Policy Evaluation

3. Places Matter!

Introduction to difference-in-differences methods

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Affordable Housing Policies

Are these policies effective, and how can they be better designed to improve social mobility?

Study this question here by focusing specifically on the role of housing vouchers for low-income families based on Chetty, Hendren, and Katz (2016 AER)

Will a given child i's earnings at age 30 (Yi) be higher if her family receives a housing voucher?

Consider:
Yi(V=1) = child's earnings if family gets voucher
Yi(V=0) = child's earnings if family does not get voucher
Goal: estimate treatment effect of voucher on child i:

$$Ti = Yi(V=1) - Yi(V=0)$$

It is very difficult to answer these questions!

Fundamental problem in empirical science: we do not observe Yi(V=1) and Yi(V=0) for the same person

We only see one of the two potential outcomes for each child!

Ideally, we want to know if they have a positive, null or a negative effect (statistical confidence), but also the economic magnitude

How can we solve this problem? RCT
Intuition: two groups are identical except for getting voucher $\Rightarrow$ difference in earnings (for example) is the causal effect of the voucher for this outcome variable

Why is it important to randomize?

Suppose we instead compared 1000 people, half of whom applied for a voucher and half of whom didn't

Could still compare average earnings in these two groups

But in this case, there is no guarantee that differences in earnings are only driven by the voucher

There could be many other differences across the groups:
• Those who applied may be more educated, better informed, or have more social skills
• Or they may live in different (worse?) areas to begin with

Adjusting for Non-Compliance

Solution: adjust estimated impact for rate of compliance

Example: suppose that only half the people offered a voucher actually used it to rent a new apartment

Suppose raw difference in earnings between those offered voucher and not offered voucher is $1,000

Then effect of using voucher to rent a new apartment must be $2,000 (since there is no effect on those who don't move)

More generally, divide estimated effect by rate of compliance:

LATE = Estimated Impact/Compliance Rate

The Moving to Opportunity (MTO) Experiment

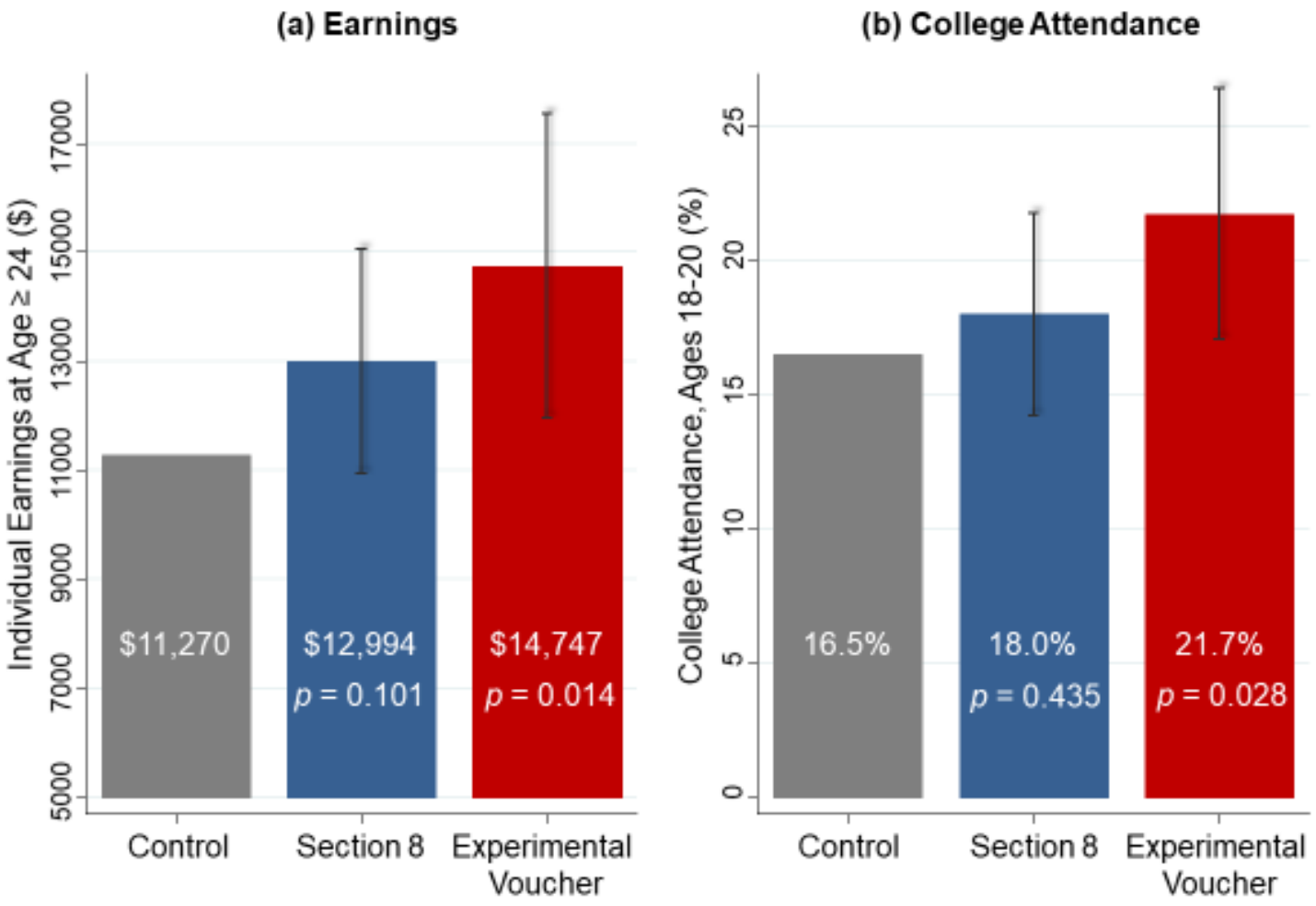
Implemented from 1994-1998 at 5 sites: Baltimore, Boston, Chicago, LA, New York

4,600 families were randomly assigned to one of three groups:

a) Experimental: offered housing vouchers restricted to low-poverty Census tracts (Compliance rate: 48%)
b) Section 8: offered conventional housing vouchers, no restrictions (Compliance rate: 66% used voucher)
c) Control: not offered a voucher, stayed in public housing

Early research on MTO found little impact of moving to a better area, but it focused primarily on adults and older youth at point of move (e.g., Kling et al., 2007 ECMA) $\Rightarrow$ did not consider exposure effects among children

