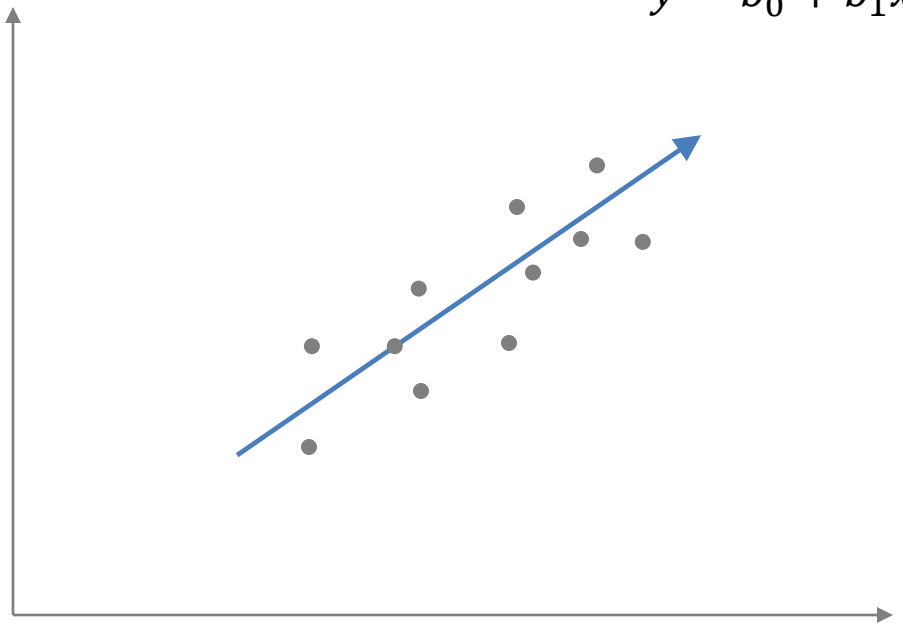
Listen: 4:30 – 15:30

http://www.radiolab.org/2010/oct/08/its-alive/

THE MATH OF CITIES

Patents / Crime / Salary
(outcomes)

$$y = b_0 + b_1 x$$



Walking Speed?

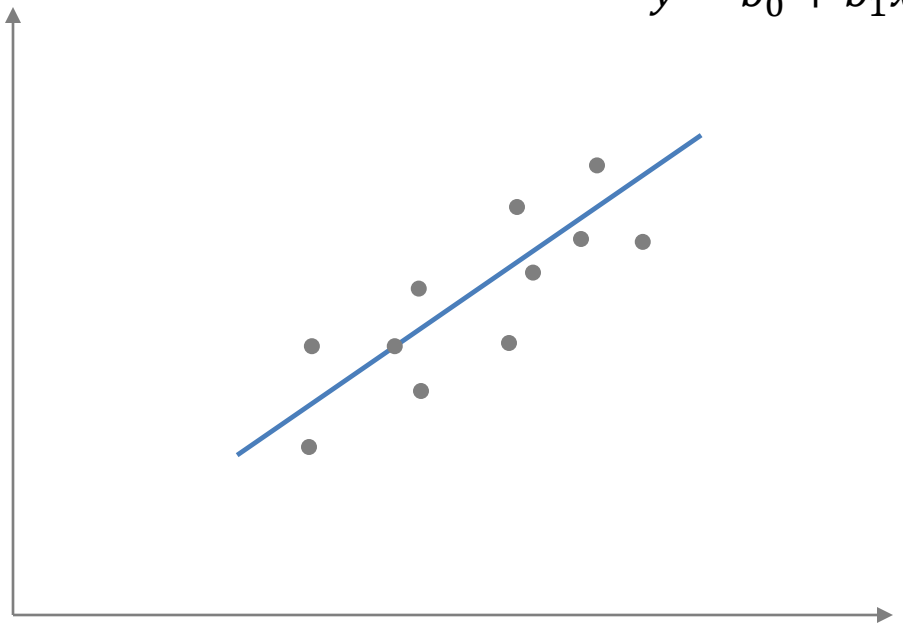
THE MATH OF CITIES

If you increase the WALKING SPEED of a city, would you increase the number of patents produced?

THE MATH OF CITIES

Patents / Crime / Salary
(outcomes)

$$y = b_0 + b_1 x$$

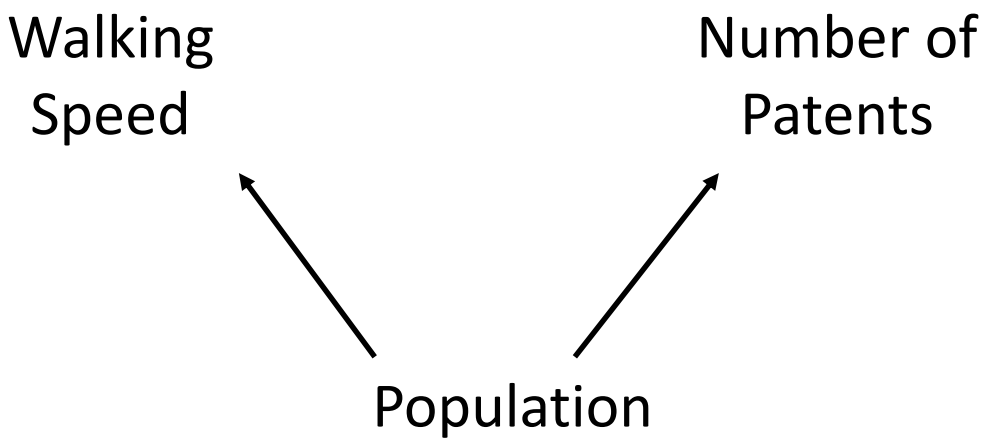


Population

THE MATH OF CITIES

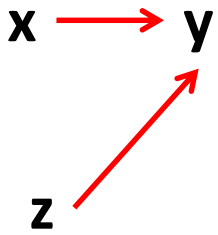
If you increase the POPULATION of a city, would you increase the number of patents produced?

The problem with correlations:

