Difference-in-Difference estimator

Presented at Summer School 2015
by Ziyodullo Parpiev, PhD
June 9, 2015
Tashkent

Today’s Class

- Non-experimental Methods: Difference-in-differences
  - Understanding how it works
  - How to test the assumptions
  - Some problems and pitfalls

Why are experiments good?

- Treatment is *random* so it's independent of other characteristics
- This independence allows us to develop an *implied counterfactual*
- Thus even though we don't observe $E[Y_0 / T=1]$ we can use $E[Y_0 \mid T=0]$ as the counterfactual for the treatment group

What if we don’t have an experiment

- Would like to find a group that is exactly like the treatment group but didn’t get the treatment
- Hard to do because
  - Lots of unobservables
  - Data is limited
  - Selection into treatment

Background Information

- Water supplied to households by competing private companies
- Sometimes different companies supplied households in same street
- In south London two main companies:
  - Lambeth Company (water supply from Thames Ditton, 22 miles upstream)
  - Southwark and Vauxhall Company (water supply from Thames)

In 1853/54 cholera outbreak

- Death Rates per 10000 people by water company
  - Lambeth 10
  - Southwark and Vauxhall 150
- Might be water but perhaps other factors
- Snow compared death rates in 1849 epidemic
  - Lambeth 150
  - Southwark and Vauxhall 125
- In 1852 Lambeth Company had changed supply from Hungerford Bridge

The effect of clean water on cholera death rates

	1849	1853/54	Difference
Lambeth	150	10	-140
Vauxhall and Southwark	125	150	25
Difference	-25	140	-165

Counterfactual 2: 'Control' group time difference. Assume this would have been true for 'treatment' group

Counterfactual 1: Pre-Experiment difference between treatment and control—assume this difference is *fixed* over time