Impacts of MTO on Children Below Age 13 at Random Assignment



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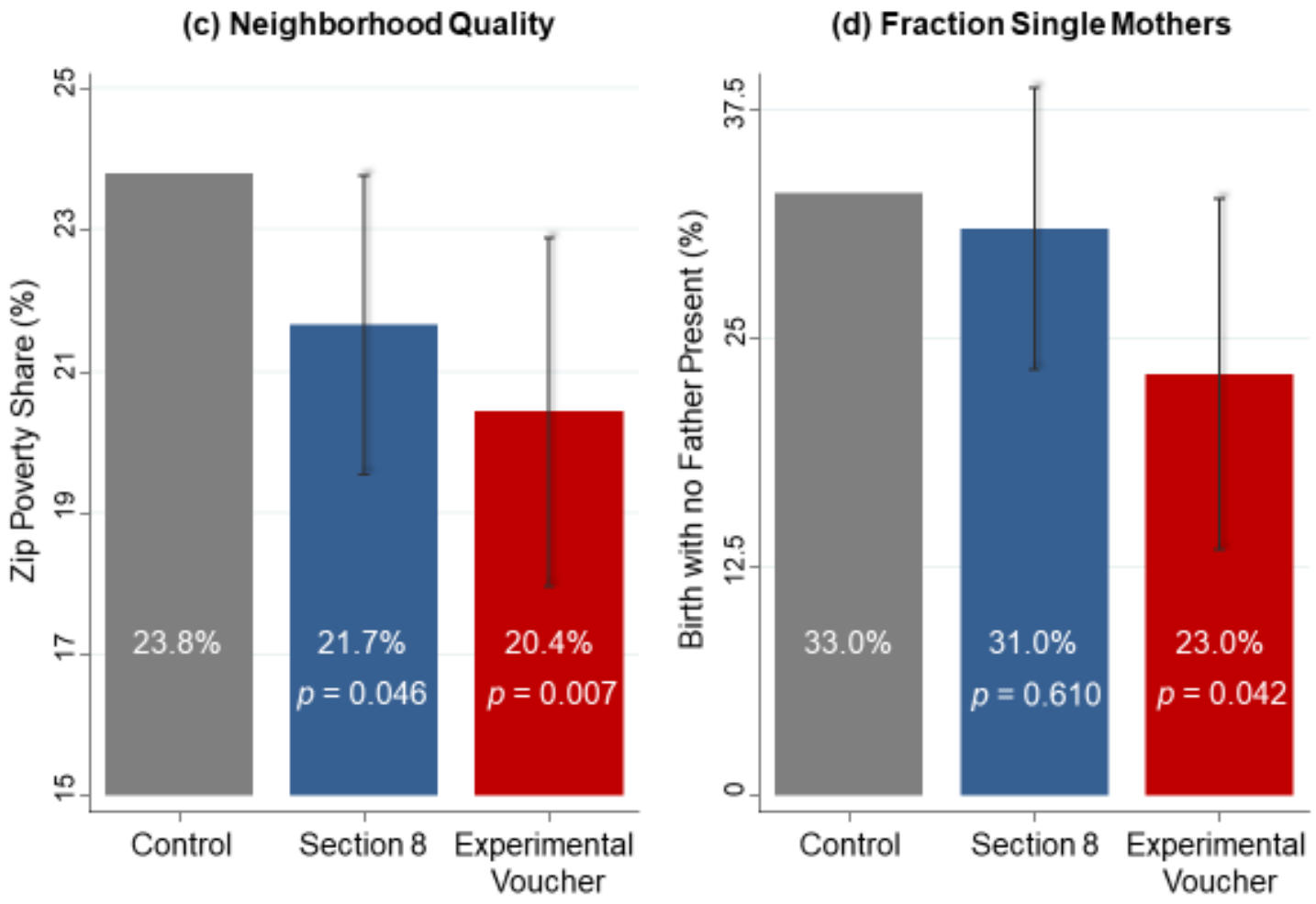
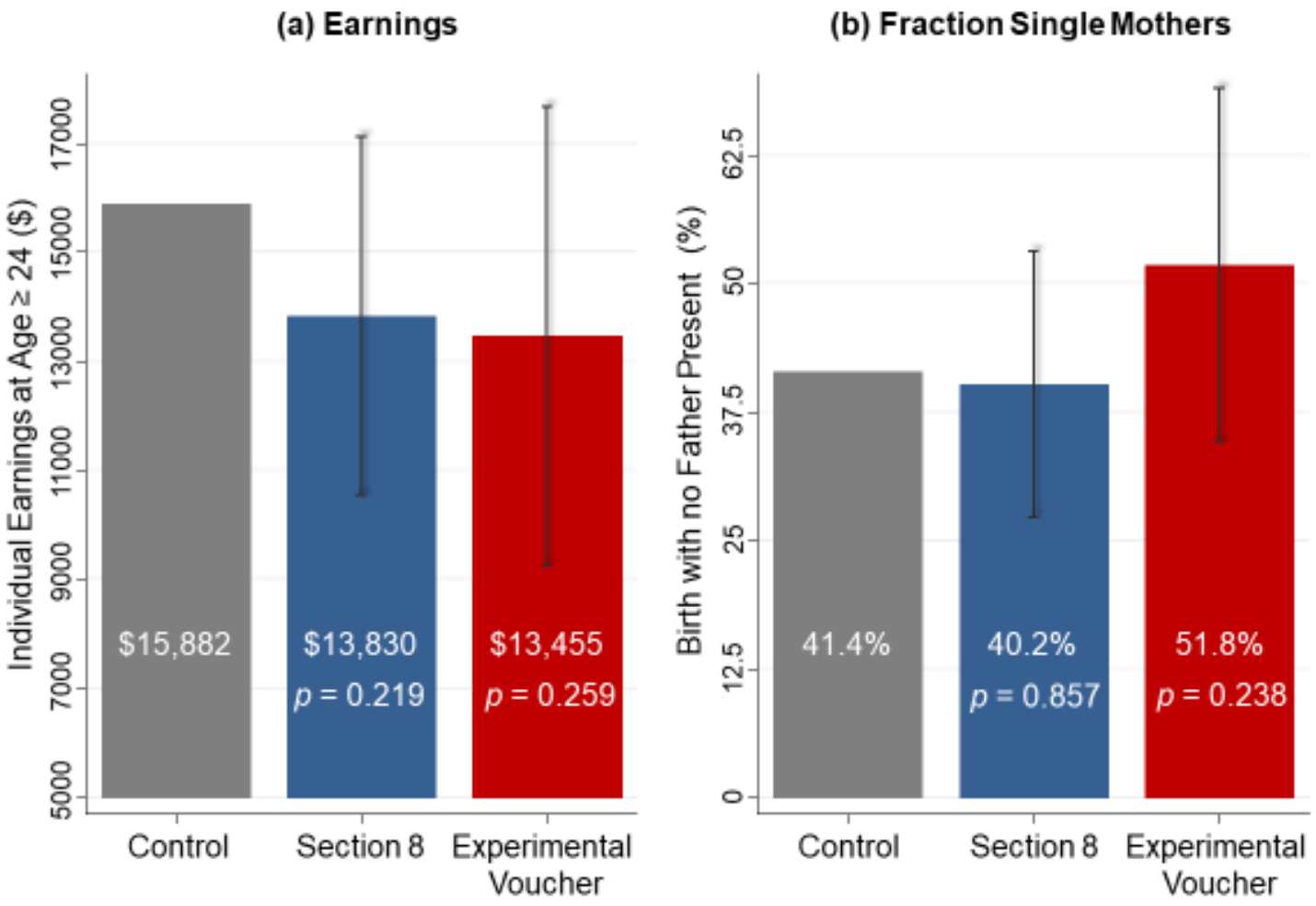
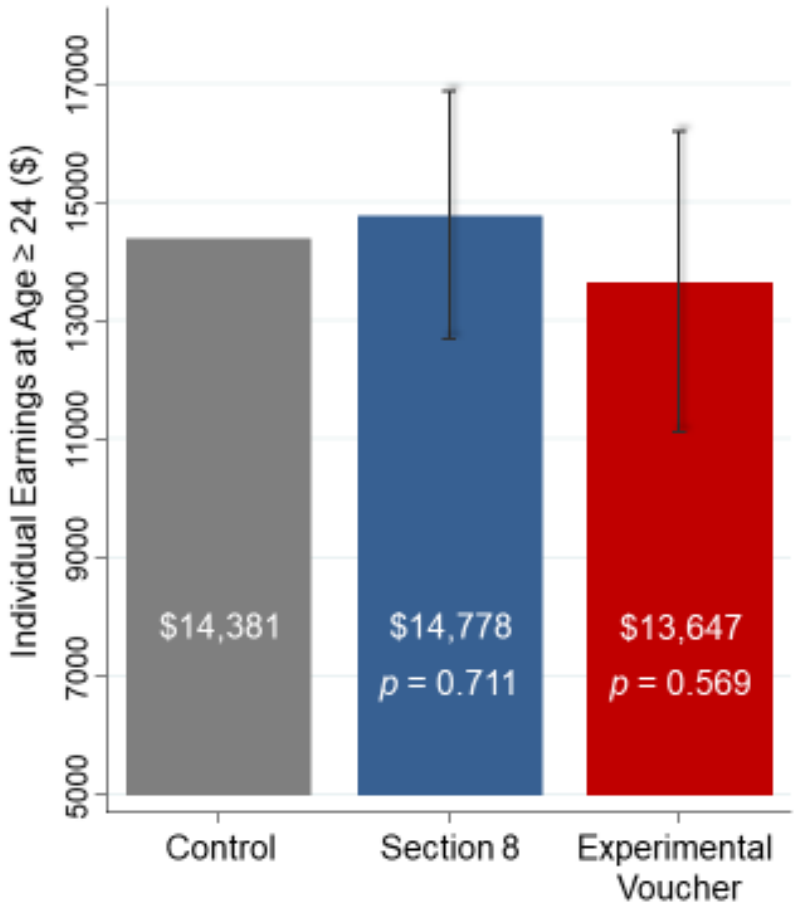


