The New Wave of Local Minimum Wage Policies: Evidence from Six Cities

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The New Wave

- 36 cities and counties and 18 states will increase their minimum wage in 2018

- 13 cities already have minimum wages of $13 and over, 10 large cities already on path to $15

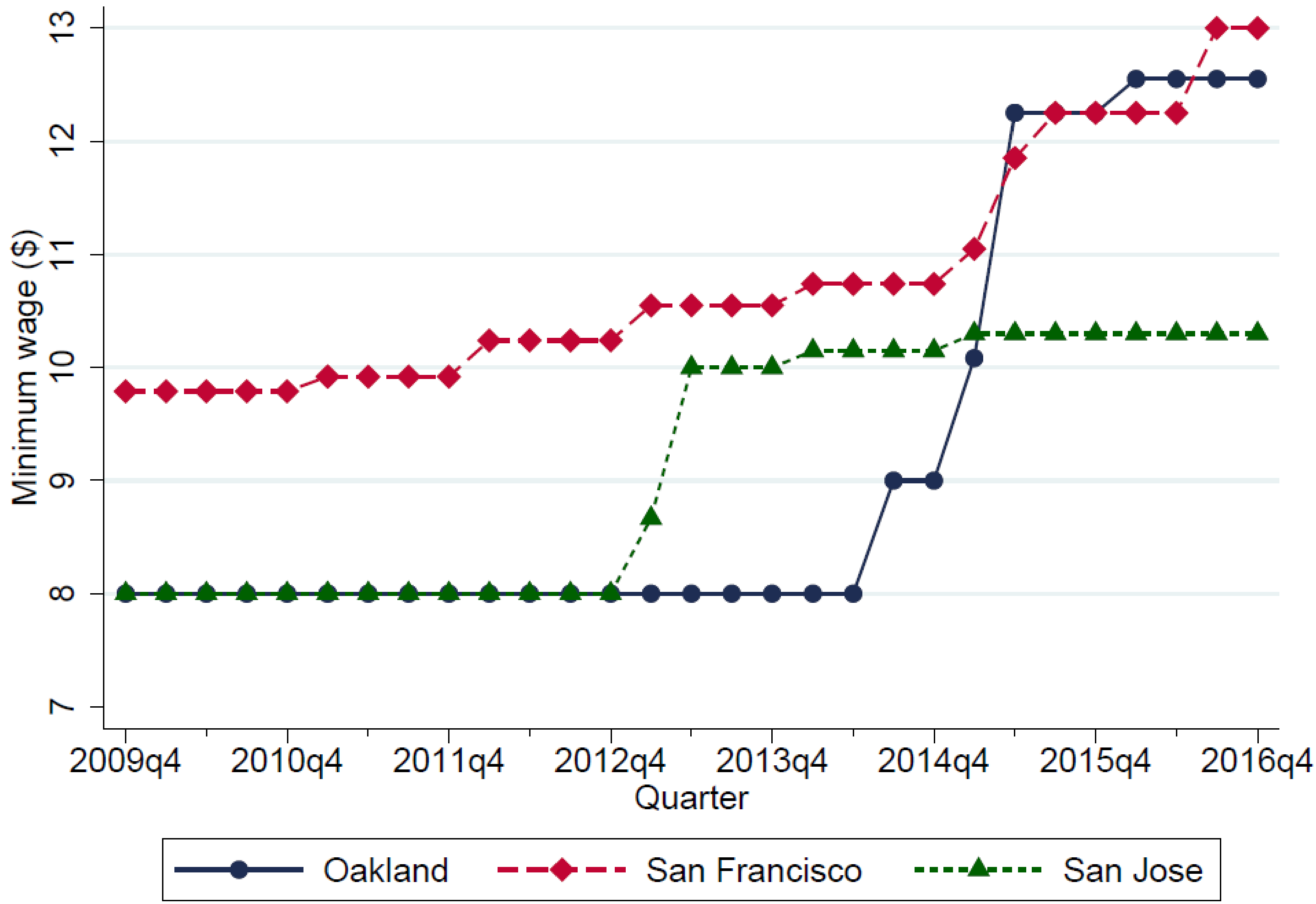
- We study effects in six large cities that are the earliest movers: Chicago, Oakland, San Francisco, San Jose, Seattle and Washington, DC