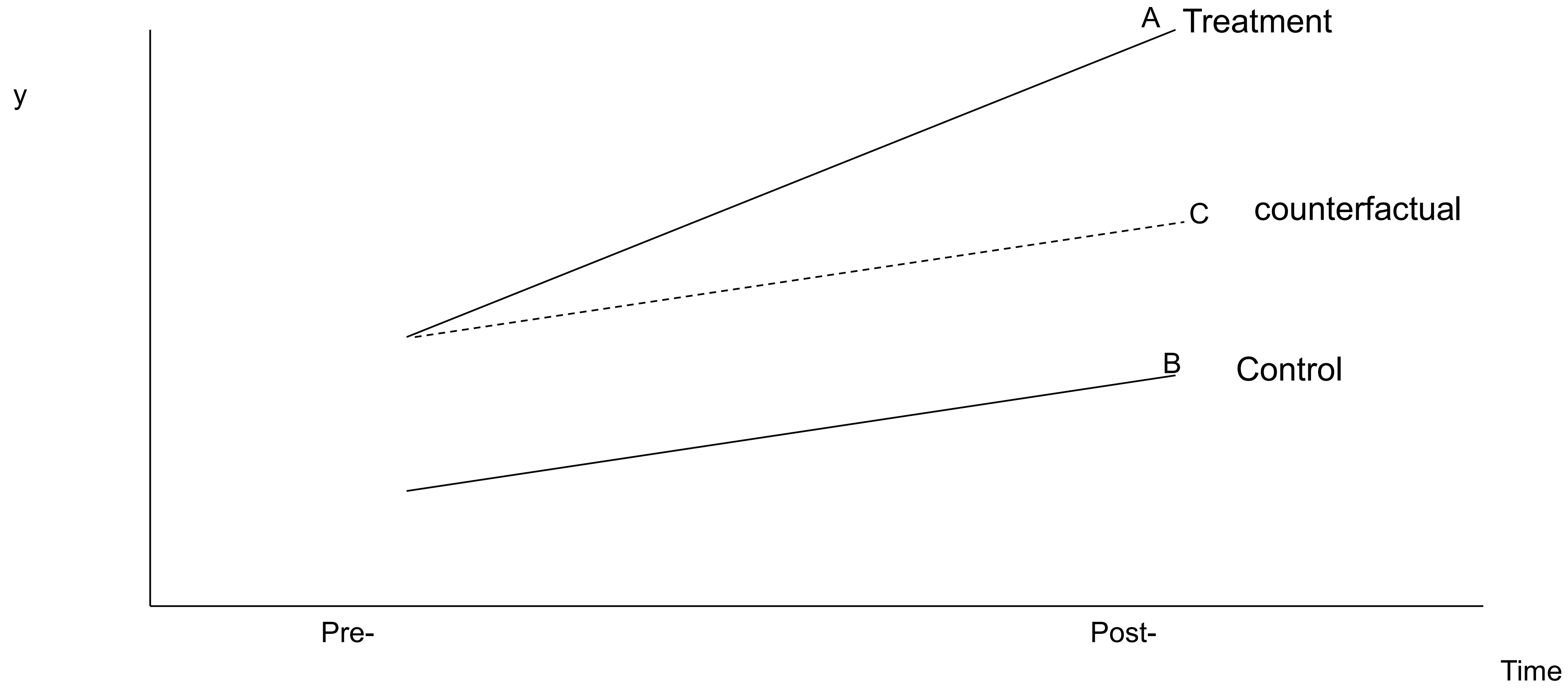
This is basic idea of D-i-D

- Have already seen idea of using differences to estimate causal effects
  - Treatment/control groups in experimental data
- We need a counterfactual because we don't observe the outcome of the treatment group when they weren't treated (i.e. $(Y_0 \mid T=1)$)
- Often would like to find 'treatment' and 'control' group who can be assumed to be similar in every way except receipt of treatment

A Weaker Assumption is..

- Assume that, in absence of treatment, difference between ‘treatment’ and ‘control’ group is constant over time
- With this assumption can use observations on treatment and control group pre- and post-treatment to estimate causal effect
- Idea
  - Difference pre-treatment is ‘normal’ difference
  - Difference pre-treatment is ‘normal’ difference + causal effect
  - Difference-in-difference is causal effect

A Graphical Representation



A – B = Standard differences estimator
C – B = Counterfactual ‘normal’ difference
A – C = Difference-in-Difference Estimate

Assumption of the D-in-D estimate

- D-in-D estimate assumes trends in outcome variables the same for treatment and control groups
  - Fixed difference over time
  - This is not testable because we never observe the counterfactual

- Is this reasonable?
  - With two periods can't do anything
  - With more periods can see if control and treatment groups 'trend together'

Some Notation

- Define:

$$\mu_{it} = E(y_{it})$$

Where i=0 is control group, i=1 is treatment

Where t=0 is pre-period, t=1 is post-period

- Standard ‘differences’ estimate of causal effect is estimate of:

$$\mu_{11} - \mu_{01}$$

- ‘Differences-in-Differences’ estimate of causal effect is estimate of:

$$(\mu_{11} - \mu_{01}) - (\mu_{10} - \mu_{00})$$

How to estimate?

- Can write D-in-D estimate as:

$$(\underbrace{\mu_{11} - \mu_{10}}_{\text{Before-After difference for 'treatment' group}}) - (\underbrace{\mu_{01} - \mu_{00}}_{\text{Before-After difference for 'control' group}})$$

- This is simply the difference in the change of treatment and control groups so can estimate as:

$$\Delta y_i = \beta(\Delta X_i) + \Delta \varepsilon_i$$

Can we do this?

- This is simply ‘differences’ estimator applied to the difference
- To implement this need to have repeat observations on the same individuals
- May not have this – individuals observed pre- and post-treatment may be different

In this case can estimate….

$$y_{it} = \beta_0 + \beta_1 X_i + \beta_2 T_t + \beta_3 X_i * T_t + \varepsilon_{it}$$

Main effect of Treatment group

(in before period because T=0)

Main effect of the After period

(for control group because X=0)

D-in-D estimate

- D-in-D estimate is estimate of β_3
- why is this?

$$p\lim\hat{\beta}_0 = \mu_{00}$$

$$p\lim\hat{\beta}_1 = \mu_{10} - \mu_{00}$$

$$p\lim\hat{\beta}_2 = \mu_{01} - \mu_{00}$$

$$p\lim\hat{\beta}_3 = (\mu_{11} - \mu_{01}) - (\mu_{10} - \mu_{00})$$

A Comparison of the Two Methods

- Where have repeated observations could use both methods
- Will give same parameter estimates
- But will give different standard errors
  - ‘levels’ version will assume residuals are independent – unlikely to be a good assumption
  - Can deal with this by clustering by group (imposes a covariance structure within the clustering variable)

Recap: Assumptions for Diff-in-Diff

- Additive structure of effects.
  - We are imposing a linear model where the group or time specific effects only enter additively.
- No spillover effects
  - The treatment group received the treatment and the control group did not
- Parallel time trends:
  - there are fixed differences over time.
  - If there are differences that vary over time then our second difference will still include a time effect.