Impacts of MTO on Children Age 13-18 at Random Assignment



Impacts of Moving to Opportunity on Adults' Earnings



Policy implications

Housing vouchers can be very effective if carefully targeted at families with young children

Vouchers should be explicitly designed to help families move to affordable, high-opportunity areas

More generally, low-income families rarely use cash transfers to move to better neighbourhoods (Jacob et al., 2015 QJE). Why don't low-income families move two opportunity?

1. Preferences: families may prefer to stay in current neighborhoods because of other amenities (e.g., commute time, proximity to family,...)

2. Barriers: families may be unable to find housing in high-opportunity areas because of lack of information, search frictions, landlords' tastes

Qualitative evidence on mechanisms

Qualitative study of 110 families interviewed for 2 hours during each process and post-move

Key lessons:

1. Scarcity: most families have extremely limited time and resources to search

2. Customization: case workers' ability to respond to each family's specific needs is crucial All of this suggests that simply providing adequate rental payments is insufficient to induce moves to opportunity $\Rightarrow$ need to provide additional customized support in each process, informational campaigns,...

Moving to Opportunity: Potential Concerns

1. Costs: is the voucher program too expensive to scale up?
Vouchers can save taxpayers money relative to public housing projects in long run

2. Negative spillovers and general equilibrium effects: does integration hurt the rich?
Evaluate this by examining how outcomes of the rich vary across areas in relation to outcomes of the poor
Empirically, did not seem to be the case, on average

3. Limits to scalability
Moving everyone from one neighborhood to another is unlikely to have significant effects $\Rightarrow$ policies that improve low-mobility neighborhoods rather than moving low-income families

Quasi-experiments using big data can address these issues

Chetty and Hendren (2018, QJE) approximate the MTO experiment studying 3 million families in the US

People who move vs. people who do not move to different areas are not comparable

But people who move when children are younger vs. older are more likely to be $\Rightarrow$ approximate experimental conditions

Key idea: exploit variation in age of child when family moves comparing the outcomes of siblings within families, studying moves triggered by displacement shocks, and exploiting sharp variation in predicted place effects

They show that neighbourhoods in which children grow up shape their earnings, college attendance, fertility, and marriage

Quasi-experiments using big data can address these issues

Quasi-experimental approach addresses limitations of MTO experiment:

- Sample size: much larger samples yield precise estimates

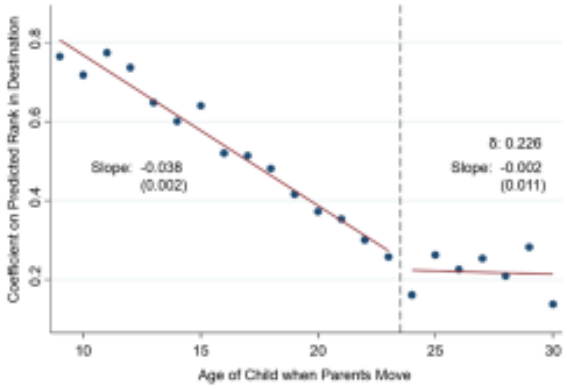
- Generalizability: results generalize to all areas of the U.S.

Limitation of quasi-experimental approach: reliance on stronger assumptions

Bottom line: reassuring to have evidence from both approaches that is consistent $\Rightarrow$ clear consensus that moving to opportunity works: in the US and in other contexts

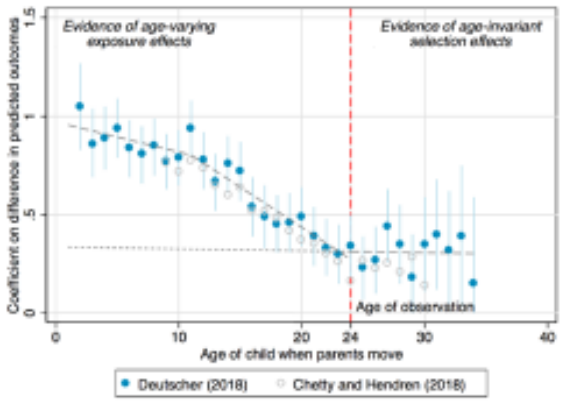
Childhood Exposure Effects Around the World

United States



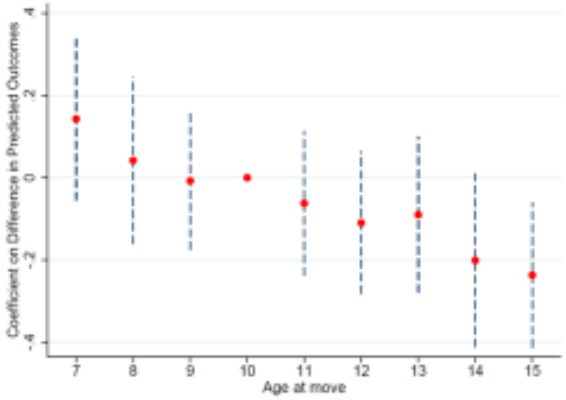
Source: Chetty and Hendren (QJE 2018)

Australia



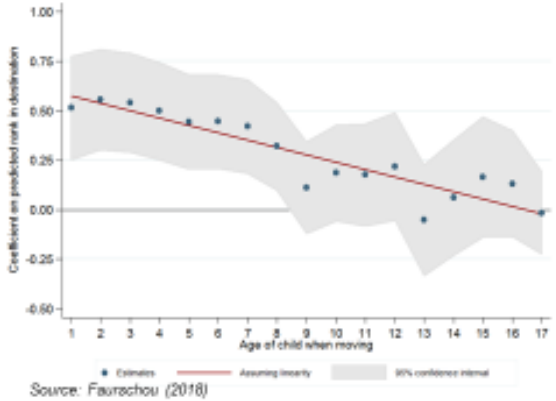
Source: Deutscher (2018)

Montreal, Canada



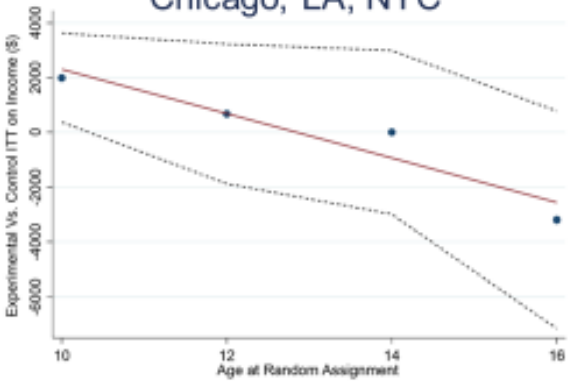
Source: Laliberté (2018)