- As of 2016q4, all were above $10, two were at $13

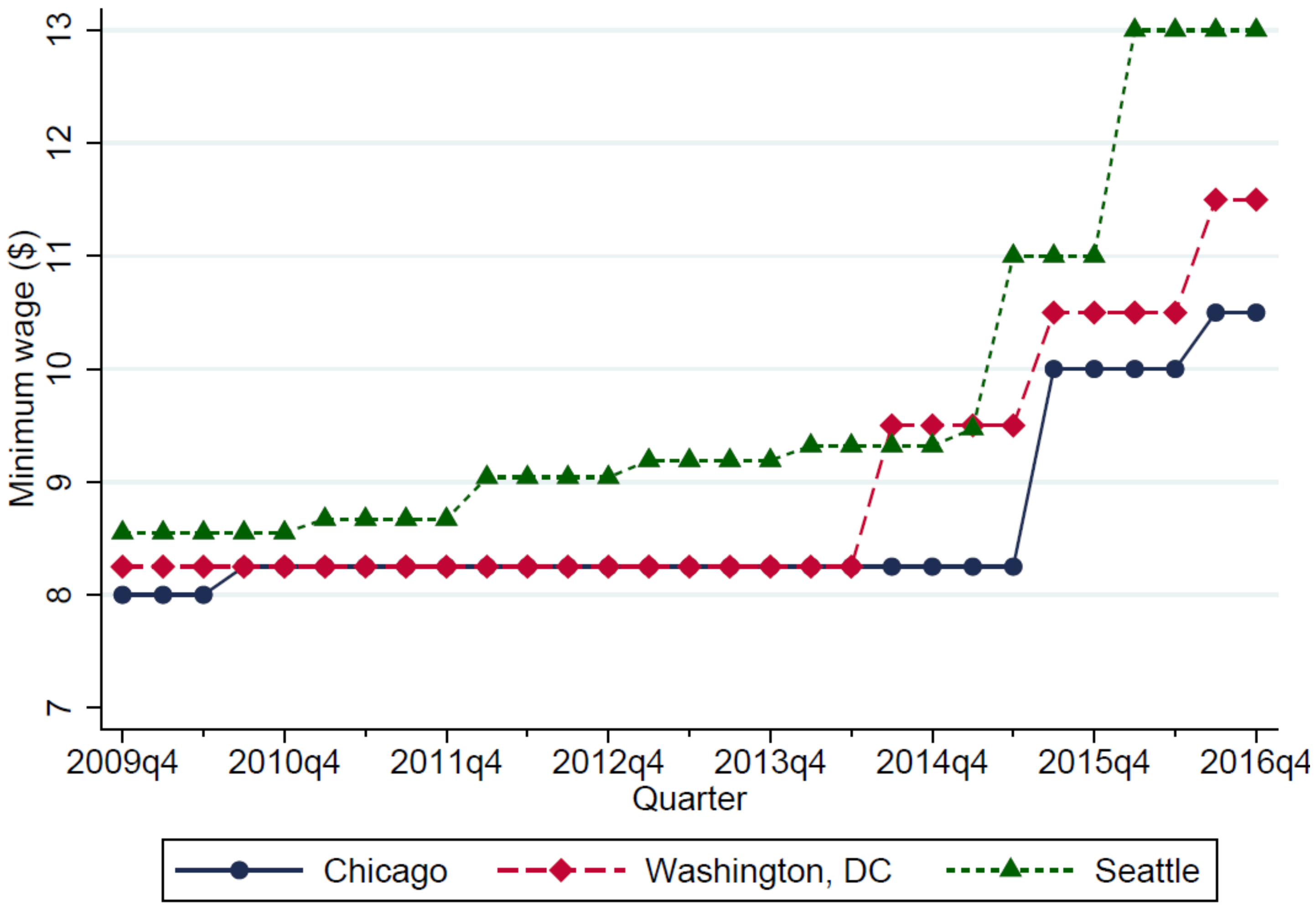
Outline

- **Design:** Compare each city to metro counties with no change in state or local MW policy, using QCEW and city extracts, 2009q4 to 2016q4
- **Methods**:
  - **Event-study analysis** (aka diff-in-diffs)
  - **Synthetic control** (Abadie et al. 2010)
    - Incorporate recent literature (Ferman and Pinto 2017)
    - Confidence intervals (Firpo and Possebom 2017)
    - Focus: Food services and drinking places
    - Falsification test: Professional services
- **Results** (from pooling SC across six cities)
  - Earnings elasticity: 0.25 [0.10, 0.40]
  - Employment elasticity: 0.07 [-0.21,0.36]

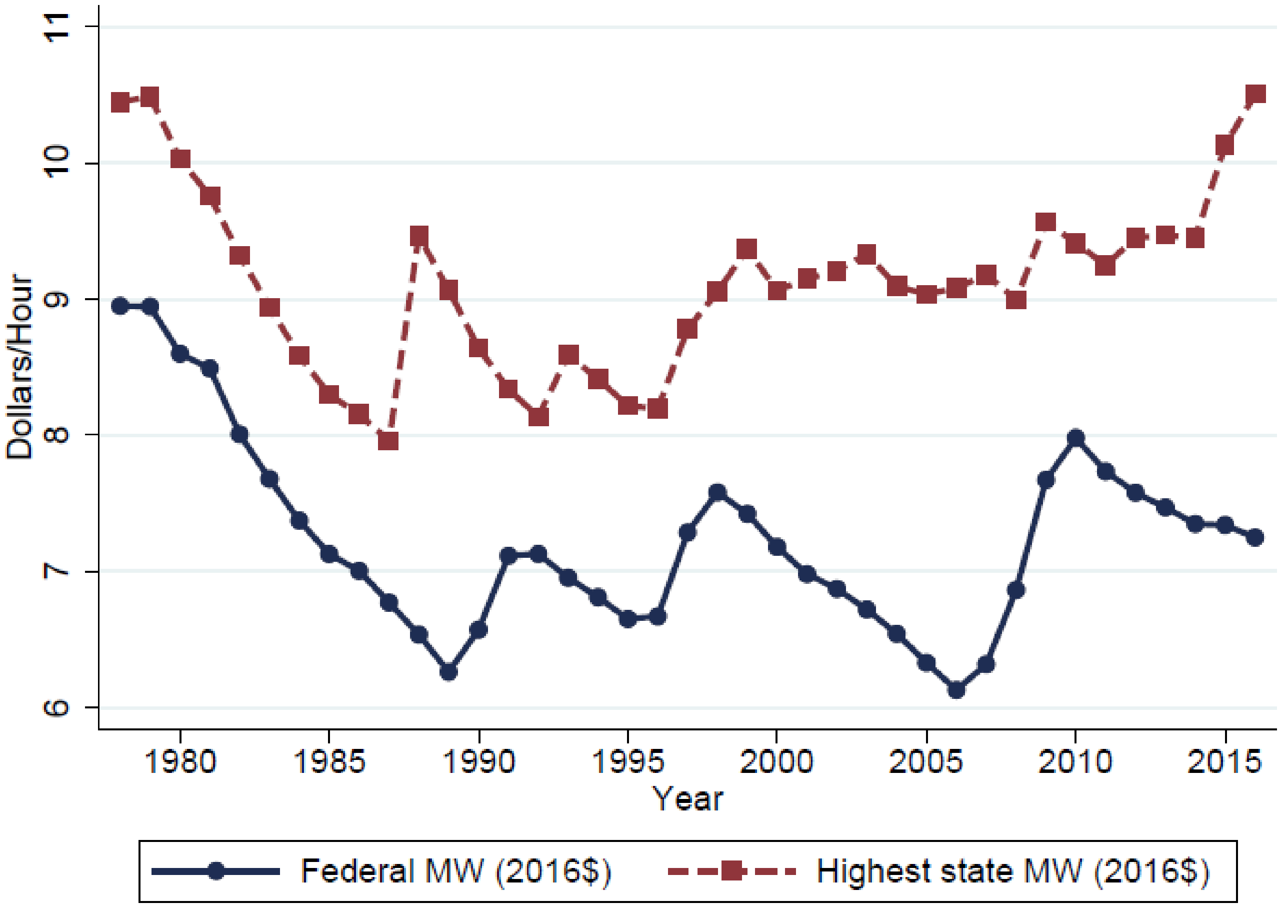
Local minimum wage policies Oakland, San Francisco and San Jose



Local minimum wage policies
Chicago, Washington, DC and Seattle



New local MWs: higher than previous state or federal policies



Research design challenges

- Finding appropriate control groups (donors) in presence of heterogeneous policies, spillover effects, non-parallel pre-trends

- Some states and surrounding counties are also increasing their MWs

- Policies are usually phased in over multiple years, implying multiple treatment events

- Our strategy: event studies and synthetic controls that account for above issues

