What Can We Conclude from the Evidence on Minimum Wages and Employment? Recent Progress

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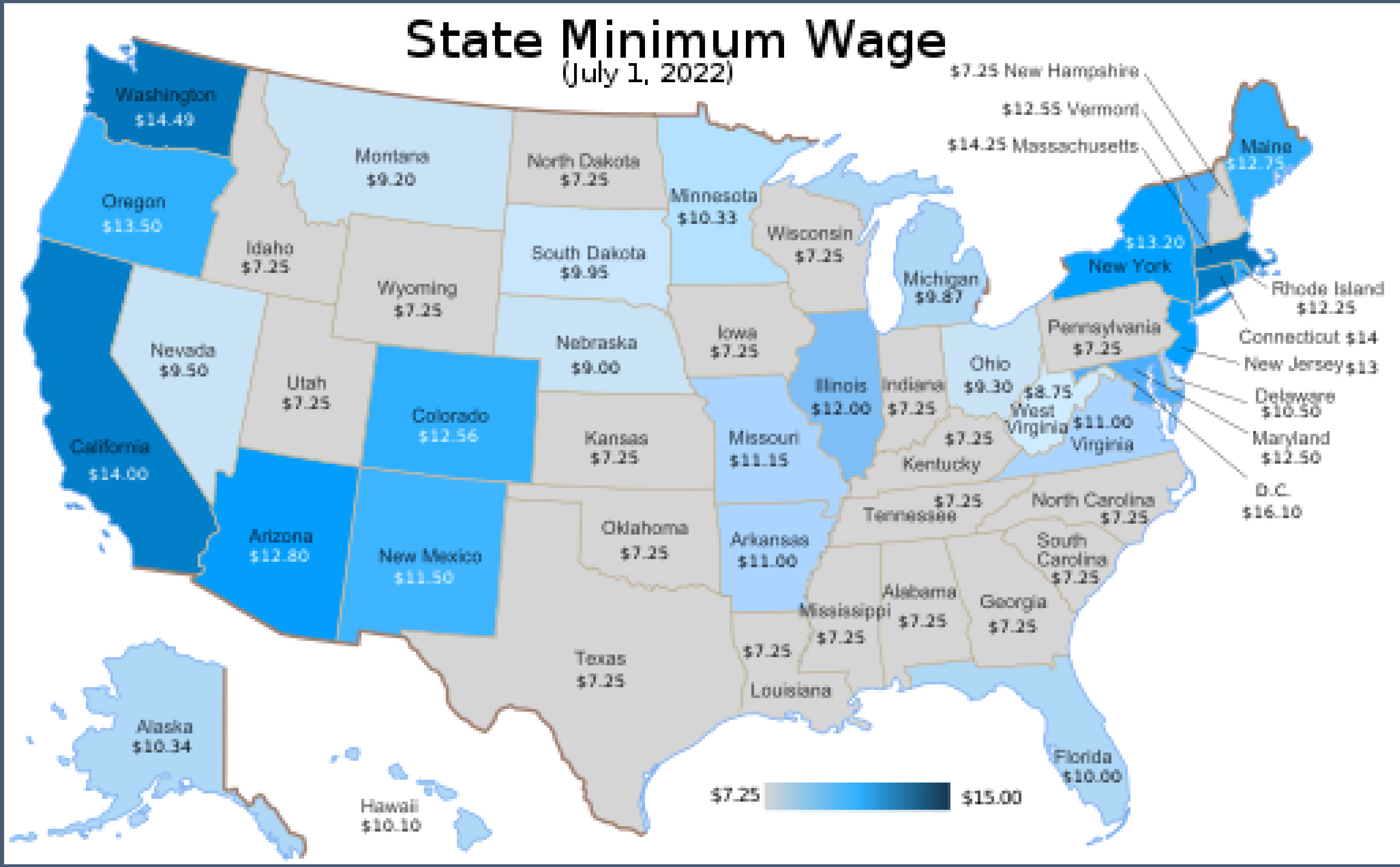
Outline

- The U.S. context is an ideal setting to study the effects of minimum wages on jobs
  – But we still haven't answered the question
- Two recent papers that – in very different ways – try to make sense of a contested research literature
  – Disagreement about what the overall body of evidence says
  – Conflicting evidence from existing empirical approaches
- Implications for policy and for research

The minimum wage (MW) “landscape” in the United States

- >30 states plus DC now have MW above federal, with average difference of around 30%
- States and cities are raising MW’s to $15 or higher
- Federal MW rose to $7.25 in 2009, no change since
- Federal MW now provides a floor for an increasingly narrow set of states, concentrated in the South (but not exclusively)

The minimum wage (MW) “landscape” in the United States



U.S. labor market should be a close to ideal "laboratory" to study MWs

- Extensive variation across time and over states (and more recently cities) in the U.S.
- This setting should allow implementation of the most convincing methods – aside from actual experiments – for estimating the effects of MWs
- More challenging in many other countries
  - National MW policy makes construction of counterfactual challenging
  - Industry variation is often bargained, making MWs almost certainly endogenous

But the question is still far from settled

- Strong claims made in both directions, in both media, and by economists doing policy advocacy work – even about $15 MW
- In the media
  - "A Statewide $15 Minimum Wage is a Bad Idea"
    - *Forbes*
  - "Why a $15 Minimum Wage is Good Economics"
    - *American Prospect*
- And by economists
  - "A $15 wage won't cost New York jobs"
    - Reich (2016)
  - "By 2022, approximately 400,000 jobs would be lost" (just in California)
    - Even and Macpherson (2017)

The dispute also persists in the research literature, in two different ways

- First dispute: Conflict over the best or most compelling way to identify MW-employment effects
- Second dispute: What does the literature even say?

What is the most compelling way to identify MW-employment effects?

- Extensive review of scores of studies, many using variation in MW changes across states (NW, 2007, 2008)
  - 2/3 find negative effects
  - 85% of more credible studies (our assessment) find negative effects
  - Larger disemployment effects when studies focus on least skilled
  - Many elasticities in range −0.1 to −0.2, with variation
- Mainly, but not exclusively, panel data evidence across states/regions (so-called "New MW Research")

Revisionist studies question this approach and reach different conclusions

- "…[V]ariation over the past two decades in minimum wages has been highly selective spatially, and employment trends for low-wage workers vary substantially across states… This has tended to produce a spurious negative relationship between the minimum wage and employment for low wage workers..." (Dube, *JEL*, 2011, p. 763)
- Motivates approaches to controlling for local shocks, including "close controls," à la Card-Krueger NJ-PA study
- Claim from doing so, in 1 high-profile paper: "[N]o detectable employment losses from the kind of minimum wage increases we have seen in the United States" (DLR, *REStat*, 2010, p. 962)

More puzzling (2nd dispute), economists can't even agree on what we disagree about

- 1. There is no job loss:
  - "Economists have conducted literally hundreds of studies based on over 160 minimum wage changes in the past thirty-five years. The best of these studies … indicate that the Act will have minimal to no adverse effects on employment…" – Reich (2019)
  - "The bulk of recent economic research on the minimum wage, as well as the best scholarship, establishes that prior increases have had little to no negative consequences and instead have meaningfully raised the pay of the low-wage workforce." – Zipperer (2019)
  - "The last decade has seen a wealth of rigorous academic research on the effect of minimum wage increases on employment, with the weight of evidence showing that previous, modest increases in the minimum wage had little or no negative effects on the employment of low-wage workers." – EPI letter, signatories include Acemoglu, Cutler, Saez, Deaton, Diamond) (2019)

Economists can't even agree on what we disagree about

- 2. There is no clear evidence of disemployment effects:
  - "[T]he literature after Myth and Measurement was about equally likely to find positive as negative employment effects of the minimum wage, with the typical estimate very close to zero." – Card and Krueger (2015, p. xvi)
  - "There is considerable support for the competitive market hypothesis that an effective minimum wage would result in lower employment… However, a few studies report zero or even positive employment responses to higher minimum wages." – Liu et al. (2016, p. 19)
  - "… despite an extensive body of empirical work of increasingly high quality, there is still considerable disagreement over the sign and strength of MW employment effects." – Hirsch et al. (2015, p. 202)

Economists can't even agree on what we disagree about

- 3. The evidence generally points to job loss:
  - "… the new conventional wisdom misreads the totality of recent evidence for the negative effects of minimum wages. Several strands of research arrive regularly at the conclusion that high minimum wages reduce opportunities for disadvantaged individuals." – Clemens (2019)
  - "My reading of the economics literature leads me to conclude that the weight of the evidence suggests that minimum wage increases lead to non-negligible employment reductions." – Strain (2019)

A puzzling disagreement

- Perfectly natural for empirical studies on a topic to reach different conclusions, and for economists to argue about the evidence
- But puzzling – and I think rare – that economists present different <u>summaries</u> of what these studies show

Today's talk – new evidence on trying to resolve the conflicting evidence

- What do we really learn from looking at the whole (US) research literature on minimum wage effects on jobs?
- Can we resolve a core conflict between conclusions from different types of studies?

"Myth or Measurement: What Does the New Minimum Wage Research Say about Minimum Wages and Job Loss in the United States?" (N&S, 2022)

- We genuinely didn't know which summaries were correct
  - … at least between the "completely mixed evidence" vs. "most evidence points to job loss"
  - Clearly important for both policy and economics to try to answer this question

Unusual question: what does the whole research literature actually say?

- In economics, not a lot of effort devoted to this
- *JEL* publishes high-quality reviews, but much more focused on understanding a topic – theories, tests, evidence, etc. – than in answering this kind of question
- There are some meta-analyses in economics – and maybe more on MWs than most other topics
  - Leave some important questions unanswered
  - May also generate misleading evidence about questions they try to answer

Meta-analysis type database

- U.S. minimum wage-employment papers published since the New Minimum Wage Research beginning with the ILRR symposium in 1992
- Drew from surveys in N&W (2007), Wolfson and Belman (*LABOUR*, 2019), and subsequent Google Scholar searches
- Studies retained if:
  - Estimated employment effects
  - Reported elasticity
- Excluded small number of time-series studies (not part of NMWR)
- Added a few other papers identified as published or forthcoming (crowdsourced)
- 70 papers total

Focused on authors' conclusions

- Entire set of estimates likely to fail to convey conclusions of paper
  - E.g., authors often report estimates they don't find as credible before reporting their preferred estimates
- Create database of each study's "preferred estimates," which could number more than one (e.g., teens and young adults)
  - In order of priority:
    - 1. Summary statements in conclusions
    - 2. Descriptions of results in tables
- Different approach to what is done in meta-analyses

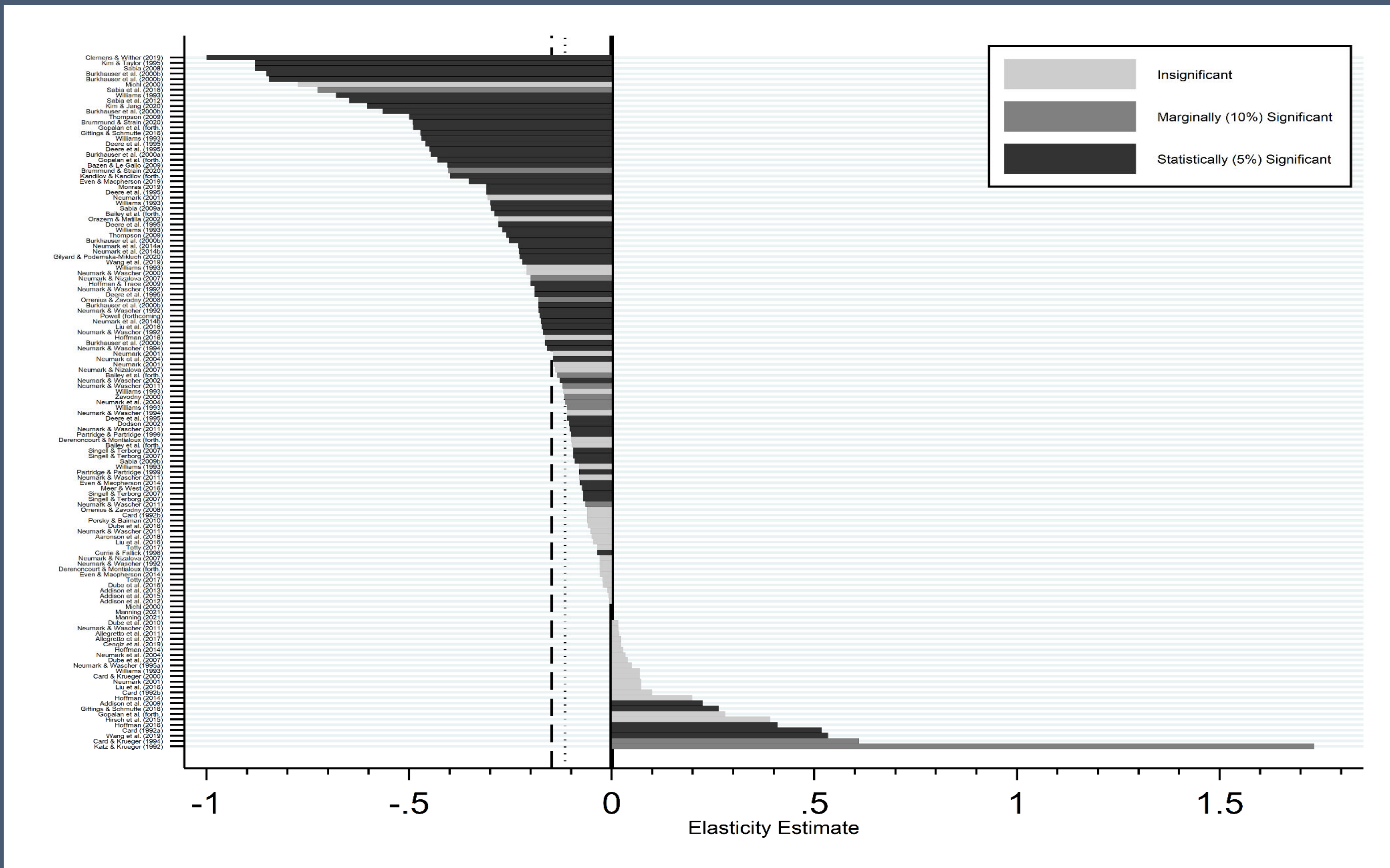
Surveyed authors to avoid subjectivity in selecting "preferred estimates"