Issue 1: Other Regressors

- Can put in other regressors just as usual
  - think about way in which they enter the estimating equation
  - E.g. if level of W affects level of y then should include ΔW in differences version

- Conditional comparisons might be useful if you think some groups may be more comparable or have different trends than others

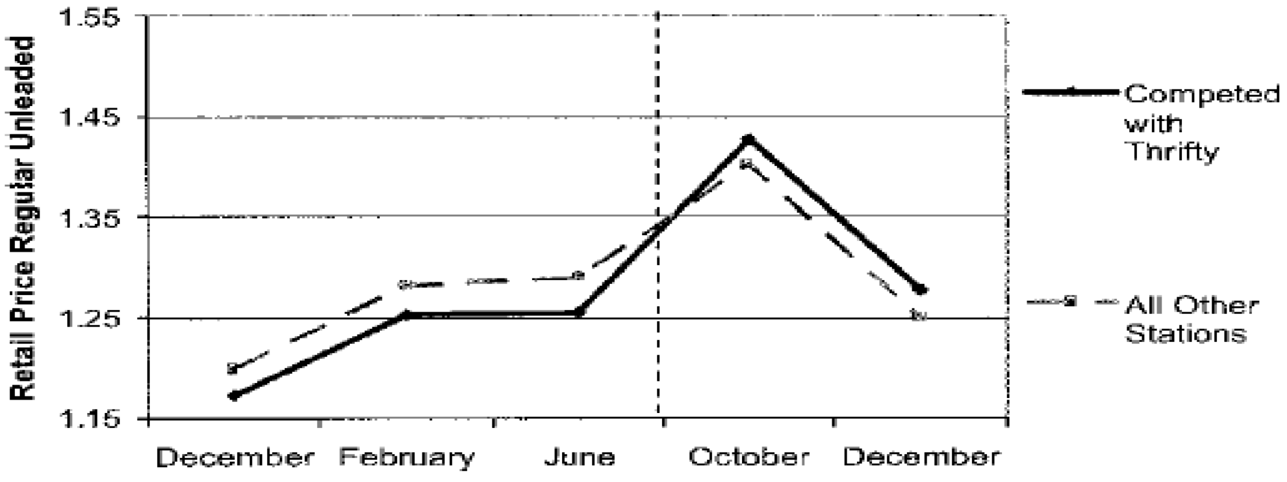
Issue 2: Differential Trends in Treatment and Control Groups

- Key assumption underlying validity of D-in-D estimate is that differences between treatment and control group would have remained constant in absence of treatment
  - Can never test this
  - With only two periods can get no idea of plausibility
  - But can with more than two periods

An Example:

- “Vertical Relationships and Competition in Retail Gasoline Markets”, by Justine Hastings, *American Economic Review*, 2004
- Interested in effect of vertical integration on retail petrol prices
- Investigates take-over in CA of independent ‘Thrifty’ chain of petrol stations by ARCO (more integrated)
  - Treatment Group: petrol stations < 1mi from ‘Thrifty’
  - Control group: petrol stations > 1mi from ‘Thrifty’
- Lots of reasons why these groups might be different so D-in-D approach seems a good idea

This picture contains relevant information…



(a) LOS ANGELES

- Can see D-in-D estimate of +5c per gallon
- Also can see trends before and after change very similar – D-in-D assumption valid

