Causal Inference

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CASA0006: Data Science for Spatial Systems

Adapted from and using slides from Ollie Ballinger

Causal questions

Inferring the effects of any treatment/policy/intervention

- What is the effect of a given treatment on a disease?
- What is the effect of climate change policy on emissions?
- What is the effect of social media on mental health?
- What is the effect of a new underground station on house prices?
- What is the effect of advertising on sales?
- What is the effect of teachers’ training intervention on student learning?

Prediction vs. Causal Inference

Prediction

- Objective: to predict what comes next based on past data.
- Focus: accuracy of predictions without necessarily understanding underlying causal mechanisms.

- Can we predict stock prices based on historical data (without necessarily understanding the factors driving market movements).
- Can we predict whether an image contains a cat or a dog based on extracted features (without understanding factors that distinguish cats from dogs).

Causal inference

- Objective: Understand causal relationships between variables, answer the ‘why’ question.
- Focus: establishing cause—and-effect relationships underlying observed correlations.

- Does minimum wage cause reductions in employment rates?
- Does a new underground line station cause displacement?

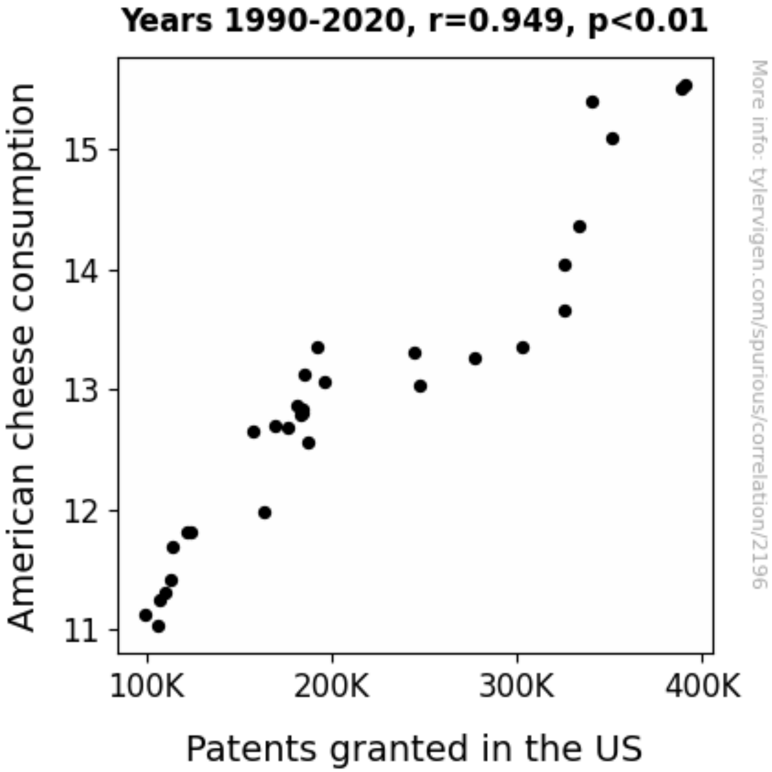
Defining causality

- Causality refers to the relationship between *cause* and *effect*
    - Direct influence of one variable on another
    - Changes in the cause variable result in changes in the effect variable

- Correlation refers to a statistical relationship between two variables.
    - Changes in one variable are accompanied by changes in another variable
    - Does not imply causation.

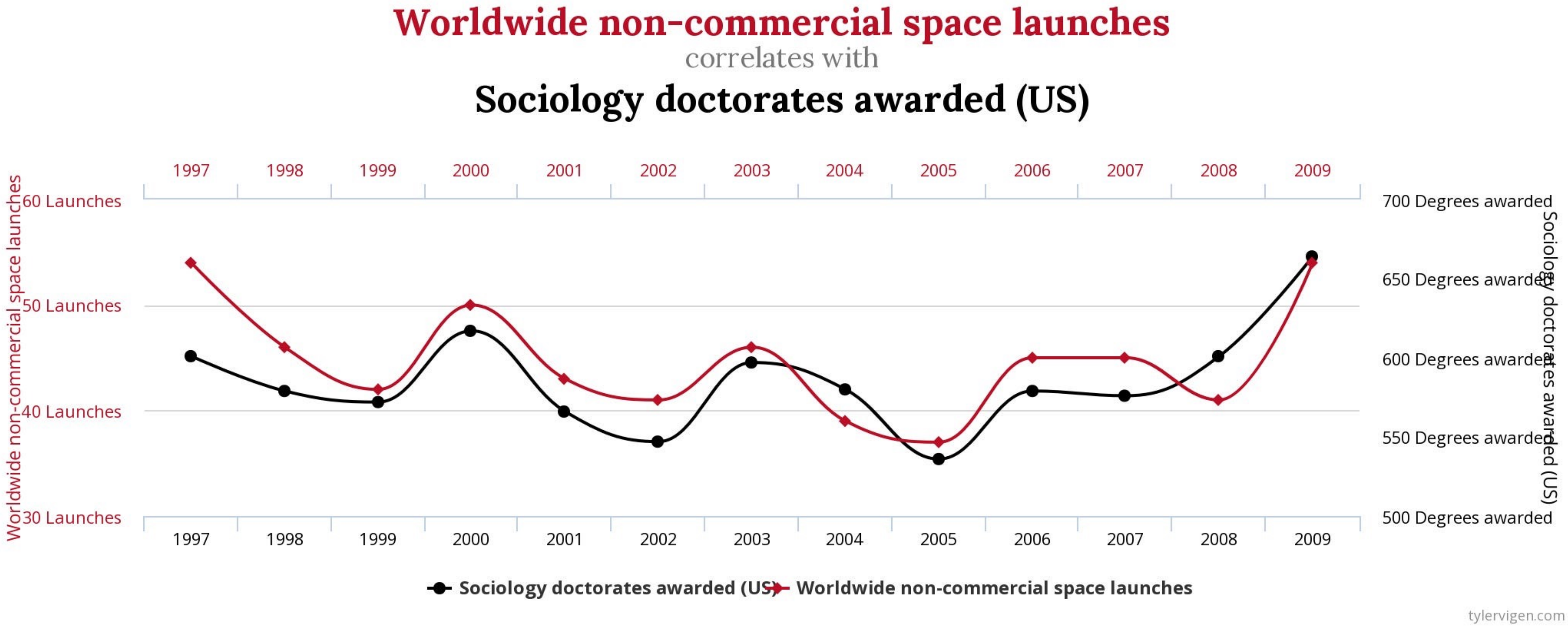
Causation vs. causality

- Ice Cream and Sunglasses Sales:
  - Correlation: during summer months, there's a correlation between higher ice cream sales and sunglasses sales
  - Causation: Both are driven by warmer weather (the cause)



Courtesy of Tyler Vigen

Causation vs. causality

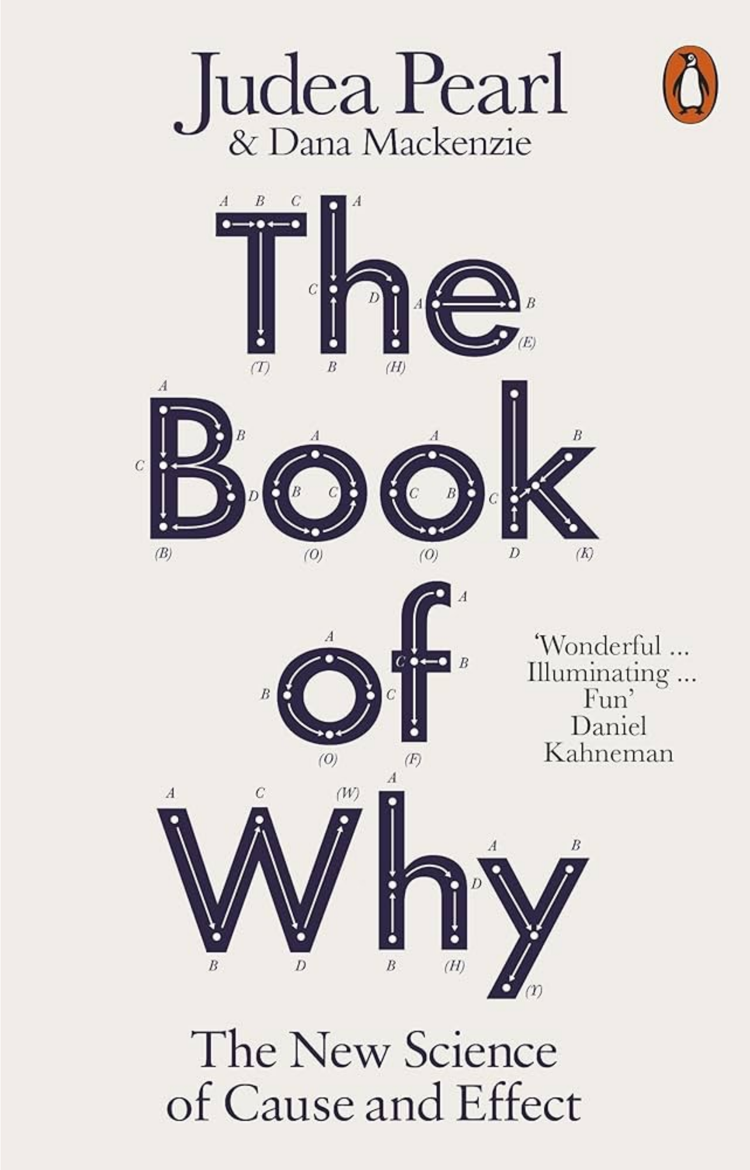


Courtesy of Tyler Vigen

Defining causality – directionality

- Smoking and lung cancer
    - Research has shown smoking is a leading cause of lung cancer.
    - While smoking increases the risk of developing lung cancer, having lung cancer does not cause smoking.
    - Directionality is from smoking to lung cancer.

- Rainfall and surface wetness
    - Rainfall causes surfaces to become wet.
    - Surface wetness does not cause rainfall.
    - Directionality is from rainfall to surface wetness.


Judea Pearl
& Dana Mackenzie
The Book of Why
'Wonderful ...
Illuminating ...
Fun'
Daniel
Kahneman
The New Science
of Cause and Effect

Potential outcomes framework

Each individual has two potential outcomes:

- Potential outcome if the individual is not treated “control outcome”
- Potential outcome if the individual receives treatment “treated outcome”

Observed factual outcome –

all we can see is what happened under one intervention

Unobserved counterfactual outcome –

what would have happened if they have received the opposite treatment

The fundamental problem of causal inference

We only ever observe one of the two outcomes.

Randomized controlled trials (RCTs)

Design an experiment where we

Randomize subjects into treatment groups and control group

- The treatment is determined randomly! There cannot be a causal parent!
- Treatment and control groups should be comparable.