- Exact text in paper, but asked in entirely neutral way:
  - For research we are doing, we would like to ask you about the elasticity of employment with respect to the minimum wage estimated in your paper:
    - 1. What estimated elasticity from your paper best captures its core conclusion? Put differently, if you had to reduce the findings of this paper to a single estimate, what would it be?
    - 2. If you believe it is impossible to capture your overall conclusion in a single estimated elasticity, please repeat this information for other estimated elasticities you believe are needed to capture your core conclusion(s).
- Used same protocol for repeated requests
- Response rate was very high: only 9 non-responses (and 1 deceased author)
- Compared our coding and survey responses – no bias one way or the other, so we use our coded responses for the non-responses

Differences relative to meta-analysis

- Focus on preferred estimates/conclusions; meta-analysis can assign lots of weight to estimates that don't reflect conclusions
- Focus to some extent on distribution of effects, not just average effect, and on how effects differ across groups/studies/time periods
- Meta-analysis often focuses on publication bias
  - Recent MW literature concludes there is little evidence of publication bias (Wolfson and Belman, *LABOUR*, 2019; Andrews and Kasy, *AER*, 2019)
  - Hard to distinguish publication bias from alternative explanations
    - E.g., more rigorous estimates also noisier, so sign can be correlated with precision for reasons unrelated to this bias

All preferred estimates



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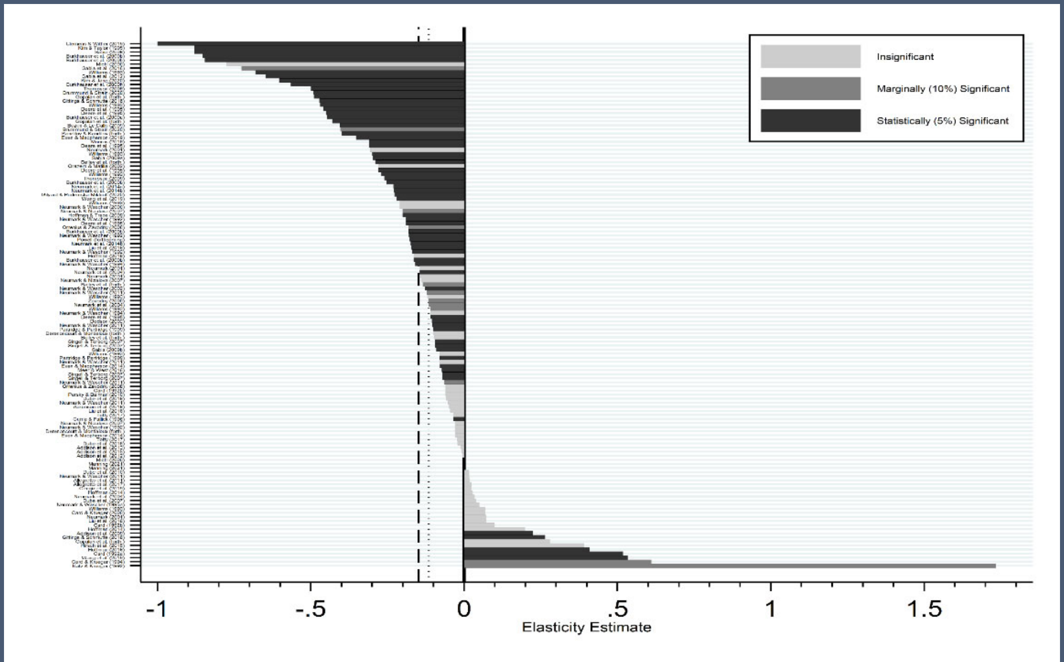
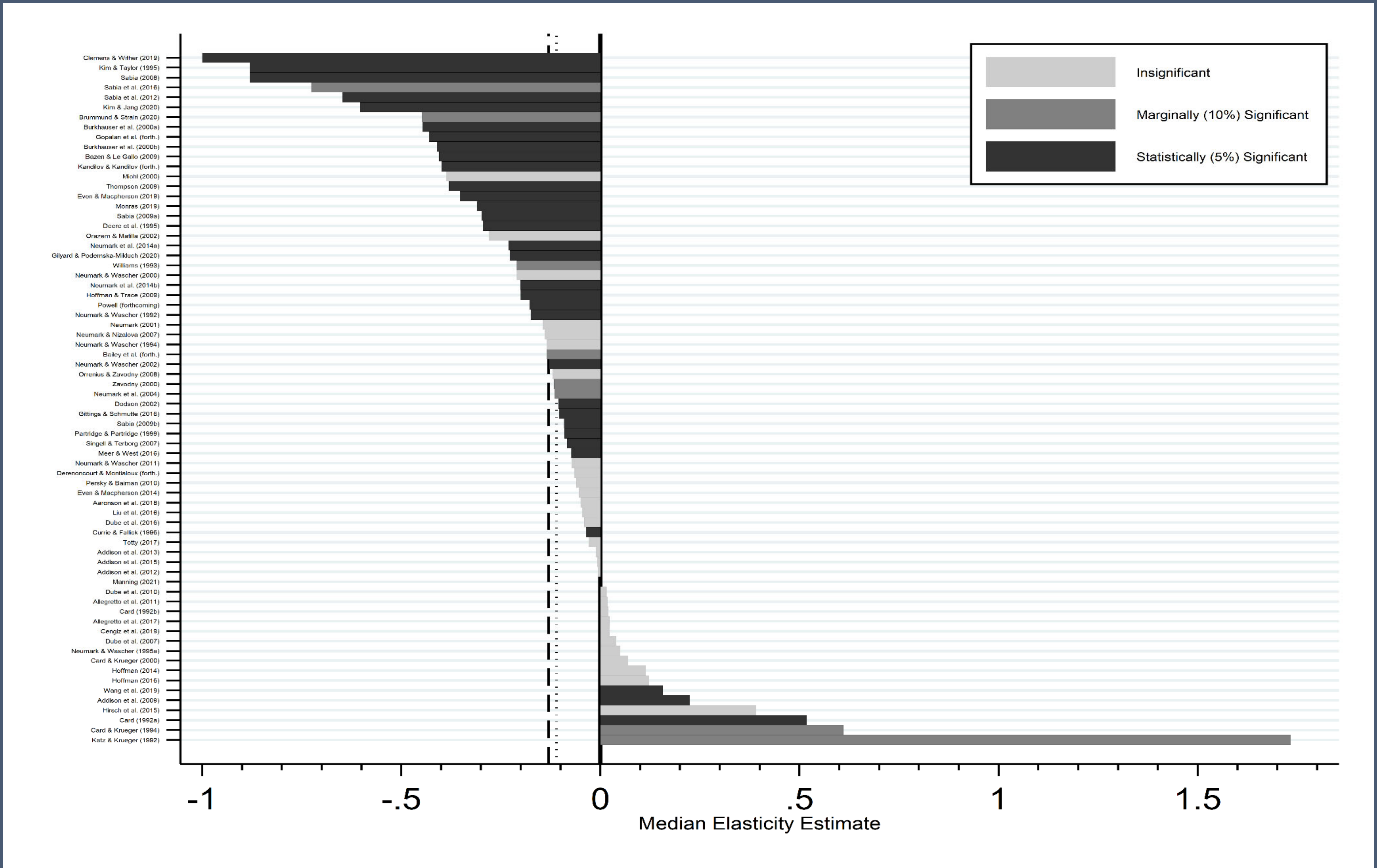
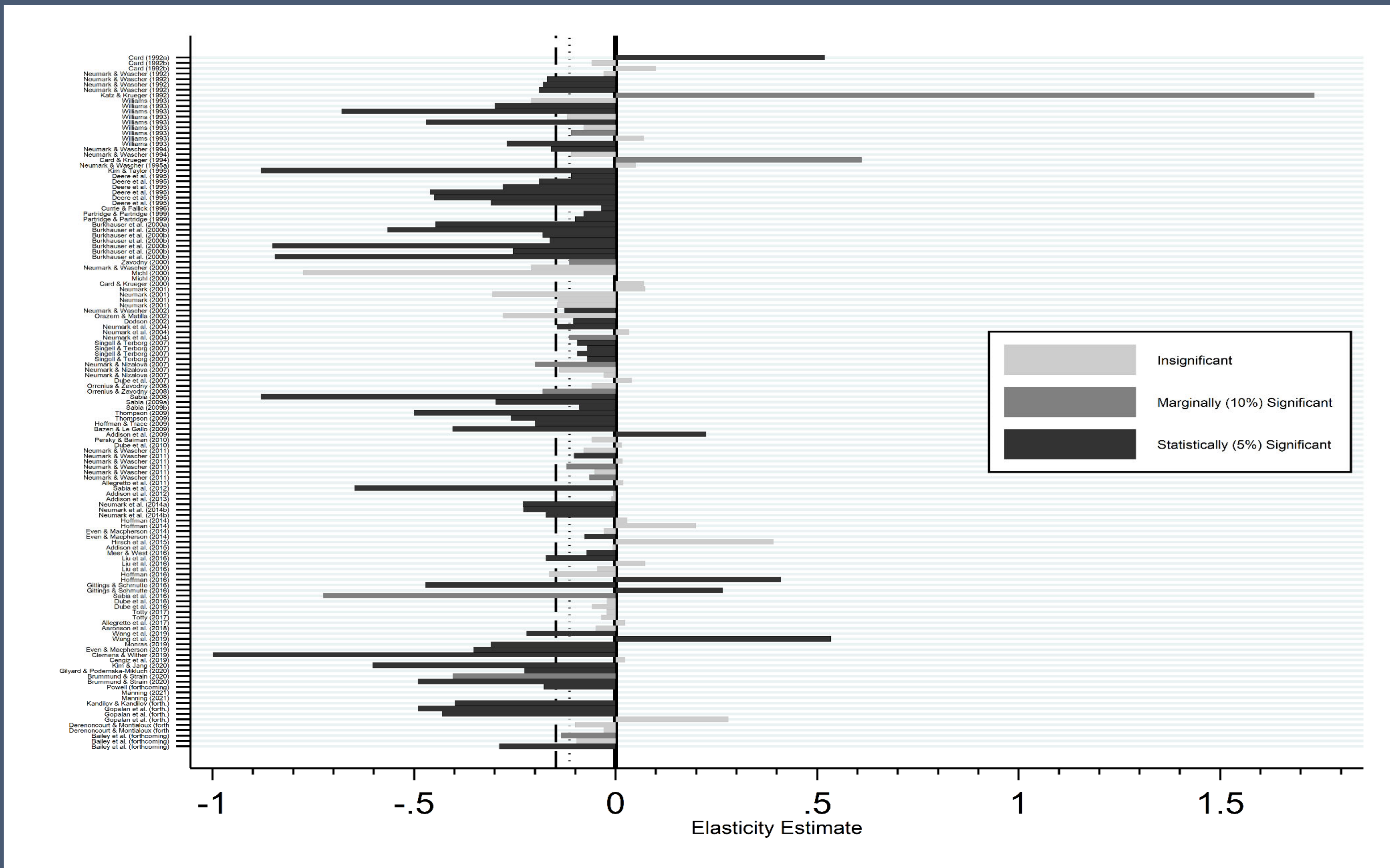


- Mean elas: −.148; median elas.: −.115
- 79.1% negative; 54.3% negative with p<.1; 46.5% negative with p<.05
- Of the large positive estimates, weaker statistical significance (so these estimates are less informative)
- Simplistic binomial treatment for null (true P(neg. effect) = .5): z-stat = 8.01
- Clearly at odds with conclusion that there is no evidence of negative effects, or negative vs. positive effects equally likely

Same conclusion using median estimates from each study



Is more recent (better?) evidence supportive of conclusion of no job loss?