Issue 3: Ashenfelter's Dip

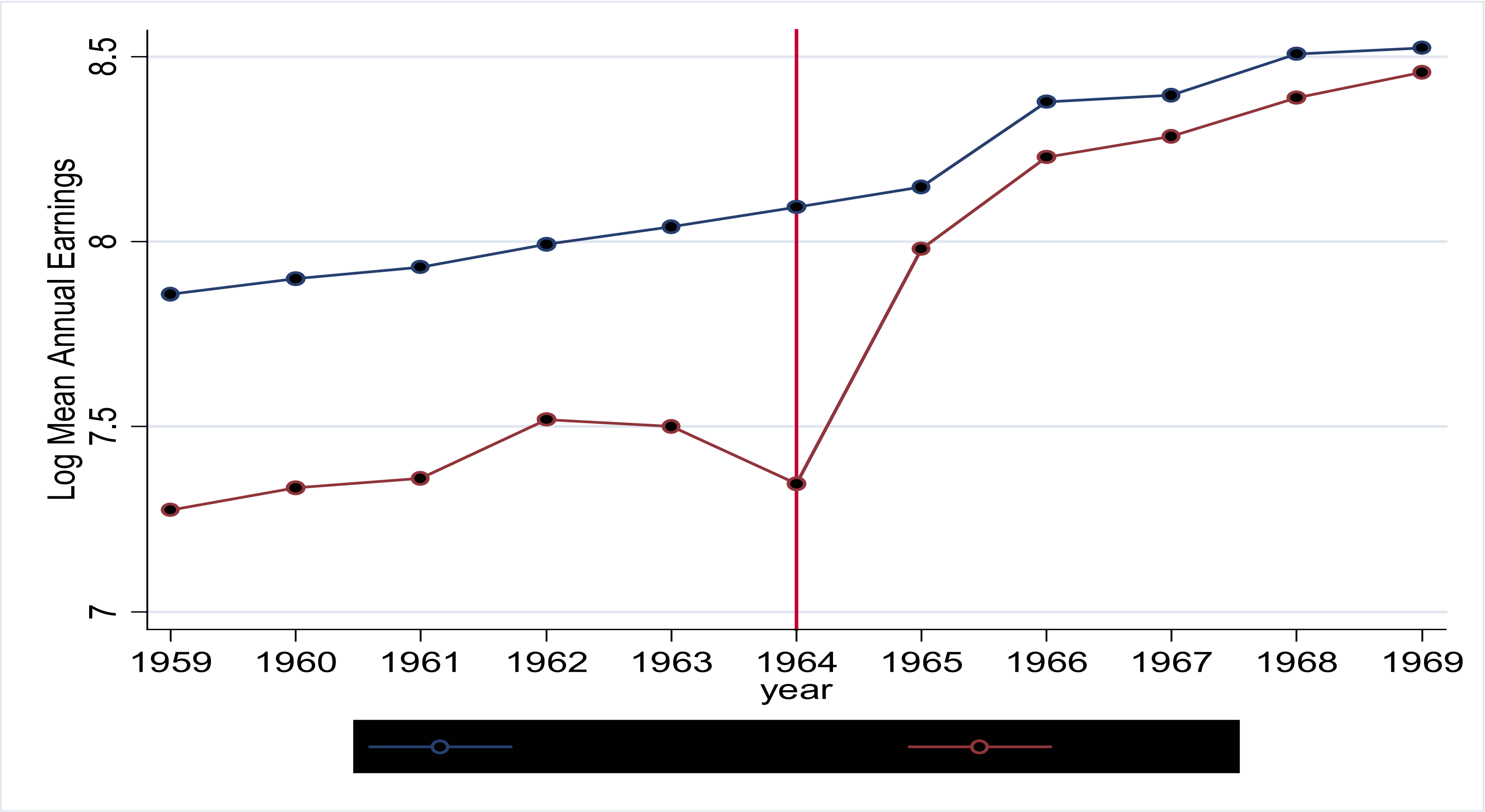
- `pre-program ***dip***', for participants
  - Related to the idea of *mean reversion*: individuals experience some idiosyncratic shock
  - May enter program when things are especially bad
  - Would have improved anyway (reversion to the mean)

- Another issue may be if your treatment is *selected* by participants then only the worst off individuals elect the treatment—not comparable to general effect of policy

Another Example…

- Interested in effect of government-sponsored training (MDTA) on earnings
- Treatment group are those who received training in 1964
- Control group are random sample of population as a whole

Earnings for period 1959-69



Things to Note..

- Earnings for trainees very low in 1964 as training not working in that year – should ignore this year
- Simple D-in-D approach would compare earnings in 1965 with 1963
- But earnings of trainees in 1963 seem to show a 'dip' – so D-in-D assumption probably not valid
- Probably because those who enter training are those who had a bad shock (e.g. job loss)

Differences-in-Differences: Summary

- A very useful and widespread approach
- Validity does depend on assumption that trends would have been the same in absence of treatment
- Often need more than 2 periods to test:
  - Pre-treatment trends for treatment and control to see if “fixed differences” assumption is plausible or not
  - See if there’s an Ashenfelter Dip