Event study analysis

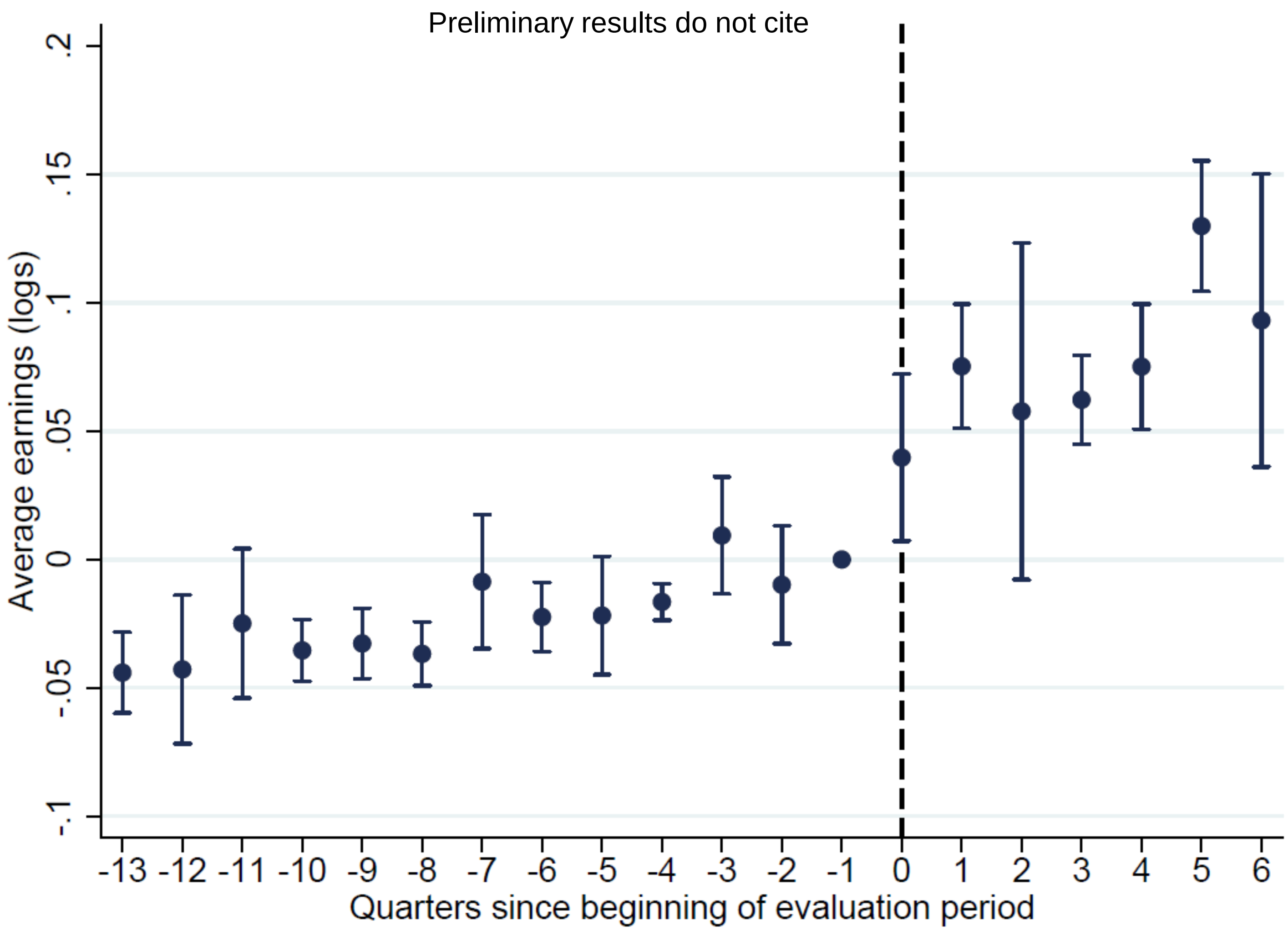
- Fit event study regressions to measure trends in earnings and employment in the cities before and after MW increase:

$$Y_{jpte} = \alpha_j + \theta_e I(MW\ City)_j + \delta_{pt} + \epsilon_{jpte}$$

where

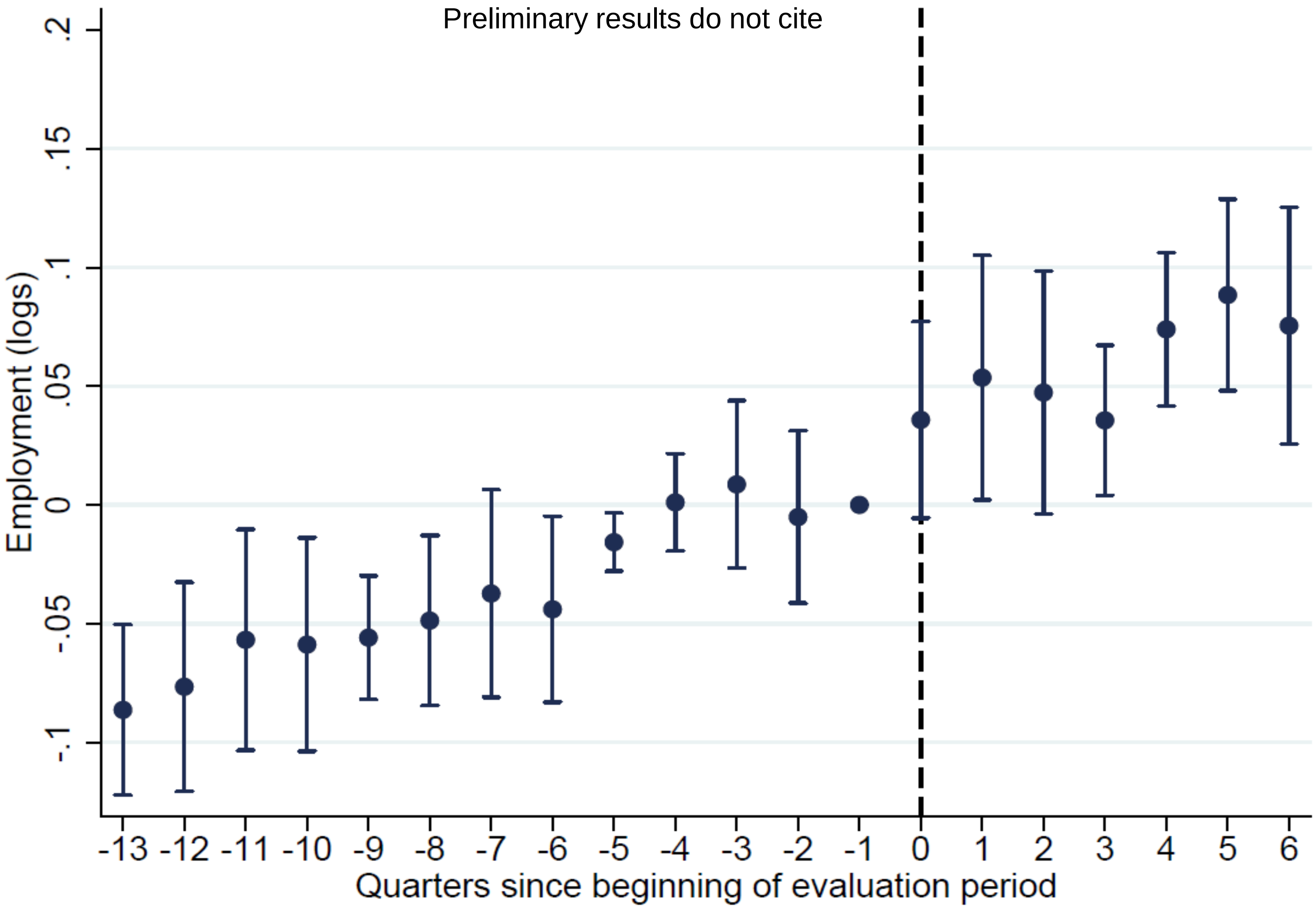
  - *j* indexes counties/cities, *t* indexes quarters
  - *p* indexes the three donor pool groups
  - *e* counts the number of quarters since the new MW policy went into effect, we omit -1 (*e=-13,…,-2,0,…,6)*
  - *Y* is either log(avg earnings) or log(employment)
  - θ_e is the average "effect" of the policy
  - δ_{pt} is a donor pool group-specific quarter effect
  - $I(MW\ City)_j$ indicates whether the county/city is one of the six cities of interest

Event study estimates, earnings



Note: Range plots report 95 percent confidence intervals.