Golden standard for causal inference – randomly assign participants to treatment and control groups to assess causal impact of an intervention

Causation in observational studies

- Can’t always randomize treatments
    - Ethical reasons: smokers vs. non-smokers for measuring the effect on lung cancer
    - Feasibility: can’t randomize public transport infrastructure investments,

Outline

1. Panel Regression
2. Case Study 1: The Mariel Boatlift
3. Difference in Differences
4. Case Study 2: Driving Bans in China

FIGHT FOR $15 AND
Walmart Workers
#FightFor15
Walmart
$15.00
PER HOUR
RAISE AMERICA
$15
Fast Food Workers
#FightFor15
HOTEL TRADES COUNCIL
HTC
AFL-CIO
Childcare Workers
#FightFor15
$
1
5
Laundry Workers
#FightFor15
Airport Workers
#FightFor15
FIGHT FOR $15
AND RAISE AMERICA
Trades
#FightFor15
Retail Workers
#FightFor15

Coalition of 62 Groups, Activists, and Legislators Oppose $15 Federal Minimum Wage

AMERICANS for TAX REFORM

60 PLUS AMERICAN ASSOCIATION OF SENIOR CITIZENS

ALASKA POLICY FORUM

AMERICAN COMMITMENT

AMERICANS FOR PROSPERITY

ALEC American Legislative Exchange Council LIMITED GOVERNMENT • FREE MARKETS • FEDERALISM

February 2, 2021

Dear Member of Congress,

On behalf of millions of taxpayers across the country, we urge you to reject proposals to increase the federal minimum wage at a time of unprecedented economic calamity, including President Joe Biden's push to raise the minimum wage to $15/hour, more than doubling the current minimum wage of $7.25/hour.

President Biden's recent $1.9 trillion "American Rescue Plan" calls on Congress to more than double the federal minimum wage to $15/hour and eliminate the "tipped" minimum $2.13/hr wage for servers. The Biden proposal likely mirrors legislation passed by the House in 2019 and reintroduced in 2021, the *Raise the Wage Act*, which increases the minimum wage to $15 by 2025, indexes it to inflation, and repeals the tipped minimum wage for servers.

A $15 minimum wage would substantially raise the cost of labor at a time when small businesses are already struggling to keep the lights on. Small businesses with thin margins would be forced to pass the costs onto consumers, which could lead to a decline in businesses, a loss of revenue, and layoffs. Businesses that have closed temporarily due to the pandemic may decide not to reopen at all in the face of a higher minimum wage, and many employers will forgo hiring new workers because they cannot afford them.

Panel Regression

Panel Data

Each point represents the employment rate of a state in a given year. How would you model the relationship between the two?

Pooled OLS

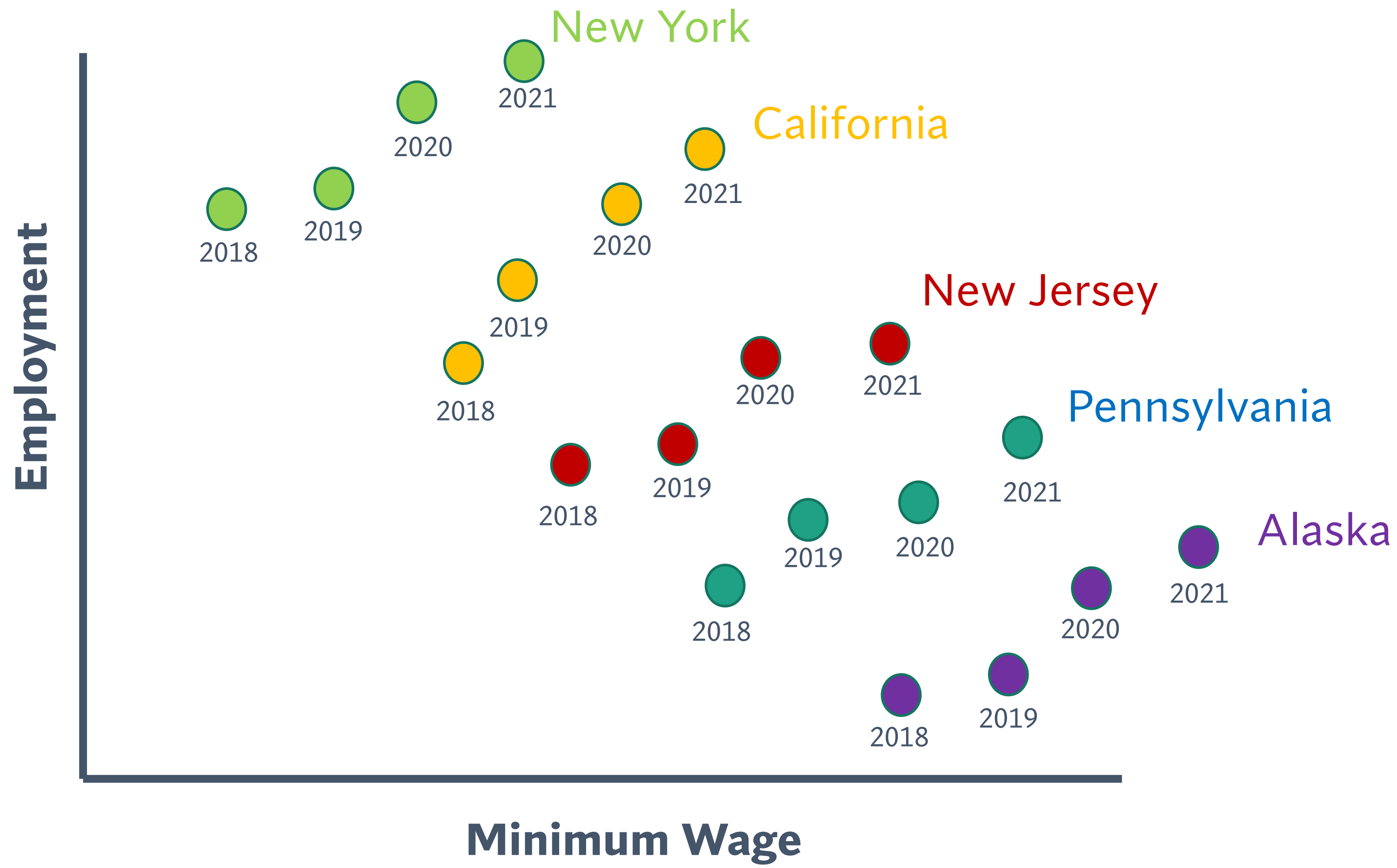
Suppose we don't distinguish between states or years, and just pool all the data together:

$$Y_i = \beta_0 + \beta_1 X_i + \varepsilon_i$$

$\beta_1 < 0$; higher minimum wages decrease employment

Fixed Individual-Level Effects

Points are now colored by state and labeled by year. How would you model the relationship between minimum wage and employment now?



Fixed-Effects Panel Regression

Fixed effects are variables that are constant over time;

- E.g. sex or ethnicity for individuals
- Geographic size or terrain for countries/states

These variables don't change or change at a constant rate over time, but they may affect the outcome

- individual ability
- State or country specific policies

!! Including fixed effects aims to account for unobserved factors that could bias the estimated relationship between the independent and dependent variables. It helps to mitigate omitted variables bias.

Fixed Effects

$$Y_{it} = \alpha_i + \beta_1 X_{it} + u_{it}$$

Y_{it}: Dependent Variable observed for individual i in time t

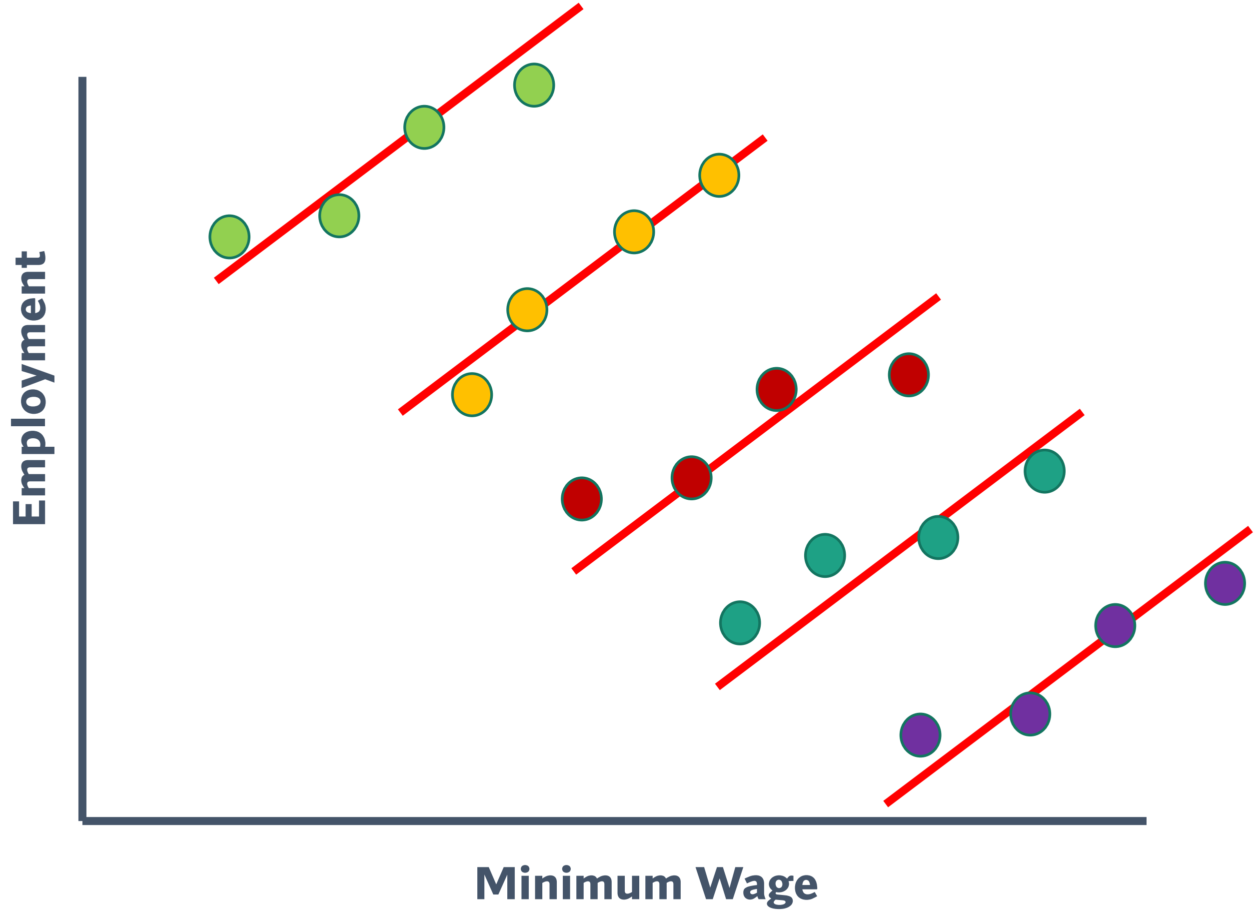
α_i: time-invariant fixed effect

β_1: slope coefficient

X_{it}: Independent Variable observed for individual i in time t

u_{it}: Error term

$$\boldsymbol{\beta_1 > 0}$$



The Fixed Effects Estimator

$$Y_{it} - \bar{Y}_i = (\alpha_i - \bar{\alpha}_i) + \beta_1(X_{it} - \bar{X}_i) + (u_{it} - \bar{u}_i)$$