Issues

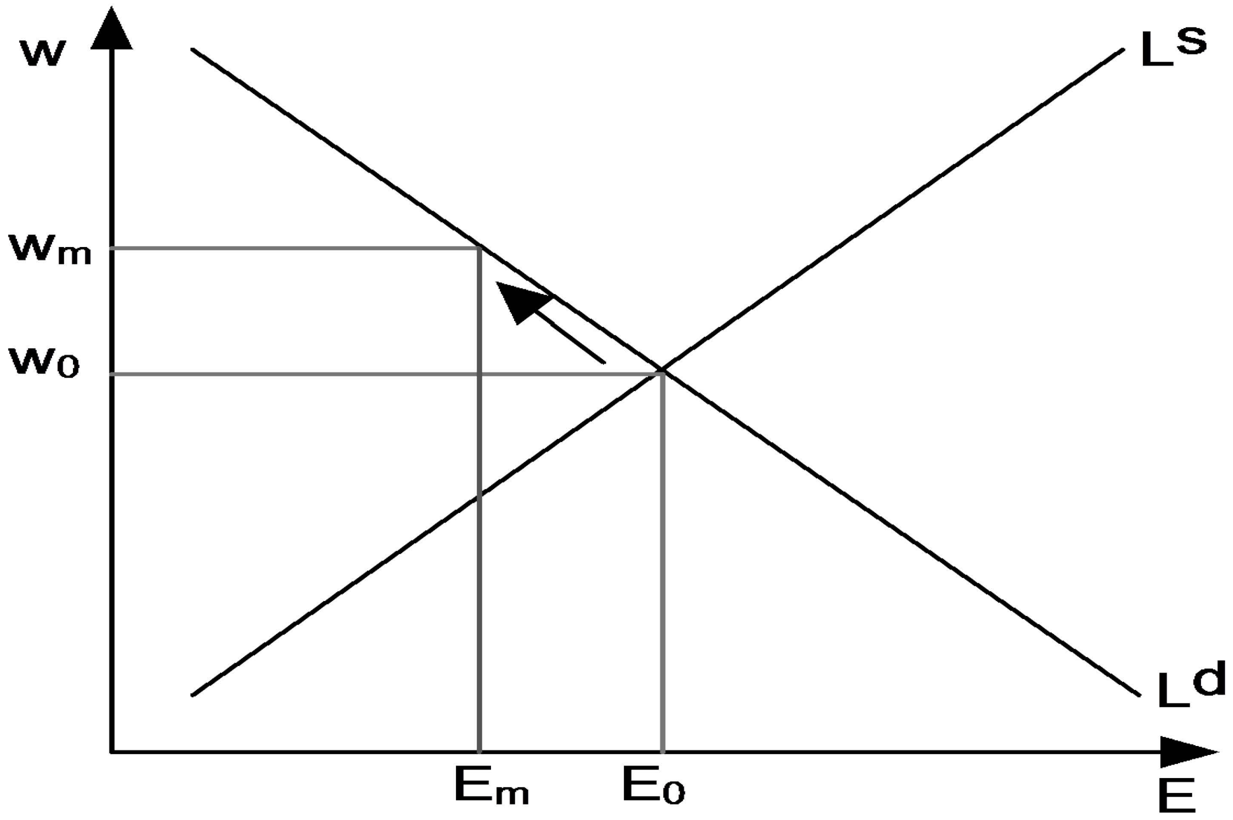
- Economic effects of minimum wages and evidence on minimum wages and employment

- The controversy on ‘conventional wisdom’ versus micro based ‘revisionist’ approach

Economic Effects of Minimum Wages

- Effect on employment/unemployment has been central issue in debate about economic effects of minimum wages.
- Standard textbook model of labour demand produces one of the clearest predictions in labour economics - minimum wages price workers out of jobs by forcing employers up their labour demand curve.

Standard Textbook Model



Standard Textbook Model

- Basic model rests upon several assumptions: complete coverage; homogeneous labour; competitive labour market; short run and long run impact the same.
- Clear prediction: the minimum wage increase results in reduced employment - the proportional reduction in employment ($\ln E_m - \ln E_0$) equals the proportionate wage increase ($\ln W_m - \ln W_0$) times the elasticity of demand η.
- Can develop more sophisticated models, but with assumption of perfect competition produce same qualitative predictions.

Two Sector Model

- Basic model can be generalised in various directions. One example is to move to a two sector model - covered/non-covered, set $E0 = 1$, $W0 = 1$.
- Demand for workers in the covered sector depends on the minimum wage, whereas demand in the uncovered sector depends upon the market wage.
- Minimum wage elasticity of employment =
- $c\eta\varepsilon \ln Wm / [1 - c + \varepsilon \ln Wm]$
- where c = proportion in covered sector, ε = elasticity of labour supply.
- If $c = 1$, $\varepsilon = \infty$ ⇨ standard one sector competitive model, η
- Example: $c = 0.7$, $\ln Wm = 0.6$, $\varepsilon = 0.3$, $\eta = -1$ ⇨ employment effect $= -0.26$.