Denmark



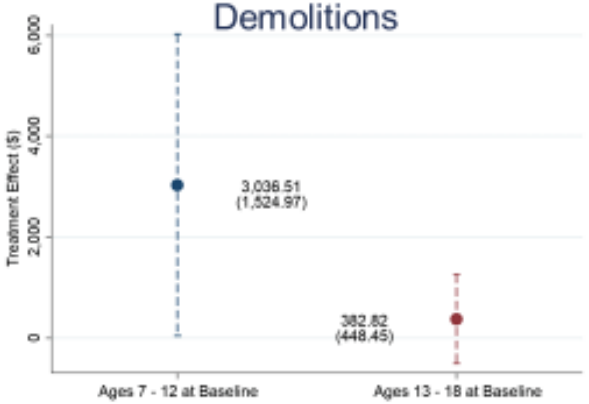
Source: Faurschou (2018)

MTO: Baltimore, Boston, Chicago, LA, NYC



Source: Chetty, Hendren, Katz (AER 2016)

Chicago Public Housing Demolitions



Source: Chyn (AER 2018)

A seminal contribution

On April 1, 1992, New Jersey raised its state minimum wage from $4.25 to $5.05. The min wage in nearby Pennsylvania stayed at $4.25 until 1996

Card and Krueger (1994), studied the change in low-wage employment in NJ to the corresponding change in PA

Key assumption: if not for the minimum wage change, low-wage employment in NJ would have trended similarly as it did in PA

CK surveyed fast food restaurants (innovative!) before and after, in both states, and famously found a positive effect from the DiD

`https://davidcard.berkeley.edu/papers/njmin-aer.pdf`

This was very controversial at the time!

The inverse relationship between quantity demanded and price is the core proposition in economic science, which embodies the presupposition that human choice behavior is sufficiently rational to allow predictions to be made. Just as no physicist would claim that "water runs uphill," no self-respecting economist would claim that increases in the minimum wage increase employment. Such a claim, if seriously advanced, becomes equivalent to a denial that there is even minimal scientific content in economics, and that, in consequence, economists can do nothing but write as advocates for ideological interests. Fortunately, only a handful of economists are willing to throw over the teaching of two centuries; we have not yet become a bevy of camp-following whores."

James M. Buchanan, 1986 Nobel laureate in economics, writing in the Wall Street Journal on April 25, 1996

Introduction to Difference-in-Difference (DD)

Two groups: Treatment group (T) which faces a change and control group (C) which does not

Compare the evolution of T group (before and after change) to the evolution of the C group (before and after change)

DD identifies the **treatment effect** if the parallel trend assumption holds: Absent the change, T and C would have evolved in parallel

DD most convincing when groups are very similar to start with

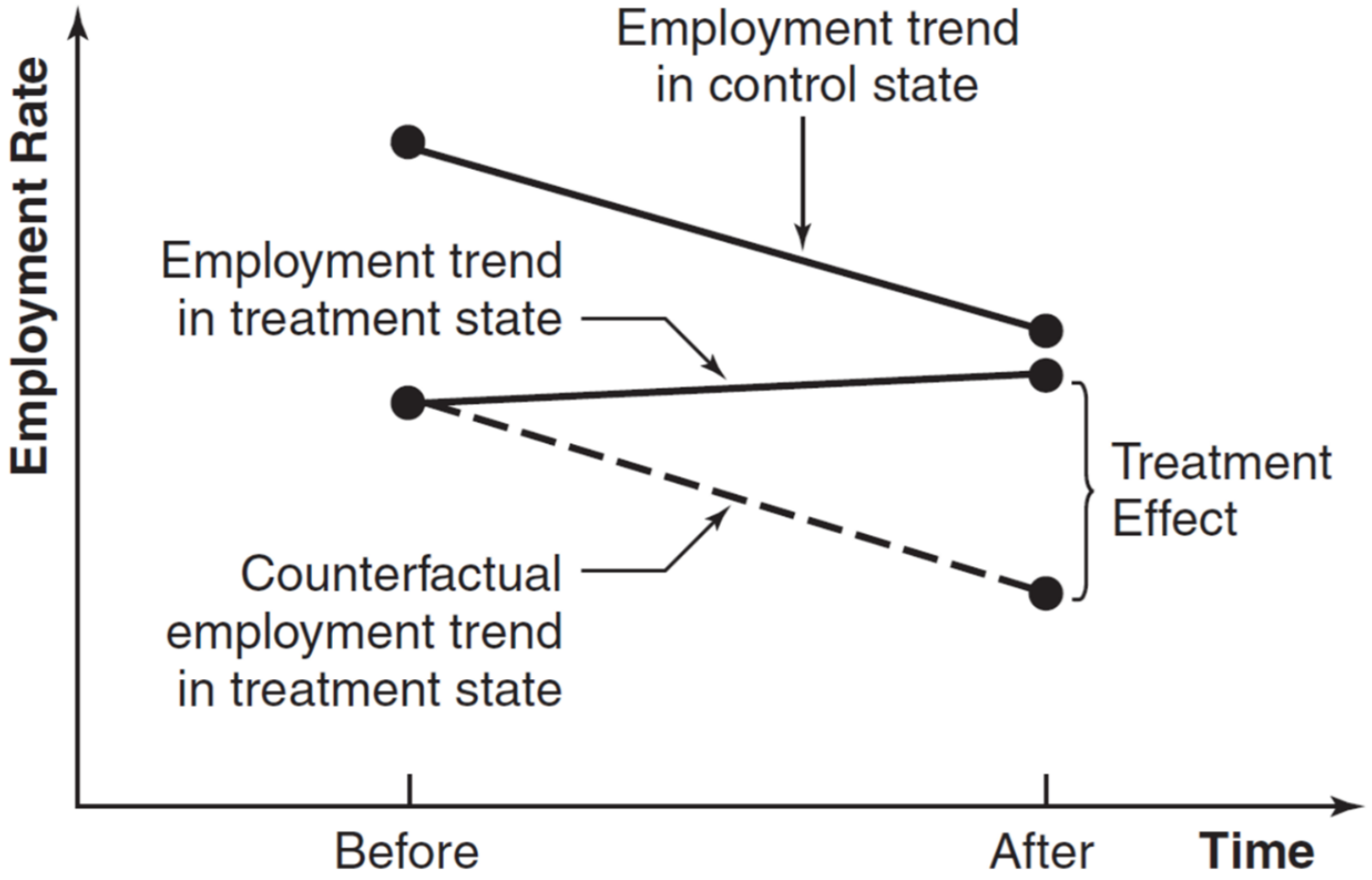
Other assumptions: no anticipation effects, no concurrent policies affecting only T or C

Why is before-after not enough?

Average employment in fast food restaurants before and after the New Jersey minimum wage increase

Variable	PA (i)	NJ (ii)	Difference, NJ – PA (iii)
1. FTE employment before, all available observations	23.33 (1.35)	20.44 (.51)	−2.89 (1.44)
2. FTE employment after, all available observations	21.17 (.94)	21.03 (.52)	−.14 (1.07)
3. Change in mean FTE employment	−2.16 (1.25)	.59 (.54)	2.76 (1.36)

Notes: Adapted from Card and Krueger (1994), table 3. The table reports average full-time-equivalent (FTE) employment at restaurants in Pennsylvania and New Jersey before and after a minimum wage increase in New Jersey. The sample consists of all restaurants with data on employment. Employment at six closed restaurants is set to zero. Employment at four temporarily closed restaurants is treated as missing. Standard errors are reported in parentheses.


Employment trend
in control state
Employment trend
in treatment state
Treatment
Effect
Counterfactual
employment trend
in treatment state
Employment Rate
Before
After
Time

Automating DiD

The canonical "2×2" DiD can be run by regression:

$$Y_{it} = \alpha_i + \lambda t + \beta D_{it} + \epsilon_{it}$$

where $D_{it} = Treatmentgroup_i$ x $Treatmentperiod_t$ = 1 if NJ post-1992 and 0 otherwise

If CK observed a panel of fast food workers, they could have included individual FE (same β if nobody moved/ quit)

Running this regression has the advantage of automating SEs on β

We can also generalize to n time periods

Problems with the 2x2 case

1. Even if we had worker level data, effectively 4 observations: can't separate β from other shocks in NJ relative to PA $\Rightarrow$ include more units in other treated and control states

2. No evidence that the parallel trend is likely to hold in this setting $\Rightarrow$ more on this when we discuss DiD event studies!