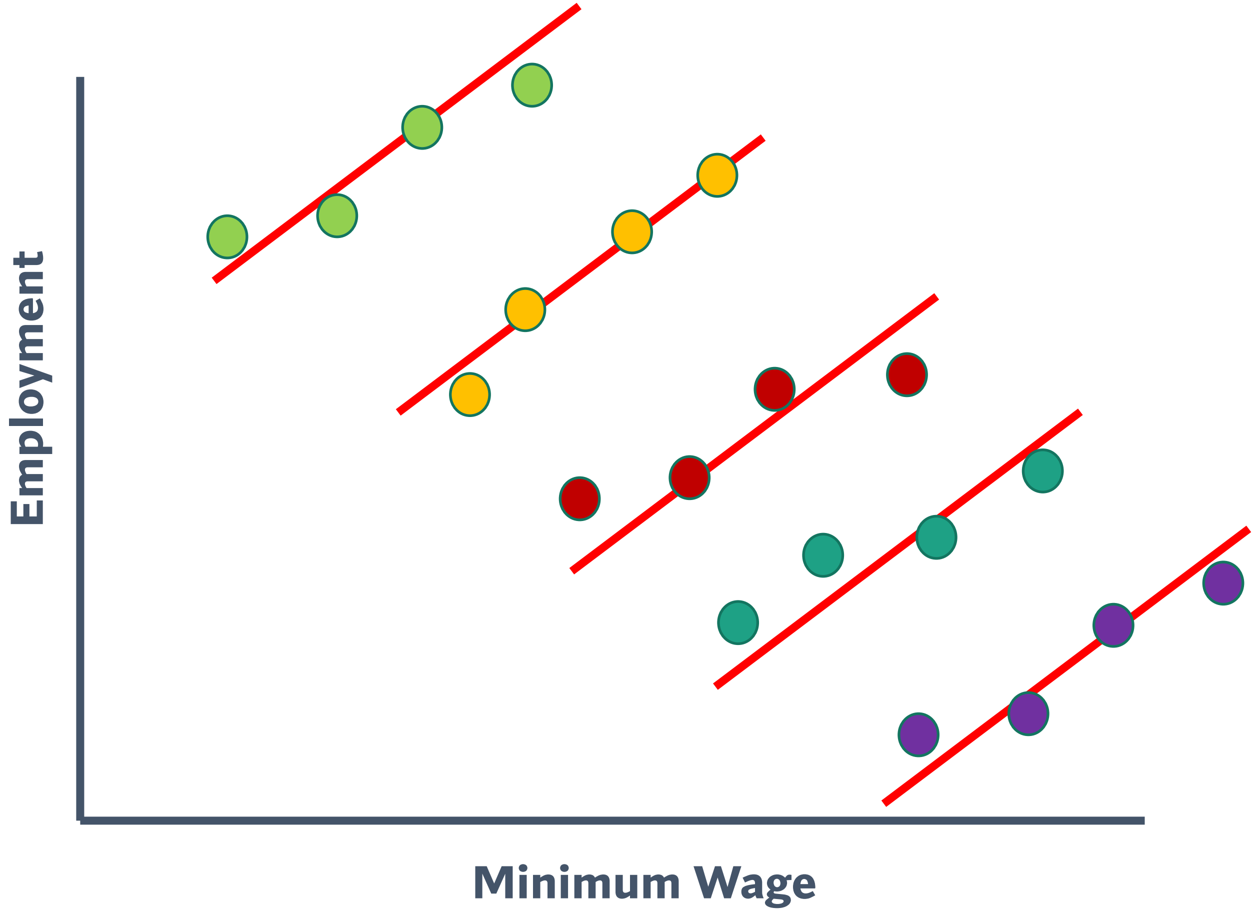
$$\ddot{Y}_{it} = \beta_1 \ddot{X}_i + \ddot{u}_{it}$$

For all $t = 1, \ldots, T$

Where:

$$\bar{Y}_{it} = \frac{1}{T}\sum_{t=1}^{T} Y_{it}$$

$$\bar{X}_{it} = \frac{1}{T}\sum_{t=1}^{T} X_{it}$$

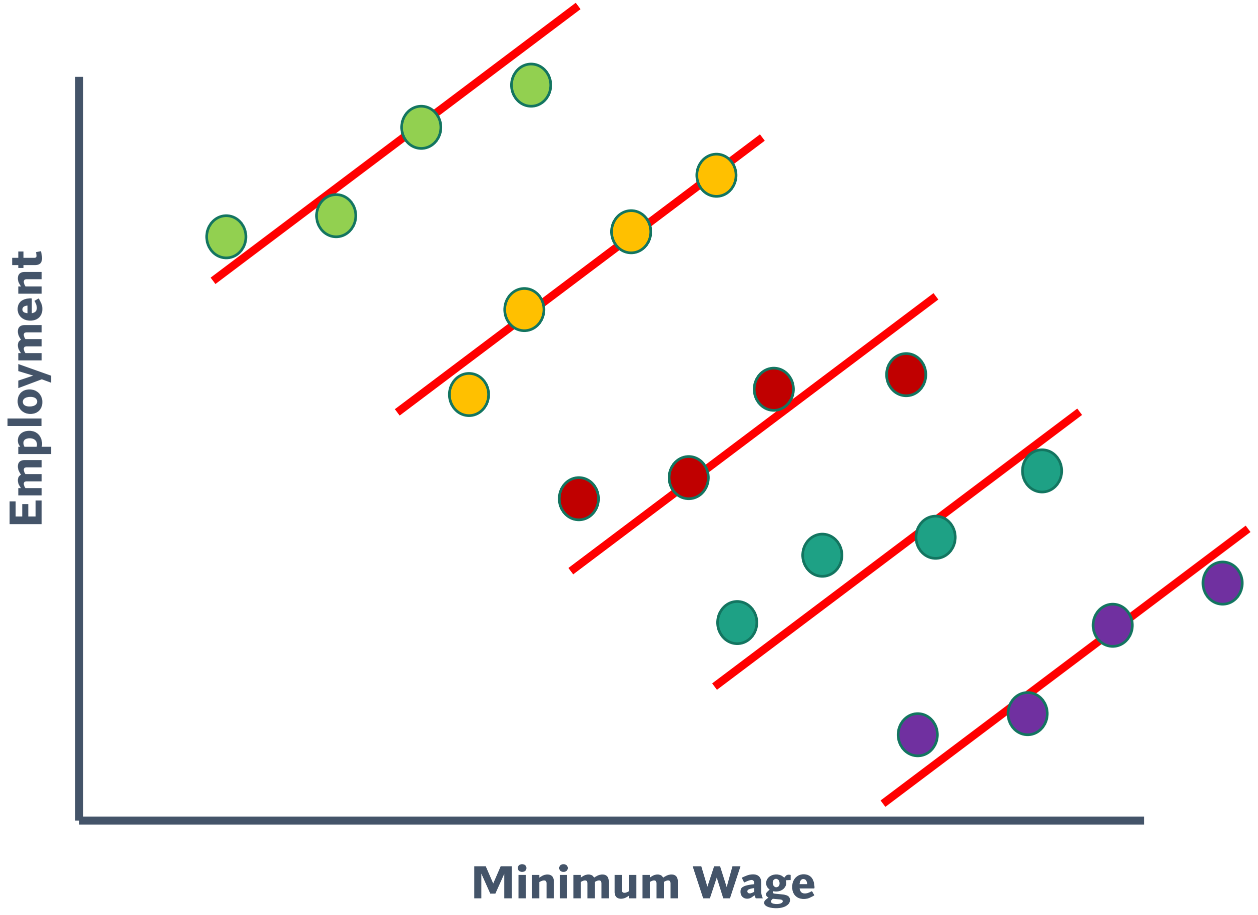


Interpretation

When we have panel data (repeat observations of the same individuals *i* over multiple time periods *t*), there are two components to the variation:

- Within
- Between

A fixed effects model allows us to control for variation **between** groups due to time-invariant characteristics, and focus on identifying meaningful variation **within** groups.



Correlation vs. Causation

Just because we observe a correlation between X and Y, we cannot say that X causes Y.

We observe that broadly, as minimum wages increase over time, poverty decreases. Our regression returns statistically significant results. Why might we hesitate in saying for certain that increasing the minimum wage decreases poverty?

Endogeneity

- Occurs when the independent variable X is correlated with the error term e.

Endogeneity

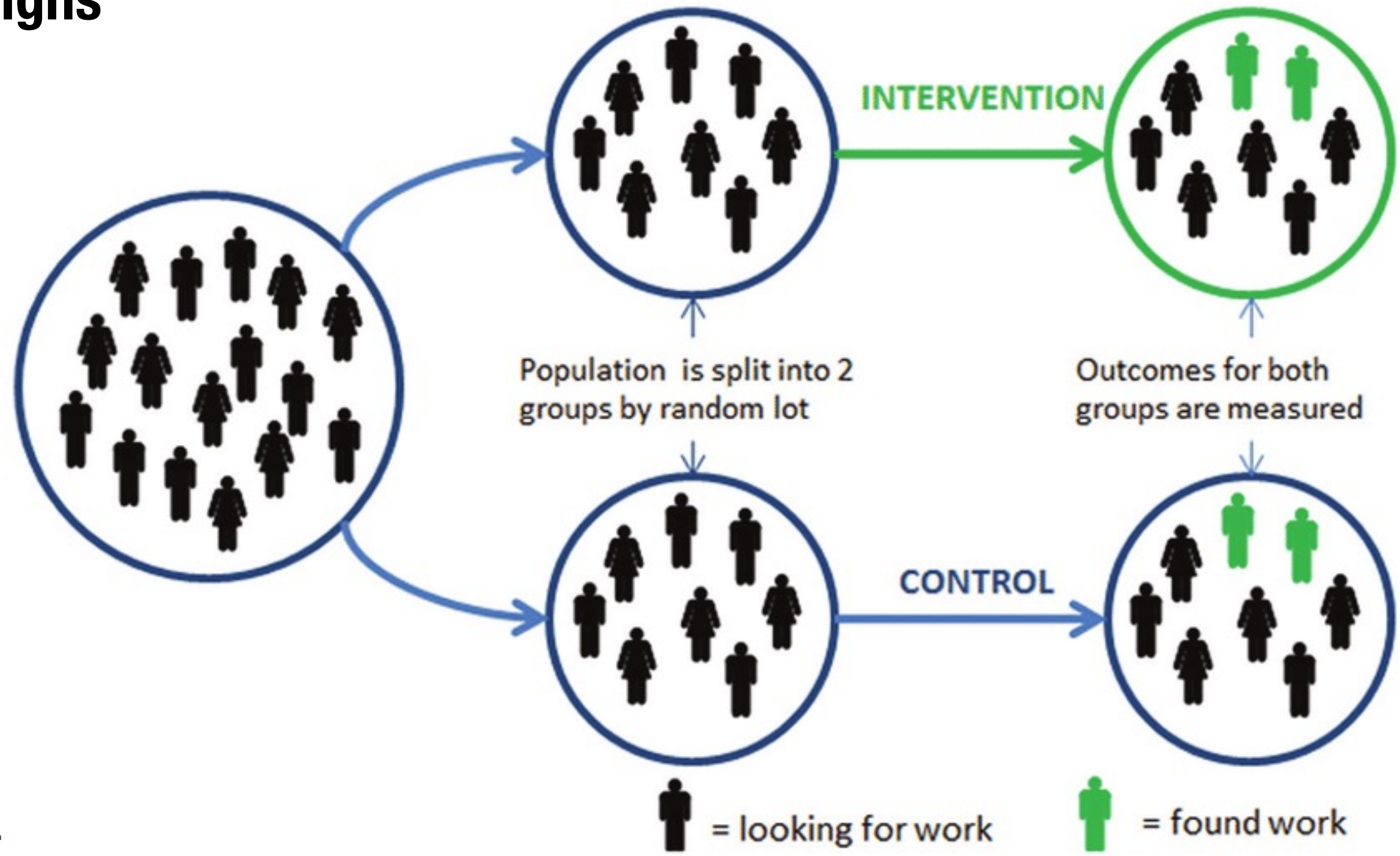
1. **Omitted Variables**
    - The relationship between X and Y is mediated by an unobserved variable Z.
    - E.g. i run a regression between the percent of a district that is forest and the total number of crimes in a year. Massive negative effect. Do trees reduce crime?
2. **Simultaneity**
    - X causes Y, but Y also causes X. In its most extreme form, we might worry about *reverse causality*.
    - e.g., If I run a panel regression across countries where x=poverty and y=conflict incidence, I'll probably see a correlation. But does poverty cause war, or does war cause poverty, or both?
3. **Selection Bias**
    - The sample is not representative of the wider population.
    - E.g. conducting phone interviews for political polling will probably overestimate conservative support because old people use landlines and tend to vote conservative.