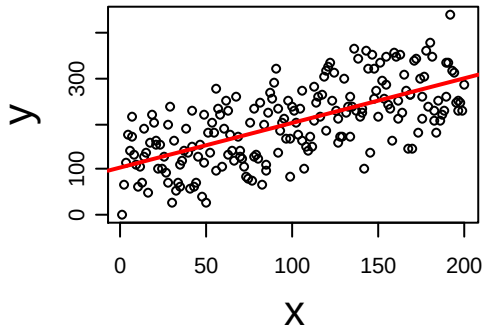


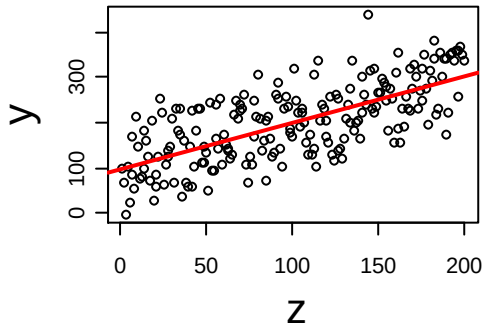
NATURE GIVES US CORRELATIONS

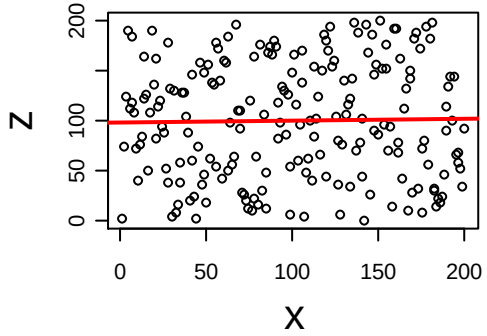
Example #1



X and Z both impact Y

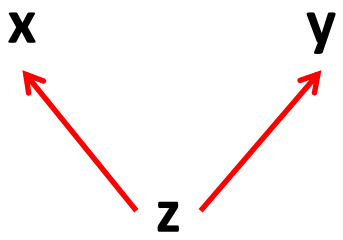




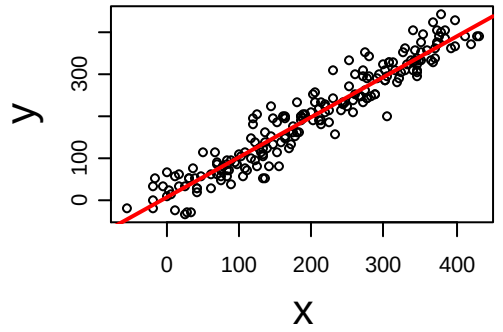


no correlation

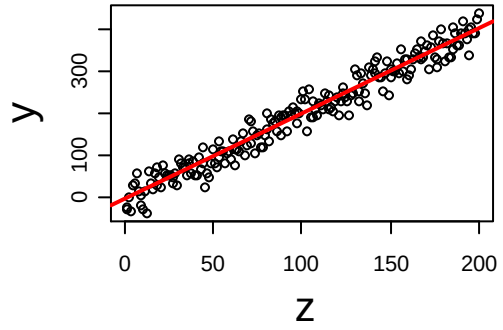
Example #2

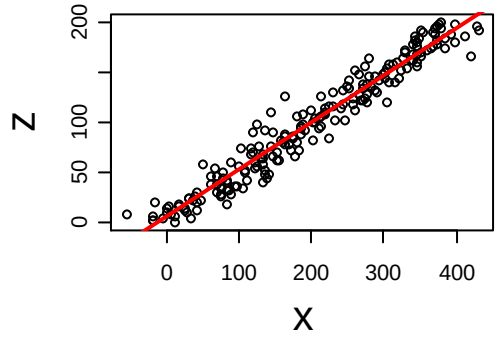


Z impacts both X and Y

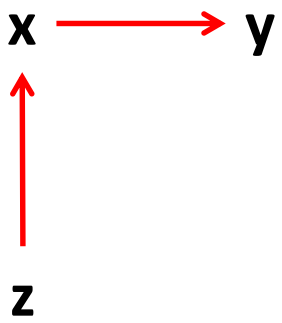


No causal relationship but high correlation

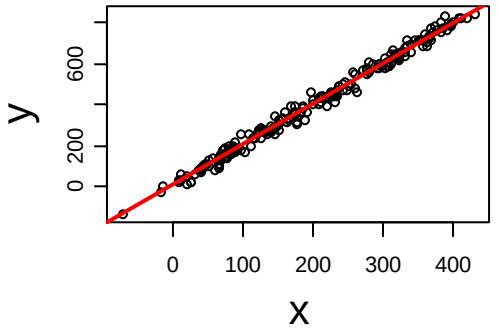


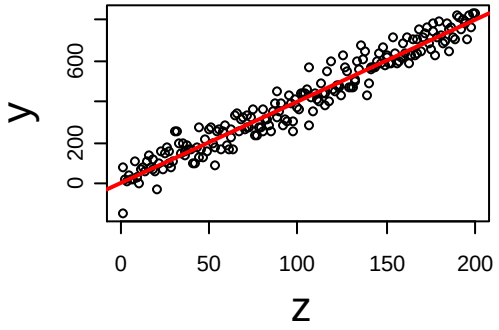


Example #3

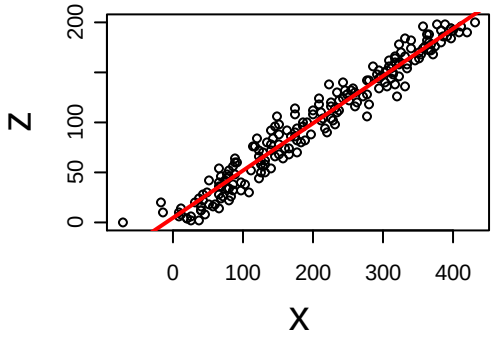


Causal chain



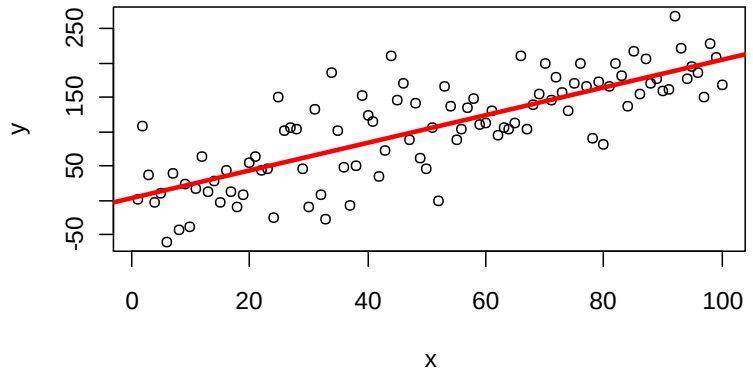


no direct causal relationship but highly correlated



Reverse causality

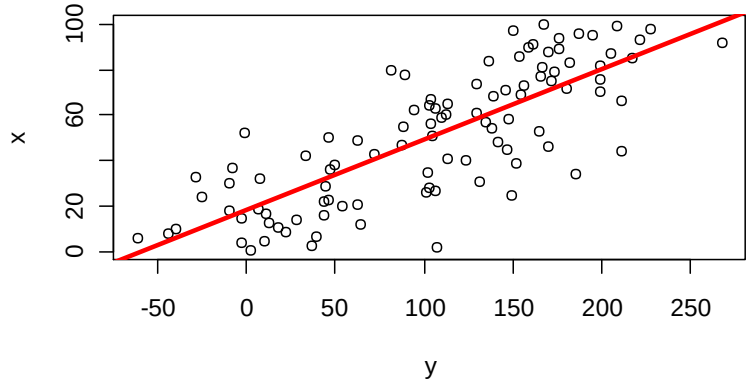
x ⟶ y



$Y = b_0 + b_1X + e$

Both models are highly significant, how do we know which one is the causal relationship?