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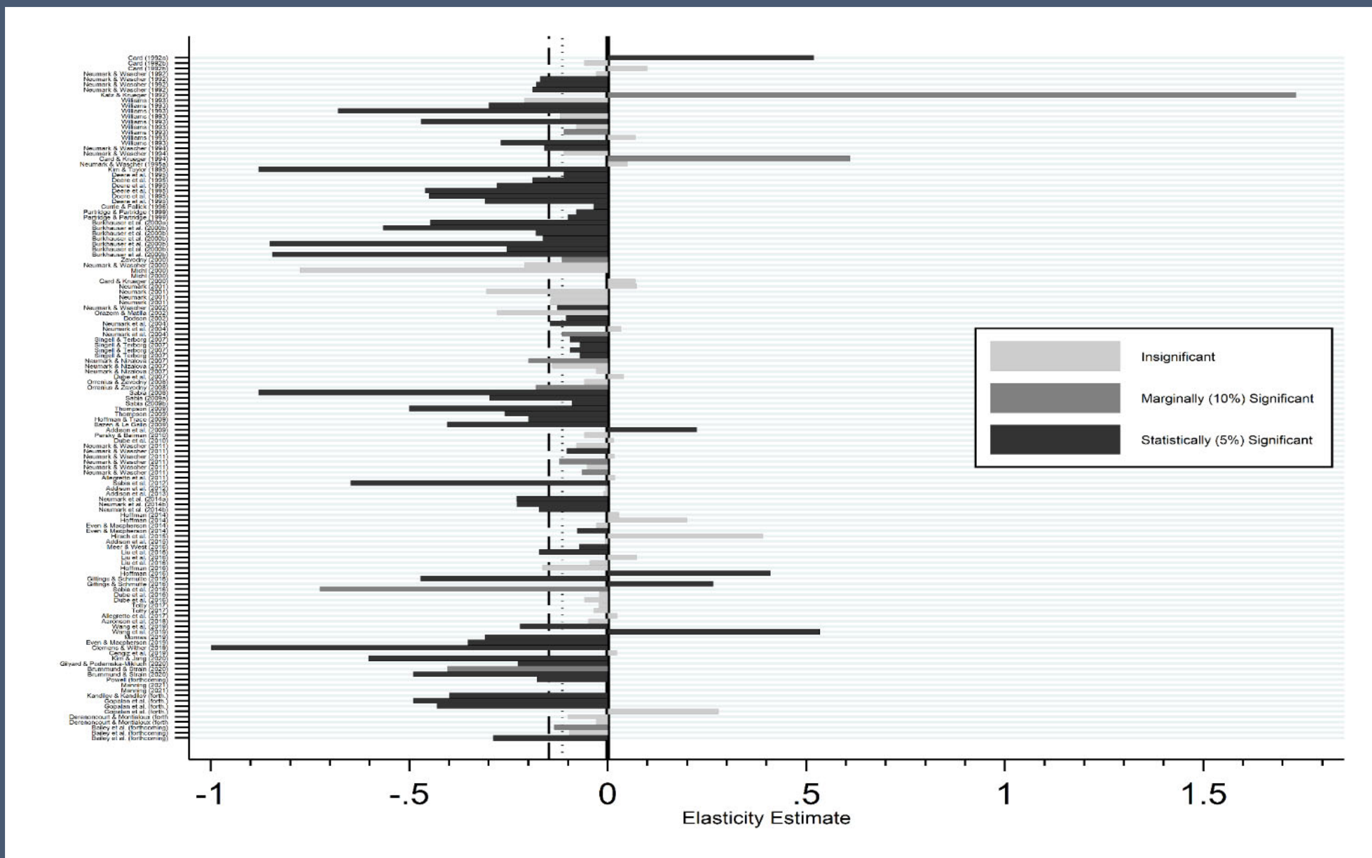


- **Regression of elasticity on publication year: coef. (p-value) = -.001 (.774)**
- **Largest positive estimates are from three of the earliest studies in the NMWR (Katz and Krueger, *ILRR*, 1992; Card, *ILRR*, 1992; Card and Krueger, *AER*, 1994)**

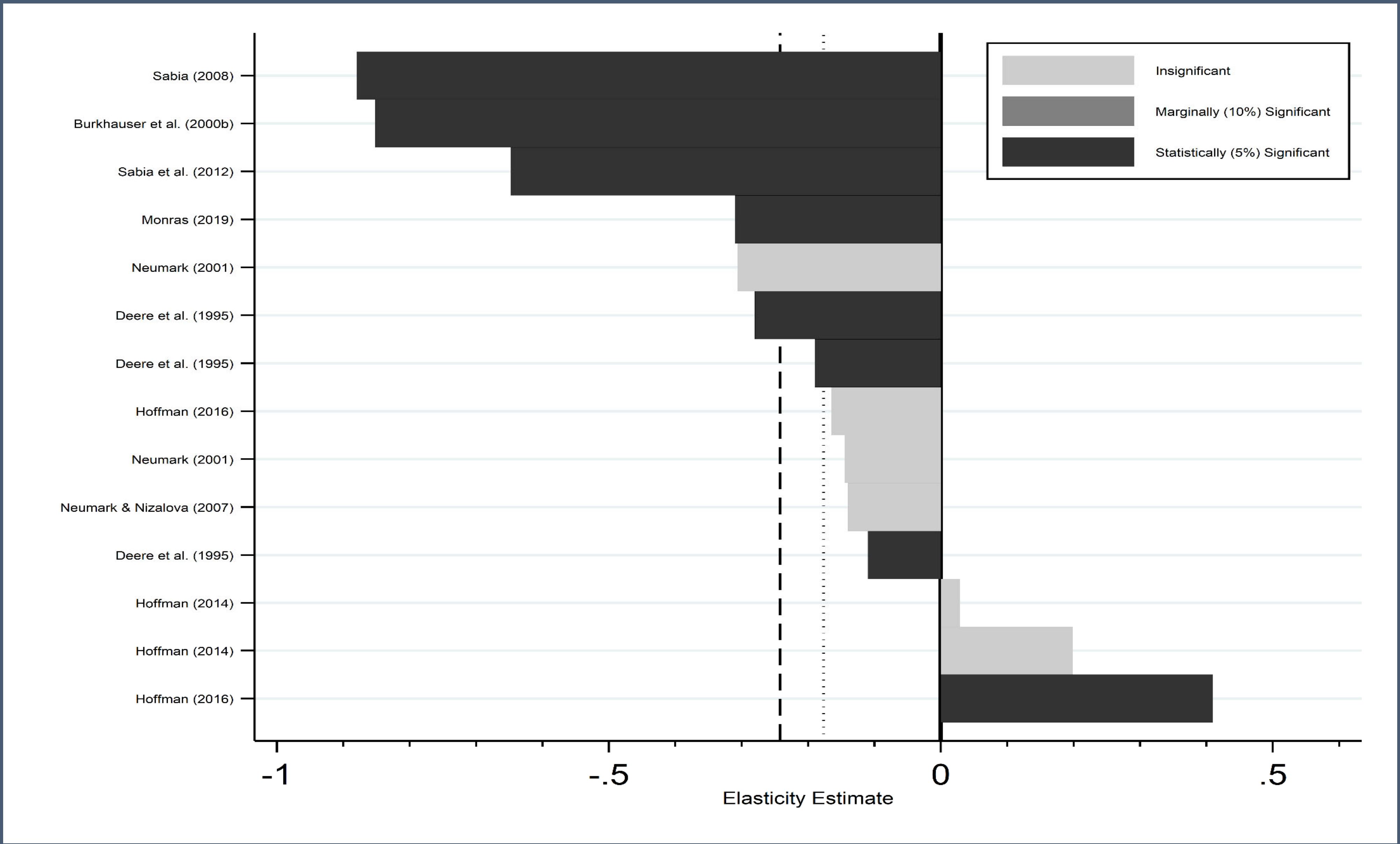
Exploring source of variation reveals modest differences

- Federal variation: effects identified from different workers, or variation “against” different state MWs
- State variation: using data across many or most states
- Case studies: one treated areas vs. one (“close,” or synthetic) control
- First two still largely/predominantly negative
- Case studies have smaller elasticities, and lower share negative
  - Mean elas: −.103; median elas.: −.083
  - 65.0% negative; 45.0% negative with $p<.1$; 40.0% negative with $p<.05$

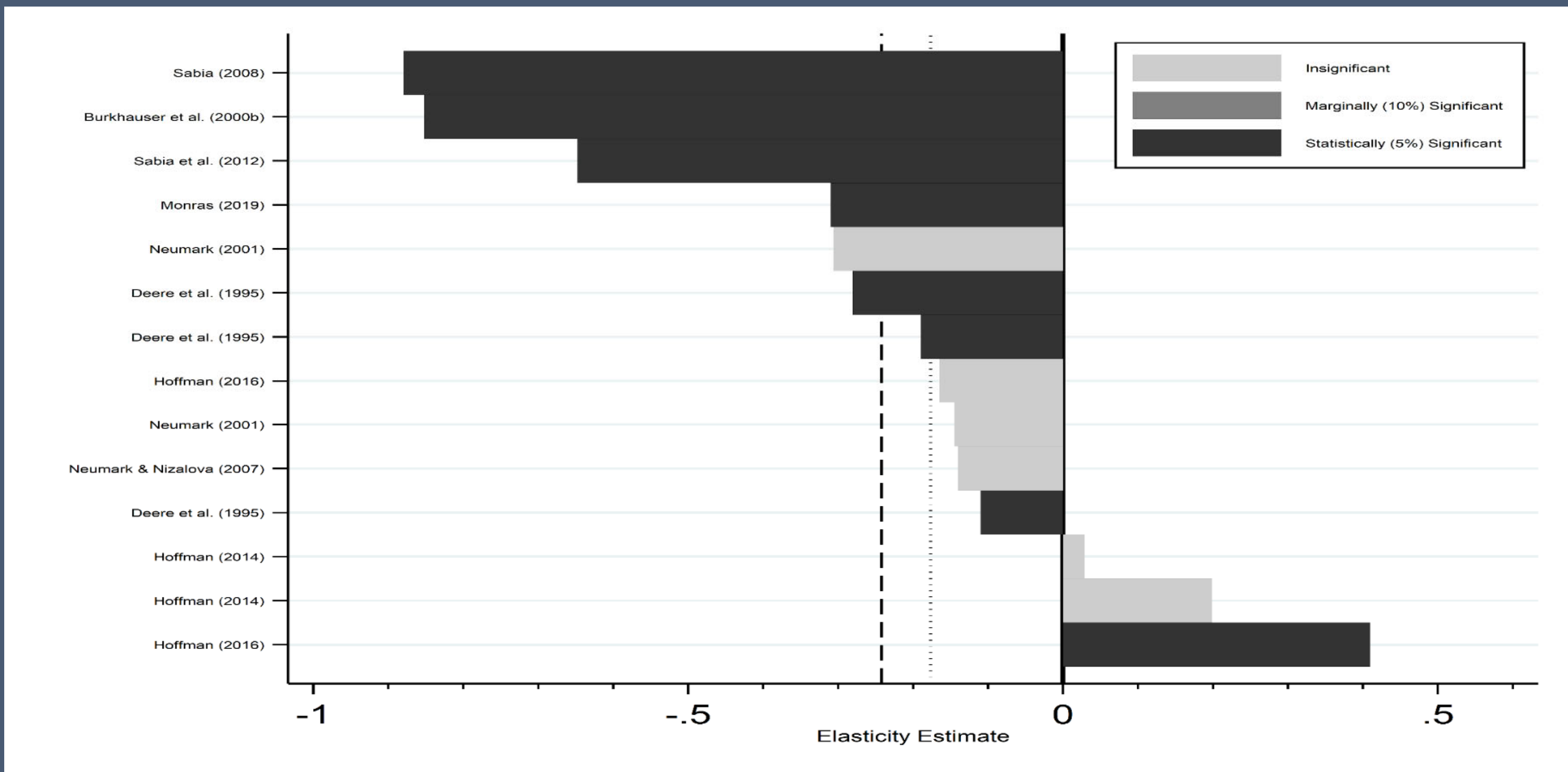
Variation by type of worker more interesting