Event study estimates, employment



Note: Range plots report 95 percent confidence intervals.

Event study estimates

	Earnings (logs)		Employment (logs)	
Effect	**0.074**	**0.040**	**0.057**	**0.019**
(s.e)	(0.017)	(0.009)	(0.021)	(0.009)
p-value	0.000	0.000	0.011	0.044
Tests of parallel trends: p-values				
All pre-increase effects equal zero	0.001	0.291	0.018	0.465
Pre-increase trend equals zero	0.004	0.063	0.000	0.079
Pre-increase trend, intercept equal zero	0.001	0.160	0.002	0.181
Population, private sector controls?	**No**	**Yes**	**No**	**Yes**
Number of cities and counties	**179**	**179**	**179**	**179**
Observations	**5132**	**5132**	**5132**	**5132**

Note: Standard errors are clustered at the state level.

Synthetic control estimation

- Synthetic control finds weights that minimize the pre-treatment MSPE between the actual and synthetic city
- To improve fit, we "demean" each city or county outcome by its pre-increase average:

  Demeaned outcomes usually within range of untreated donor pool outcomes

  Reduces bias from stationary time effects (Ferman and Pinto 2017)

- Predictors: All values of outcome of interest during pre-increase period
- Estimate of effect: Average difference between actual and synthetic after the MW increases

Synthetic control inference

- Use placebo tests to construct p-values and confidence intervals
- Invert the test statistic to find confidence intervals (Firpo and Possebom 2017)
- Test statistic: Ratio of the post-increase to pre-increase MSPE

$$RMSPE_j \equiv \frac{\frac{1}{T - T_0 + 1} \sum_{t=T_0}^{T} \left(Y_{jt} - \hat{Y}_{jt}\right)^2}{\frac{1}{T_0 + 1} \sum_{t=1}^{T_0 - 1} \left(Y_{jt} - \hat{Y}_{jt}\right)^2}$$

where *j* indexes counties/cities, *t* indexes quarters, T *is the number of periods,* T_0 the period in which the MW increases

- p-values are based on number of counties with larger RMSPE than the treated city

Policy affects donor pool eligibility

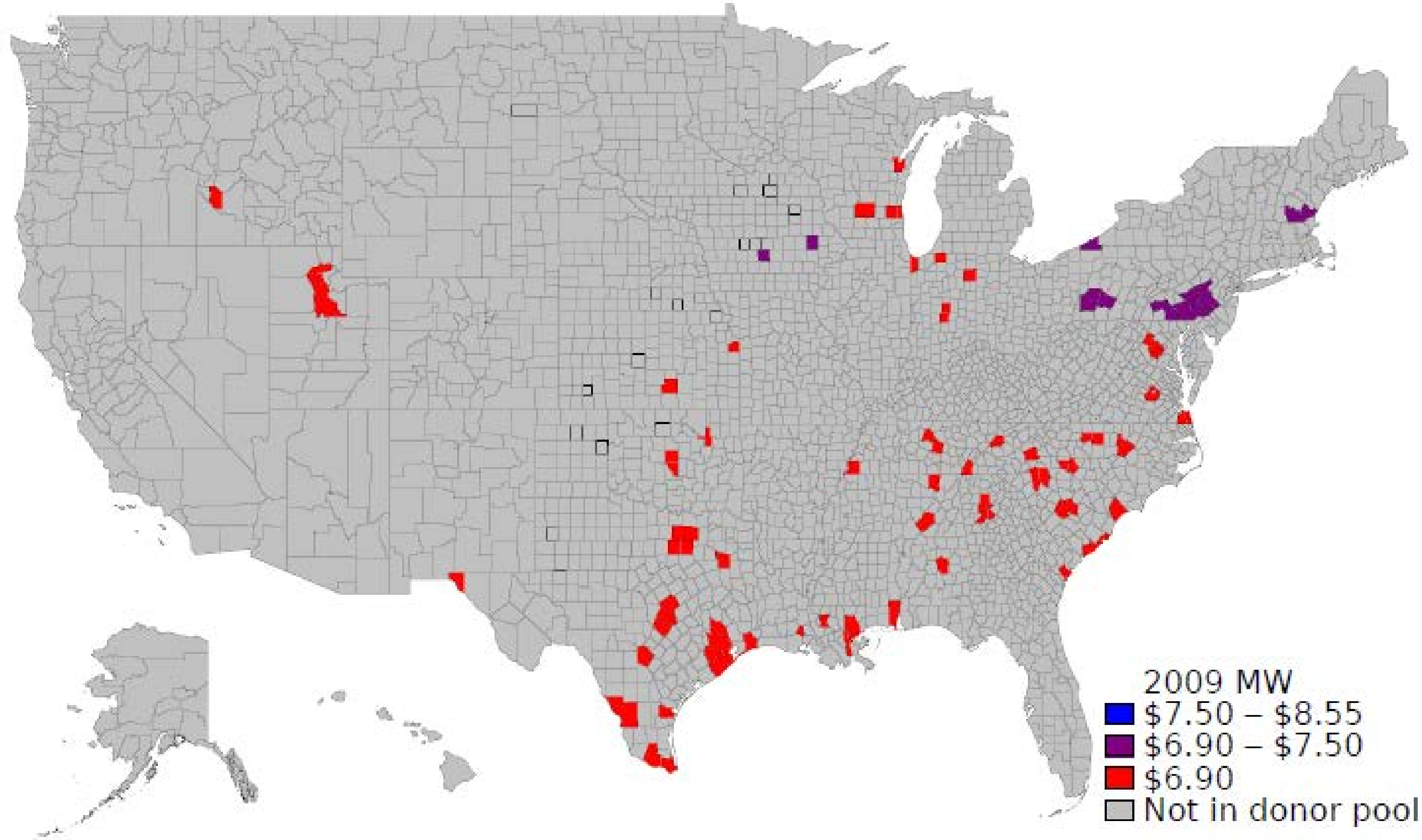
City	Pre-period	Evaluation period	MW growth	Donor pool
Chicago	2010q3--2015q2	2015q3--2016q2	19.2%	No increases
Oakland	2009q4--2014q2	2015q2--2016q3	43.8%	No increases
San Jose	2009q4--2012q4	2013q2--2014q3	23.1%	No increases
Wash. DC	2009q4--2014q2	2014q3--2016q4	21.9%	No increases
San Francisco	2009q4--2015q1	2015q2--2016q4	11.4%	Indexed to inflation
Seattle	2009q4--2015q1	2015q2--2016q4	24.1%	Indexed to inflation