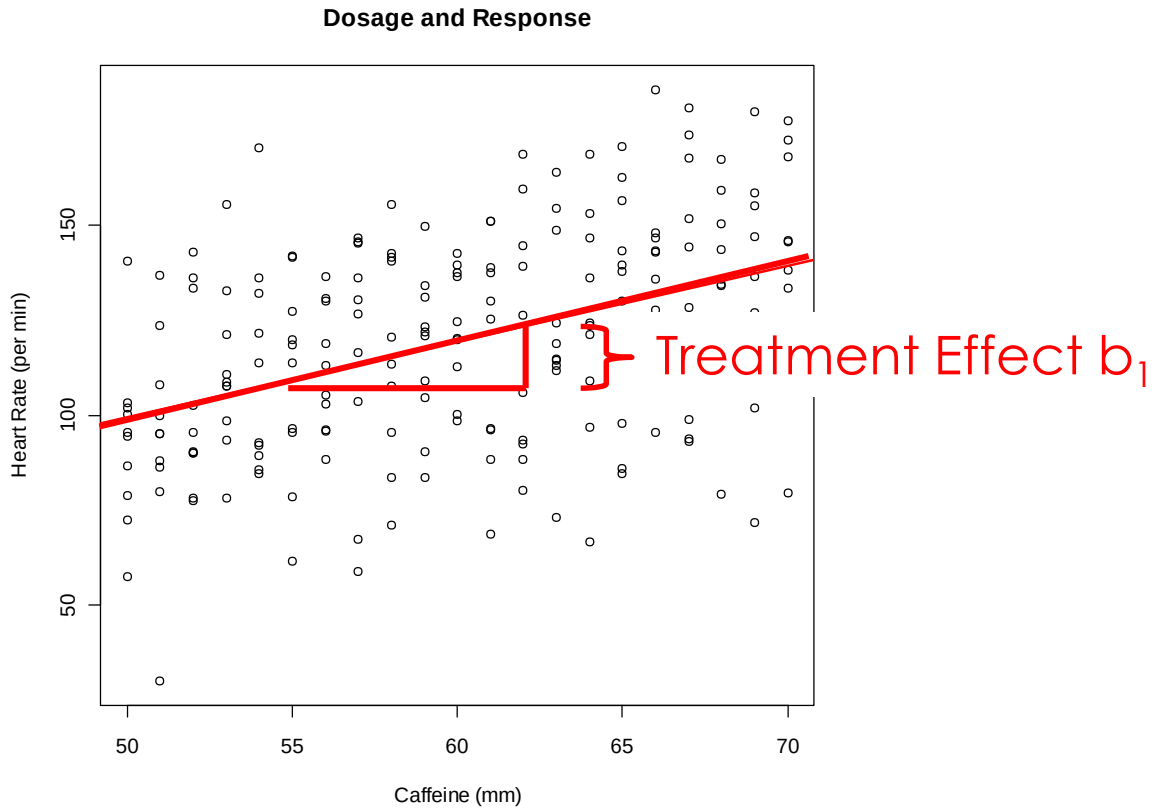
x ⟵ y



$X = b_0 + b_1Y + e$

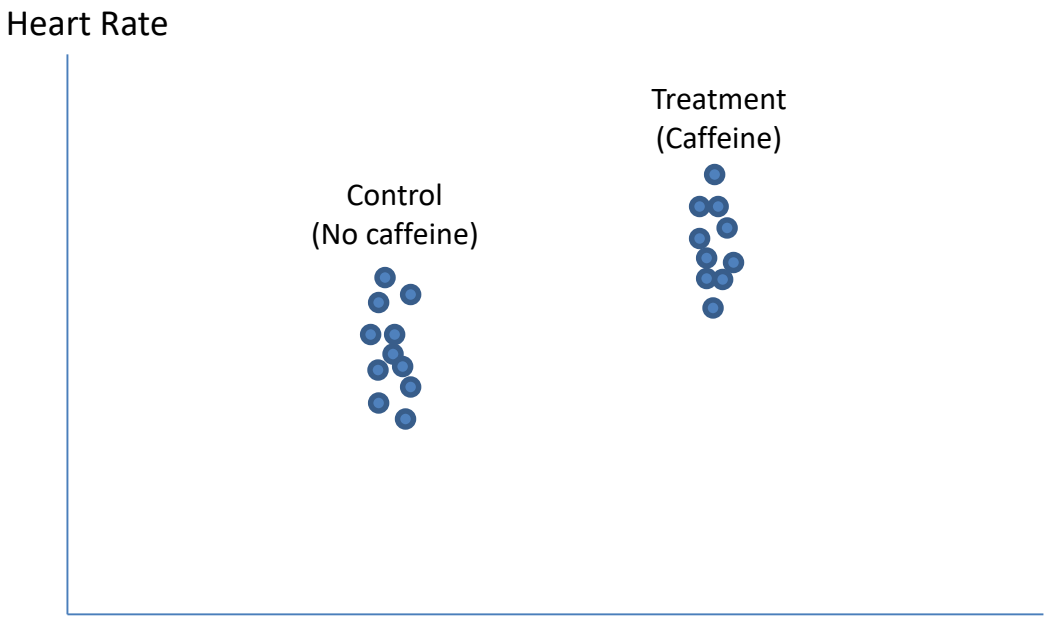
THE PROGRAM EVALUATION FRAMEWORK

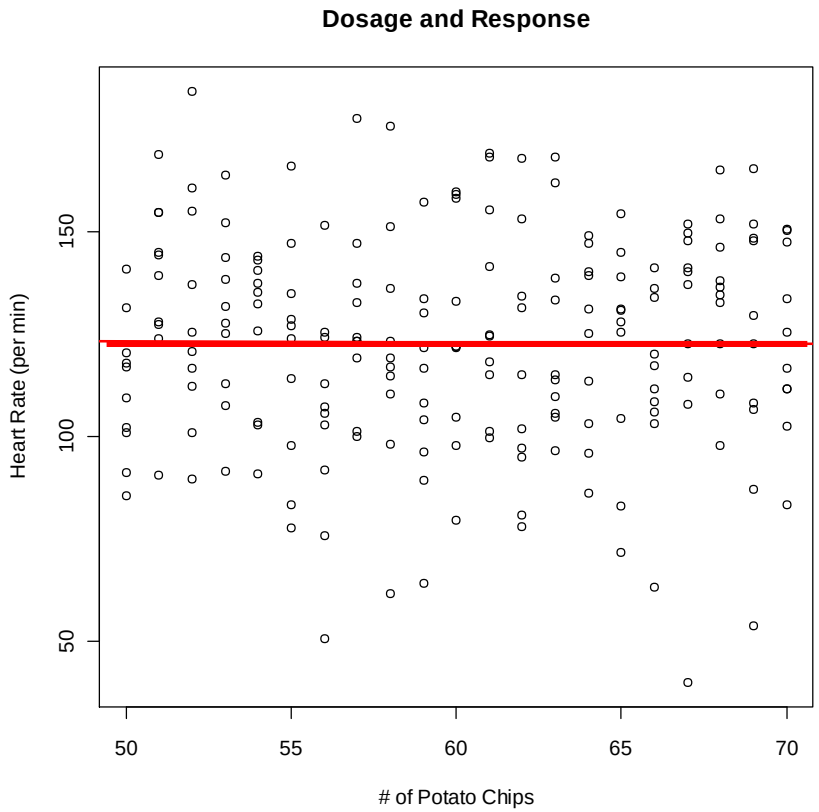
$$Heartrate = b_0 + b_1 \cdot Caffeine + \varepsilon$$



DISCRETE TREATMENT CASE: MODEL IS THE SAME EXCEPT CAFFEINE IS A DUMMY NOT A LEVEL

$$Heartrate = b_0 + b_1 \cdot Caffeine + \varepsilon$$

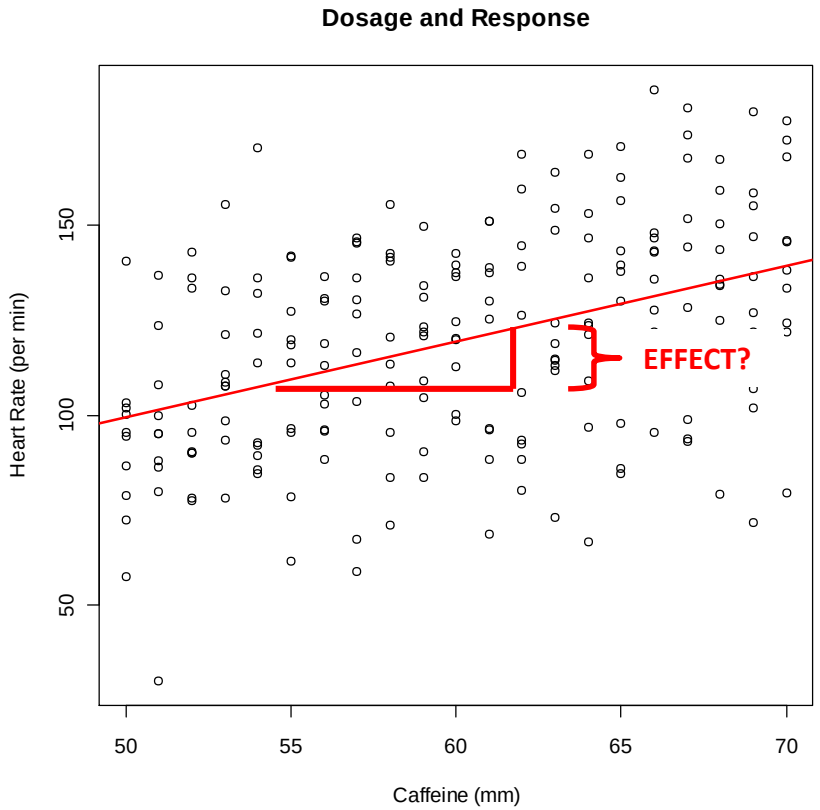


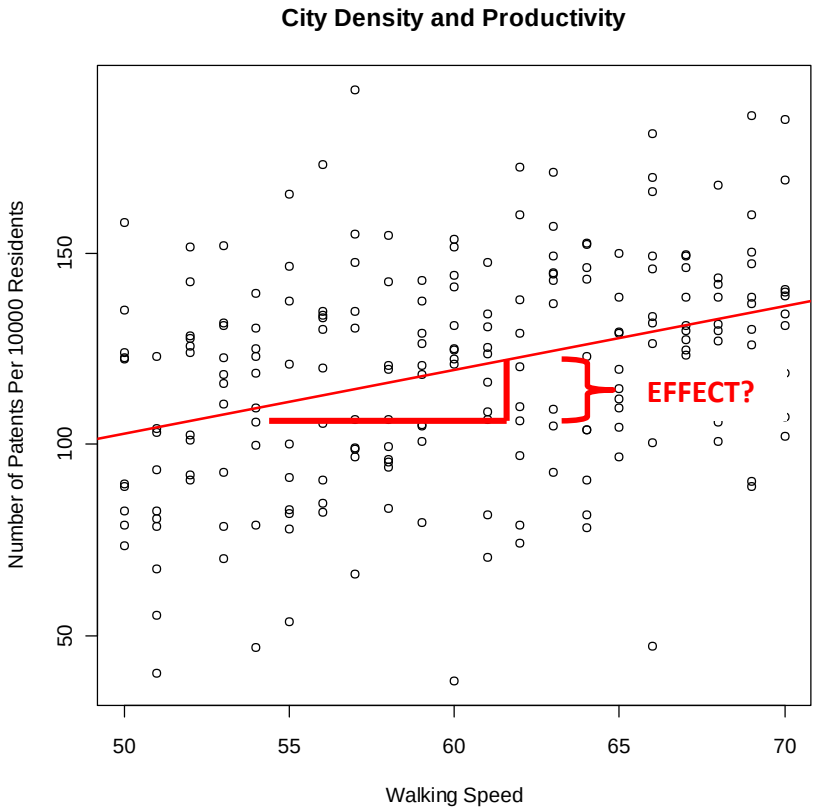


No effect of treatment on outcome

HOW DO WE KNOW WHEN THE INTERPRETATION IS CAUSAL?