- Effects similar for teens and young adults
  - Mean elas: −.17 to −.19; median elas.: −.12 to −.16
  - 80-83% negative; 57-58% negative with p<.1; 42-46% negative with p<.05
- Effects more strongly negative for less-educated
- Effects much closer to zero for low-wage industries

Less-educated

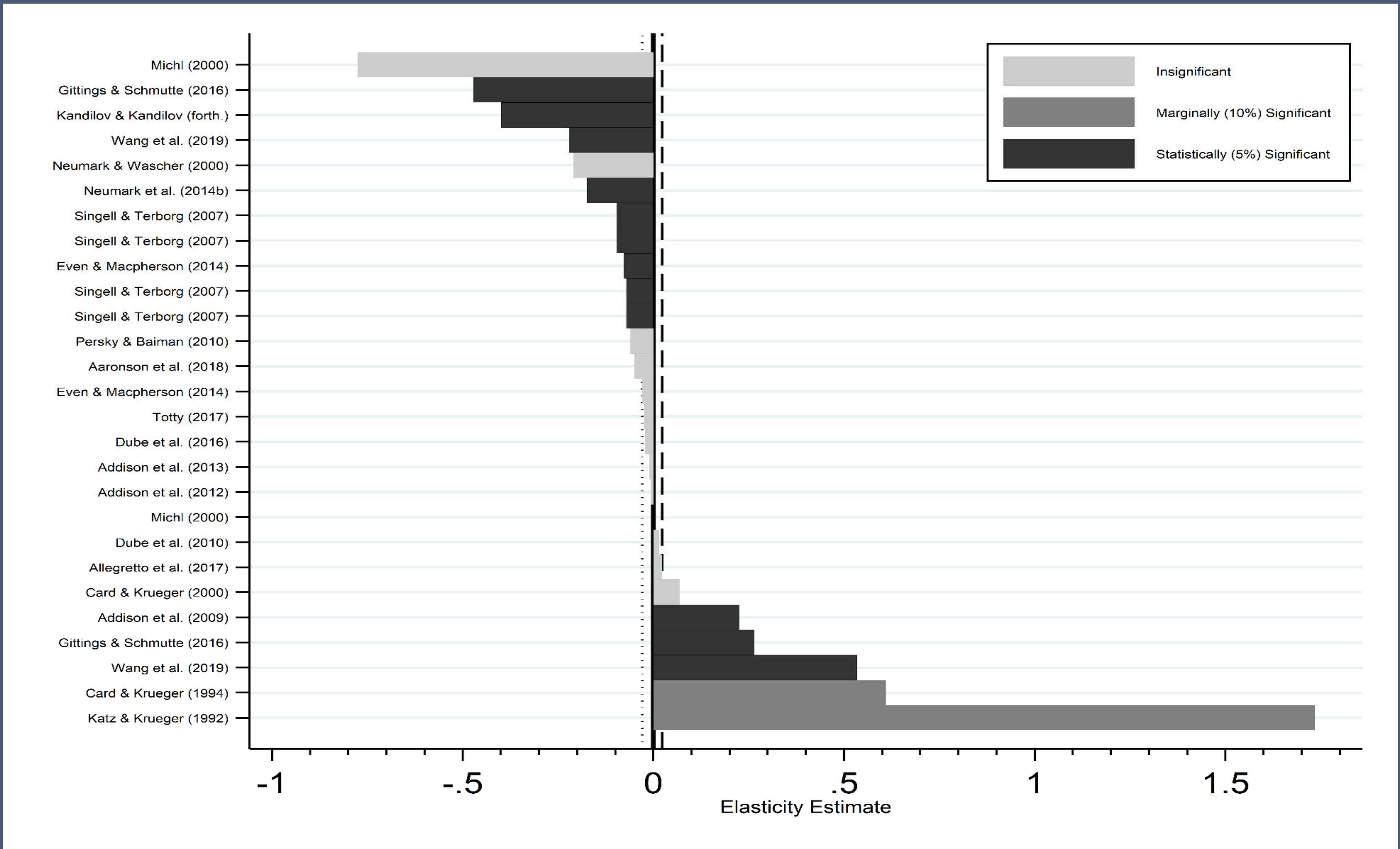


Less-educated

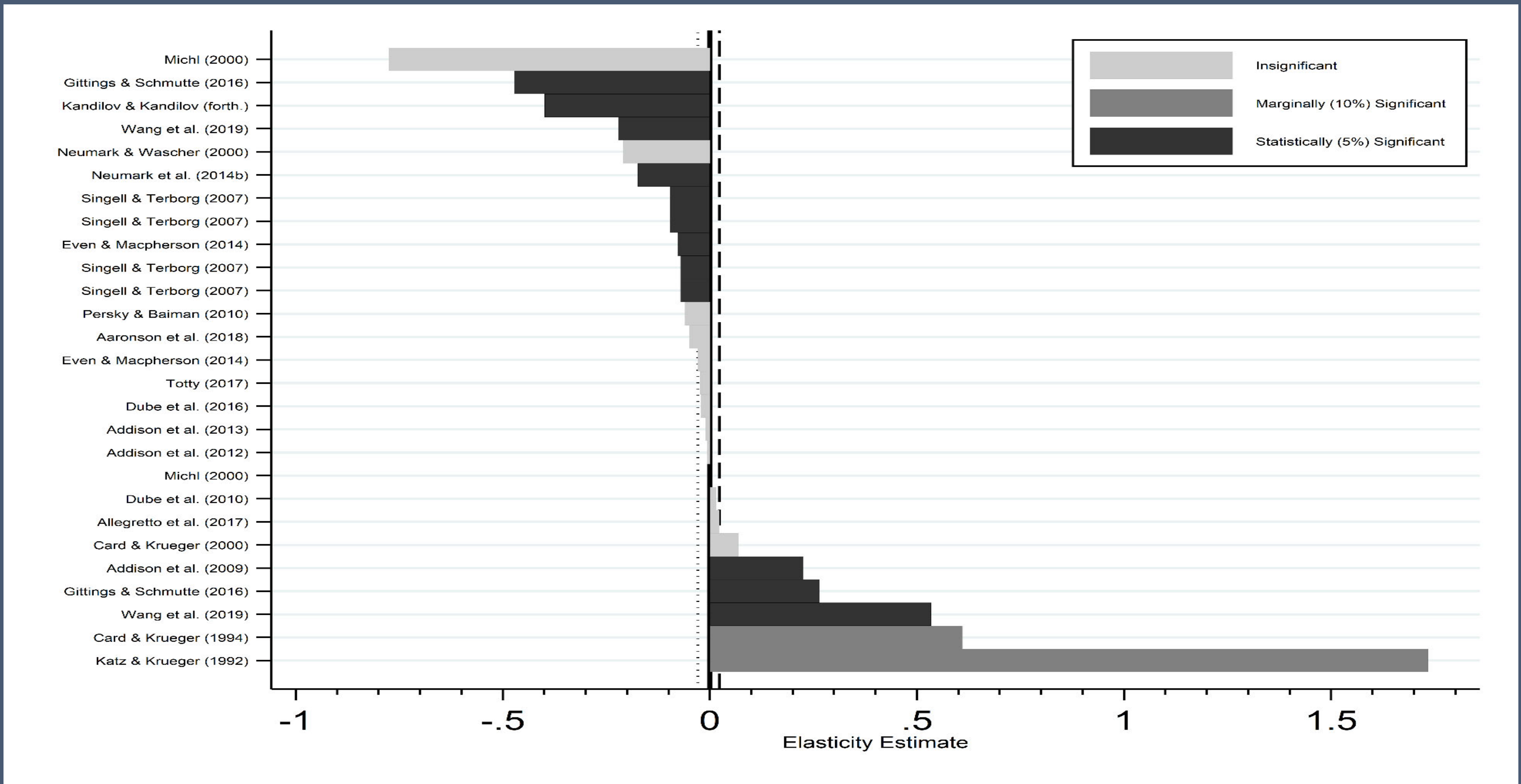


- Mean elas: −.242; median elas.: −.177
- 79% negative; 50% negative with p<.1; 50% negative with p<.05

Low-wage industries



Low-wage industries

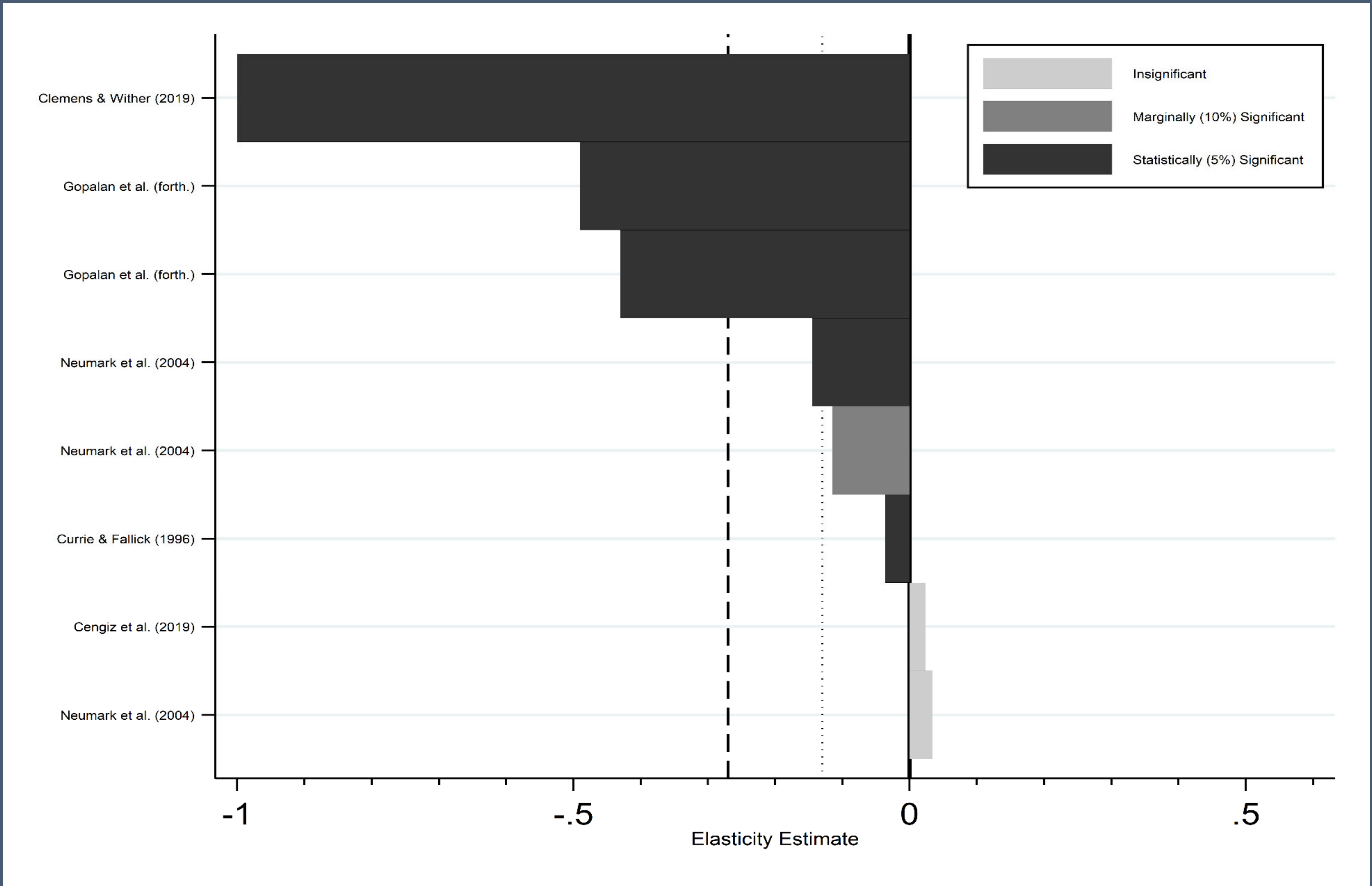


- Mean elas: .023; median elas.: −.029
- 67% negative; 33% negative with p<.1; 33% negative with p<.05

What should we conclude from studies of low-wage industries?

- These industries (by definition) have lower shares of low-wage workers, but still many higher-wage workers
- Labor-labor substitution may mask much larger (gross) disemployment effects on the least skilled
  - Substitution *has to be* within industry, whereas substitution for low-wage teens need not be toward higher-wage teens
- Evidence for low-wage industries relevant for asking what happens to employment in an industry, but perhaps not the most interesting policy question ("Does a higher MW help the lowest-wage workers?")