QCEW county data on earnings and employment by industry

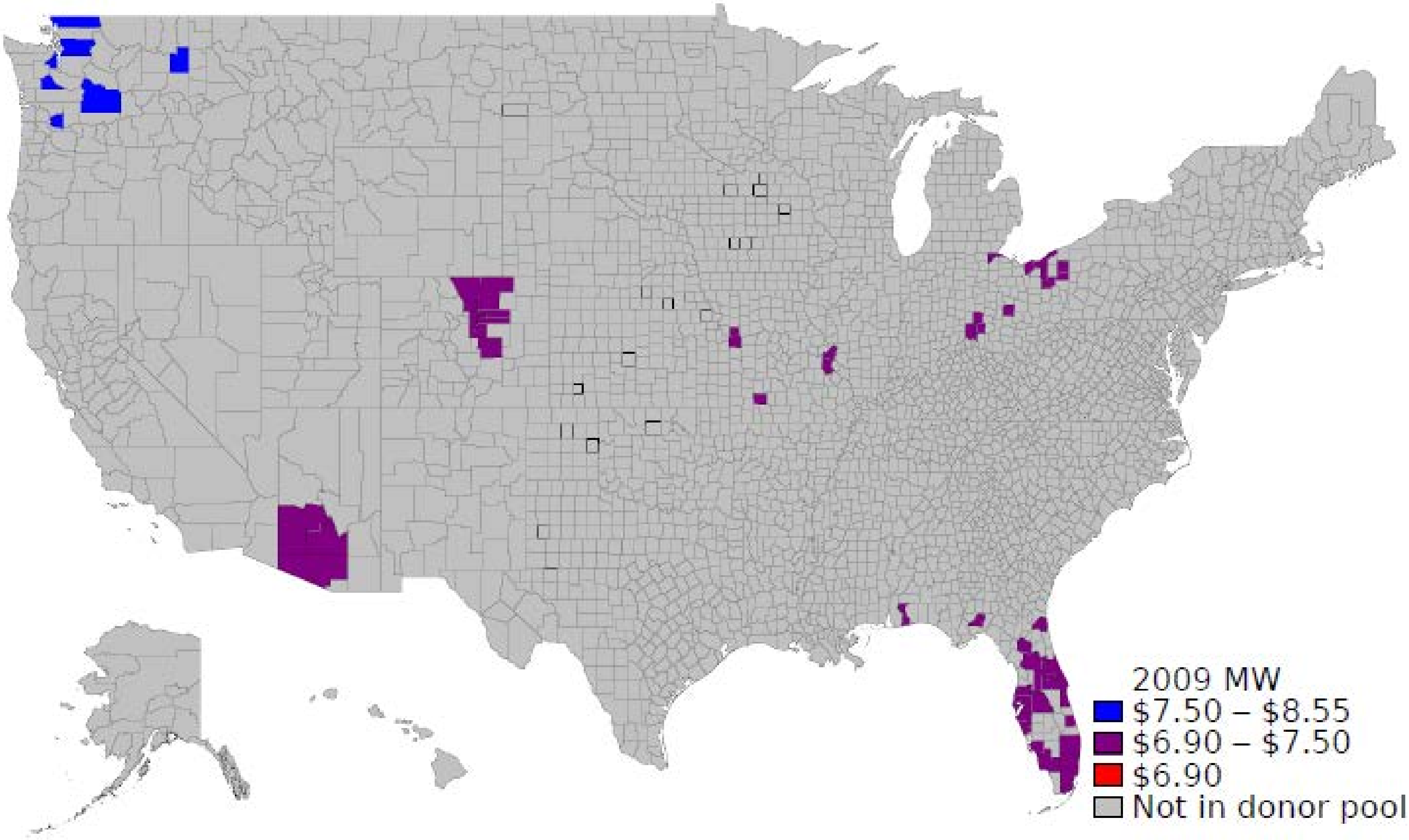
We restrict our donor pool to counties:

- In a metro area with at least 200k population
- “Clean” -- meaning no state or local MW policy
- Similar MW -- no changes or indexation