When is b_1 an impact, and when is it just a relationship in data?





THE SELECTION PROBLEM IN EVALUATION RESEARCH

Microfinance example of bias from selection INTO a study group

Number of each "type" of person in the study

	NOT Entrepreneurial	Entrepreneurial
No Loan	30	15
Takes a Loan	20	35

You are more likely to take a loan if you know you are good at business

Average weekly income after loan period

	NOT Entrepreneurial	Entrepreneur
No Loan	$10	$20
Takes a Loan	$10	$20

Income not impacted by the loan

Takes Loan?

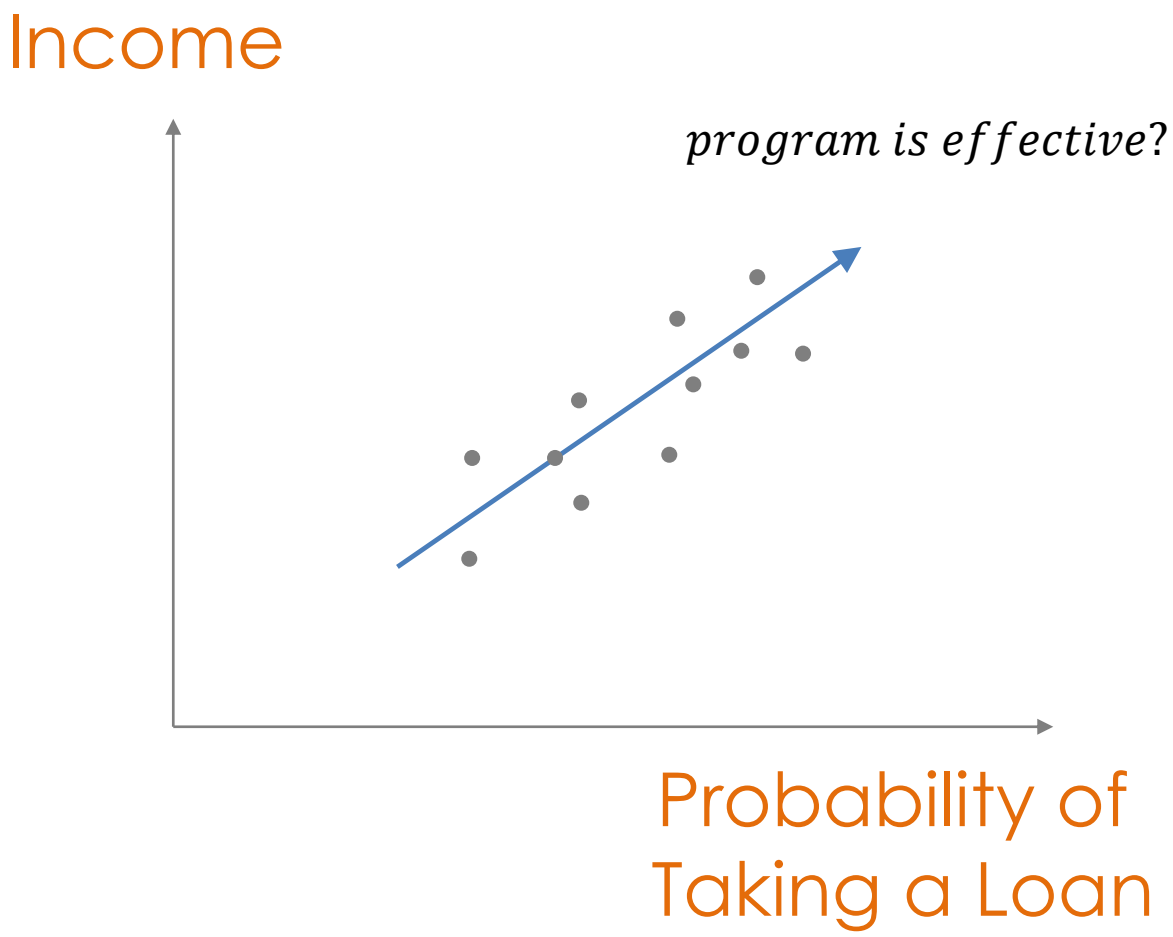
$$NO: \quad \frac{30 \cdot \$10 + 15 \cdot \$20}{45} = \$13.33$$

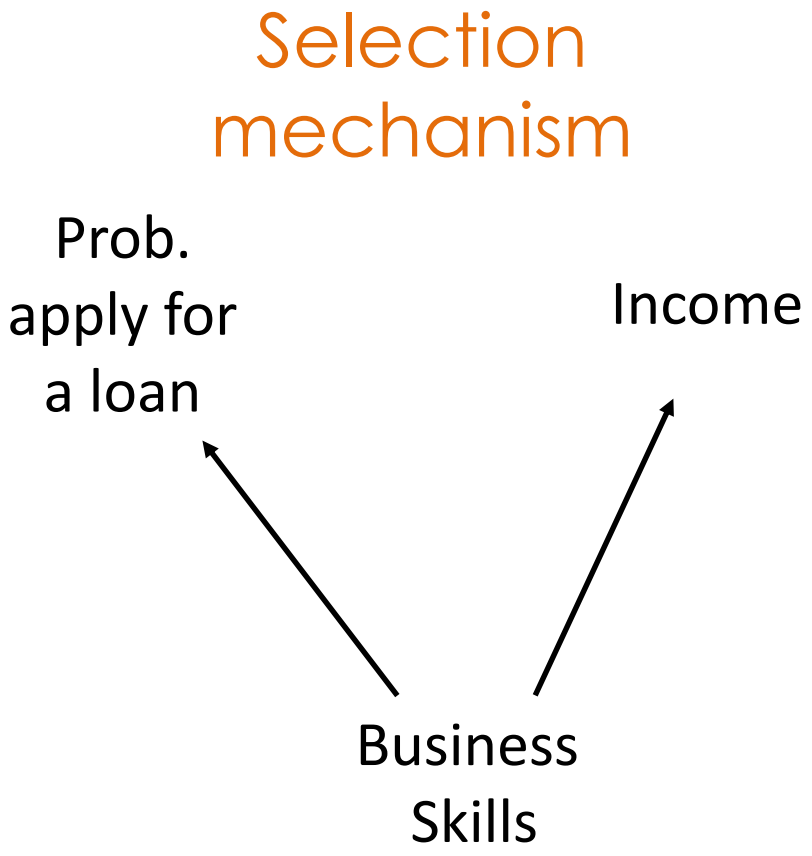
$$YES: \quad \frac{20 \cdot \$10 + 35 \cdot \$20}{55} = \$16.37$$

The loan appears to have an impact!

Even though we know it didn't.

CORRELATION VERSION OF LAST EXAMPLE:





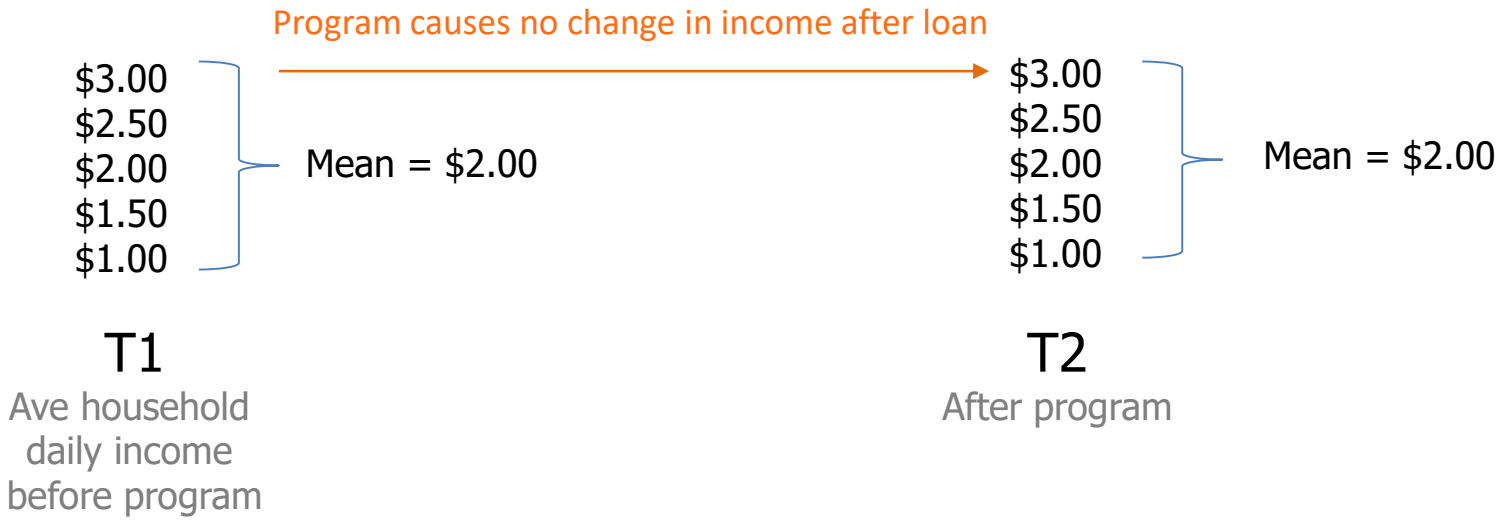
“Selection” Problem

Those that participate in the program
are different from those that do not participate.