Implications

- Only pertinent question is ‘how negative is the negative effect on employment?’
- Minimum wage hurts the people it sets out to help by pricing them out of work – even more the case since low skill people more likely to be low paid

Evidence

- Early empirical work largely supportive of basic model → 'conventional wisdom'.
- Usually based on aggregate time series studies of US employment/unemployment rates and minimum wages, usually focussing on teenagers

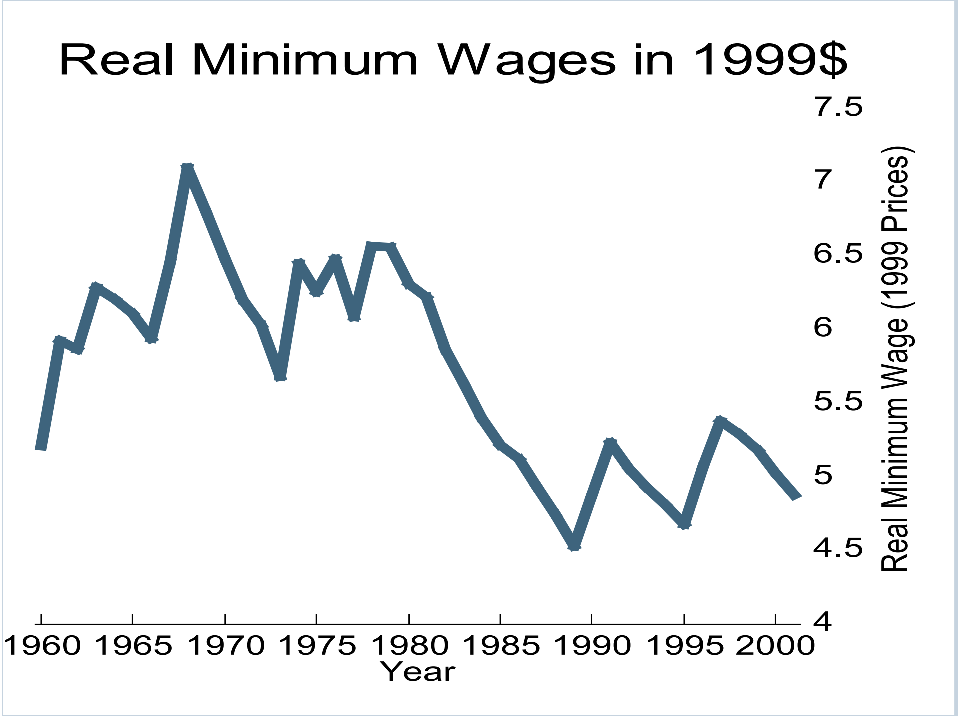
$$Y_t = g(MW_t, X_{1t},......X_{kt}) + e_t$$

where Y_t = employment / unemployment to population ratios (usually in logs), X_{it} = aggregate demand and supply variables (teenagers in training programmes, school enrollment, time trend), MW_t = minimum wage index (e.g. Kaitz index).

- Brown, Gilroy, Kohen (1982) Journal of Economic Literature - say "consensus" reached: minimum wages reduce teenage employment with elasticities in the -0.1 to -0.3 range.

Observations on Time Series Evidence: Re-Appraisal

1). US minimum wage fell strongly in real terms in the 1980s



Re-Appraisal (Continued)

Extending the samples of teenage employment studies into the 1980s produces much smaller, often statistically insignificant, elasticities below the ‘consensus’ range (around -0.07) (Card and Krueger, 1995).

Minimum wage effects on employment seem small (centring in on zero).

Or – labour demand curve inelastic so that employment not very sensitive to changes in minimum wages.

- What is best conceptual way to evaluate economic effect of minimum wage?
- ‘Before and after’ micro work more closely approximates the theoretical approaches that talk about labour markets with and without minimum wage floors – sometimes referred to as ‘revisionist’ approach.

Methodological Issues in Newer Research

- Corresponds better to theoretical concepts as adopts before and after approach, with treatment and control groups.
- If E is employment, T and C denote treatment and controls and 1 and 2 are the before and after treatment periods then an estimate of the impact of treatment is:

$$(E_{T2} – E_{C2}) – (E_{T1} – E_{C1})$$

or

$$(E_{T2} – E_{T1}) – (E_{C2} – E_{C1})$$

Most famous piece is Card and Krueger's (1994) New Jersey / Pennsylvania comparison

New Jersey/Pennsylvania Comparison (Card and Krueger, 1994)

- Can be viewed as case study of fast food industry.
- Surveyed fast food restaurants in New Jersey and Pennsylvania in February-March and November-December 1992.
- In April 1992 the New Jersey minimum wage went up from the federal minimum level of $4.25 to $5.05 but the minimum in Pennsylvania remained at $4.25.