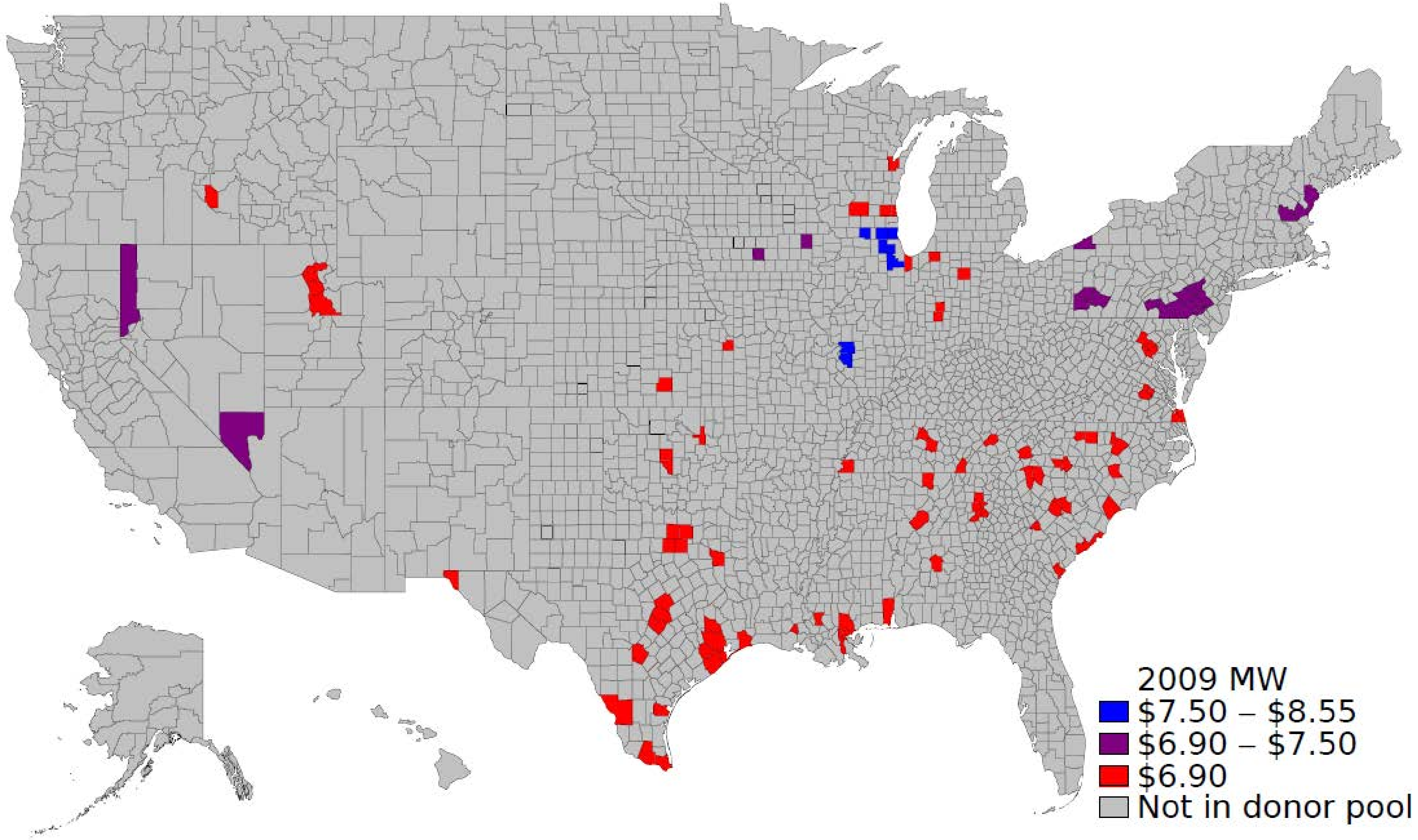
Oakland, San Jose, DC: eligible donors



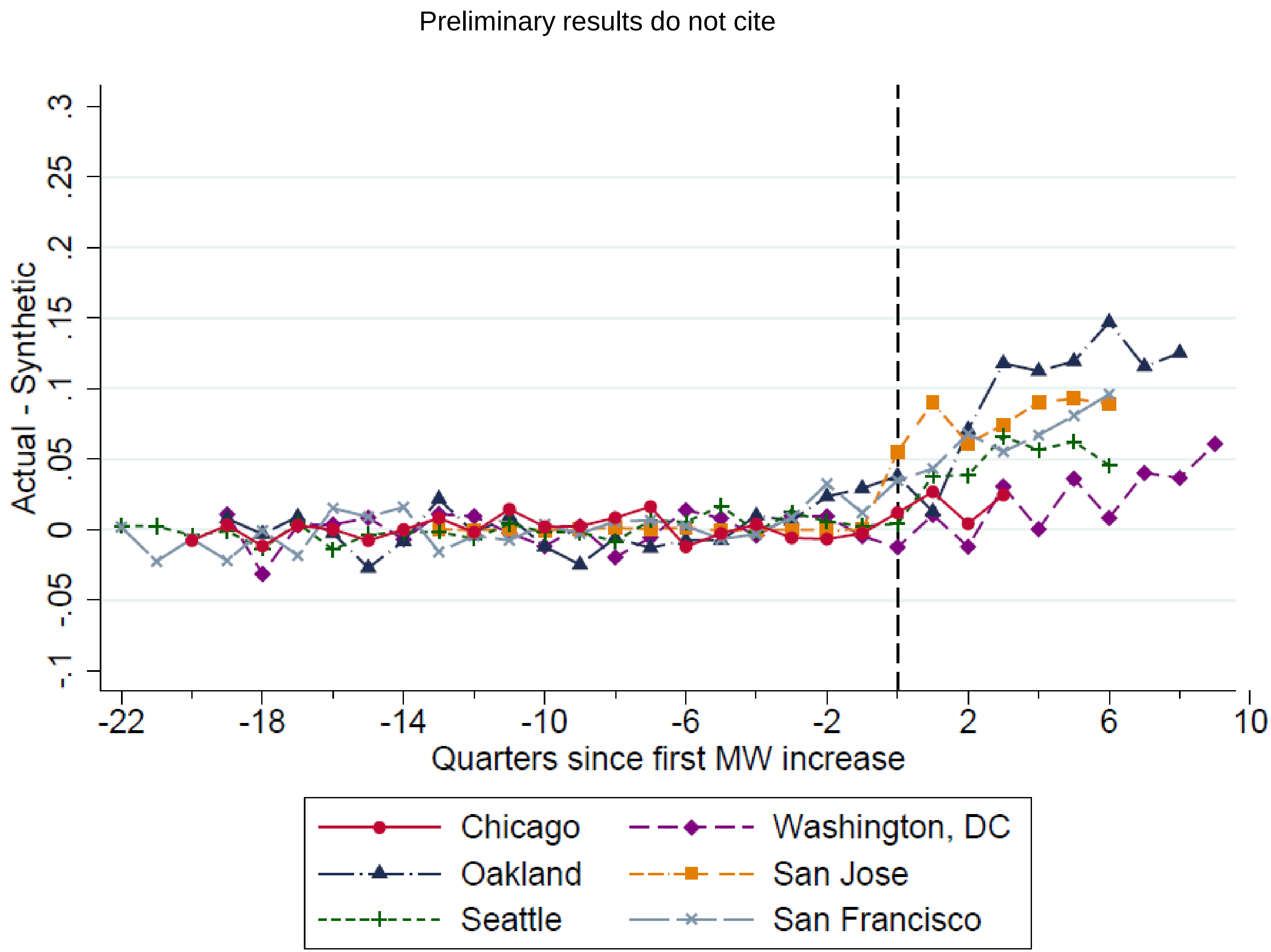
San Francisco and Seattle: eligible donors