Not as many studies, but job loss looks worst for directly affected



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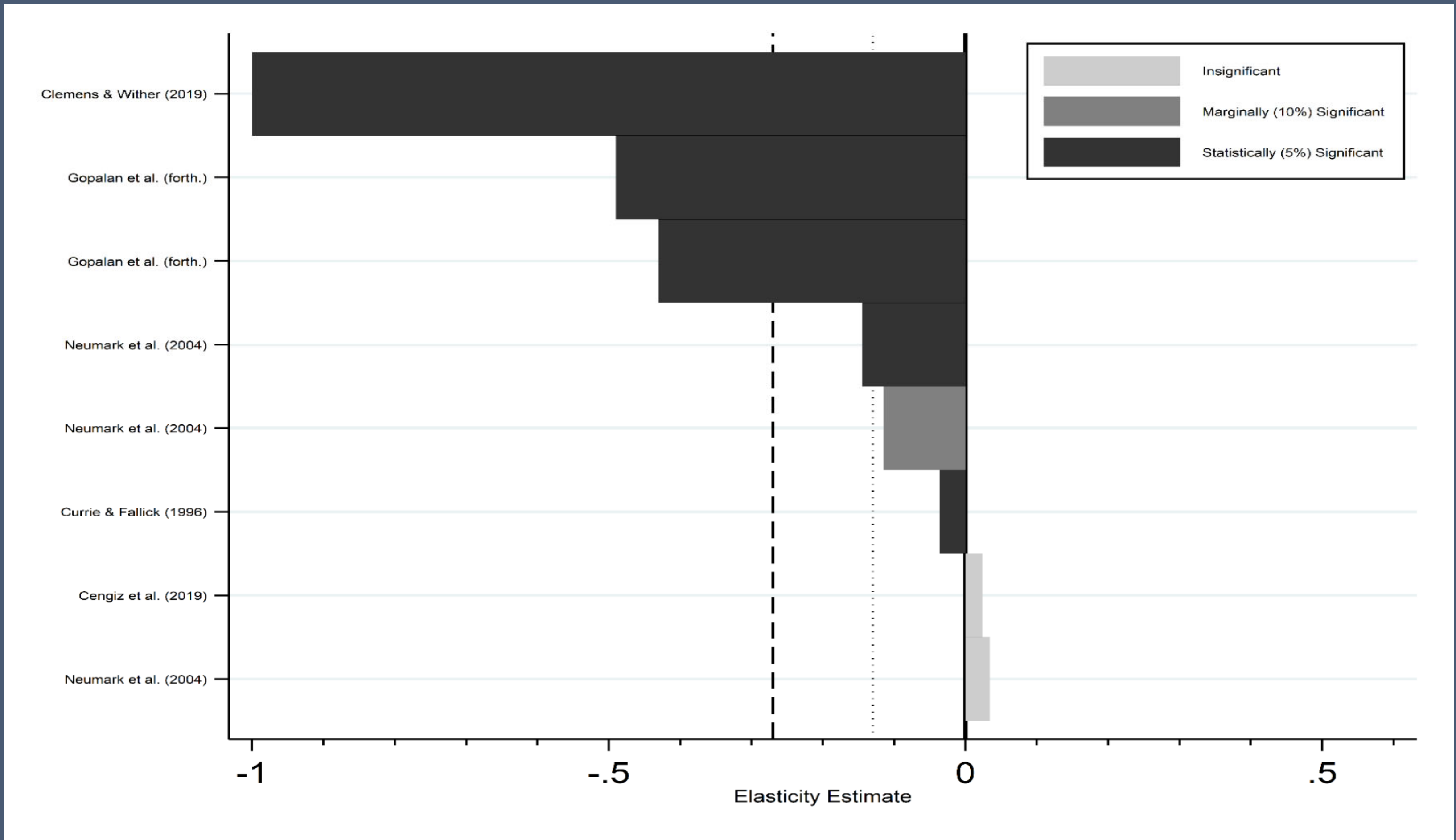

- Mean elas: −.270; median elas.: −.130
- 75% negative; 63% negative with p<.1; 63% negative with p<.05
- Elasticities *should* be larger for those directly affected

Summary: some heterogeneity (as we knew), but evidence predominantly negative

Figure	% negative	% negative, p ≤ .1	% negative, p ≤ .05	% positive	% positive, p ≤ .1	% positive, p ≤ .05
1: All	**79.1**	54.3	46.5	**20.9**	5.4	3.9
3: Median study estimates	**75.7**	50.0	41.4	**24.3**	7.1	4.3
4: Federal variation	**82.4**	52.9	47.1	**17.6**	2.9	0.0
5: State variation	**80.8**	57.5	47.9	**19.2**	4.1	4.1
6: Case studies	**65.0**	45.0	40.0	**35.0**	15.0	10.0
7: Teens	**80.0**	57.8	42.2	**20.0**	2.2	2.2
8: Young adults	**82.5**	57.1	46.0	**17.5**	1.6	1.6
9: Less educated	**78.6**	50.0	50.0	**21.4**	7.1	7.1
10: Low-wage industries	**66.7**	33.3	33.3	**33.3**	18.5	11.1
12: Directly-affected workers	**75.0**	75.0	62.5	**25.0**	0.0	0.0

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Multiple regression: the low-wage industry studies tend to deliver small effects – not the case studies

	Preferred elasticities						Median preferred elasticities		
	Magnitude				Negative	Negative and significant at 5% level	Magnitude		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Intercept	-0.148*** (0.031)	-0.151** (0.069)	-0.159** (0.057)	-0.149* (0.079)	0.921*** (0.067)	0.656*** (0.150)	-0.141** (0.067)	-0.232*** (0.052)	**-0.189** (0.072)**
Intercept + State	N/A	-0.158*** (0.036)	N/A	-0.195*** (0.066)	0.939*** (0.069)	0.734*** (0.112)	-0.166*** (0.034)	N/A	**-0.256*** (0.060)**
Intercept + Case Study	N/A	-0.103 (0.088)	N/A	-0.225 (0.135)	0.830*** (0.130)	0.749*** (0.181)	-0.103 (0.088)	N/A	**-0.277** (0.131)**
Intercept + Young Adults	N/A	N/A	-0.184*** (0.038)	-0.153** (0.072)	0.827*** (0.060)	0.536*** (0.114)	N/A	-0.175*** (0.034)	**-0.128* (0.067)**
Intercept + Low-wage Industry	N/A	N/A	0.023 (0.081)	0.069 (0.154)	0.671*** (0.118)	0.244 (0.157)	N/A	0.012 (0.075)	**0.083 (0.152)**
Intercept + Directly Affected	N/A	N/A	-0.270* (0.138)	-0.229 (0.158)	0.734*** (0.151)	0.682*** (0.179)	N/A	-0.277** (0.128)	**-0.218 (0.145)**
Intercept + Low-ed	N/A	N/A	-0.224** (0.094)	-0.202** (0.098)	0.850*** (0.151)	0.470** (0.208)	N/A	-0.237*** (0.089)	**-0.199** (0.097)**

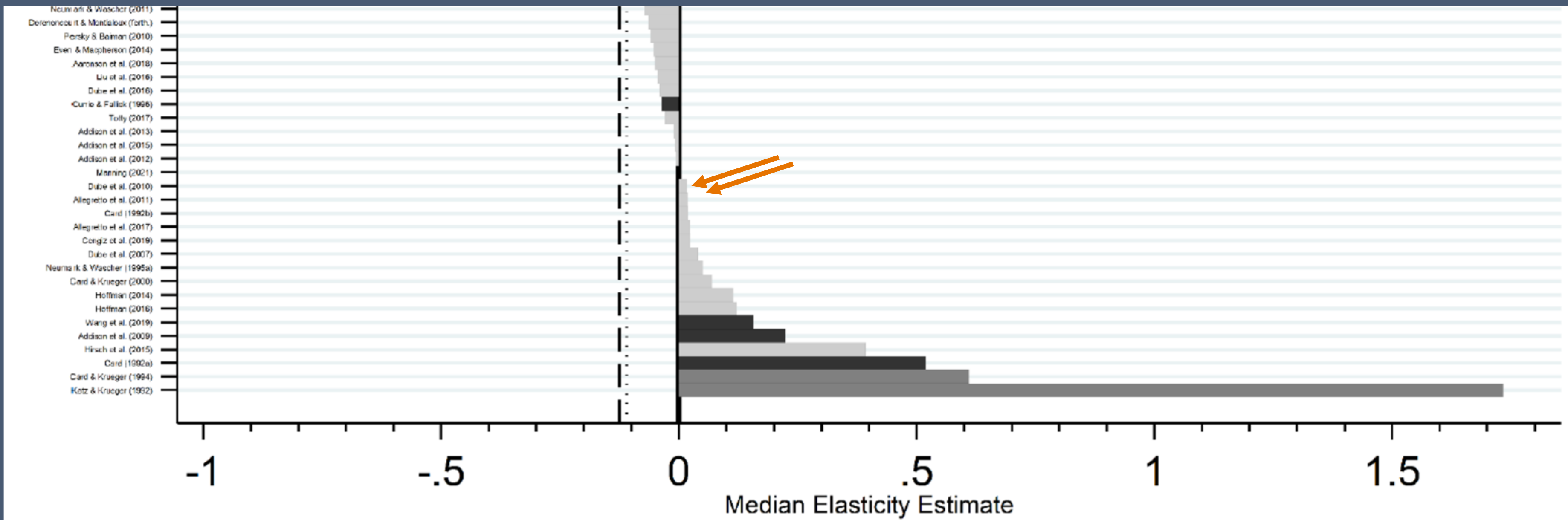
Research concludes that minimum wages reduce employment—unless you discard a lot of it

- Our conclusion: results on employment contested, but more studies, and greater variety of methods, point to job loss
- *"… concluding that the research evidence as a whole fails to find disemployment effects of minimum wages requires discarding or ignoring most of the evidence on low-skilled workers…"*
- Still leaves an open question: Should we do this? Are the studies that find no employment effects the only valid ones?
  - Some quotes from earlier (and some responses to this paper): "yes"

"What's Across the Border? Re-Evaluating the Cross-Border Evidence on Minimum Wage Effects" (J,N & R-L, 2022)

- **Core conflict is between panel data estimators that use many states as potential controls/counterfactual, vs. geographically close controls (pairs of counties on state borders) to estimate MW effects**

Most notably, from earlier median estimates graph…



"What's Across the Border?
Re-Evaluating the Cross-Border Evidence on Minimum Wage Effects"
(J,N & R-L, 2022)

- Core conflict is between panel data estimators that use many states as potential controls/counterfactual, vs. geographically close controls (pairs of counties on state borders) to estimate MW effects
- We implement a similar strategy, but using close control areas that more plausibly capture the same unobserved shocks that occur in areas where the MW increased
- We find very different results – evidence of job loss consistent with other estimates
- We present evidence that the DLR "border county" strategy introduces positive bias into estimated MW effects on employment, hence masking adverse employment effects

Helps answer question posed by first paper

- "Which evidence should we discard?"
- DLR (and other work by these authors) clearly argue it should be the standard panel data estimates
- Based on evidence I'll discuss today, we disagree

Two-way fixed effects models were the norm in earlier "new minimum wage research"

- Standard two-way FE model (state-by-year obs.)

$$E_{st} = \beta MW_{st} + X_{st}\gamma + D_s\theta + D_t\lambda + \varepsilon_{st}$$

  - Generally produces negative "consensus" estimates, elasticities for low-skilled groups near 0, vs. −0.1 to −0.2

"Close-controls" approach takes issue with two-way fixed effects model

- Dube et al. (2010, DLR) and Allegretto et al. (2011, ADR) critique/solution
  - MW chosen endogenously w.r.t. ε, but using close controls avoids problem
  - Let r denote region including subset of s (DLR: counties in bordering pairs; ADR: states in Census divisions)
  - Assume shocks common to r

$$E_{st} = \beta MW_{st} + X_{st}\gamma + D_s\theta + D_t\lambda + D_t \cdot D_r^T\eta + \varepsilon_{st}$$

  - Motivation/intuition: time-by-region dummies control for shocks correlated with MW
  - β identified as long as there is within-region variation

"Close-controls" approaches generally find no disemployment effect

Authors	Employment elasticity and groups studied	Data/approach
Geographically-proximate designs		
Dube, Lester, and Reich (2010)	**Near zero for teens and restaurant workers**	**Paired counties on opposite sides of state borders**
Allegretto, Dube, and Reich (2011)	**Near zero for teens**	**States compared only to those in same Census division**
Gittings and Schmutte (2016)	**Near zero for teens; larger negative elasticities in markets with short non-employment durations (−0.1 to −0.98) and smaller positive elasticities in markets with long non-employment durations (0.2 to 0.46)**	**States compared only to those in same Census division**
Addison et al. (2013)	**Varying sign, more negative, generally insignificant for restaurant workers and teens (although stronger negative at height of Great Recession)**	**Similar methods to Dube et al. (2010) and Allegretto et al. (2011) restricted to 2005-10 period**
Slichter (2016)	**-0.04 (teens)**	**Comparisons to bordering counties and other nearby counties**
Liu et al. (2016)	**−0.17 (14-18 year-olds)**	**Comparisons within Bureau of Economic Analysis (BEA) Economic Areas (EA) that cross state lines, with controls for EA-specific shocks**