3. These SEs are not clustered, and cannot be in this case $\Rightarrow$ same as in 1.

Place-based policies

Increasing regional inequality has become a major concern for policymakers both in the US and Europe.

Place-based policies, which directly subsidise regions that are economically lagging behind, are a prominent instrument in policymakers' toolkits

The US currently devotes about $60 billion to place-based policies – mostly through business tax incentives (Slattery and Zidar 2020)

Between 2014 and 2020, the EU spent more than €350 billion – about a third of its budget – on regional policies (Ehrlich and Overman 2020, JEP)

Empowerment Zone (EZ) Program

Busso, Gregory and Kline (2013, AER) conduct the first microfounded equilibrium welfare evaluation of a large-scale place based policy

The EZ program is a series of incentives to encourage investment in the neediest urban and rural areas

It consists of spatially targeted investments, such as employment tax credits (roughly 20% wage subsidy) and block grants (infrastructure improvement, training programs, access to credit,...)

(Theoretical model +) Empirical strategy involves comparing EZ neighborhoods to rejected and future EZ areas using a difference-in-differences estimator

Detroit

Chicago

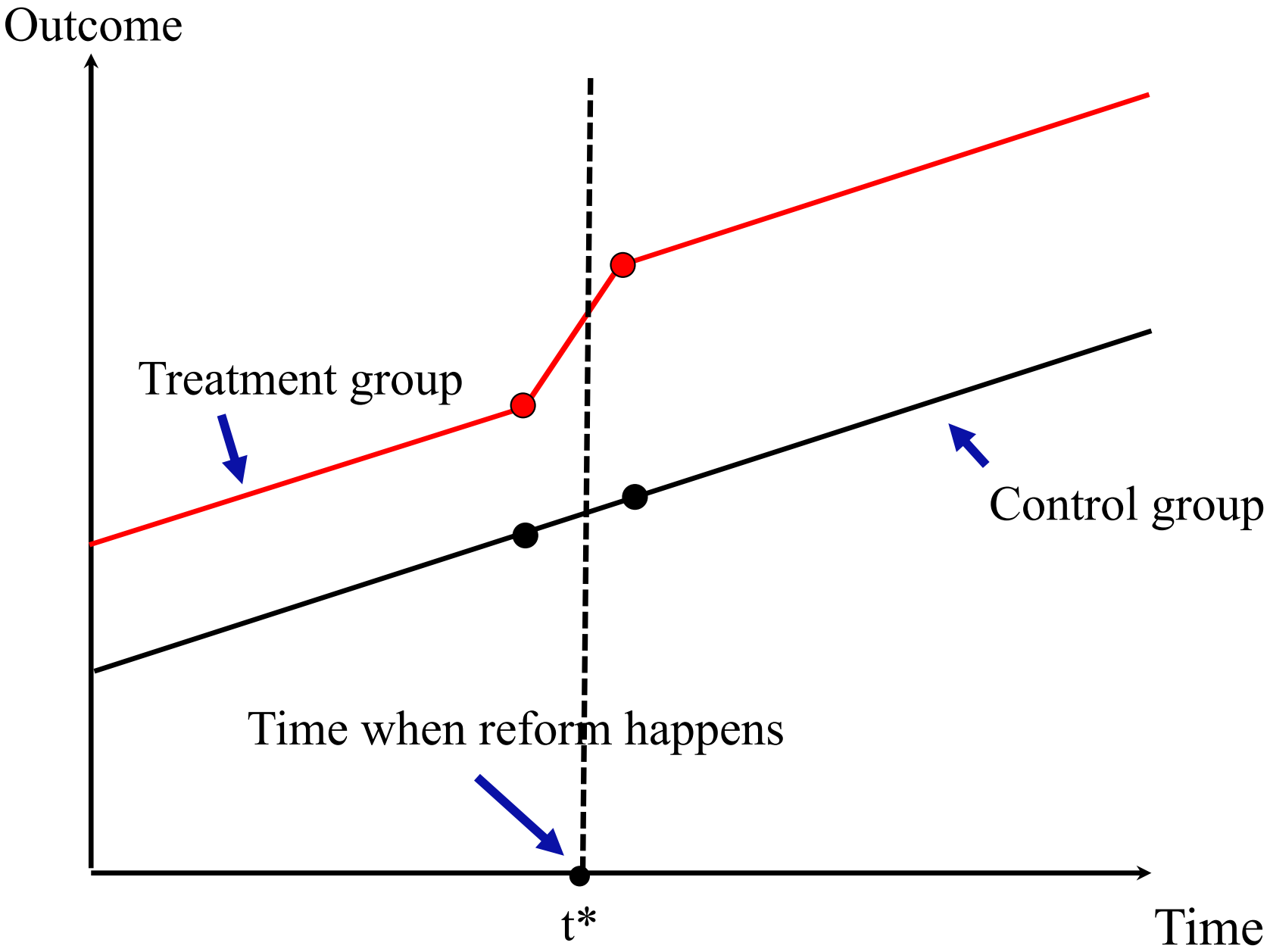
Before EZ

Camden (inside EZ), New Jersey, *1993*

After EZ

Same street in Camden (inside EZ), New Jersey, *2003*

Difference-in-Difference Econometric Method



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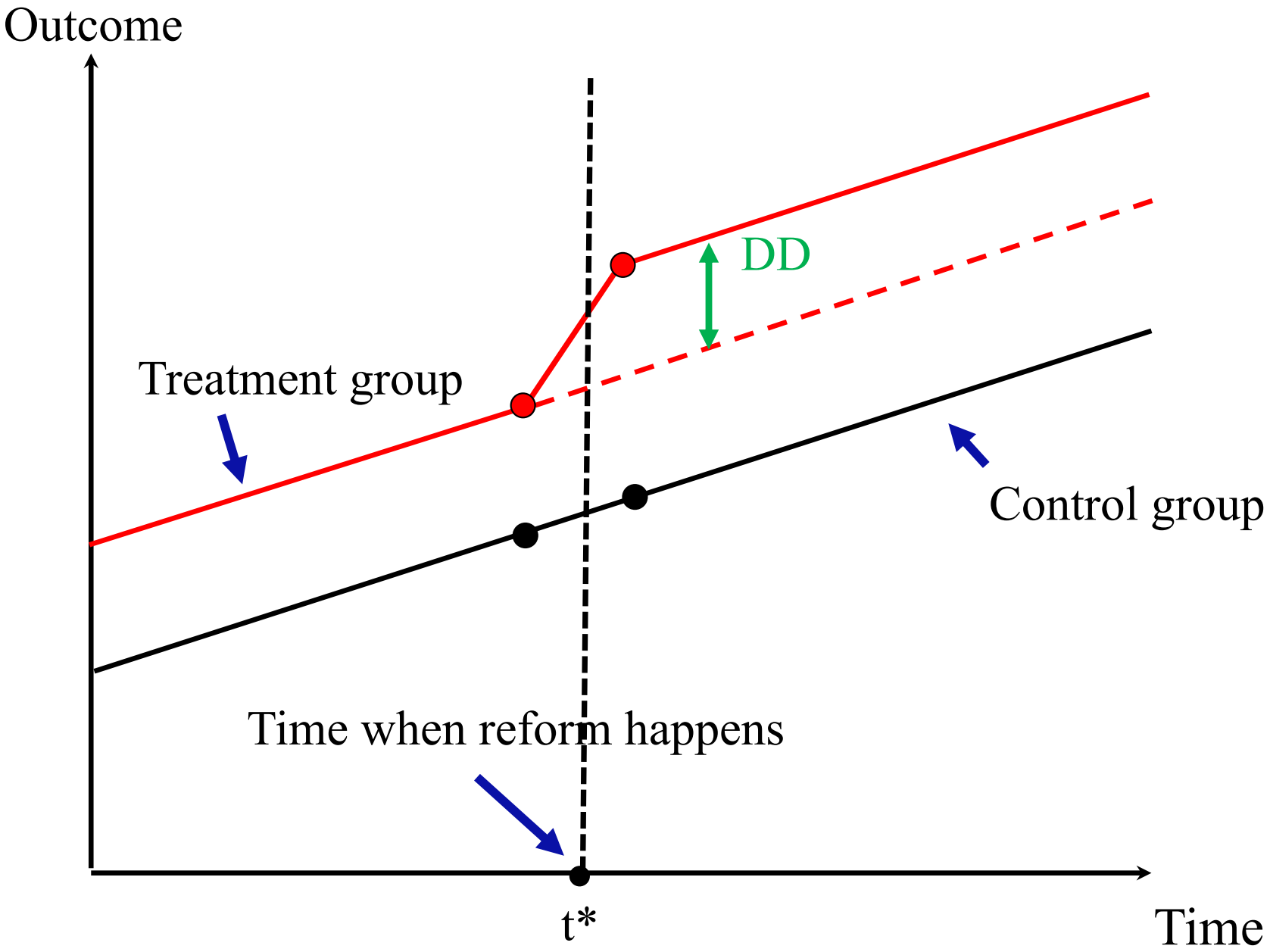
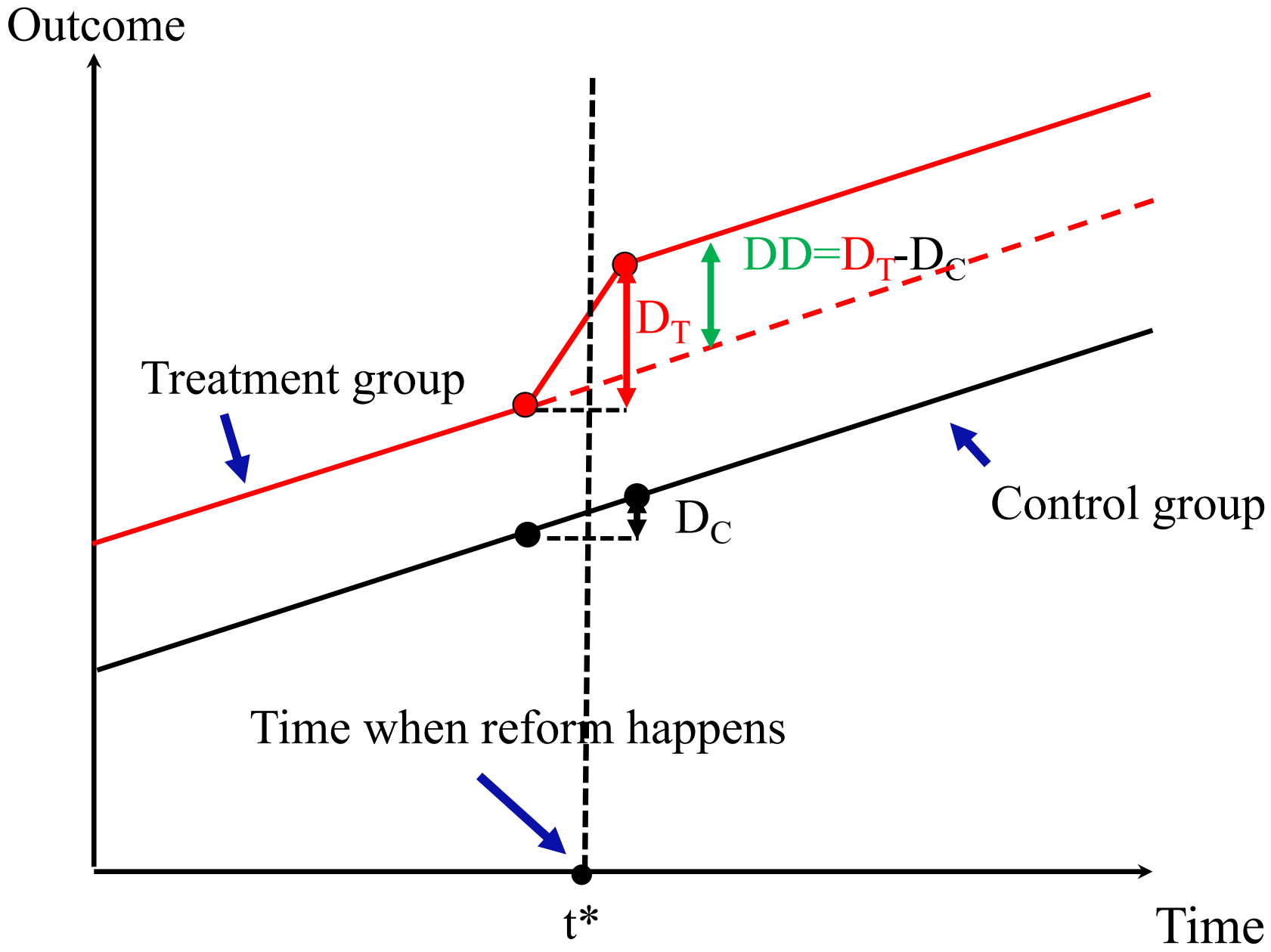


Difference-in-Difference Econometric Method



Busso et al. (2013, AER) results

Significant increase in employment and wages for a poor population

Negligible cost of living increase but possible windfall gain to homeowners

Little change in demographic composition but only 57% of households in same house as 5 years ago

Risk of gentrification and landlord capture over longer run

Evaluating Local Business Incentives

Consider how business incentives affect:

1. **Workers** benefit from employment, higher wages, lower local prices, lower taxes, high-quality government services