Minimum Wages and Employment:
A Case Study of the Fast-Food Industry in
New Jersey and Pennsylvania

David Card and Alan B. Krueger

American Economic Review 84(4),
September 1994: 772-793.

- Question of Interest

"How do employers in a low-wage labor market respond to an increase in the minimum wage?"

- Approach

Compare employment of teenagers in New Jersey and eastern Pennsylvania before and after the increase in the minimum wage in NJ from $4.25 to $5.05 on April 1, 1992.

- Methods of Analysis
  - Difference-in-Differences
  - Regression Analysis

- <u>Data</u>
  - Phone survey of fast-food restaurants in NJ and eastern Penn (personal interview for 28 stores in wave 2)
    - Wave 1: late Feb. and early Mar. 1992
    - Wave 2: Nov. and Dec. 1992
    - Restaurants: Burger King, KFC, Wendy's and Roy Rogers
    - 371 restaurants were interviewed in both waves
- What if employment increased when the MW in NJ increased? Did the increase in MW cause the increase in employment?

- “Correlation does not mean causation.” Could be that econ conds improved, which caused E to go up.

- Difference-in-Differences Approach
  - Premise – Economic conditions in NJ and eastern Penn are the same.
  - Compare E change in NJ before and after the MW change, with the E change in eastern Penn in the same time period.
  - If NJ and eastern Penn E grow at the same rate, MW would have had no effect.
  - If E in Penn grew or stayed the same, and E in NJ fell → evidence that MW decreases E.

- Results of Difference-in-Difference Analysis

Employment in Typical Fast-Food Restaurants (in FTEs)

	NJ	E Penn
Before change	20.4	23.3
After change	21.0	21.2
Difference	0.6	-2.1

Difference-in-Differences = 0.6 – (-2.1) = **2.7**

NJ: Employment increased after MW.
E Penn: Employment decreased after MW → depressed economy in E Penn and NJ.

Despite declining economic conditions, in E Penn (and presumably NJ), employment ***increased*** in NJ.

- <u>Regression Analysis</u>
  - Equation (1a): ✦E = a + bX + cNJ
    - ✦E = change in employment from wave 1 to wave 2 at a given restaurant
    - X = set of characteristics of the restaurant
    - NJ = 1 if restaurant is in New Jersey;
      NJ = 0 if restaurant is in eastern Pennsylvania.
      - If c < 0, ✦E lower for NJ than for Penn restaurants after the MW
      - If c > 0, ✦E higher for NJ
      - If c = 0, no difference in the change in E

- Equation (1b): ✦E = a' + b'X + c'GAP
  - GAP = 0 for stores in Penn
  - GAP = 0 for stores in NJ with W_1 👓 \$5.15
  - GAP = (5.05 – W_1)/ W_1 for other stores in NJ
  - GAP is the proportional increase in wages needed to meet the new MW.
    - If c' < 0, an increase in the required wage hike, results in a negative change in employment.
    - If c' > 0, ✦E higher as stores have to pay more to meet the MW requirement.
    - If c' = 0, no difference in the change in E for stores that have to increase wages more.

- Regression Results

  - Equation (1a): ✦E = a + bX + cNJ
    - c >0

  - Equation (1b): ✦E = a’ + b’X + c’GAP
    - c’ >0

  - Indicates MW is positively associated with ✦E.
  - Results robust across many specification tests.

- Conclusion

April, 1992 rise in the MW did not decrease employment of teens in fast-food restaurants in New Jersey.

- Why Counter-Intuitive Result?

  - Survey data may not accurately measure actual employment. (Neumark and Wachter (2000) use establishment data with same methodology and find a zero or slightly negative effect.)

  - Fast-food restaurants may not be representative of low-wage employers, e.g., there may be fewer opportunities to substitute capital for labor.

  - Employers may need more time to adjust to minimum wage changes than comparing just before and just after a change.

- <u>Why Counter-Intuitive Result?</u>
  - Economic health of the fast-food market in E Penn may be an imperfect match for NJ.
  - Adjustment may begin before MW takes effect.