Issue DLR and ADR raise taken seriously; approach has prompted 2 alternative approaches

- Other econometric approaches to construction of counterfactual
  - Powell (2022), data driven (synthetic control) approach, using all MW increases and their continuous variation
  - Estimated elasticity = −0.18
- Alternative identification strategies to isolate effects of MW from potentially correlated shocks

DDD is natural alternative approach

- Within-state variation in how people or areas are affected can provide other group exposed to shock, but not policy change
- E.g., Thompson (2009) studies effect of federal increases in counties with high share affected

$$E_{ct} = \beta POST_t \cdot H_c + X_{ct}\gamma + H_c\psi + D_s\theta + POST_t\lambda + \varepsilon_{ct}$$

- DDD estimator: identifies MW effect from differential change in employment in counties where the minimum wage increase affected more workers (H = 1) versus fewer workers
  - But less prone to bias from shocks because (a) uses federal variation and (b) effect identified for subregions of states

DDD strategies

- Related approach in Clemens and Wither (2016): look at low-wage and very low-wage workers, with H becoming indicator for the latter
  - MW identified from differential effect of on lowest-wage vs. other low-wage workers – who are arguably affected by the same shocks that could be correlated with MW, but not by MW

"DD/DDD" approaches generally find strong disemployment effects

Authors	Employment elasticity and groups studied	Data/approach
Other approaches		
Thompson (2009)	**−0.3 (for teen employment share)**	**Low-wage counties vs. higher-wage counties in states**
Clemens and Wither (2014)	**Appx. −0.97, for those directly affected by minimum wage increase**	**Targeted/affected workers versus other low-wage workers in states affected by federal increases**

IV strategy

- Baskaya and Rubinstein (2015) create IV based on interaction between federal MW and historical probability that federal MW binds in state
  - Solve problem that candidate IVs – like political tilt – may be fully absorbed by state and year Fes
- Define F_s as fitted value from model for probability that state lets federal MW bind

$$F_s = P(Z_s\pi)$$

  - Z_s: cross-state measures of political preferences, and prop. of early years federal MW was binding
- First stage combined with standard two-way FE estimator

$$MW_{st} = \varphi F_s MW^F_t + X_{st}\gamma + D_s\theta + D_t\lambda + \varepsilon_{st}$$

IV estimate is strongly negative

Authors	Employment elasticity and groups studied	Data/approach
Other approaches		
Baskaya and Rubinstein (2015)	**−0.3 to −0.5 for teens**	**States, using federally-induced variation as instrumental variable**

Different methods, different results

- Key studies using close controls generally find no evidence of disemployment effect
- Other strategies (DDD, IV, SC) generally do
- Limited exceptions (haven't shown them all)
- All address the same problem of shocks potentially correlated with MW increases – so issue is not whether these shocks are considered, but <u>how</u>

One potential problem with DLR's close-controls analysis (main issue we consider): bad controls

- I've criticized the Allegretto et al. (2011) paper treating states in same Census division as close controls (because they aren't)
- Liu et al. (2016) use BEA Economic Areas that cross state lines
  - Regions that are supposed to have similar and integrated economies and hence similar shocks on both sides of border
  - Estimated elasticity (14-18) = −0.17
- Are cross-border counties, without regard to whether they are in similar economic areas (<u>or especially if they aren't</u>), bad controls for capturing common shocks?

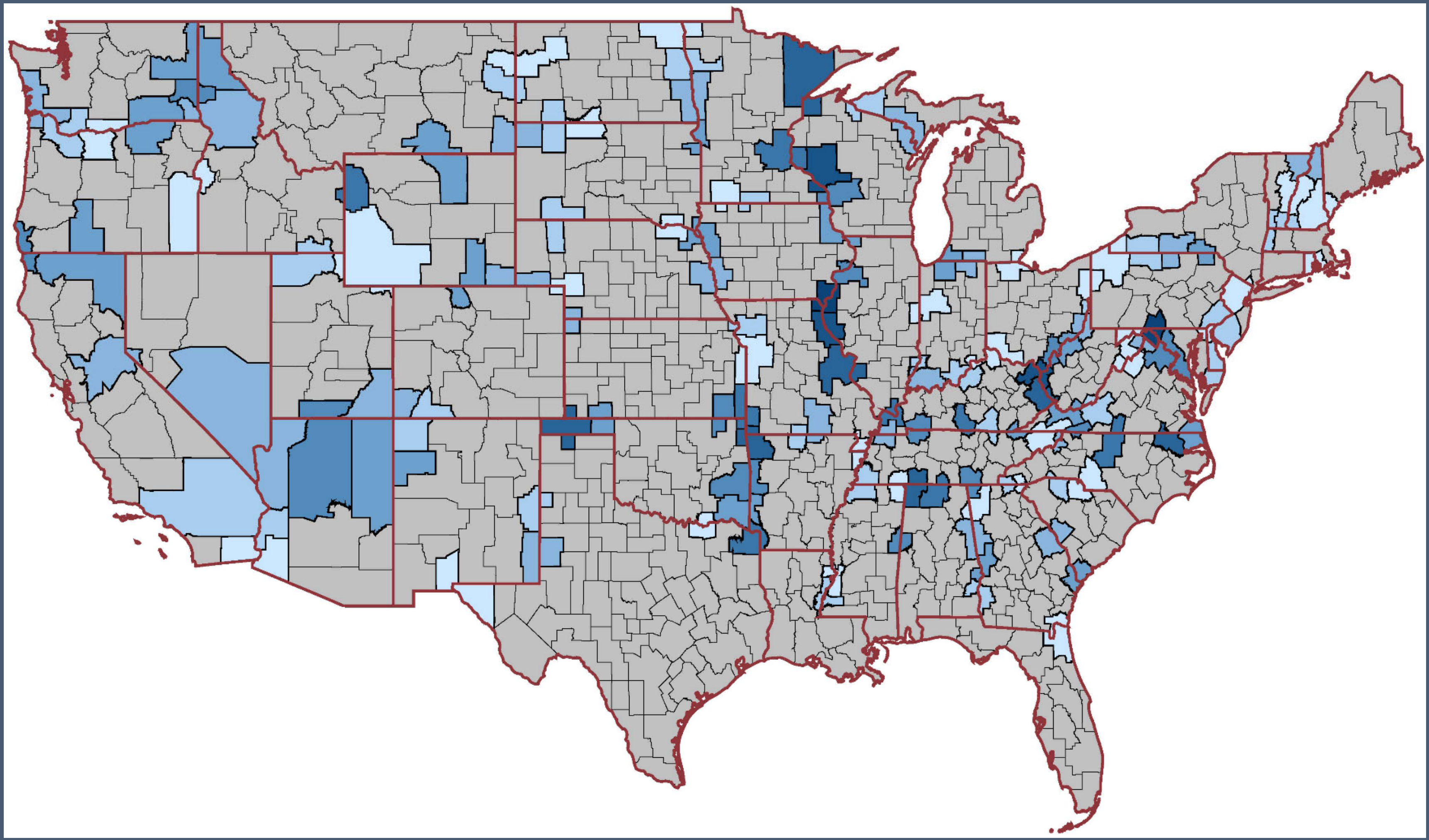
2 of the 3 authors of DLR have made the same argument!

- IRLE working paper by Allegretto, Dube, and Reich (2009), studying teen employment, uses cross-border counties in commuting zones (based on Census's journey-to-work data)
  - Using commuting zones "is appealing because these areas are not only contiguous; they are also demonstrably linked with each other by an economically meaningful criterion"

Re-evaluation of DLR results

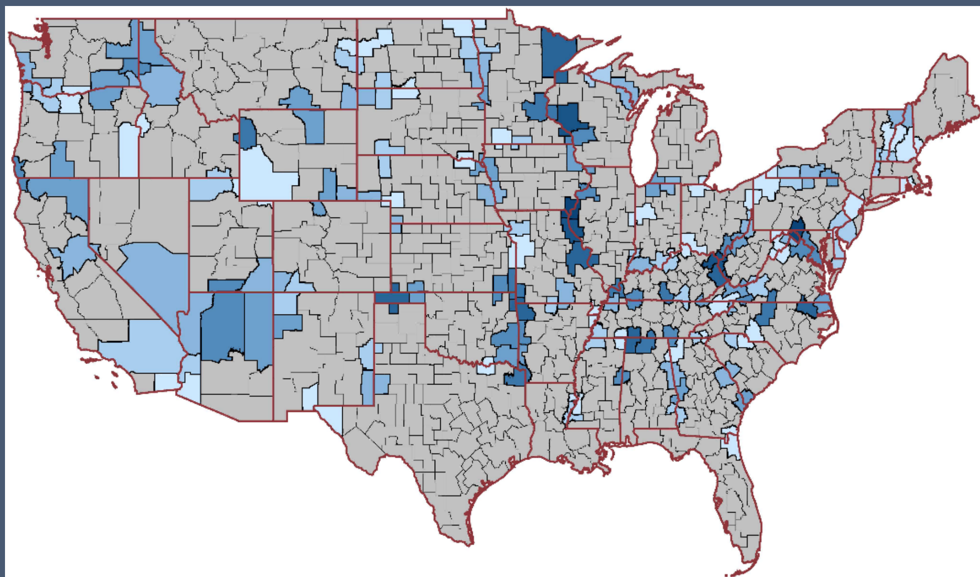
- Use cross-border areas of commuting zones in multi-state commuting zones (MSCZs)
- Use restaurant employment
  - Start with QCEW data (as in DLR), but then turn to CBP data (Autor et al. 2013 & 2016), which has much better coverage because QCEW data is suppressed for many counties
    - E.g., with QCEW we get 316 complete cross-border pairs and 73 within MSCZs; with CBP the numbers increase to 1,181 and 151
    - Coverage for counties increases from 26.75% to 98.65%, and for MSCZ counties from 47.71% to 98.69% (less impact because MSCZ counties are larger)
    - We also add 10 more years of data

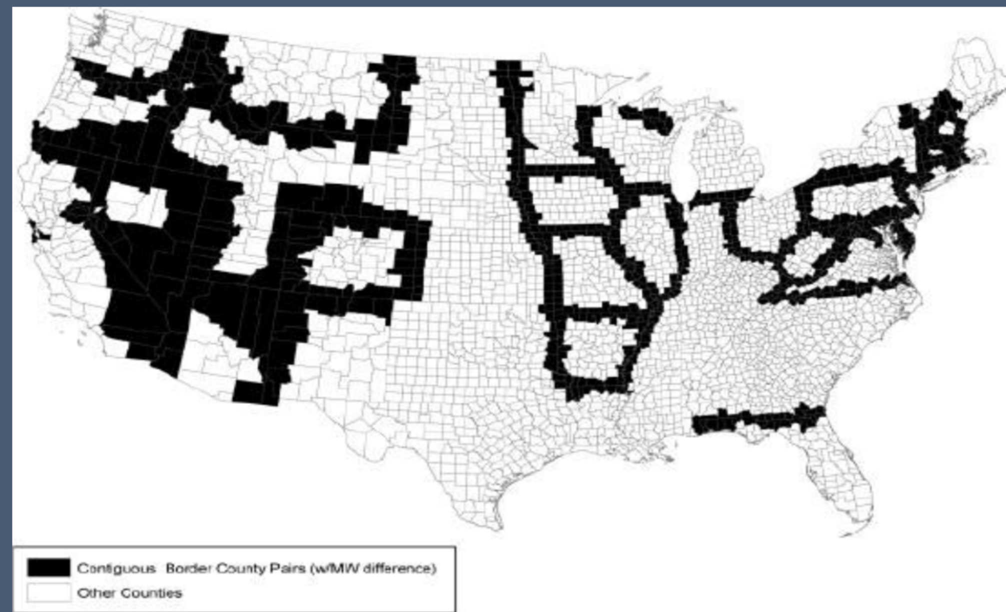
MSCZ map: geographically dispersed



The 137 MSCZs. Note: All counties assigned to CZ; small subset are multi-state.

MSCZ map vs. DLR cross-border county pairs





- Counties in MSCZs have nearly twice the population of counties in cross-border pairs that in not in MSCZs. (More urban, a point I come back to.)
- Maps not strictly comparable. RH map (from DLR) is only cross-border counties with some MW variation along the border. But you can see there are lot of cross-border county pairs that aren't in the same MSCZ.