Experimental Designs

To measure the **causal effect** of an intervention, we need to isolate its effect.

In experimental settings, we can do this through randomization.

But what about non-experimental settings?



Case Study 1: Immigration and Employment

Does immigration increase unemployment

Many argue that immigration is undesirable because low-skilled immigrants may displace low-skilled or less-educated US citizens in the labor market.

Anecdotal evidence for this claim includes newspaper accounts of hostility between immigrants and natives in some cities, but the empirical evidence is inconclusive.

Card's (1990) study of the effect of immigration on the employment of American citizens, using a Difference in Differences design and the Mariel Boatlift.


PICK UP TODAY • SEE PAGE 60

EXCLUSIVE REPO

MIGRANTS TAKE ALL NEW JOBS IN BRITAIN

And they go to the front of the housing queue

FOREIGN workers have taken every new job in Britain for the past four years, astonishing figures show.

The total of migrant employees since 2003 has soared by 740,000, while the number of Britons in work has gone into reverse and dropped by 120,000. This means

By Tom Whitehead
Home Affairs Correspondent

that foreign workers filled all the extra 620,000 jobs which were created during those four years.

The revelation is a severe embarrassment for Gordon Brown and makes a mockery of his recent pledge to create "British jobs for British workers". The Prime Minister's discomfort deepened when an investigation was launched into how white Britons are being left behind in the housing queue.

The damaging figures emerged just three days after ministers twice had to revise statistics on the number of foreign workers and jobs created

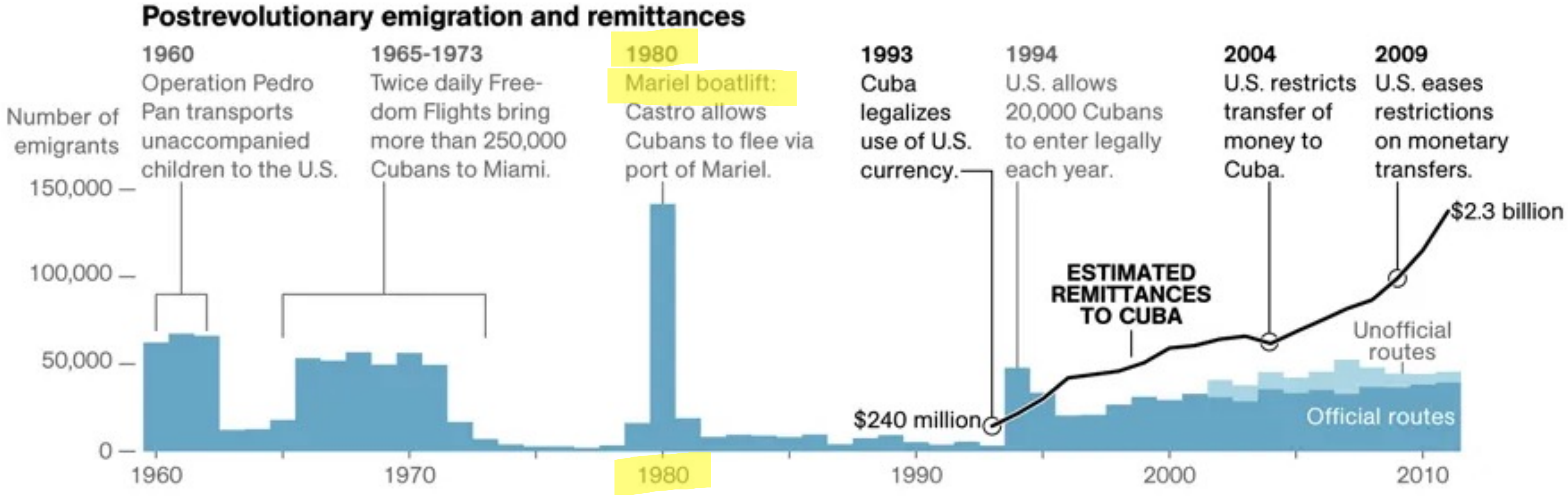
TURN TO PAGE 6


ABAJO
CASTRO

The Mariel Boatlift

In 1980 there was a mass emigration of Cubans to the United States.

The exodus was driven by a stagnant economy that had weakened under the grip of a U.S. trade embargo.



The Mariel Boatlift

"Those who have no revolutionary genes, those who have no revolutionary blood...we do not want them, we do not need them,"

Castro declared in a May 1, 1980 speech. In a stance that reversed the government's closed emigration policy, Castro told Cubans who wanted to leave Cuba to leave, and directed would-be emigrants to go to the Port of Mariel.

David Card's 1990 study

Research question: Did the influx of Cuban immigrants during the Mariel Boatlift in 1980 had a negative impact on the employment and wages of American citizens in Miami.

Difference-in-Differences (DID) Design: Aimed to compare changes in outcomes over time between a treatment group (Miami) and a control group (other cities) before and after the intervention (Mariel Boatlift).

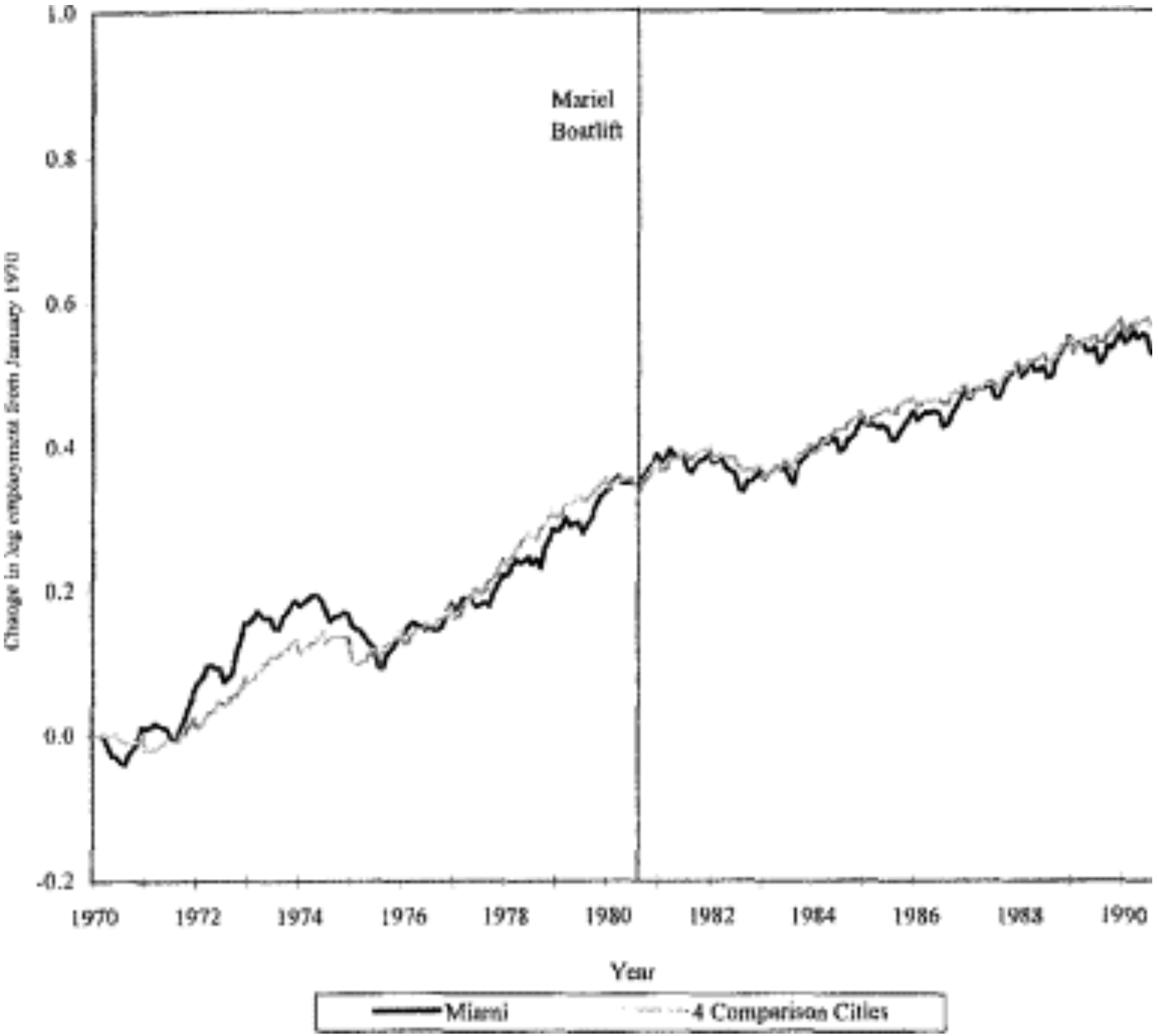
Experimental Design

Data: Current Population Survey (CPS), same data we've been using in the workshops

Treatment Group: Miami

Control Group: Atlanta, Los Angeles, Houston, and Tampa-St. Petersburg. These cities were chosen because, like Miami, they have large Black and Hispanic populations and because discussions of the impact of immigrants often focuses on the consequences for minorities. Most importantly, these cities appear to have employment trends similar to those in Miami at least since 1976.