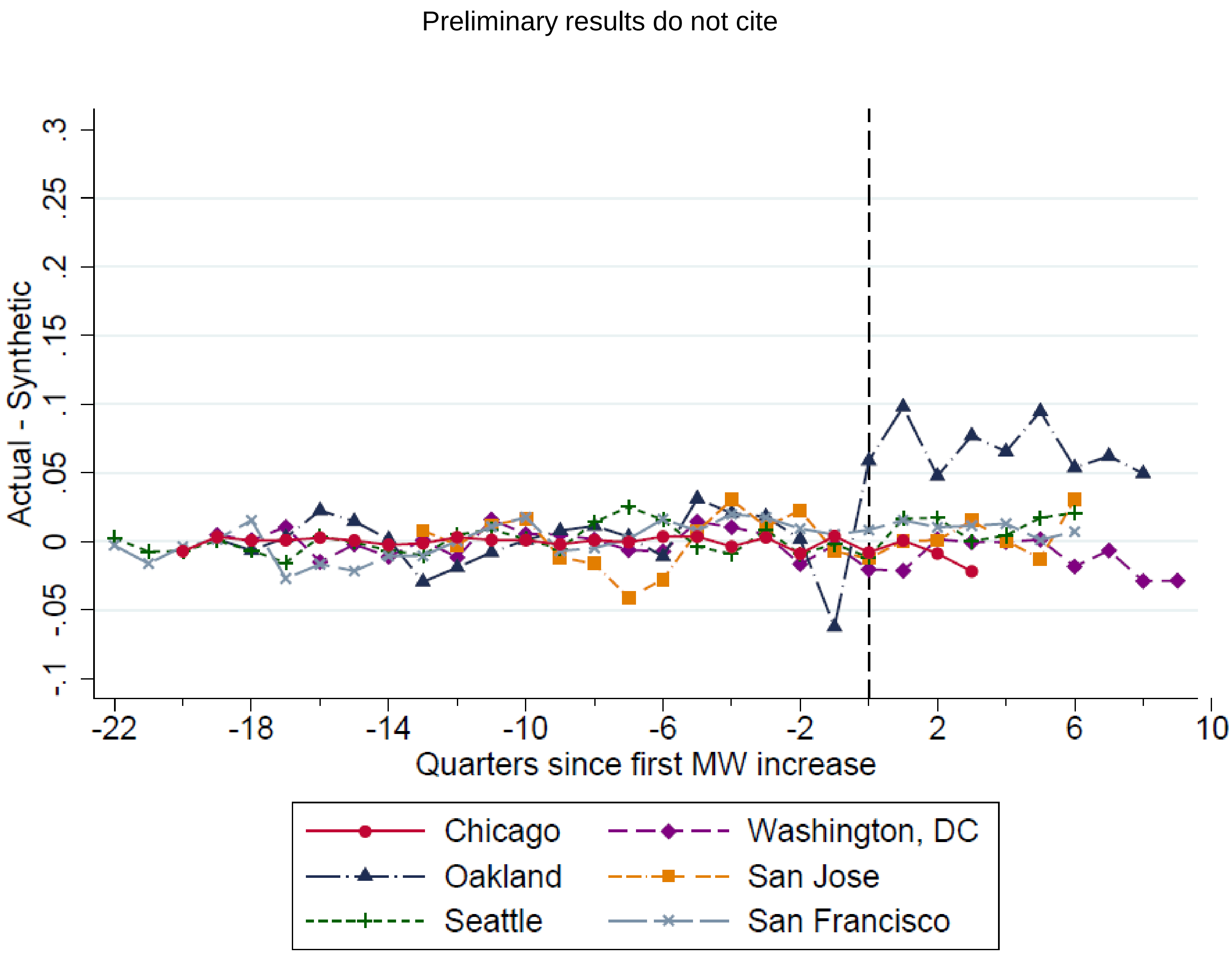
Chicago: eligible donors



Synthetic control estimates, earnings



Synthetic control estimates, employment



Synthetic control estimates by city

	Chicago	Oakland	San Jose	Seattle	San Francisco	Washington DC
		Earnings (log)				
Effect	0.02	0.12	0.08	0.04	0.06	0.02
p-value	0.45	0.14	0.04	0.02	0.03	0.52
95% CI	[-0.03,0.07]	[-0.30,0.55]	[0.03,0.13]	[0.02,0.07]	[0.01,0.12]	[-0.18,0.22]
Mean effect, donor pool	0.00	0.00	0.00	0.00	0.00	0.00
		Employment (log)				
Effect	-0.01	0.07	0.01	0.01	0.01	-0.01
p-value	0.30	0.50	1.00	0.77	0.97	0.75
95% CI	[-0.04,0.02]	[-∞,∞]	[-∞,∞]	[-0.06,0.07]	[-0.08,0.10]	[-0.21,0.19]
Mean effect, donor pool	0.00	0.00	0.00	0.00	0.00	0.00
Donor pool size	113	99	99	60	60	99
Pre-increase periods	20	19	13	22	22	19

Preliminary results do not cite

Synthetic control estimates: Prof. services (falsification)

	Chicago	Oakland	San Jose	Seattle	San Francisco	Washington DC
		Earnings (log)				
Effect	0.02	0.03	0.03	0.00	0.06	0.00
p-value	0.28	0.33	0.32	0.56	0.54	0.06
95% CI	[-0.05,0.08]	[-0.25,0.31]	[-0.77,0.83]	[-0.12,0.12]	[-0.25,0.53]	[-0.03,0.02]
Mean effect, donor pool	0.00	0.00	0.00	0.00	0.00	0.00
		Employment (log)				
Effect	0.01	0.07	-0.07	0.00	0.10	0.00
p-value	0.36	0.68	0.71	0.82	0.39	0.90
95% CI	[-0.03,0.05]	[-∞,∞]	[-∞,∞]	[-0.03,0.04]	[-∞,∞]	[-0.10,0.11]
Mean effect, donor pool	0.00	0.00	-0.02	0.00	0.00	0.00
Donor pool size	113	99	99	60	60	99
Pre-increase periods	20	19	13	22	22	19

Preliminary results do not cite

Pooling synthetic control to find elasticity

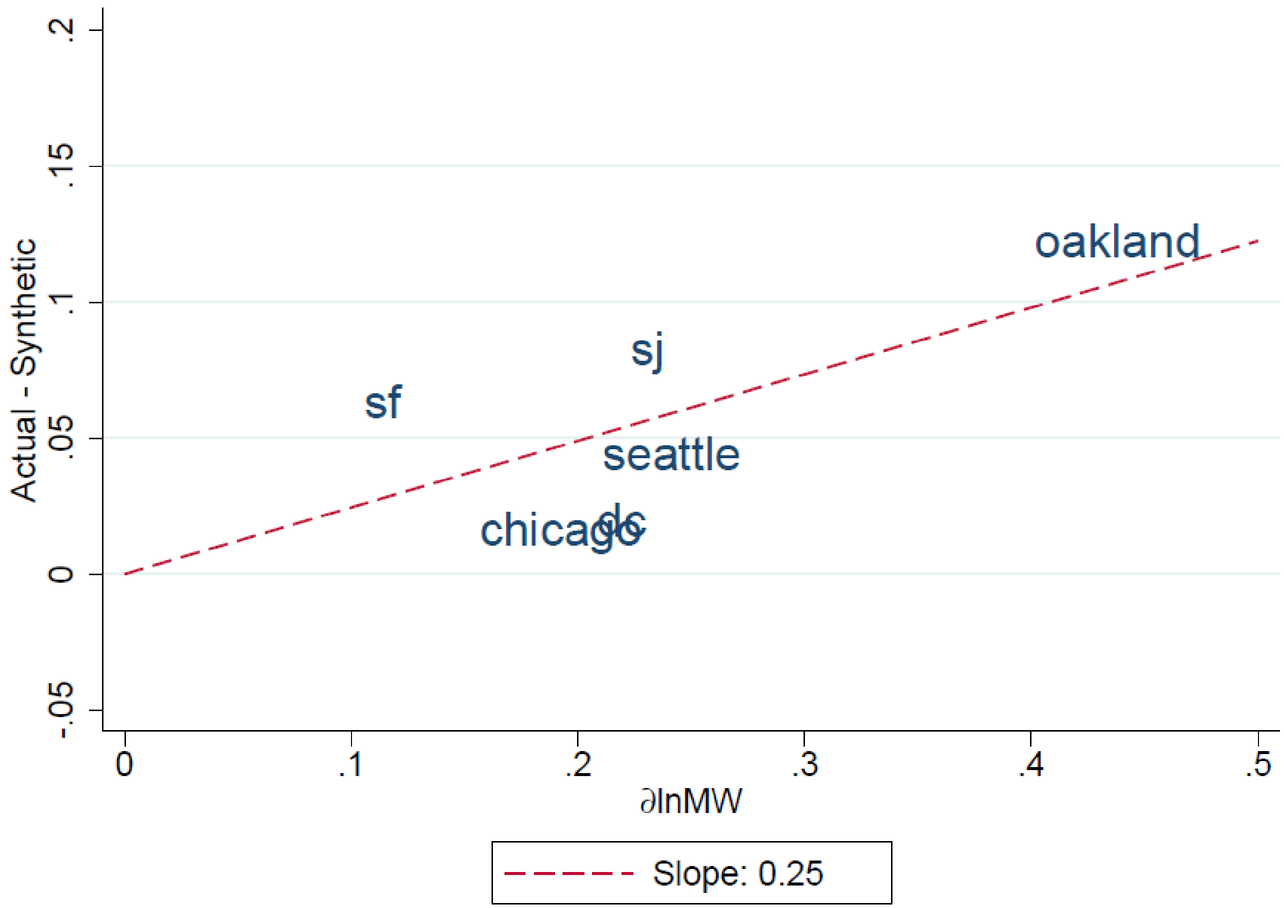
- Cities with larger minimum wage increases have larger earnings effects
- To find the implied elasticities wrt the MW we divide our causal estimates by the mandated minimum wage increase
- Example (Oakland, log earnings):

$$\frac{0.12 \text{ (effect estimate)}}{0.44 \quad \text{(MW growth)}} = 0.27$$

- For inference we use a pooled version of the RMSPE: ratio of the six city average post-increase MSPE to average pre-MSPE
- Constant elasticity model implies effects would lie along a regression line…