This is the biggest problem in impact evaluation!

Microfinance example of bias from selection OUT OF a study group

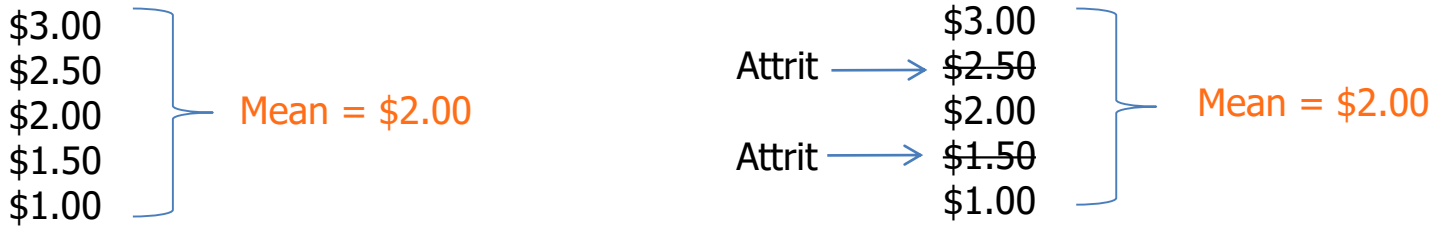
Reflexive design



$$T2 - T1 = 0$$

causal estimate is unbiased

Random Attrition Example

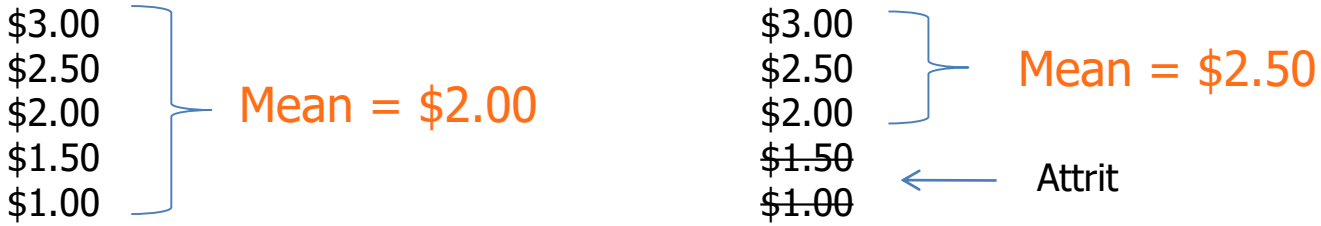


T2 - T1 = 0

Impact study accurately represents program effects

Program is not determined to be effective (no change)