Treatment period: pre/post 1980



1.0
0.8
0.6
0.4
0.2
0.0
-0.2
Change in log employment from January 1970
Mariel
Boatlift
1970
1972
1974
1976
1978
1980
1982
1984
1986
1988
1990
Year
Miami
4 Comparison Cities

Results

Unemployment

	Group	Year		
		1979 (1)	1981 (2)	1981–1979 (3)
	Whites			
(1)	Miami	5.1 (1.1)	3.9 (0.9)	−1.2 (1.4)
(2)	Comparison cities	4.4 (0.3)	4.3 (0.3)	−0.1 (0.4)
(3)	Miami-Comparison Difference	0.7 (1.1)	-0.4 (0.95)	−1.1 (1.5)
	Blacks			
(4)	Miami	8.3 (1.7)	9.6 (1.8)	1.3 (2.5)
(5)	Comparison cities	10.3 (0.8)	12.6 (0.9)	2.3 (1.2)
(6)	Miami-Comparison Difference	−2.0 (1.9)	−3.0 (2.0)	−1.0 (2.8)

Results

Between 1981 and 1979, the unemployment rate for Blacks in Miami rose by about 1.3%, though this change is not significant.

Unemployment rates in the comparisons cities rose even more, by 2.3%. The difference in these two changes, – 1.0%, is a DD estimate of the effect of the Mariel immigrants on the unemployment rate of Blacks in Miami.

In this case, the estimated effect on the unemployment rate is actually negative, though not significantly different from zero.

Findings

The Mariel immigrants increased the Miami labor force by 7%, and the percentage increase in labor supply to less-skilled occupations and industries was even greater because most of the immigrants were relatively unskilled.

The Mariel influx appears to have had virtually no effect on the wages or unemployment rates of less-skilled workers, even among Cubans who had immigrated earlier.

The author suggests that the ability of Miami's labor market to rapidly absorb the Mariel immigrants was largely owing to its adjustment to other large waves of immigrants in the two decades before the Mariel Boatlift.

Difference-in-Differences

What is Difference-in-Differences?

Differences-in-differences strategies are simple panel-data methods applied to sets of group means in cases when certain groups are exposed to the causing variable of interest and others are not.

This approach, which is transparent and often at least superficially plausible, is well-suited to estimating the effect of sharp changes in the economic environment or changes in government policy.

Counterfactuals

Difference in differences requires a few things:

1. A treatment located at a point in time
2. Two distinct groups for which we have measurement pre-and post- treatment
3. Parallel pre-treatment trends in the outcome variable y
4. No simultaneous treatment occurring around our treatment of interest

These allow us to construct a valid counterfactual:

- We can argue that the control group in the post-treatment period acts as a valid representation of the treatment group's behaviour in the absence of treatment. We can thus interpret the difference between the two as the causal effect of the treatment.

Do Minimum Wages Decrease Poverty?

A bill signed into law on November 1989 raised the federal minimum wage from $3.35 per hour to $3.8 effective April 1, 1990.

A further increase from $4.25 per hour to $5.05 on April 1, 1992.

New Jersey experiences the wage increase in 1992 but not Pennsylvania.

David and Krueger run two rounds of survey right before and after the rise – fast-food restaurants (leading employer of low-wage workers) asking about employment rates.

Correlation versus Causation

We cannot draw causal conclusions by observing simple before-and-after changes in outcomes, as factors other than the treatment may influence the outcome over time.

- E.g. New Jersey may have also implemented new social programmes, improved public transport, etc. which would also influence poverty.

We cannot simply compare NJ and PA directly due to differences in unobservable characteristics between the states.

- E.g. geographic size, economic structure, population, governance, etc.

Difference-in-differences

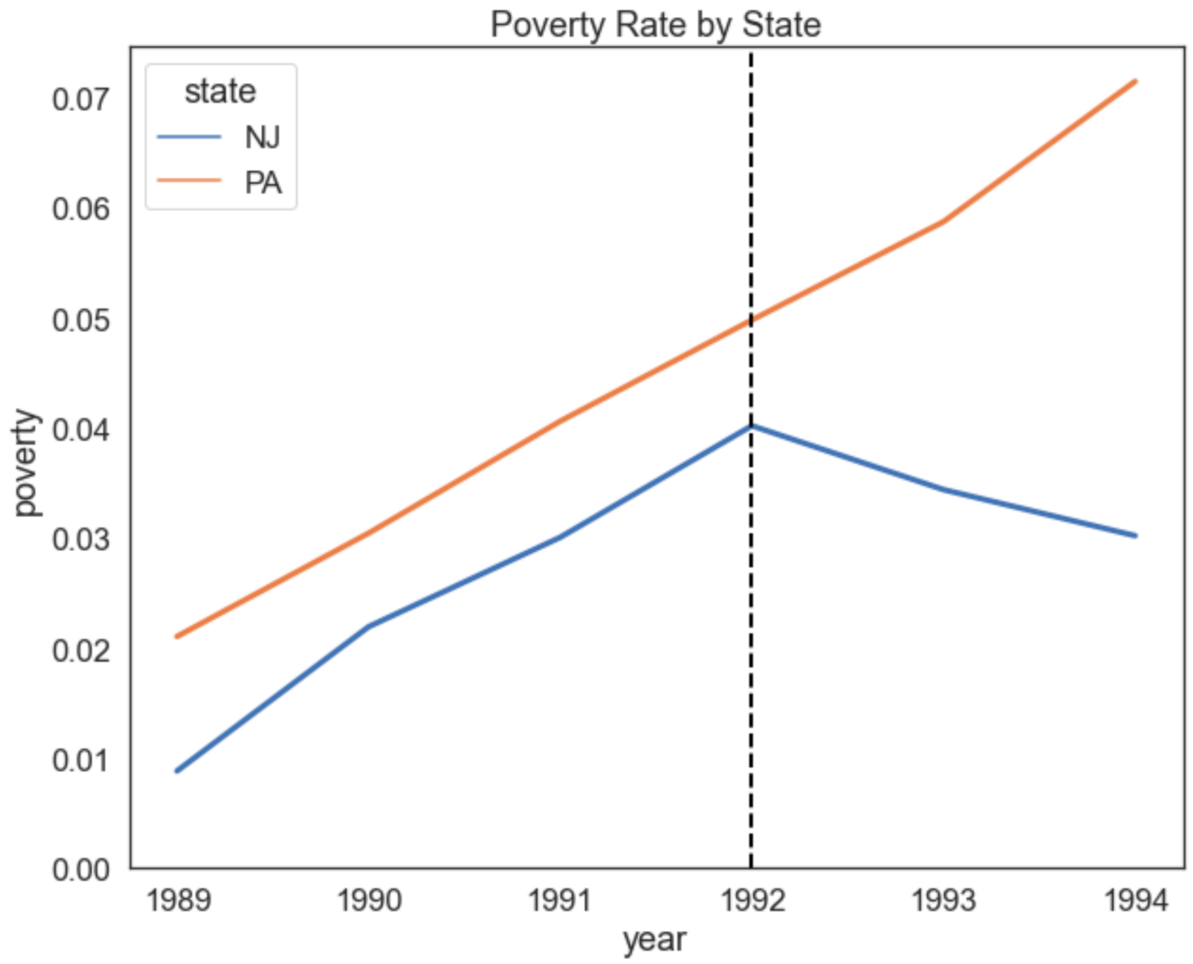
Difference-in-differences combines these two methods to compare the before-and-after changes in outcomes for treatment and control groups and estimate the overall impact of the treatment.

$$Y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Treat_i \times Post_t + \varepsilon_{it}$$

	Control	Treatment
Before	β_0	$\beta_0 + \beta_2$
After	$\beta_0 + \beta_1$	$\beta_0 + \beta_1 + \beta_2 + \beta_3$

Do Minimum Wages Decrease Poverty?

State	Year	Treatment	Post	Poverty
NJ	1989	1	0	0.1
NJ	1990	1	0	0.2
NJ	1991	1	0	0.3
NJ	1992	1	1	0.25
NJ	1993	1	1	0.2
NJ	1994	1	1	0.15
PA	1989	0	0	0.2
PA	1990	0	0	0.3
PA	1991	0	0	0.4
PA	1992	0	1	0.5
PA	1993	0	1	0.6
PA	1994	0	1	0.7



The Four Scenarios

$$Y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Treat_i \times Post_t + \varepsilon_{it}$$

Control Group, Before Treatment:

- $Y_{it} = \beta_0 + \beta_1 0 + \beta_2 0 + \beta_3 0 \times 0 + \varepsilon_{it} = \beta_0$

Control Group, After Treatment

- $Y_{it} = \beta_0 + \beta_1 1 + \beta_2 0 + \beta_3 0 \times 1 + \varepsilon_{it} = \beta_0 + \beta_1$

Treatment Group, Before Treatment

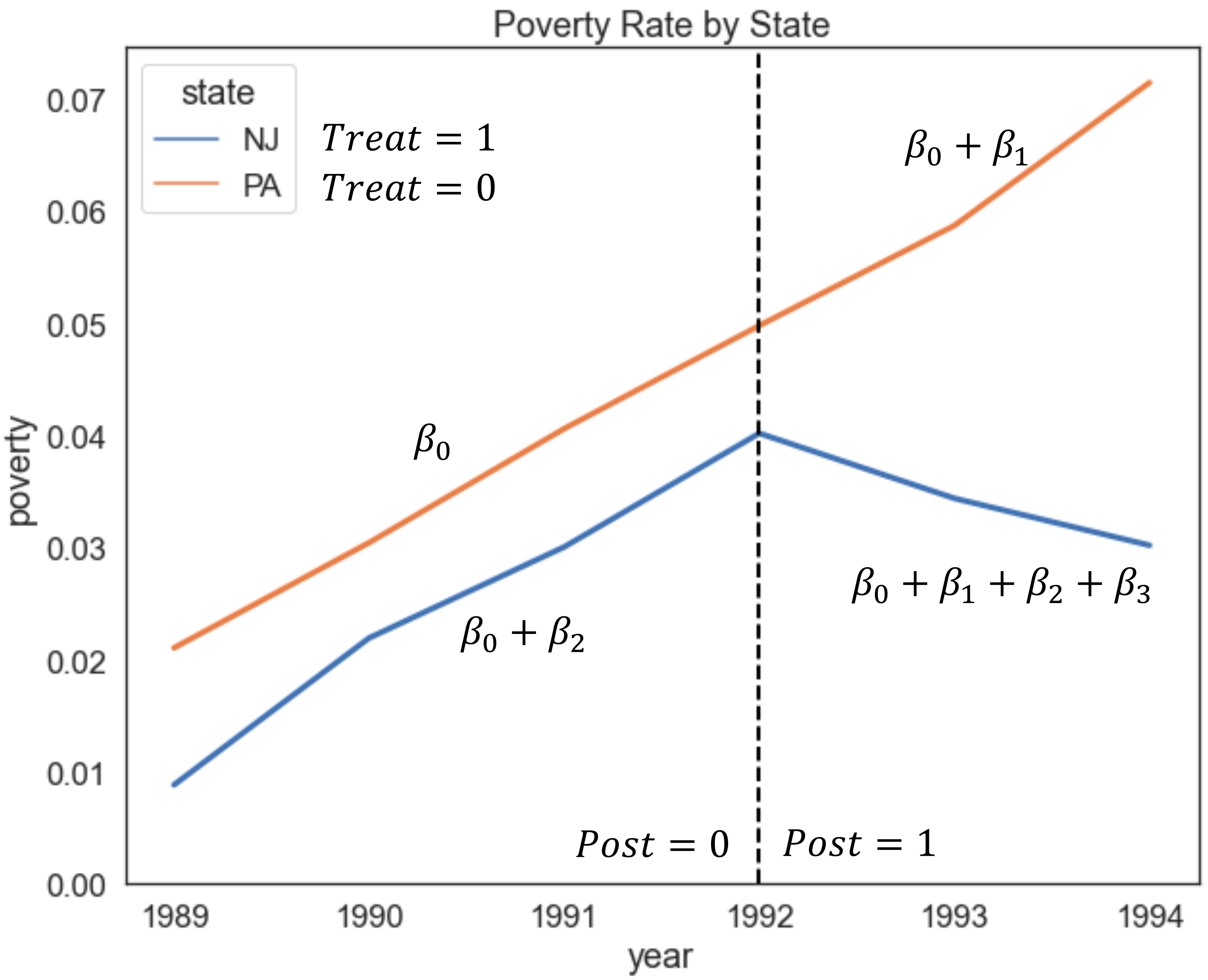
- $Y_{it} = \beta_0 + \beta_1 0 + \beta_2 1 + \beta_3 1 \times 0 + \varepsilon_{it} = \beta_0 + \beta_2$