Identification of Employment Effects

$$\Delta E_i = a + bX_i + cNJ_i + e_i$$

$$\Delta E_i = a' + b'X_i + c'GAP_i + e'_i$$

Where GAP = 0 for P stores and NJ stores with $W_{1i} \geq \$5.05$ and = $(5.05 - W_{1i}) / W_{1i}$ for other NJ stores/

New Jersey/Pennsylvania Comparison (Continued)

TABLE 3—AVERAGE EMPLOYMENT PER STORE BEFORE AND AFTER THE RISE IN NEW JERSEY MINIMUM WAGE

	Stores by state			Stores in New Jersey[a]			Differences within NJ[b]	
Variable	PA (i)	NJ (ii)	Difference, NJ − PA (iii)	Wage = \$4.25 (iv)	Wage = \$4.26–\$4.99 (v)	Wage ≥ \$5.00 (vi)	Low–high (vii)	Midrange–high (viii)
1. FTE employment before, all available observations	23.33 (1.35)	20.44 (0.51)	−2.89 (1.44)	19.56 (0.77)	20.08 (0.84)	22.25 (1.14)	−2.69 (1.37)	−2.17 (1.41)
2. FTE employment after, all available observations	21.17 (0.94)	21.03 (0.52)	−0.14 (1.07)	20.88 (1.01)	20.96 (0.76)	20.21 (1.03)	0.67 (1.44)	0.75 (1.27)
3. Change in mean FTE employment	−2.16 (1.25)	0.59 (0.54)	2.76 (1.36)	1.32 (0.95)	0.87 (0.84)	−2.04 (1.14)	3.36 (1.48)	2.91 (1.41)
4. Change in mean FTE employment, balanced sample of stores[c]	−2.28 (1.25)	0.47 (0.48)	2.75 (1.34)	1.21 (0.82)	0.71 (0.69)	−2.16 (1.01)	3.36 (1.30)	2.87 (1.22)
5. Change in mean FTE employment, setting FTE at temporarily closed stores to 0[d]	−2.28 (1.25)	0.23 (0.49)	2.51 (1.35)	0.90 (0.87)	0.49 (0.69)	−2.39 (1.02)	3.29 (1.34)	2.88 (1.23)

Notes: Standard errors are shown in parentheses. The sample consists of all stores with available data on employment. FTE (full-time-equivalent) employment counts each part-time worker as half a full-time worker. Employment at six closed stores is set to zero. Employment at four temporarily closed stores is treated as missing.

[a]Stores in New Jersey were classified by whether starting wage in wave 1 equals \$4.25 per hour ($N = 101$), is between \$4.26 and \$4.99 per hour ($N = 140$), or is \$5.00 per hour or higher ($N = 73$).

[b]Difference in employment between low-wage (\$4.25 per hour) and high-wage (≥ \$5.00 per hour) stores; and difference in employment between midrange (\$4.26–\$4.99 per hour) and high-wage stores.

[c]Subset of stores with available employment data in wave 1 and wave 2.

[d]In this row only, wave-2 employment at four temporarily closed stores is set to 0. Employment changes are based on the subset of stores with available employment data in wave 1 and wave 2.

Employment Models

TABLE 4—REDUCED-FORM MODELS FOR CHANGE IN EMPLOYMENT

	Model				
Independent variable	(i)	(ii)	(iii)	(iv)	(v)
1. New Jersey dummy	2.33 (1.19)	2.30 (1.20)	—	—	—
2. Initial wage gap[a]	—	—	15.65 (6.08)	14.92 (6.21)	11.91 (7.39)
3. Controls for chain and ownership[b]	no	yes	no	yes	yes
4. Controls for region[c]	no	no	no	no	yes
5. Standard error of regression	8.79	8.78	8.76	8.76	8.75
6. Probability value for controls[d]	—	0.34	—	0.44	0.40

Notes: Standard errors are given in parentheses. The sample consists of 357 stores with available data on employment and starting wages in waves 1 and 2. The dependent variable in all models is change in FTE employment. The mean and standard deviation of the dependent variable are −0.237 and 8.825, respectively. All models include an unrestricted constant (not reported).

[a]Proportional increase in starting wage necessary to raise starting wage to new minimum rate. For stores in Pennsylvania the wage gap is 0.

[b]Three dummy variables for chain type and whether or not the store is company-owned are included.

[c]Dummy variables for two regions of New Jersey and two regions of eastern Pennsylvania are included.

[d]Probability value of joint F test for exclusion of all control variables.

Card and Krieger (1994) dataset (fastfood.dta) can be obtained here:

http://www.stat.ucla.edu/projects/datasets/