MSCZs representative of U.S. economy and MW policy

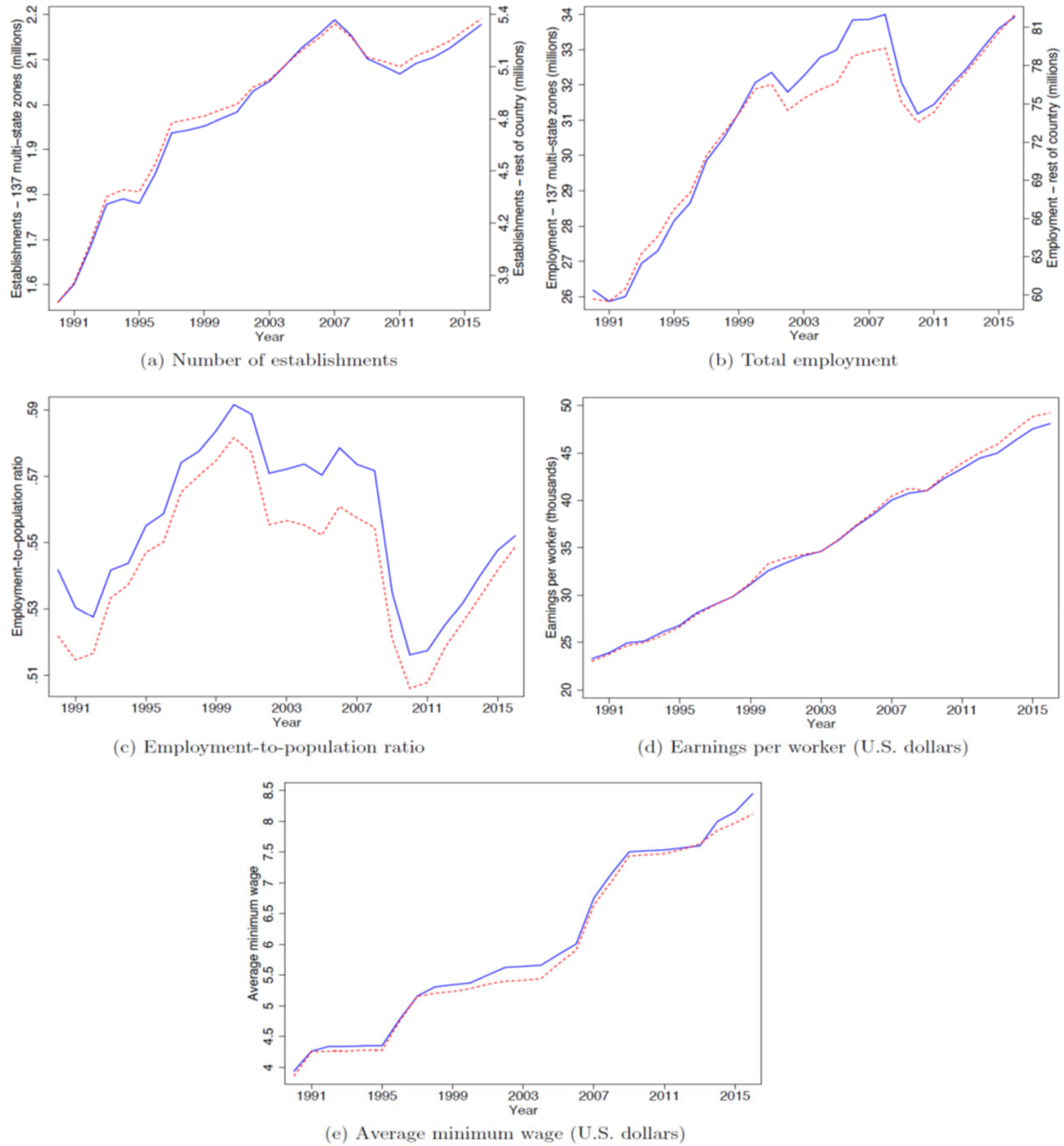


Figure 2: Comparison between commuting-zone groups: 137 multi-state commuting zones (solid blue) and rest of the country (dashed red)

Changing from all cross-border counties to cross-border areas in MSCZs changes the answer sharply

Table 1: Replication and re-analysis of Dube, Lester, and Reich (2010) using 1990-2006 QCEW data

	Specification (5)		Specification (6)	
	(1)	(2)	(3)	(4)
A. DLR's Contiguous Border County-Pair Sample				
(a) ln(employment)				
ln(minimum wage)	-0.137*	-0.112	0.057	0.016
	(0.072)	(0.079)	(0.088)	(0.076)
(b) ln(earnings)				
ln(minimum wage)	0.232***	0.221***	0.200***	0.189***
	(0.033)	(0.034)	(0.050)	(0.047)
Observations	70,620	70,582	41,712	41,676
B. Multi-state Commuting Zone-Pair Sample				
(a) ln(employment)				
ln(minimum wage)	-0.212***	-0.186**	-0.128*	-0.141**
	(0.069)	(0.072)	(0.070)	(0.070)
(b) ln(earnings)				
ln(minimum wage)	0.232***	0.226***	0.222***	0.208***
	(0.041)	(0.042)	(0.071)	(0.064)
Observations	13,266	13,264	9,636	9,634
Pair–period effects			Y	Y
Total private sector		Y		Y

Equally true for CBP data

Table 3: Pair-approach estimation of minimum wage responses with CBP 1990-2016 data

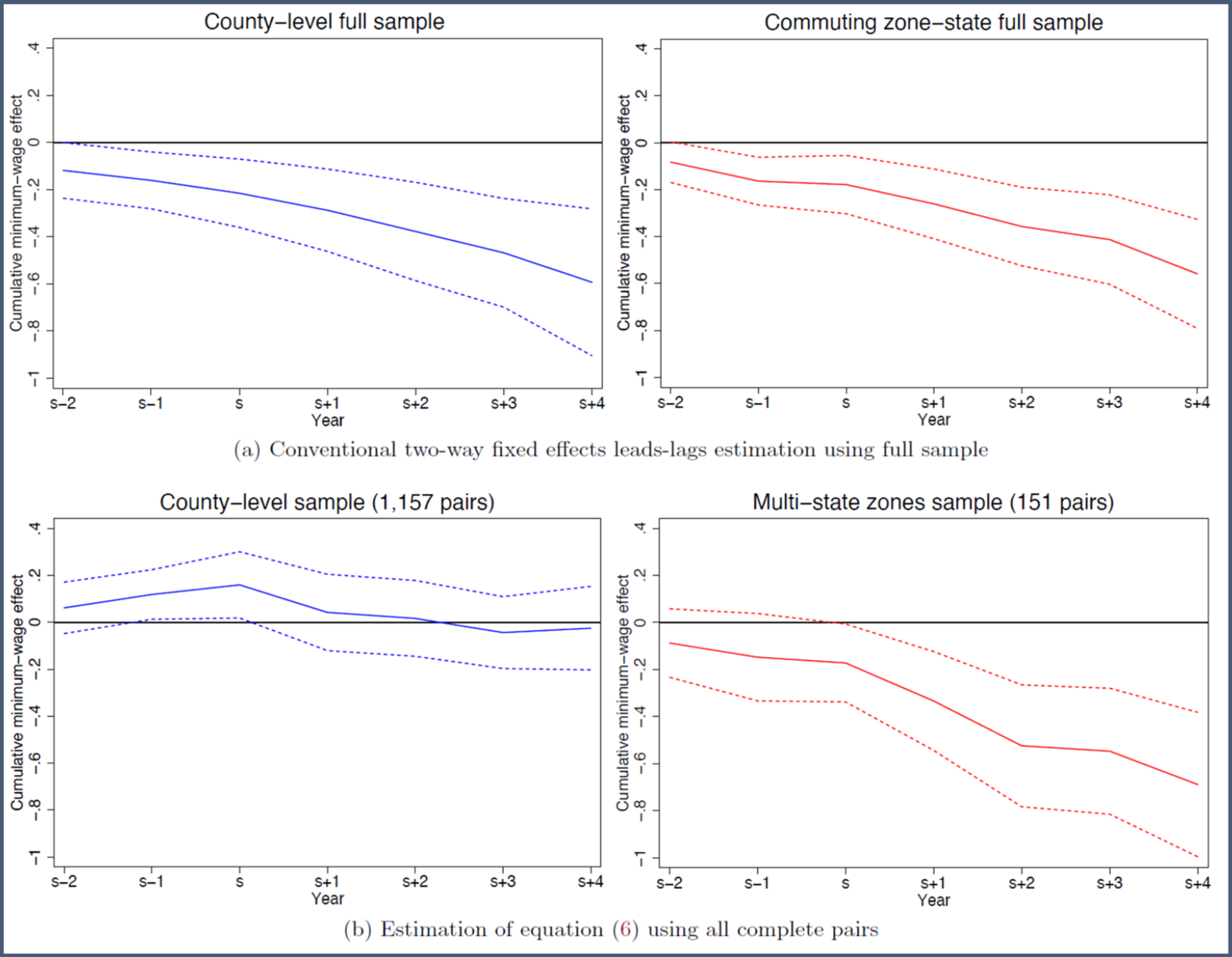
	Multi-state zones		Contiguous counties	
	(1)	(2)	(3)	(4)
A. ln(employment)				
ln(minimum wage)	-0.242**	-0.255***	-0.081	-0.023
	(0.120)	(0.082)	(0.063)	(0.056)
ln(employment$^-$)	0.159	0.073	0.193***	0.154***
	(0.098)	(0.089)	(0.053)	(0.051)
ln(population)	0.934***	1.116***	0.979***	1.000***
	(0.179)	(0.184)	(0.100)	(0.081)
B. ln(earnings)				
ln(minimum wage)	0.163***	0.198***	0.156***	0.211***
	(0.055)	(0.044)	(0.044)	(0.029)
ln(earnings$^-$)	0.113	-0.047	0.044	0.017
	(0.138)	(0.034)	(0.056)	(0.022)
ln(population)	0.085	0.068	0.027	0.040
	(0.084)	(0.049)	(0.042)	(0.031)
Zone-state effects	Y	Y		
County effects			Y	Y
Pair–period effects	Y	Y	Y	Y
DLR data pairs		Y		Y
Number of pairs	151	71	1,165	309
Observations	8,134	3,830	62,228	16,670

Robust to ending year for analysis (DLR vs. latest consistent CBP data)

Table 4: Evolution of the minimum wage elasticity of employment with pair approach and CBP data

	Multi-state zones		Contiguous counties	
Period	(1)	(2)	(3)	(4)
1990–2006	-0.291***	-0.214**	-0.166**	-0.059
	(0.095)	(0.088)	(0.065)	(0.058)
1990–2007	-0.301***	-0.239***	-0.169***	-0.076
	(0.092)	(0.084)	(0.061)	(0.055)
1990–2008	-0.327***	-0.262***	-0.175***	-0.086
	(0.093)	(0.084)	(0.061)	(0.055)
1990–2009	-0.337***	-0.270***	-0.170***	-0.086
	(0.092)	(0.084)	(0.058)	(0.055)
1990–2010	-0.338***	-0.275***	-0.159***	-0.083
	(0.094)	(0.086)	(0.055)	(0.054)
1990–2011	-0.333***	-0.275***	-0.141**	-0.075
	(0.094)	(0.087)	(0.054)	(0.054)
1990–2012	-0.341***	-0.284***	-0.127**	-0.071
	(0.095)	(0.087)	(0.053)	(0.054)
1990–2013	-0.323***	-0.285***	-0.106*	-0.064
	(0.099)	(0.087)	(0.053)	(0.056)
1990–2014	-0.275**	-0.278***	-0.081	-0.048
	(0.112)	(0.089)	(0.056)	(0.059)
1990–2015	-0.258**	-0.260***	-0.084	-0.033
	(0.117)	(0.086)	(0.057)	(0.059)
1990–2016	-0.242**	-0.255***	-0.081	-0.023
	(0.120)	(0.082)	(0.063)	(0.056)
Zone-state effects	Y	Y		
County effects			Y	Y
Pair–period effects	Y	Y	Y	Y
DLR data pairs		Y		Y
Number of pairs	151	71	1,165	309

Longer-term differences more pronounced (2 years of leads and 4 years of lags)



Where are we so far?

- **TWFE estimates give conventional negative MW elasticities, whatever data we use**
- **Close-control estimate using MW variation within cross-border county pairs is smaller, close to zero**
- **Close-control estimate using MW variation within cross-border areas *in MSCZs* is negative, as large or larger than TWFE estimate**

Where are we so far?

- **TWFE estimate give conventional negative MW elasticities, whatever data we use**
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- **Why?**

Heterogeneous effects: monopsony?