Treatment Group, After Treatment

- $Y_{it} = \beta_0 + \beta_1 1 + \beta_2 1 + \beta_3 1 \times 1 + \varepsilon_{it} = \beta_0 + \beta_1 + \beta_2 + \beta_3$

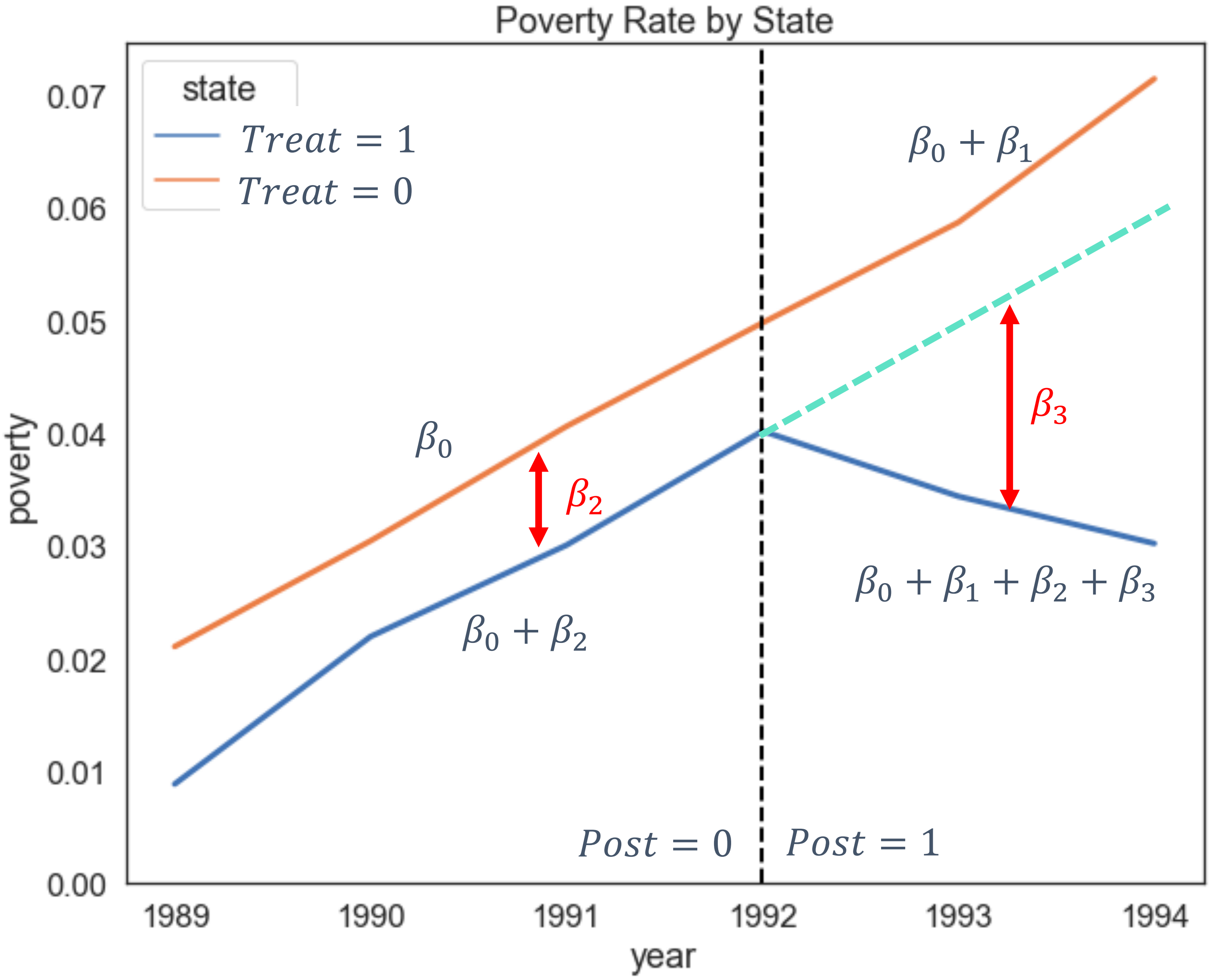
$$Y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Treat_i \times Post_t + \varepsilon_{it}$$

	Control	Treatment
Before	β_0	$\beta_0 + \beta_2$
After	$\beta_0 + \beta_1$	$\beta_0 + \beta_1 + \beta_2 + \beta_3$



$$Y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Treat_i \times Post_t + \varepsilon_{it}$$

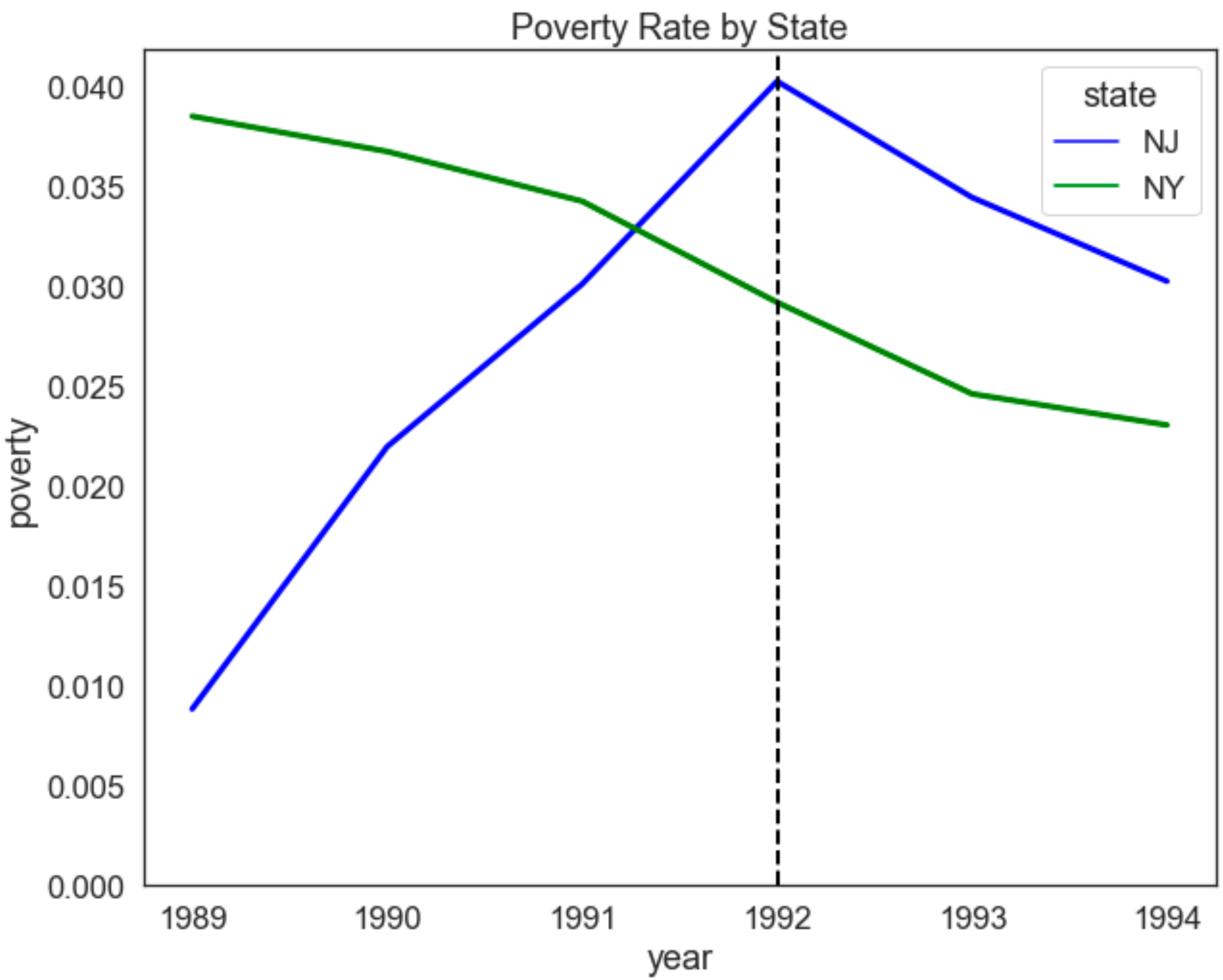
	Control	Treatment	Treatment -Control
Before	β_0	$\beta_0 + \beta_2$	β_2
After	$\beta_0 + \beta_1$	$\beta_0 + \beta_1 + \beta_2 + \beta_3$	$\beta_2 + \beta_3$
After-Before	β_1	$\beta_1 + \beta_3$	β_3



Parallel Trends Assumption

Treatment and control would have developed in the same way in the absence of treatment.

We can provide suggestive evidence of this if we have data on pre-years.



Assessing Parallel Trends

1. Visual Assessment (are pre-treatment trends parallel)
2. Perform a placebo test using a fake treatment group. The fake treatment group should be a group that was not affected by the program. A placebo test that reveals zero impact supports the equal-trend assumption.
3. Perform a placebo test using a fake outcome. A placebo test that reveals zero impact supports the equal-trend assumption.
4. Perform the difference-in-differences estimation using different comparison groups. Similar estimates of the impact of program confirms the equal-trend assumption.

Case Study 2: Air quality and Driving Restrictions

Exercise

Let's think about how we would explore the relationship between air quality and driving restrictions in China.