Non-Random Attrition Example

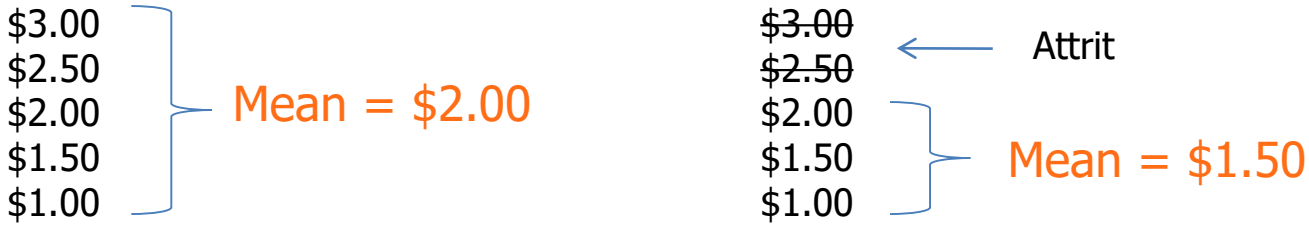


$$T2 - T1 \neq 0$$

We over-estimate program effects

Program appears to be effective

Non-Random Attrition Example



$$T2 - T1 \neq 0$$

We under-estimate program effects

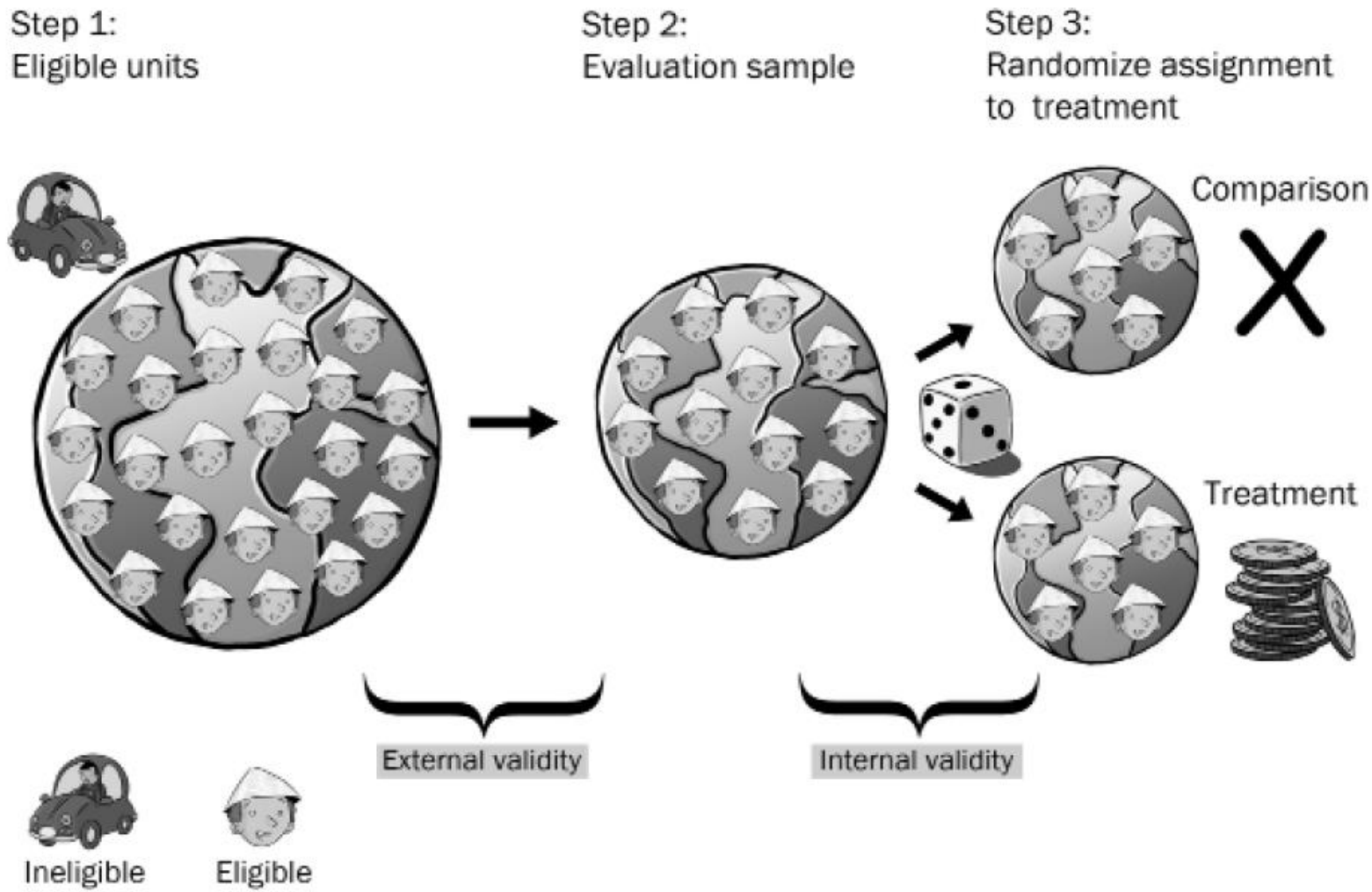
Program appears to harm families

RANDOMIZED CONTROL TRIALS (RCT'S): THE "GOLD STANDARD" FOR INTERNAL VALIDITY

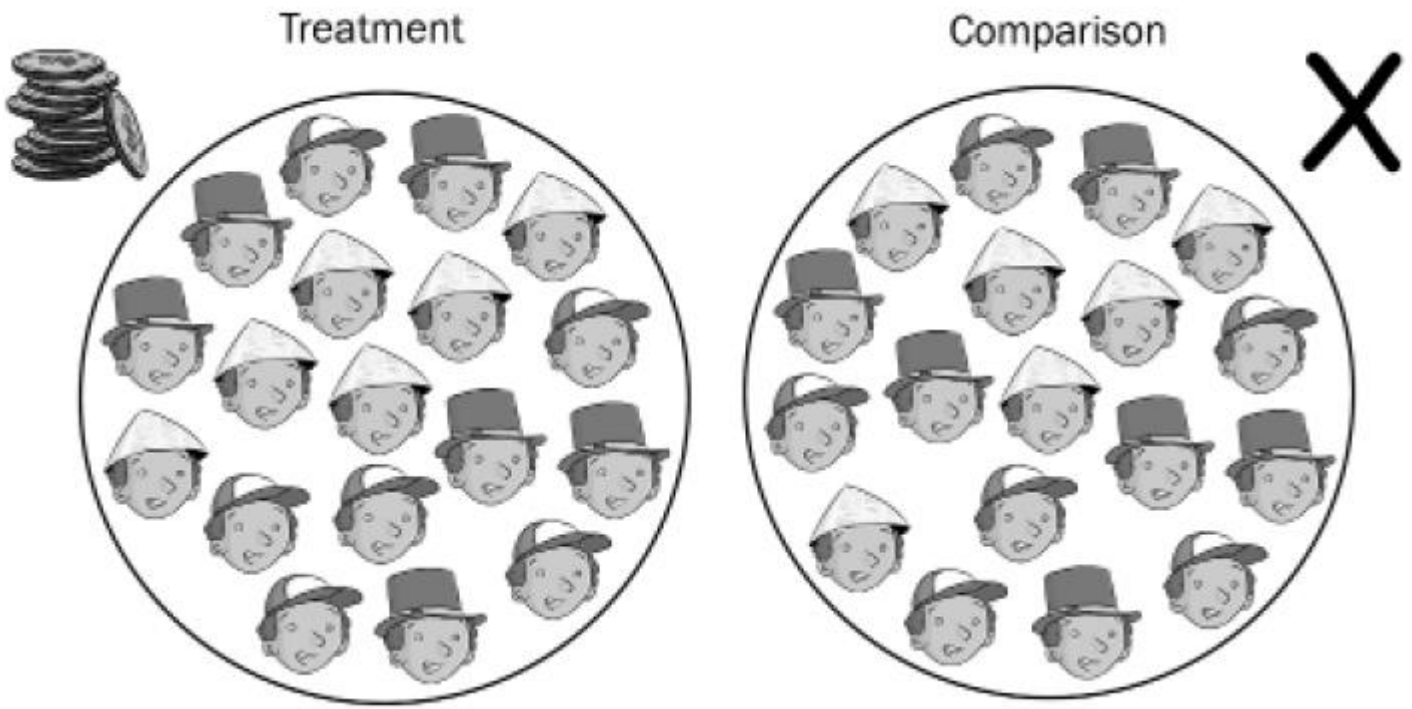
Gertler, P. J., Martinez, S., Premand, P., Rawlings, L. B., & Vermeersch, C. M. (2016). *Impact evaluation in practice.* The World Bank.

>> Chapter 4. Randomized Selection Methods

Figure 4.3 Steps in Randomized Assignment to Treatment



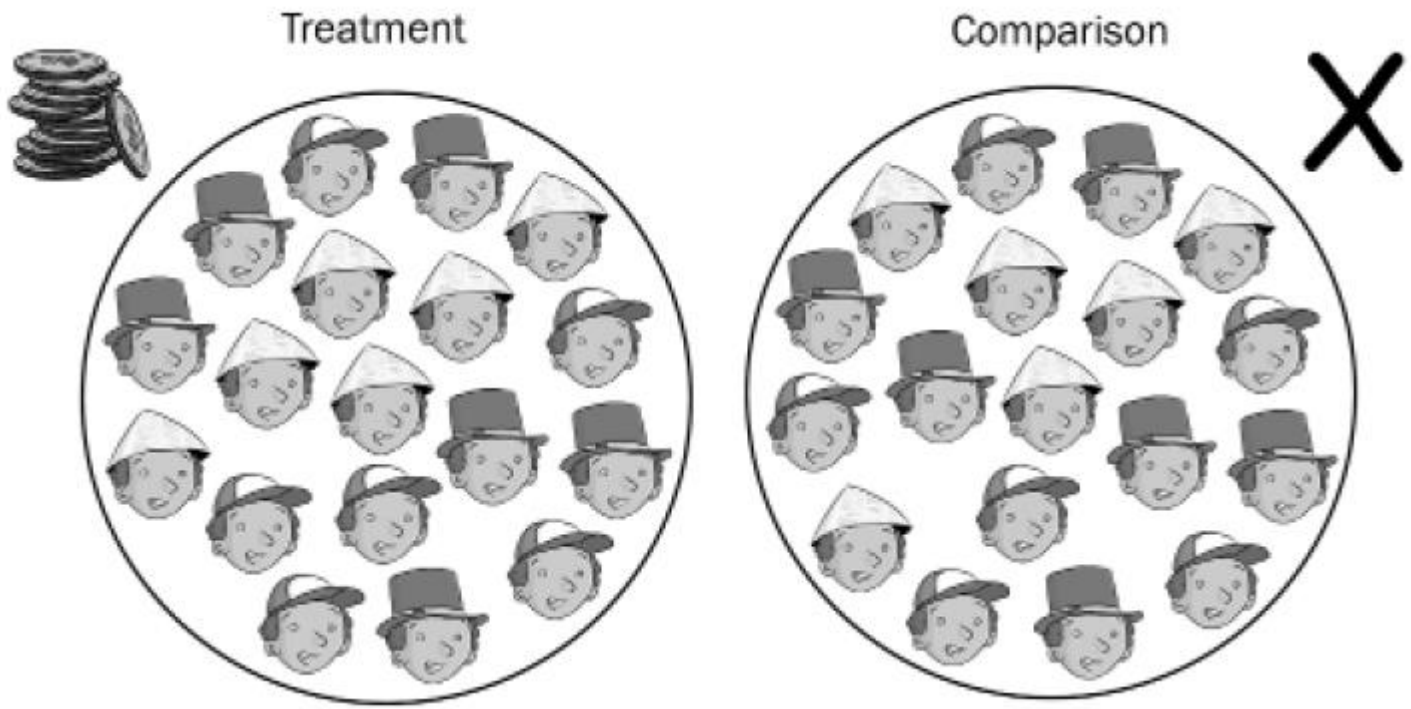
Our counterfactual framework is
valid / robust when the
groups only DIFFER BY THE TREATMENT
but are OTHERWISE "IDENTICAL"



When true, we can interprett the differences in group outcomes
after the treatment period to be caused by the treatment

How do we test the criteria:

groups only OTHERWISE "IDENTICAL" ???



HAPPY RANDOMIZATION

If we have a group of 100 people and we randomly assign them to two groups, 50 people each, how often would we expect the average weight of each group to differ?

If we have a group of 100 people and we randomly assign them to two groups, 50 people each, how often would we expect the average weight of each group to differ?

MATHEMATICALLY: ALWAYS !!!

weights will never be exactly identical

so what do we mean by “different”?

If we have a group of 100 people and we randomly assign them to two groups, 50 people each, how often would we expect the average weight of each group to differ?

STATISTICALLY:

TEST OF GROUP MEANS

Use a t-test and select a
level of confidence
that we are comfortable with

If we have a group of 100 people and we randomly assign them to two groups, 50 people each, how often would we expect the average weight of each group to differ?

STATISTICALLY:

TEST OF GROUP MEANS

What does alpha=0.05 mean?

[--- 95% confidence interval ---]

If we have a group of 100 people and we randomly assign them to two groups, 50 people each, how often would we expect the average weight of each group to differ?