2. **Capital owners** benefit from higher after-tax-and-incentive profits, product demand, productivity

3. **Politicans** benefit from increases in re-election odds, campaign contributions, pork provision opportunities $\Rightarrow$ we call this Political economy considerations

There are several policy instruments with which to maximize these objective functions

Some Local Business Tax Incentives

1. **Lowering the corporate tax rate:** lowers tax bill, encouraging entry of new firms and expansion of existing firms

2. **Narrowing the corporate tax base:** lower tax bill for set of firms, based on activity/industry. Encourages entry of new firms in that industry/increase in targeted activity

3. **Offering firm-specific tax incentives:** offer one firm a subsidy for their commitment to locate in the jurisdiction and create a certain level of (skilled) employment and investment
• Hard-to-build relationship-specific capital with local suppliers

Costs to using firm-specific tax incentives

1. It is hard to pick winners

• Rationale for targeting assumes local governments can identify "high-benefit" firm and forecast effect on local economy

• Estimating agglomeration economies very difficult

2. It is hard to know if firm is inframarginal

3. Lack of transparency leaves incentives exposed to political capture

4. Most distressed places may not be able to afford to compete

5. Providing generous incentives requires raising revenue from other taxes

• Social cost of higher taxes grows quickly with size of tax

• Benefit of incentives diminish with size of incentives

Slattery and Zidar (2020 JEP)

2008 VW Deal in Tennessee: chooses Chattanooga for new assembly plant and promises 2,000 empl and $1B investment

TN grants VW a subsidy worth $558 million

- Local property tax abatements over 30 years ($200M)
- Enhanced state job and investment tax credits ($200M)
- Property given to VW ($81M)
- Worker training ($30M)
- Road construction ($43M) + Rail line upgrades ($3.5M)

Initially considered "more than 100 candidate sites"

- Runner up: Huntsville, AL offers $386 million package

Differences in Employment Between Winner and Runner-up

Interesting, but just one case study...