Seven Chinese cities have recently instituted driving bans; these cities are

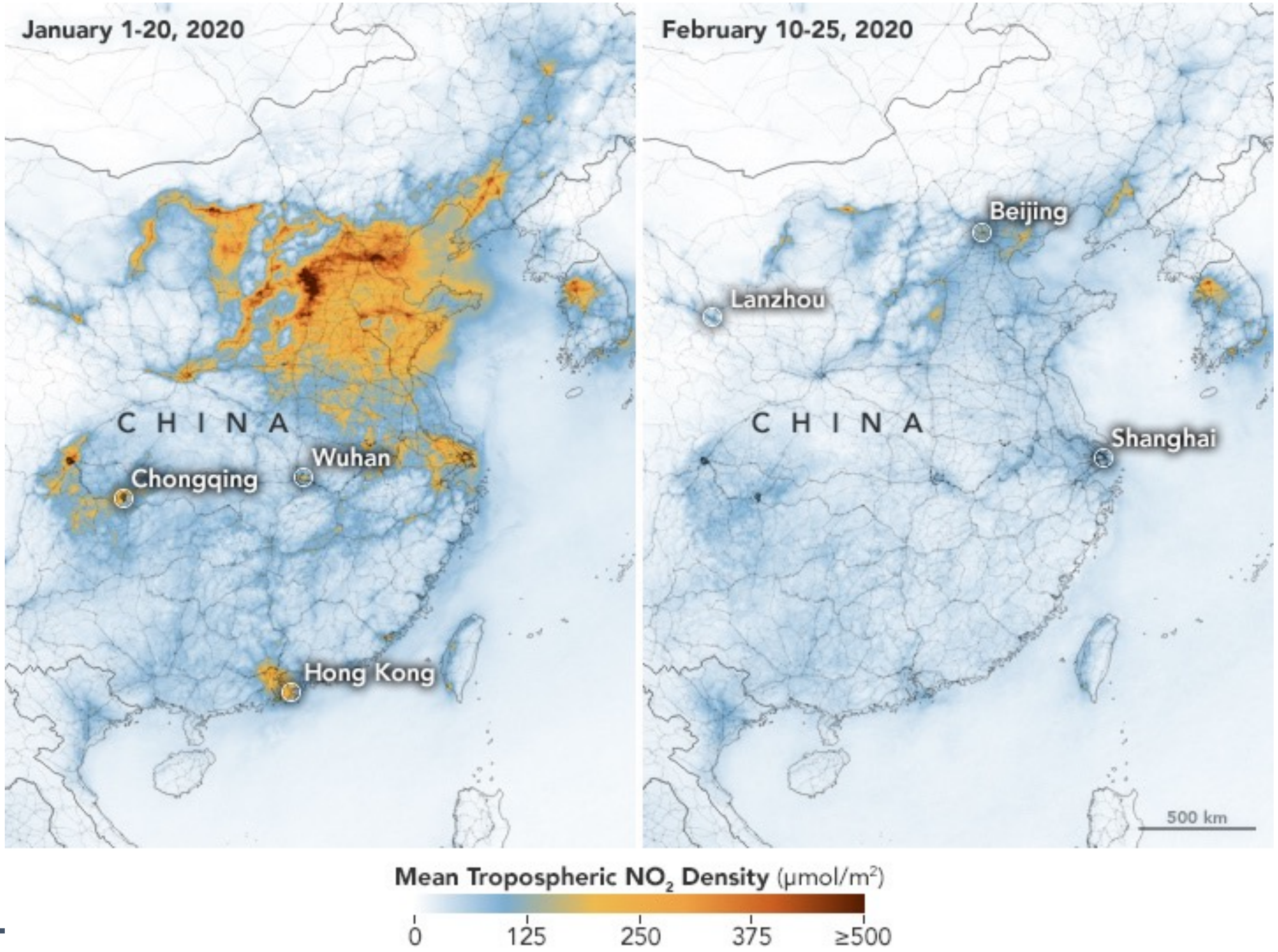
- Tianjin, Nanchang, Hangzhou, Lanzhou, Guiyang, Chengdu, and Changchun

Suppose we have some data on the daily amount of traffic in these cities, and some data on pollution

Air Quality Data

Pollution dropped significantly during the coronavirus pandemic

Air quality was measured using satellite-borne sensors of various pollutants such as Nitrogen Dioxide (NO2).



Design a Panel Regression

Question: What is the effect of driving restrictions on air quality in China?

Data: A panel dataset with monthly observations of air quality in every major Chinese city, spanning 2000-2022

$$Y_{it} = \alpha_i + \beta_1 X_{it} + u_{it}$$

Y_{it}: ???

α_i: ???

β_1: ???

X_{it}: ???

Omitted Variables

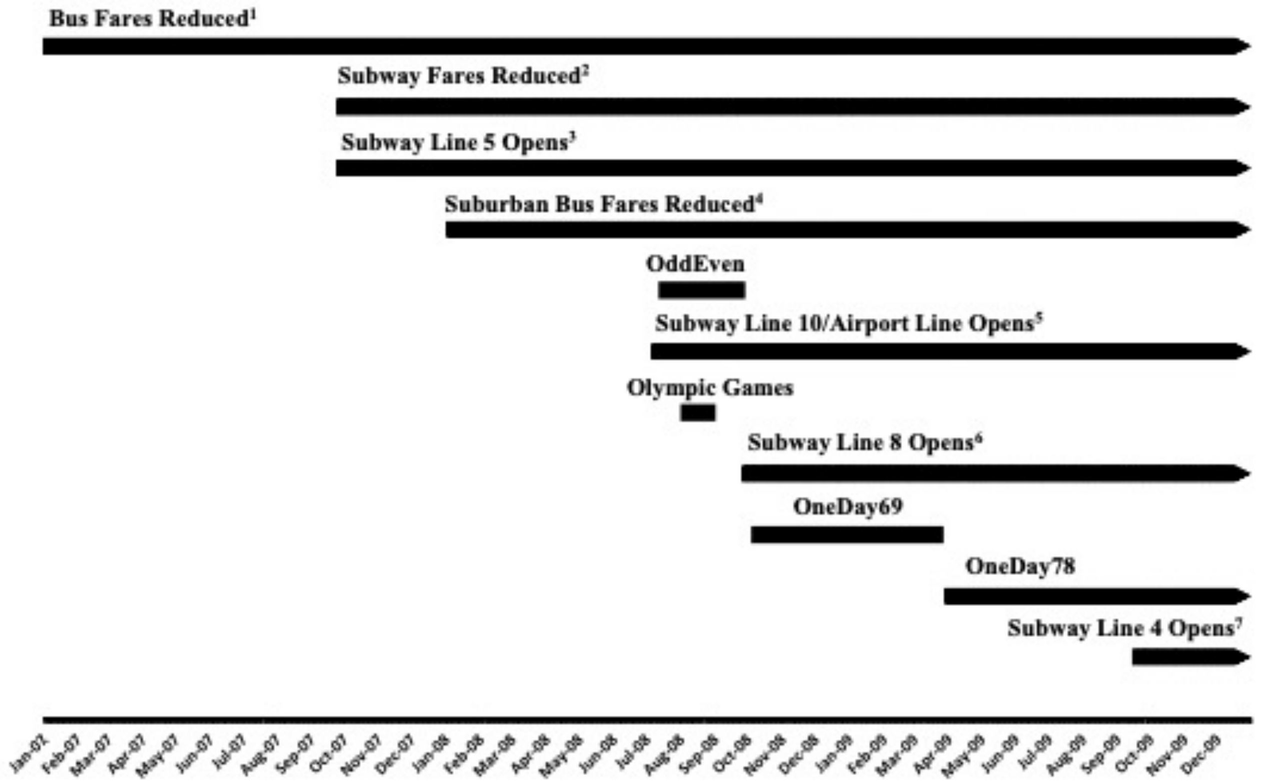
Air quality metrics like AQI are closely related to other atmospheric variables like moisture and temperature.

- The restrictions came in to effect in July, when it tends to be hot and humid.

Are cars the only source of air pollution?

Simultaneity

Figure 2 Timeline of Pollution-Relevant Policies



[1] Bus fares reduced from RMB 1 per trip to 0.4 for regular bus pass holders and to 0.2 for student pass holders. [2] Subway fares reduced from RMB 2 per transfer to RMB 2 per trip regardless of number of transfers. [3] Runs south to north. [4] Fares on suburban routes lowered by 60% for adults and 80% for students. "Suburban" routes connect the ten districts and counties outside the inner city with the eight city districts inside. [5] Runs southeast to northwest including the airport. [6] Serves the Olympics Park area. Opened on a more limited basis earlier to serve Olympic athletes and tourists. [7] Runs south to northwest.

Selection Bias

It has been argued that compositional changes in monitoring stations over time may reflect systematic government decisions to close stations in highly polluted places to improve air quality statistics with out actually improving air quality

- E.g. Andrews, S. Q. (2008). "Inconsistencies in Air Quality Metrics: 'Blue Sky' Days and PM10 Concentrations in Beijing," Environmental Research Letters, 3, 1 – 14.

Driving Restrictions

Viard and Fu, 2013

Driving restrictions are used in numerous cities around the world to reduce pollution and congestion.

Such restrictions may be ineffective either due to non-compliance or compensating responses such as inter-temporal substitution of driving or adding second vehicles.

If effective, they may lower economic activity by increasing commute costs and reducing workers' willingness to supply labor for given compensation.

Motivation

There is little empirical evidence of driving restrictions' effect on pollution and none about their effect on economic activity.

We examine both effects under driving restrictions imposed by the Beijing government since July 20, 2008. The restrictions, based on license plate numbers, initially prevented driving every other day and later one day per week.