STATISTICALLY:

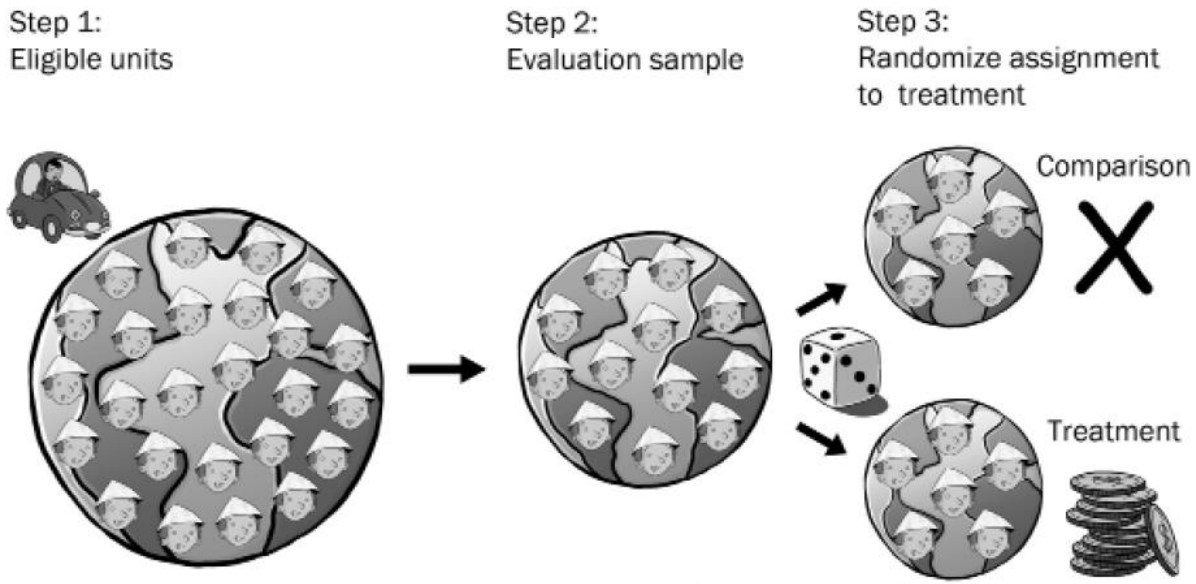
TEST OF GROUP MEANS

5 times out of 100 the two samples are drawn from the same population (or weight distribution), but we will still consider them to be different.

What does alpha=0.05 mean?

[--- 95% confidence interval ---]

How often will randomization "fail"?



By definition of a test at a 95% confidence level, each measured characteristic like weight will differ

5 out of 100 times

"Unhappy Randomization"

Unhappy randomization is not failed randomization (process applied improperly), rather just bad luck of the draw

Table 4.1 Case 3—Balance between Treatment and Comparison Villages at Baseline

Household characteristics	Treatment villages (N = 2964)	Comparison villages (N = 2664)	Difference	*t*-stat
Health expenditures ($ yearly per capita)	14.48	14.57	−0.09	−0.39
Head of household's age (years)	41.6	42.3	−0.7	−1.2
Spouse's age (years)	36.8	36.8	0.0	0.38
Head of household's education (years)	2.9	2.8	0.1	2.16*
Spouse's education (years)	2.7	2.6	0.1	0.006
Head of household is female = 1	0.07	0.07	−0.0	−0.66
Indigenous = 1	0.42	0.42	0.0	0.21
Number of household members	5.7	5.7	0.0	1.21
Has bathroom = 1	0.57	0.56	0.01	1.04
Hectares of land	1.67	1.71	−0.04	−1.35
Distance to hospital (km)	109	106	3	1.02

"contrasts"

Source: Authors' calculation.

* Significant at the 5 percent level.

The most important table in every study: comparisons of treatment and control group characteristics

For the counterfactual to be **valid**, the groups can ONLY differ by the treatment, not by any measured traits.

Is this problematic?

What is the appropriate test for "identical" or equivalent groups?

We should observe no differences in measured traits.
Assume a 95% confidence interval.

Bonferroni Correction:

When we want to be 95% confident that two groups are the same, and we can measure those groups using a set of contrasts, then our decision rule is no longer to reject the null (that the groups are the same) if the p-value < 0.05. A “contrast” is a comparison of means of any measured characteristic between two groups.

If we have a 5% chance of observing a p-value of less than 0.05 for each contrast, then the probability of observing at least one contrast with a p-value that small is greater than 5%! It is actually n*0.05 (minus prob of observing multiple < 0.05 at same time) where n is the number of contrasts.

So if we want to be 95% confident that the groups are different (not just the contrasts), we have to adjust our decision rule to α/n.

For example, if we have 10 contrasts, then our decision rule is now 0.05/10, or 0.005. The p-value of at least one contrast must be below 0.005 for us to conclude that the groups are different.

```
x1 <- rbinom( 10000, 6, 0.05 )
table( x1 ) / 10000
y1 <- rbinom( 10000, 6, 0.05/6 )
table( y1 ) / 10000
```

Test for Group Equivalence

TABLE 2

Background Characteristics of Students in Treatment and Control Groups (Total numbers of cases in parentheses)

Characteristic	*All students in the study*			*All students with scores three or four years after application*		
	Choice students	*Control students*	*p value*[a]	*Choice students*	*Control students*	*p value*[a]
Math scores before application	39.7 (264)	39.3 (173)	.81	40.0 (61)	40.6 (33)	.86
Reading scores before application	38.9 (266)	39.4 (176)	.74	42.1 (60)	39.2 (33)	.35
Family income	10,860 (423)	12,010 (127)	.14	10,850 (143)	11,170 (25)	.84
Mothers' education 3 = some college 4 = college degree	4.2 (423)	3.9 (127)	.04	4.1 (144)	3.8 (29)	.15
Percent married parents	24 (424)	30 (132)	.17	23 (145)	38 (29)	.11
Parents' time with children 1 = 1–2 hours/week 2 = 3–4 hours/week 3 = 5 or more	1.9 (420)	1.8 (130)	.37	1.9 (140)	1.7 (27)	.26
Parents' education expectations of children 4 = college 5 = graduate school	4.2 (422)	4.2 (129)	.85	4.2 (142)	3.7 (27)	.01

a. The tests of significance are suggestive of the equivalence of the two groups. Technically, tests of significance should be done at each point of random assignment, but the number of cases at each point is too few for such tests to be meaningful.

Smallest p-value in table

New alpha = 0.05 / 6 = 0.0083

0.04 > 0.0083

Do not reject :: Groups are equivalent

RCT versus Natural Experiments:

1. **RCT assumes complete control over the assignment process**

2. **Natural Experiments often utilize randomization:**

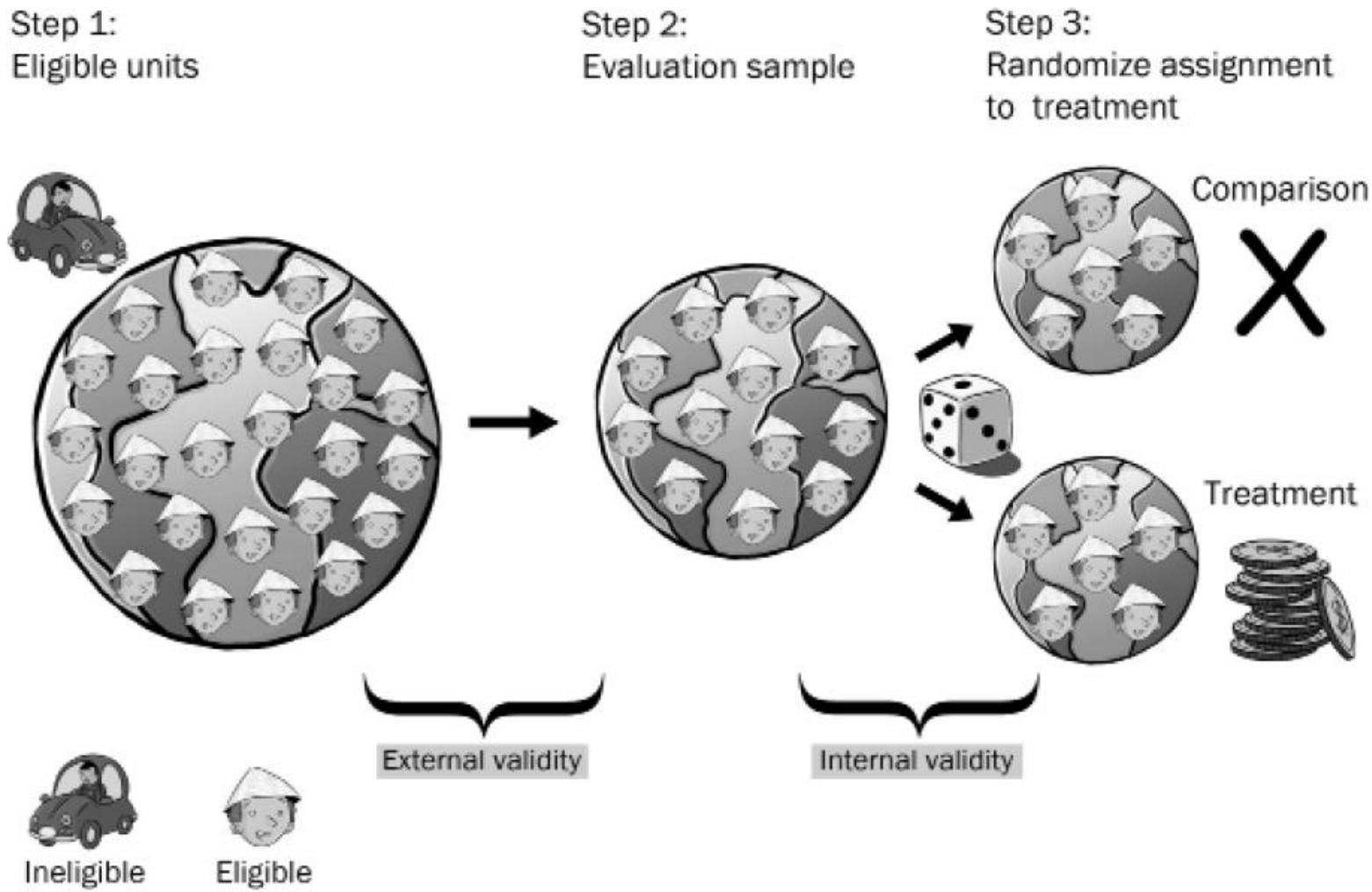
   → **Charter School lotteries**

   → **Vietnam draft**

3. **Quasi-Experimental techniques can use other methods to create group equivalence (for example, matching)**