Pooled analysis for earnings effects lie along a regression line

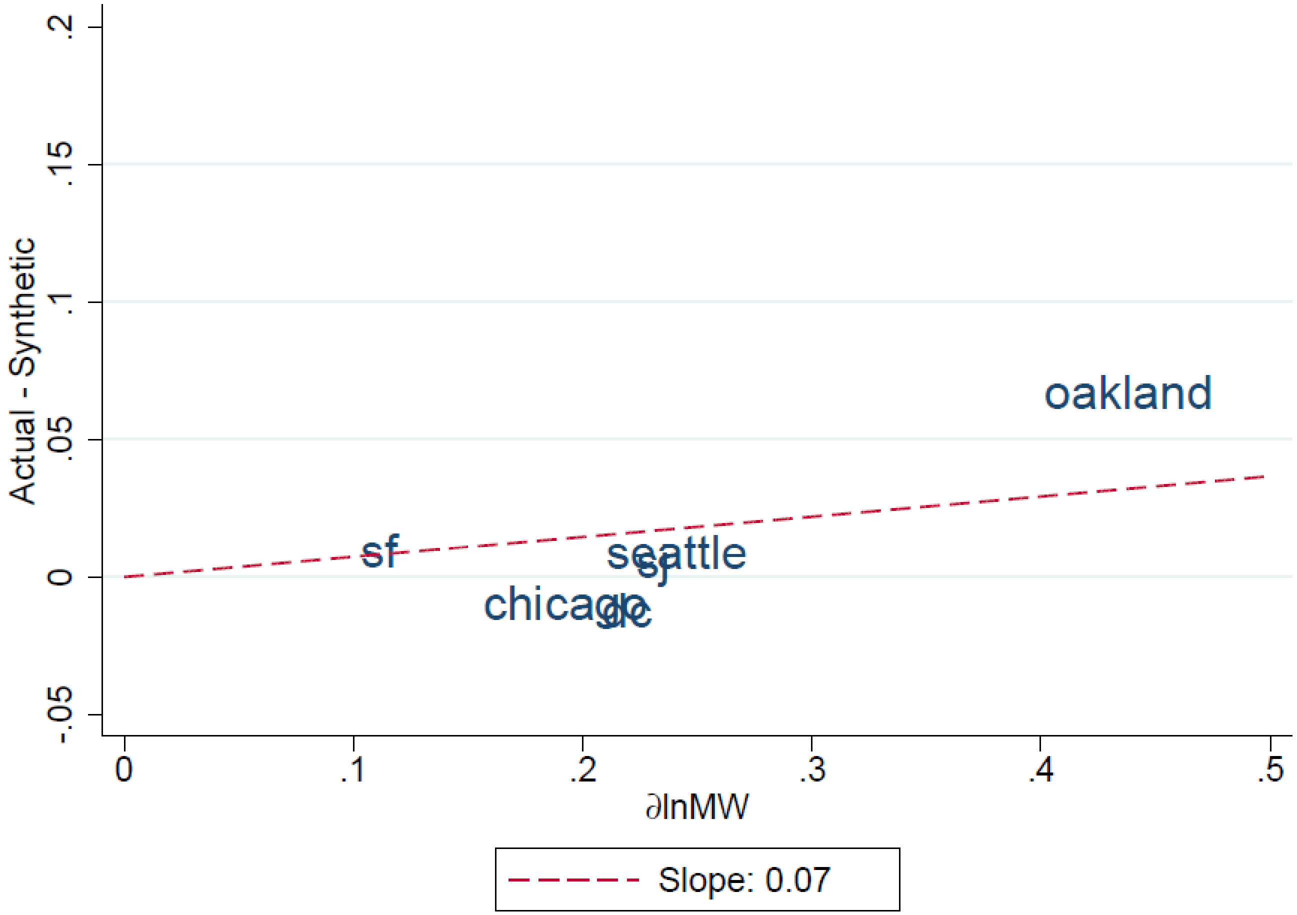
Preliminary results do not cite



Confidence interval [0.10,0.40]

Pooled analysis for employment effects do not exhibit this pattern

Preliminary results do not cite



Confidence interval [-0.21,0.36]

Pooled synthetic control estimates, by sector

	Food services	FSR + LSR	FSR	LSR	Professional
Average earnings (log)					
Elasticity	**0.25**	**0.26**	**0.19**	**0.43**	**0.08**
95% CI	[0.10,0.40]	[0.18,0.36]	[0.11,0.28]	[0.27,0.58]	[-0.25,0.40]
Employment (log)					
Elasticity	**0.07**	**0.05**	**0.09**	**0.01**	**0.07**
95% CI	[-0.21,0.36]	[-0.23,0.33]	[-0.11,0.30]	[-0.55,0.54]	[-0.45,0.60]

Confidence intervals are based on placebo tests with 10,000 combinations.

Interpreting causal effects

- QCEW elasticities combine changes in wage, hours and compositions of firms and workers

  Ex: If sector fires all its low-wage workers, average wage *increases*

  Hours*:* In CPS, weekly wage effect is same as hourly wage effect for hourly workers (Cendiz et al. 2017)

  Composition: For restaurants, small compositional shift to full-service, but no employment change in existing restaurants (Aaronson et al. 2017)

- Small employment and large positive earnings effects are consistent with either

  Greater annual earnings for incumbent low-wage workers **OR** substantial L-L substitution that benefits higher-skilled workers

Substantial L-L substitution?

Do MW employers replace their low-skilled workers?

- Age, education shares: Cendiz et al. 2017 examine 23 groups– no sig. effects. Same for gender, race/ethnicity and skill level of tasks
- Retail: Giuliano (2013) finds *increase* in teen employment
- Changes in tasks but not skill levels in restaurants, retail and other sectors (Aaronson and Phelan 2017)
- Automatable job share falls, for some demographic groups only (Lordan and Neumark 2017); possible that non-automatable unskilled jobs increase

Credible studies do not find that employers replace significant numbers of low-wage workers with higher-skilled workers

More work to be done

- **Add more cities and time periods to sample**
- **Alternative estimators (e.g., IFE)**
- **Synthetic control to do list**
  - Include more sectoral analyses

    Subsets of retail, nursing home workers
  - Falsification tests on other higher pay sectors
  - Relax donor pool restrictions

Stay tuned. We will release our six city report in early 2018 and subsequent reports thereafter!

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