Difference-in-Difference (DD) Event studies

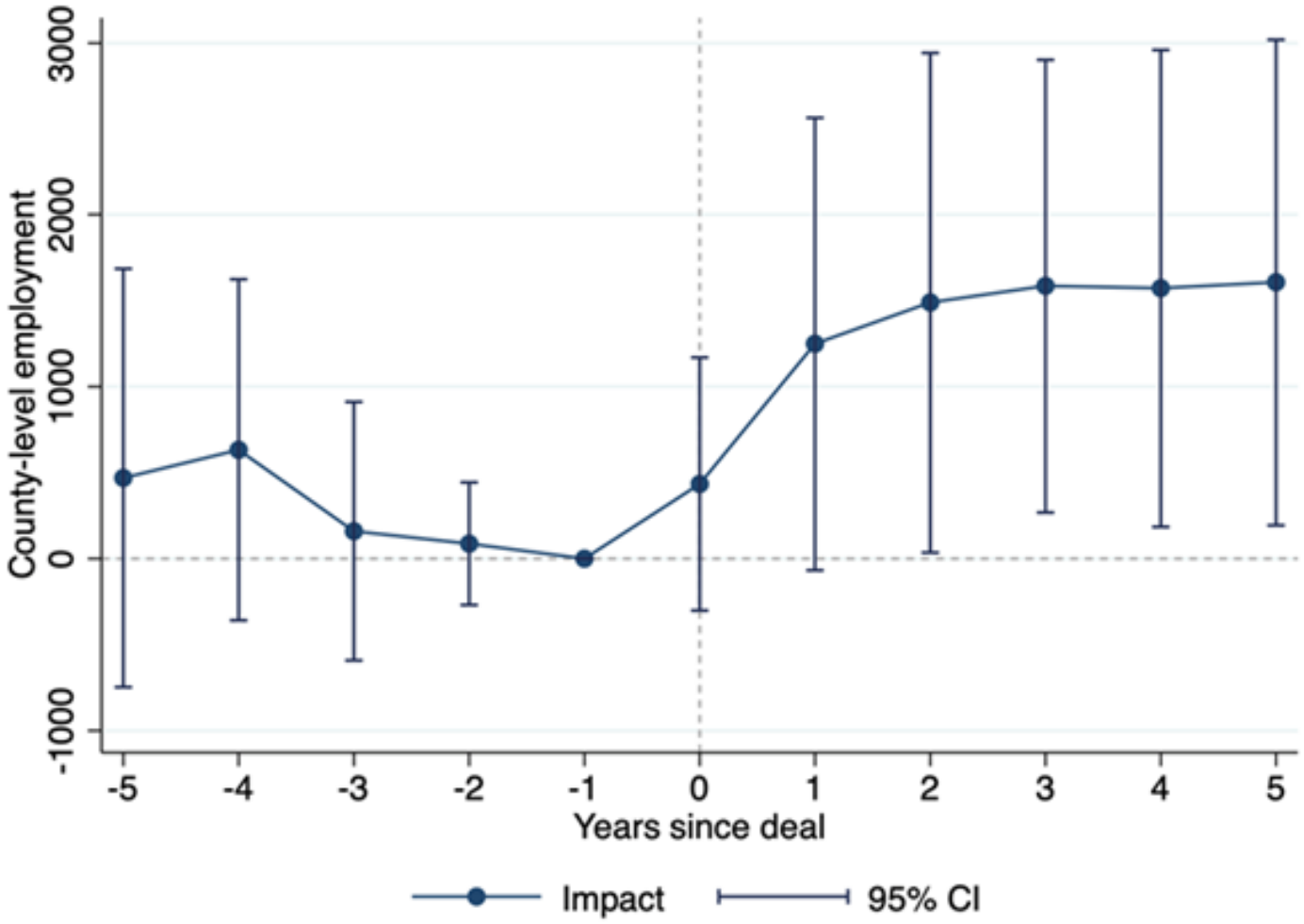
Event studies are generalizations of the DD design

The coefficients plot the behavior of outcomes in the treatment group relative to the control group before ($\Rightarrow$ show evidence that the PTA is likely to hold!) and after treatment ($\Rightarrow$ dynamic effects!)

A recent revolution: when treatment dates are staggered, then the ES approach is potentially a more efficient means of pooling together several different DDs, even in cases where all the units get treated $\Rightarrow$ huge recent literature starting in Goodman-Bacon (2021, JoE), Chaisemartin and D'Haultfœuille (2020, AER), Borusyak et al. (2021), Callway and Sant'Anna (2021, JoE),

Note: we cannot test the PTA! Honest parallel trends $\Rightarrow$ Roth (2022)

Expand event study design to compare "winner" to "runner-up" counties for deals between 2002-2012:



Central government policies can have heterogeneous local effects

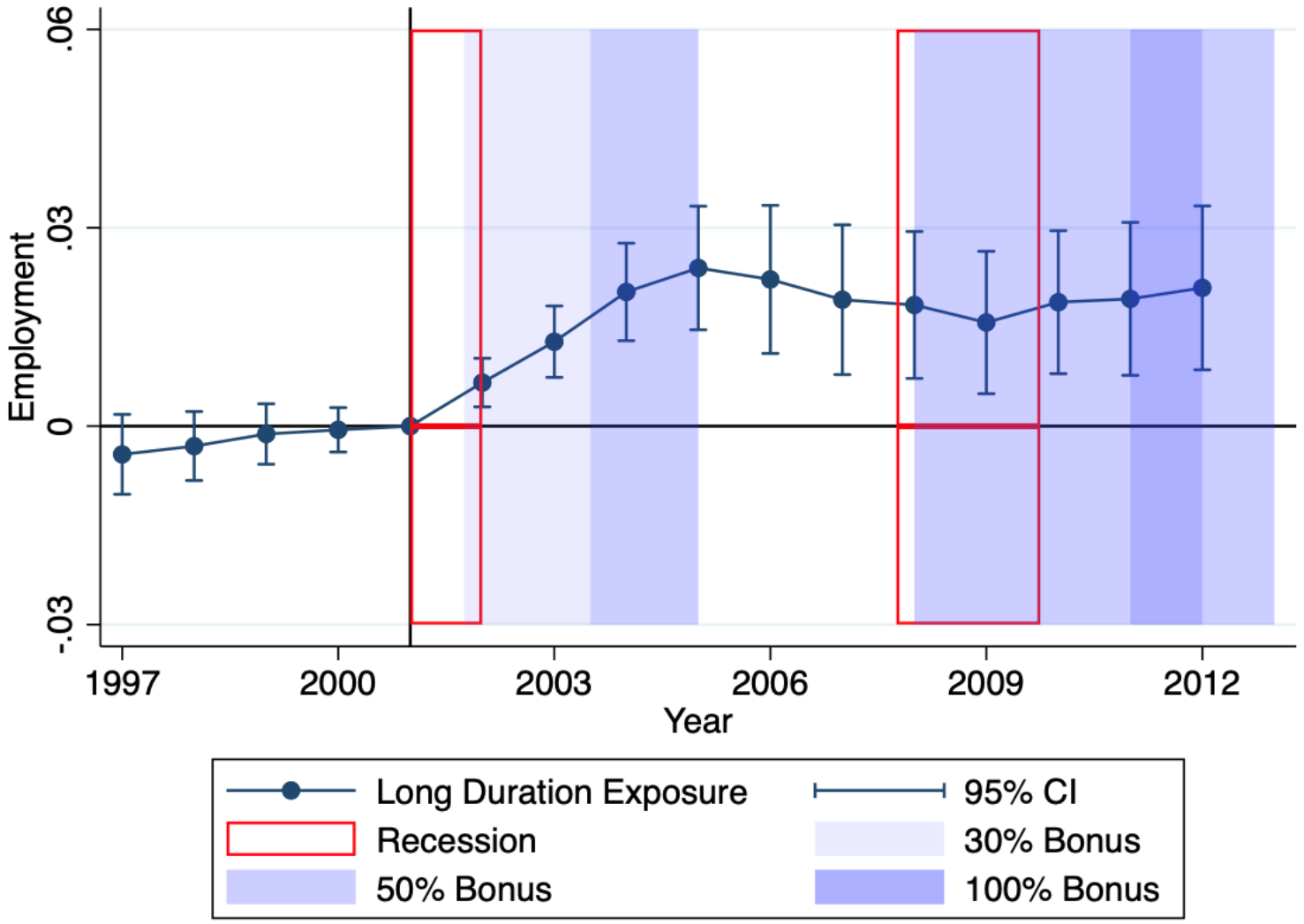
Garnett, Ohrn, and Suarez Serrato (2020, AER: I) study the impact of "bonus depreciation" that allows firms to deduct an additional % of capital expenditures in the 1st year of an asset