ATTRITION

Figure 4.3 Steps in Randomized Assignment to Treatment



Tests for Selection-Into Study Group

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Non-Random Attrition

If the people that leave a program or study are different than those that stay, the calculation of effects will be biased.

The Fix:

Examine characteristics of those that stay versus those that leave.

Non-Random attrition tests for selection OUT of the study group

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Percent married parents	24 (424)	30 (132)	.17	23 (145)	38 (29)	.11
Parents' time with children 1 = 1–2 hours/week 2 = 3–4 hours/week 3 = 5 or more	1.9 (420)	1.8 (130)	.37	1.9 (140)	1.7 (27)	.26
Parents' education expectations of children 4 = college 5 = graduate school	4.2 (422)	4.2 (129)	.85	4.2 (142)	3.7 (27)	.01

a. The tests of significance are suggestive of the equivalence of the two groups. Technically, tests of significance should be done at each point of random assignment, but the number of cases at each point is too few for such tests to be meaningful.

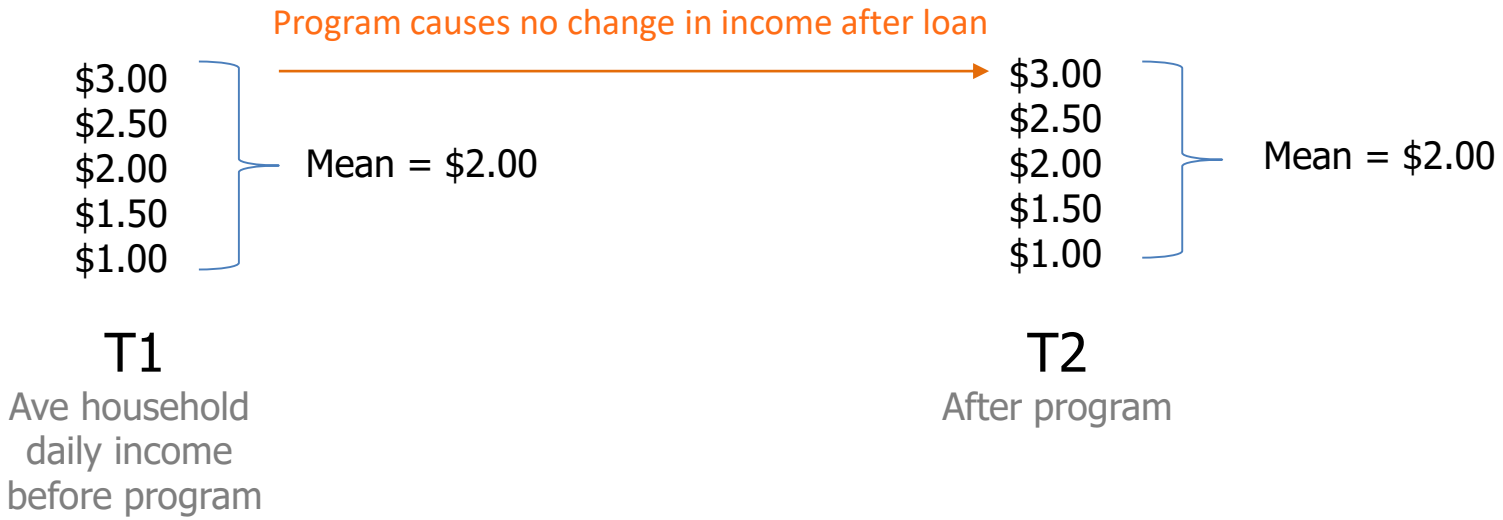
Do group traits differ after attrition occurs?

Attrition is natural, the question is whether it is random (will not change the groups) or non-random (will change the groups)

Can also be tested by comparing traits of those that stay to those that leave.

Microfinance example of bias from selection OUT OF a study group

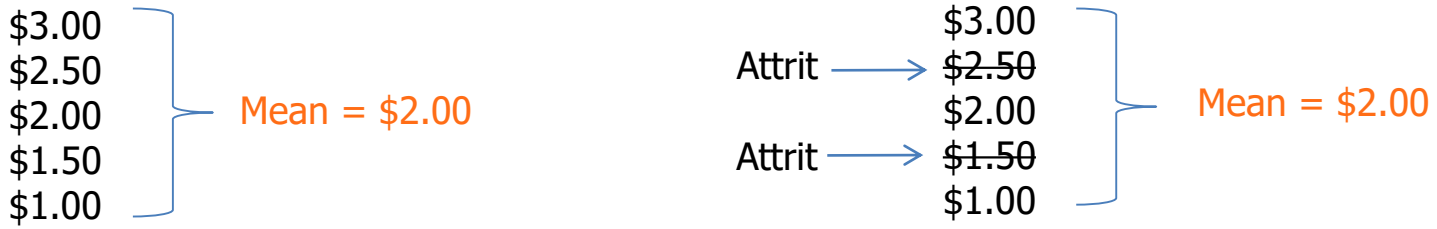
Reflexive design



$$T2 - T1 = 0$$

causal estimate is unbiased

Random Attrition Example

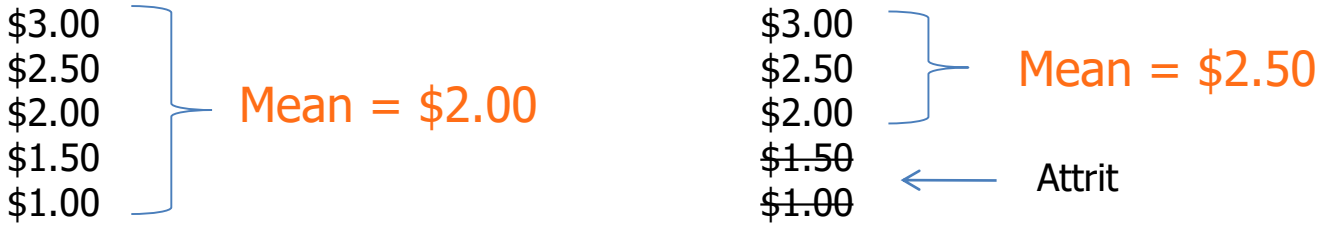


T2 - T1 = 0

Impact study accurately represents program effects

Program is not determined to be effective (no change)

Non-Random Attrition Example

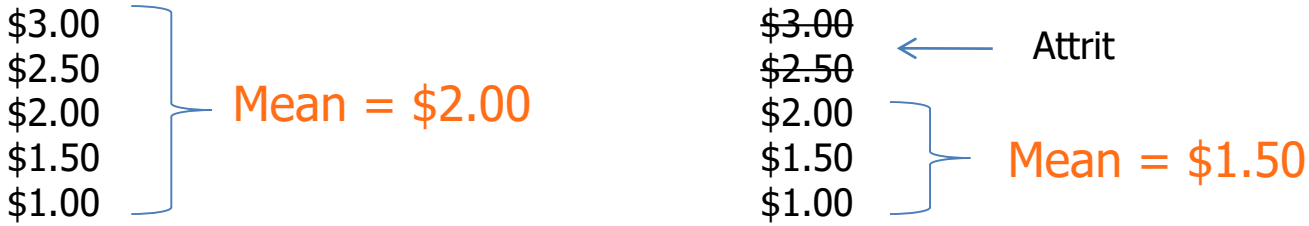


$$T2 - T1 \neq 0$$

We over-estimate program effects

Program appears to be effective

Non-Random Attrition Example



$$T2 - T1 \neq 0$$

We under-estimate program effects

Program appears to harm families