Figure 2 Timeline of Pollution-Relevant Policies

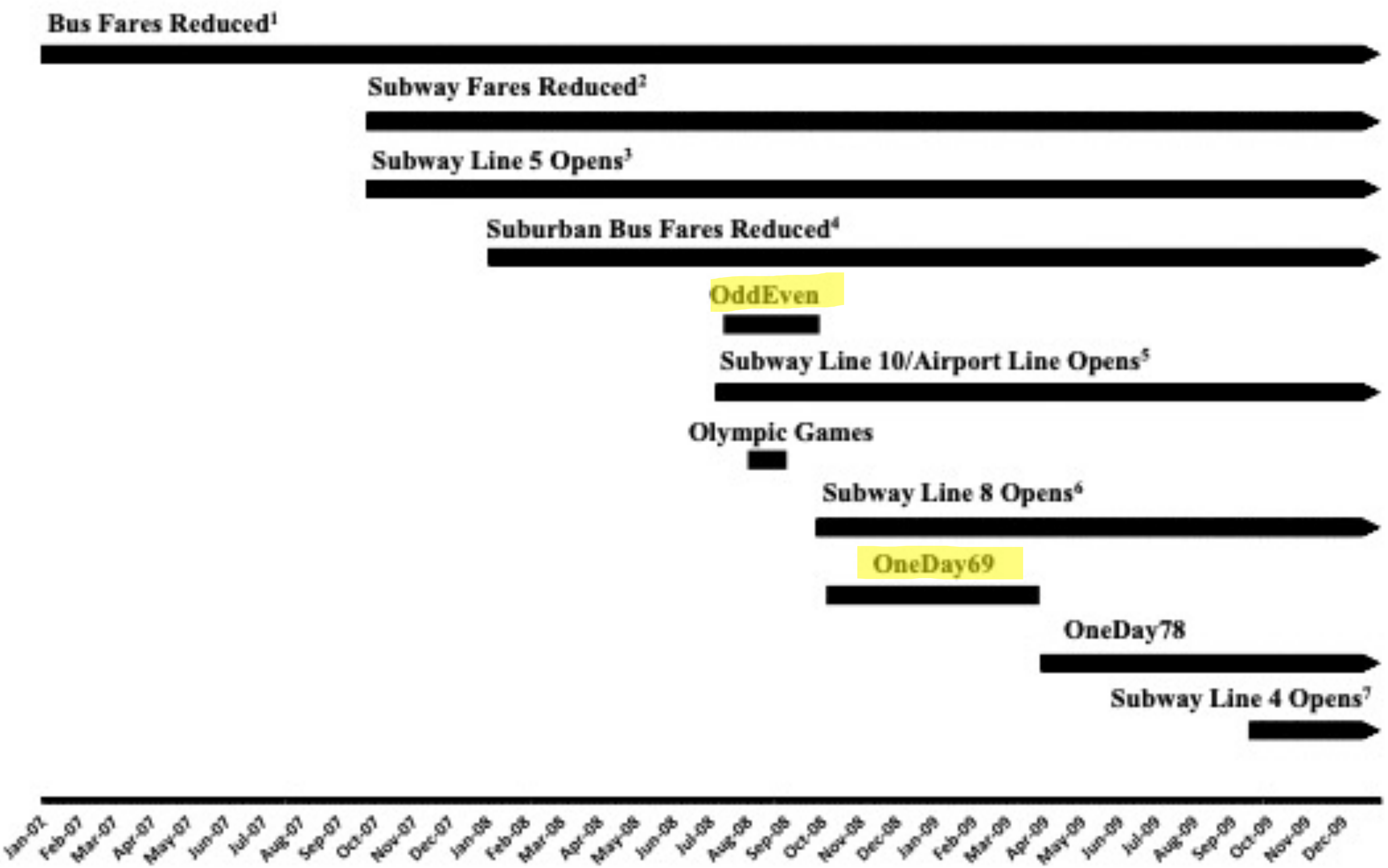
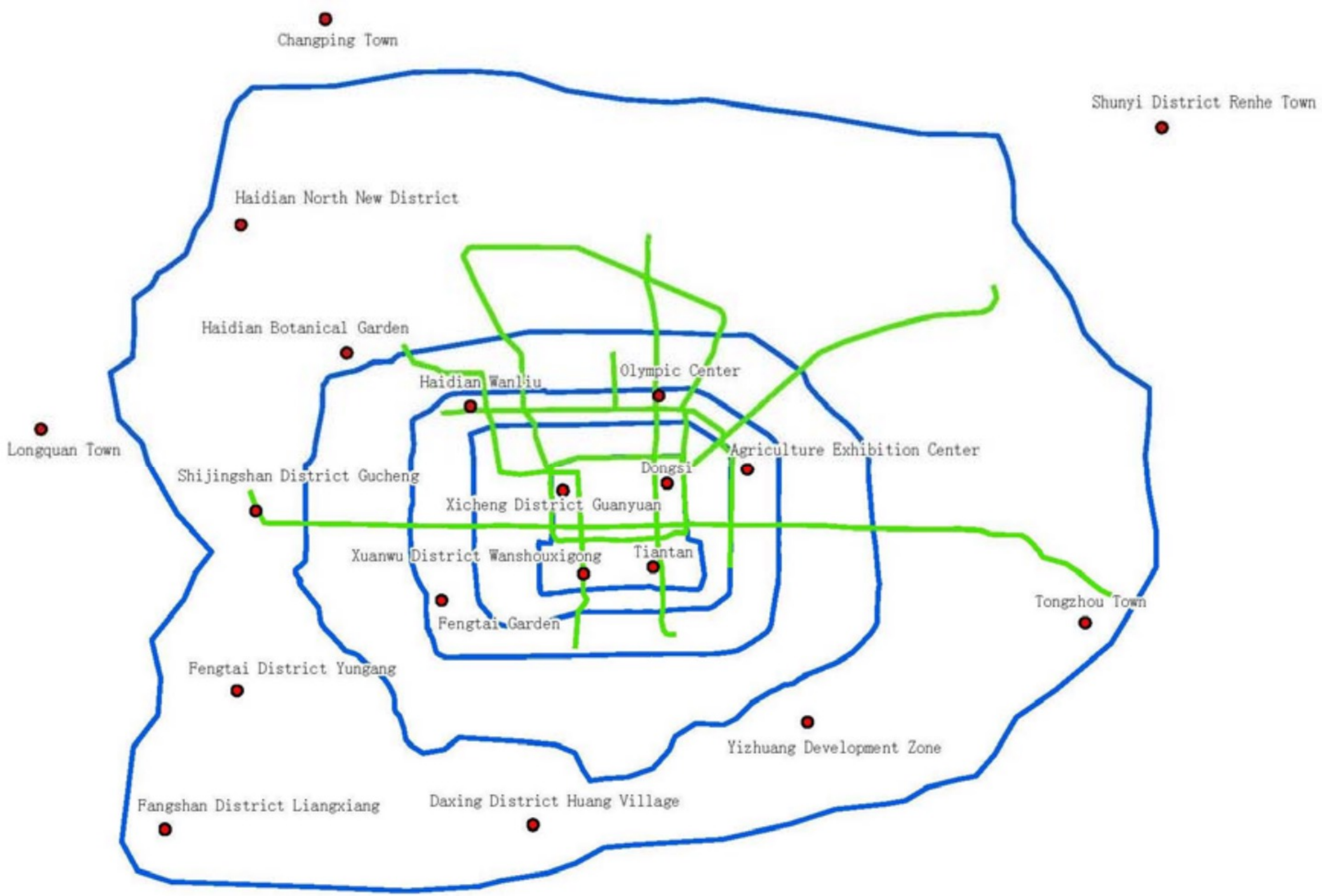


Figure 1 Map of Beijing Monitoring Station Locations in 2008 and 2009



Experimental Design

Question: What is the effect of driving restrictions on air quality in Beijing?

Data: A panel dataset with daily measures of Beijing air pollution at the individual monitoring-station level, spanning January 1, 2007 to December 31, 2009.

Treatment Group: Air quality monitoring stations near roads

Control Group: Air quality monitoring stations far from roads

Treatment period: After the implementation of the driving restriction.

Experimental Design

"Particulate matter's ambient properties dictate that it is deposited within a few kilometers of its release. We exploit this to develop a differences-in-differences (DD) approach that combines time-series variation with spatial variation in monitoring stations' locations to eliminate other explanations besides cars for the pollution reduction."

$$\log\left(API_{st}^{S}\right) = \alpha_s + \sum_{i=1}^{11} \beta_{2i} m_{t\in i} + \beta_3 WE_t + \beta_4 HO_t + \beta_5 BR_t + \beta_6 OE_t + \beta_7 OD_t$$

$$+ \beta_8 OD_t * WE_t + \sum_{k=1}^{K} \beta_{9k} Z_{stk} + DD_{st} + f_s\left(t\right) + \varepsilon_{st}^{S},$$

Identifying Assumptions

The identifying assumption for our DD estimation is that, conditional on the other covariates, station-specific unobserved factors affecting the API are uncorrelated with the treatment.

That is, **unobserved factors do not vary systematically with distance from a major road during the policy periods relative to before.**

This assumption may not hold if stations closer to a major road have different long-term pollution trends than those further away. This might be the case if, for example, traffic patterns changed differently over time on major roads relative to smaller roads.

Results

The effects are highly significant with either a 45- and 60-day window showing declines of

- 12% and 19% for “far” stations
- 16% and 23% for “near” stations.

DD Estimates using Log Station-Level, Daily API in Discontinuity Samples

	45-Day Window	60-Day Window
OddEven	-0.1192 ***	-0.1899 ***
	(0.0153)	(0.0166)
"Near"*OddEven	-0.0447 **	-0.0414 **
	(0.0199)	(0.0205)
R^2	0.8083	0.7248
Number of Stations	8	8
Station Fixed Effects	Yes	Yes
N	718	958

Dependent variable is log of daily API at eight monitoring stations inside the restricted area. Robust standard errors clustered at the station level in parentheses. * = 10% significance, ** = 5% significance, *** = 1% significance. All regressions include an Olympics dummy, maximum temperature, average humidity, total rainfall, hours of sunshine, wind speed quartiles, wind direction dummies, interactions between wind speed and wind direction, dummy for weekends, and dummy for days with API less than 50. The number of observations is not evenly divisable by the number of stations due to missing values.

Detailed Results

Instead of just using a binary near/far cutoff, these results use continuous distance

Table 5 DD Estimates using Log Station-Level, Daily API (2007 – 2009), N = 8,361

	(1)	(2)	(3)	(4)
	Distance to Ring Roads			Distance to Class I Roads
	Near/Far		Quadratic Distance	
OddEven	-0.1396 ***	-0.1400 ***	-0.4617 ***	-0.1927 ***
	(0.0129)	(0.0129)	(0.0457)	(0.0221)
Near*OddEven	-0.0580 ***	-0.0536 ***		-0.0540 **
	(0.0119)	(0.0117)		(0.0255)
OddEven*Distance			0.5110 ***	
			(0.1054)	
OddEven*Distance^2			-0.2425 ***	
			(0.0588)	
OneDay	-0.1160 ***	-0.1155 ***	-0.3290 ***	-0.1254 ***
	(0.0089)	(0.0085)	(0.0350)	(0.0145)
Near*OneDay	-0.0248 **	-0.0204 **		-0.0440 *
	(0.0098)	(0.0102)		(0.0226)
OneDay*Distance			0.4150 ***	
			(0.0862)	
OneDay*Distance^2			-0.2160 ***	
			(0.0466)	
Before OddEven Trend		-0.0330	0.0100	-0.0247
		(0.0281)	(0.0320)	(0.0424)
Near*(Before OddEven Trend)		-0.0072	-0.0797	0.0063
		(0.0586)	(0.0597)	(0.0750)
Break Trend		-9.3796 ***	-6.8852 ***	-3.7289
		(0.8287)	(1.7231)	(2.4161)
Near*(Break Trend)		2.5255	-0.2934	-6.6855 **
		(2.3474)	(3.3823)	(3.0088)
During OneDay Trend		0.3334 ***	0.3817 ***	0.3463 ***
		(0.0323)	(0.0348)	(0.0534)
Near*(During OneDay Trend)		0.0027	0.0116	0.0840
		(0.0512)	(0.0779)	(0.0577)
R^2	0.6059	0.6053	0.4179	0.4173
Station Fixed Effects	Yes	Yes	Yes	Yes
Number of Stations	8	8	8	8

Findings

Pollution drops more at stations closer to a major road. This means confounding factors are related to proximity to a major road and therefore traffic flow. We consider, and rule out, changes in gasoline prices, parking rates, number of taxis, emissions standards, and government-imposed working hours.

The OddEven policy reduces the API by 12.5%

The OneDay policy reduces the API by 16.0%

Any Questions?