Exploit 2002 Job Creation and Worker Assistance Act: 30% bonus depreciation, increased to 50% in 2003-2004, then canceled in 2005 and re-implemented at 50% for 2008-17 (aside from 100% in 2011)

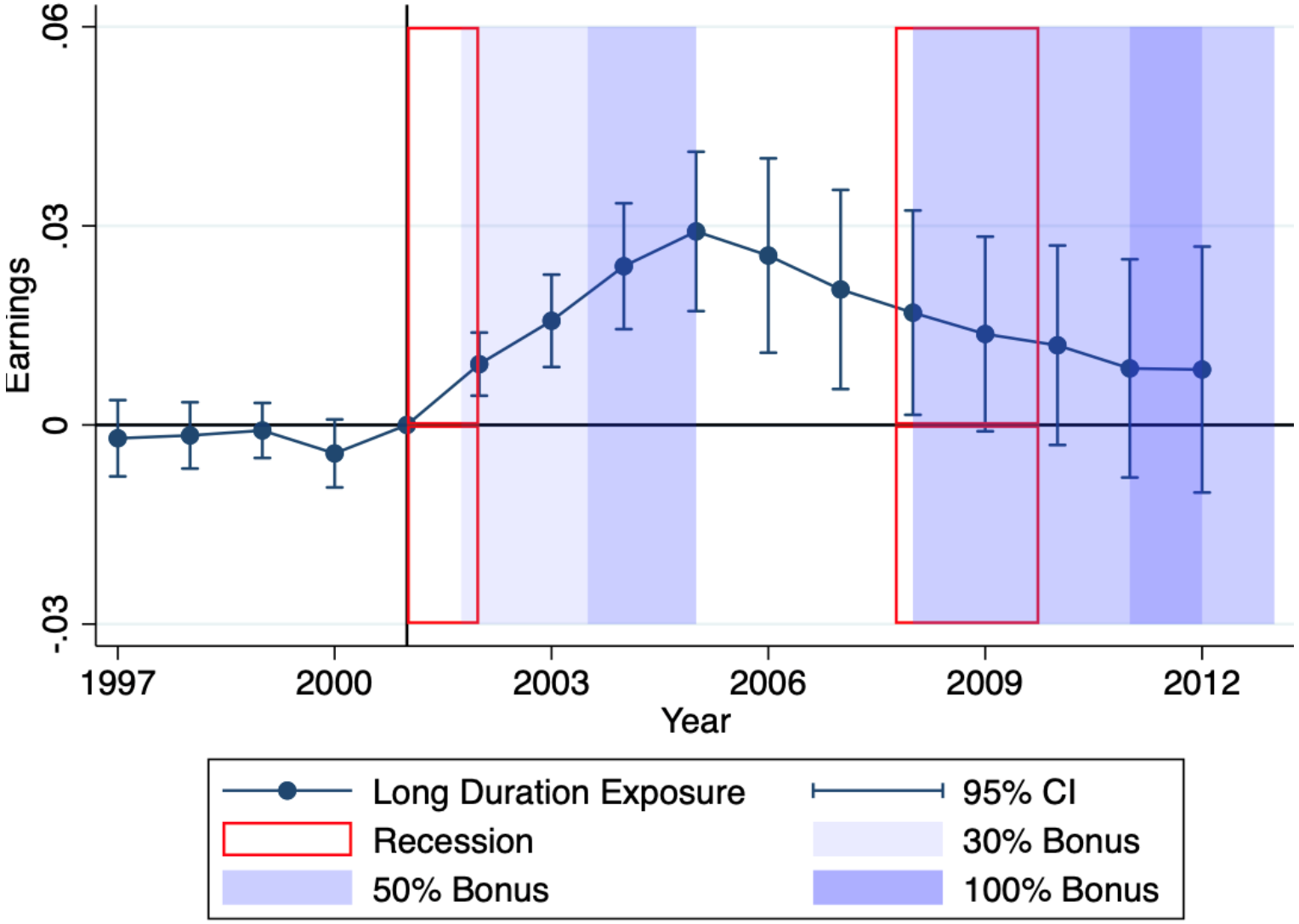
Bonus depreciation has larger effects where firms invest in longer-lived assets: Classify industries by those with long-lived assets and construct fraction of local employment pre-treatment (in 2001) that is in these industries as the intensity measure $\Rightarrow$ DD Event study with treatment intensity

Measure a county's exposure to bonus depreciation by interacting industry-level heterogeneity in the benefit of bonus depreciation with industry location data

A. Employment



B. Earnings


.06
.03
0
-.03
Earnings
1997
2000
2003
2006
2009
2012
Year
Long Duration Exposure
95% CI
Recession
30% Bonus
50% Bonus
100% Bonus

Other interesting papers on place-based policies

Kline and Moretti (2014, QJE) study the long-run effects of the 1930S Tennessee Valley Authority (TVA). Using as controls areas that were proposed but never approved by Congress, they find that the TVA led to large gains in agricultural employment (reversed when the subsidies ended). Gains in manufacturing continued to intensify (agglomeration economies)

Mayer, Mayneris, and PY (2017, JoEG) study the impact of the French 'Zones Franches Urbaines' (ZFUs) and find a positive and sizable impact on the prob to locate in the ZFU part of municipalities. However, this positive effect is entirely due to within-municipality diversion effects

Criscuolo et al. (2019, AER) exploit changes in the area-specific eligibility criteria (dictated by EU rules) for a program

to support jobs through investment subsidies. Areas eligible for higher subsidies significantly increased jobs and reduced unemployment. This effect exists solely for small firms: large companies accept subsidies without increasing activity. There are positive effects on investment and employment for incumbent firms, but not productivity

What was the impact of the introduction of tolls in former SCUT highways for workers and firms?

Volume 20, Issue 6
November 2020

JOURNAL ARTICLE

The effects of highway tolls on private business activity—results from a natural experiment

David B Audretsch, Dirk Christian Dohse, João Pereira dos Santos ✉

Journal of Economic Geography, Volume 20, Issue 6, November 2020, Pages 1331–1357
https://doi.org/10.1093/jeg/lbaa003
Published: 26 March 2020 **Article history** ▾

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Journal of Urban Economics
Volume 136, July 2023, 103569

Nobody's gonna slow me down? The effects of a transportation cost shock on firm performance and behavior ☆

Catarina Branco [a], Dirk C. Dohse [b] ✉, João Pereira dos Santos [c 1] ✉, José Tavares [d] ✉

Data: accounting results for the universe of 300k firms in 12k 7-digit postal codes, per year

`https://www.sciencedirect.com/science/article/pii/S0094119023000384`