- One potential response is effects are heterogeneous, and depend on which counties (or areas more broadly) identify the MW effects
  - Could even explain the TWFE vs. DLR cross-border county results – latter uses only the MW variation along state borders for cross-border county pairs, which tend to be less densely populated (more rural)
- We use NETS to calculate HHI by firm, in the restaurant industry, for each county, at baseline (first available 1992, so we adjust starting period for analysis)
  - Incorporate interaction of MW with HHI (following recent papers): monopsony predicts positive interaction – and is it larger for DLR's cross-border counties?
- No evidence this is the explanation – MW effects across different samples/approaches unchanged
  - Prediction is less negative employment effect *over range of lower MWs*, but no evidence of this

No evidence of monopsony regardless of subset of counties considered

Table 10: Monopsony power in the pair-approach estimation of minimum wage responses for different cross-border county-pair samples

	All cross-border contiguous counties not in same CZs	*Only cross-border contiguous counties in same MSCZs*	*All cross-border counties (incl. non-contiguous) in same MSCZ*	*Subset of (3), excludes contiguous cross-border counties*	*Subset of (1), cross-border counties not in any MSCZ*
	(1)	(2)	(3)	(4)	(5)
ln(minimum wage)	-0.021	-0.162	-0.231**	-0.263*	0.101
	(0.077)	(0.100)	(0.110)	(0.137)	(0.142)
$\ln(\text{MW}) \times (\text{HHI} - \overline{\text{HHI}})$	-0.605	-0.558	-0.823	-0.904	-0.463
	(0.456)	(0.673)	(0.540)	(0.559)	(0.641)
$\ln(\text{employment}^{-})$	0.237***	0.174**	0.192***	0.201***	0.288***
	(0.062)	(0.070)	(0.050)	(0.052)	(0.075)
ln(population)	0.938***	0.894***	0.889***	0.890***	0.855***
	(0.128)	(0.151)	(0.107)	(0.127)	(0.153)
County effects	Y	Y	Y	Y	Y
Pair–period effects	Y	Y	Y	Y	Y
Number of pairs	843	322	986	664	395
Observations	41,496	15,940	48,912	32,972	19,354
Summary statistics for 1992 HHI $\in (0,1]$***:***					
Mean ($\overline{\text{HHI}}$)	0.109	0.094	0.097	0.097	0.122
Standard deviation	0.130	0.123	0.130	0.131	0.142
Minimum	0.002	0.003	0.001	0.001	0.003
Maximum	1	1	1	1	1
Number of counties	927	456	740	557	462

Concentration higher in less dense counties in col (5) vs. col (3), but no evidence of predicted monopsony-MW relationship.

Second potential problem with *DLR's* close-controls analysis: positive bias

- Analogous to Griliches (1979) showing that within-family estimates of returns to schooling may not be less biased
- Assume two years of data, treated areas a, control areas a'
- Form DD estimator

$$(\Delta E_a - \Delta E_{a'}) = \beta \Delta MW_a + (\Delta X_a - \Delta X_{a'}) \cdot \gamma + (\Delta \varepsilon_a - \Delta \varepsilon_{a'})$$

- Intuition behind close-controls approach: common shock $\Delta\mu_a$ in $\Delta\varepsilon_a$ and $\Delta\varepsilon_{a'}$ that is differenced out
- But suppose shocks not identical (just like identical twins don't have identical ability), so $\Delta\mu_a \neq \Delta\mu_{a'}$

Within-area estimates (I)

- Ignoring X, bias (inconsistency) in within-area estimate is

$$Cov(\Delta\mu_a - \Delta\mu_{a'}, \Delta MW_a)/Var(\Delta MW_a)$$

  – Reasonable to expect that $Cov(\Delta\mu_a - \Delta\mu_{a'}, \Delta MW_a)$ is smaller for close controls
  – But $Var(\Delta MW_a)$ is generally lower in nearby states, because of strong regional pattern to MWs (i.e., increase in NE state likely across border from other NE state with MW above federal MW)
  – Thus not clear that bias shrinks, and could easily be increased instead
- Key point for us that could explain results: $Cov(\Delta\mu_a - \Delta\mu_{a'}, \Delta MW_a)$ could be positive (larger) when close-controls don't come from areas exposed to similar shocks, and the bias can be exacerbated because denominator is $Var(\Delta MW_a)$, not $Var(MW_a)$

Within-area estimates (II)

- Why might $Cov(\Delta\mu_a - \Delta\mu_{a'}, \Delta MW_a)$ be positive when close-controls don't come from areas exposed to similar shocks (i.e., cross-border counties not in same MSCZ)?
  - Close controls are similar in other ways that might influence MWs (like political orientation), so MW variation may have more to do with MW policy responses to endogenous shocks
  - Doesn't happen when shocks are the same in the close controls (like with MSCZs)
- More generally, the cross-state variation in standard panel data estimates may be more exogenous than the cross-border county variation in areas not subject to the same shocks
  - Clemens: "If these areas are so damn similar, why do they have different MW increases?"

Potential bias from spillovers

- **Negative bias ("exacerbation") if jobs move across the border in response to MW increase**
- **Positive bias ("attenuation") if, as in some search models, MW increase leads more people from across the border to search where MW increased, leading employers to open more jobs**

Simultaneously present evidence on both questions

- Distinguish cross-border county pairs
  - (1) Cross-border contiguous counties *not* in same MSCZ
  - (2) Only cross-border contiguous counties in same MSCZ
  - (3) Add non-contiguous (and non-border) counties in same MSCZ
  - (4) Only non-contiguous (and non-border) counties in same MSCZ
- Is evidence more consistent with DLR only with counties not in same MSCZ – (1) vs. (2)?
- What is estimate from MSCZ close-control approach using the most possible county pairs – (3)?
- What do we get from the most "spillover free" estimate – (4)?

TWFE estimates largely unaffected by different county samples

Table 7: TWFE estimation of minimum wage responses for different county-level samples using CBP 1990-2016 data

	All cross-border contiguous counties not in same CZs	*Only cross-border contiguous counties in same MSCZs*	*All cross-border counties (incl. non-contiguous) in same MSCZ*	*Subset of (3), excludes contiguous cross-border counties*
	(1)	(2)	(3)	(4)
A. ln(employment)				
ln(minimum wage)	-0.316***	-0.293***	-0.395***	-0.414***
	(0.112)	(0.101)	(0.120)	(0.146)
ln(employment$^-$)	0.088	0.046	0.079	0.080
	(0.054)	(0.070)	(0.053)	(0.058)
ln(population)	1.074***	1.091***	1.016***	1.011***
	(0.096)	(0.119)	(0.105)	(0.110)
B. ln(earnings)				
ln(minimum wage)	0.254***	0.181***	0.238***	0.236***
	(0.050)	(0.058)	(0.043)	(0.047)
ln(earnings$^-$)	0.184**	0.155**	0.145**	0.126***
	(0.072)	(0.076)	(0.058)	(0.042)
ln(population)	0.018	0.071	0.058	0.046
	(0.039)	(0.056)	(0.038)	(0.039)
County effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Number of counties	929	458	742	557
Observations	24,904	12,331	19,955	14,974

Answer very different for close-controls estimates

Table 8: Pair-approach estimation of minimum wage responses for different cross-border county-pair samples using CBP 1990-2016 data.

	All cross-border contiguous counties not in same CZs	*Only cross-border contiguous counties in same MSCZs*	*All cross-border counties (incl. non-contiguous) in same MSCZ*	*Subset of (3), excludes contiguous cross-border counties*
	(1)	(2)	(3)	(4)
A. ln(employment)				
ln(minimum wage)	-0.047	-0.160	-0.244*	-0.286
	(0.075)	(0.107)	(0.145)	(0.189)
ln(employment$^-$)	0.194***	0.191***	0.197***	0.201***
	(0.061)	(0.068)	(0.046)	(0.046)
ln(population)	0.982***	0.971***	0.924***	0.908***
	(0.115)	(0.143)	(0.093)	(0.102)
B. ln(earnings)				
ln(minimum wage)	0.156***	0.156**	0.221***	0.253***
	(0.056)	(0.072)	(0.062)	(0.066)
ln(earnings$^-$)	0.043	0.046	0.043	0.041
	(0.063)	(0.062)	(0.043)	(0.048)
ln(population)	0.005	0.108	0.093*	0.088
	(0.047)	(0.079)	(0.049)	(0.058)
County effects	Y	Y	Y	Y
Pair–period effects	Y	Y	Y	Y
Number of pairs	843	322	986	664
Observations	44,914	17,314	52,928	35,614

Answer very different for close-controls estimates

	All cross-border contiguous counties not in same CZs	*Only cross-border contiguous counties in same MSCZs*	*All cross-border counties (incl. non-contiguous) in same MSCZ*	*Subset of (3), excludes contiguous cross-border counties*
	(1)	(2)	(3)	(4)
A. ln(employment)				
ln(minimum wage)	-0.047	-0.160	-0.244*	-0.286
	(0.075)	(0.107)	(0.145)	(0.189)

(1) vs. (2): consistent with positive bias in non-MSCZ cross-border counties, but *not* MSCZ cross-border counties

(3): Most complete MSCZ close-controls sample

(4): Spillover free estimate slightly larger, consistent if anything with attenuation bias

Some evidence shocks are "more common" for county-border pairs *within* MSCZs

Table 6: Within-pair correlations of controls and Bartik shocks

	Contiguous county pairs				Pairs within multi-state zones			
	Δ_1	Δ_2	Δ_3	Δ_4	Δ_1	Δ_2	Δ_3	Δ_4
A. DLR's Quarterly Data (1990-2006)								
Priv. employment	0.26	0.34	0.35	0.31	0.28	0.43	0.45	0.44
Population	0.38	0.39	0.39	0.40	0.50	0.52	0.54	0.54
B. CBP Yearly Data (1990-2016) — All Complete Pairs								
Employment^{-}	0.10	0.15	0.17	0.21	0.19	0.30	0.31	0.34
Population	0.35	0.42	0.45	0.45	0.52	0.58	0.61	0.63
Bartik shock	0.72	0.76	0.72	0.74	0.83	0.84	0.82	0.83

Within-pair correlations higher by > 30% on average for county pairs within MSCZs.

Finally, evidence from DLR test for bias from estimating model with "pre-trends"

- Estimate models with adding three leads as well as contemporaneous effect (we use years, they used quarters)
- Test for "effect" of leads
  - Some negative lead is consistent with anticipation effects, but a positive lead cannot be rationalized this way and is more indicative of positive bias from differencing

Answer very different for close-controls estimates

Table 9: Testing for restaurant employment pre-trends for different samples

	All cross-border contiguous counties not in same CZs	*Only cross-border contiguous counties in same MSCZs*	*All cross-border counties (incl. non-contiguous) in same MSCZ*	*Subset of (3), excludes contiguous cross-border counties*
	(1)	(2)	(3)	(4)
B. With pair-period effects				
$\hat{\beta}_3$	0.048	-0.036	-0.024	-0.022
	(0.073)	(0.081)	(0.074)	(0.087)
$\hat{\beta}_1$	0.162*	-0.029	-0.169	-0.244
	(0.089)	(0.120)	(0.143)	(0.187)
$\hat{\beta}_0$	0.011	-0.169	-0.420*	-0.552*
	(0.100)	(0.160)	(0.214)	(0.285)
Trend ($\hat{\beta}_1 - \hat{\beta}_3$)	0.114**	0.007	-0.145	-0.222
	(0.053)	(0.068)	(0.104)	(0.143)
County effects	Y	Y	Y	Y
Pair-period effects	Y	Y	Y	Y
Number of pairs	841	322	986	664
Observations	39,462	15,304	46,680	31,376

“Pre-trend” always negative for TWFE estimates (not shown).

But *positive and significant* for cross-border county pairs *not* in same MSCZ.

DLR got the wrong answer, and we think we know why

- Implementing DLR estimator using cross-border counties in same MSCZ reverse their findings: evidence of disemployment effects is strong
- Findings from pre-trends analysis (and other evidence) consistent with positive bias in DLR's analysis from using cross-border counties that aren't in same MSCZ
- <u>Core conclusions come from exact same approaches and tests for bias that DLR advocate</u>
  - … <u>and geographic controls in the close-controls approach that some of the same co-authors advocated in other work done concurrently or earlier</u>

Combined evidence reinforces conclusions that higher minimum wages cost jobs

- From looking at extensive literature, this conclusion is supported
- One can only reach opposite conclusion by leaning heavily on close-controls studies (and discarding the others)
- But re-evaluation of core study using this method turns out to support the same conclusion that higher minimum wages reduce employment

Implications for policy and research

- Does <u>not</u> mean that higher MW is a bad policy or doesn't deliver benefits on net, but <u>does</u> mean we need to consider tradeoffs, and get more evidence on costs and benefits and their incidence
  - I think this recent work should shift the debate and research toward these latter questions
  - Brings more to the fore comparisons with other policies
- Growing literature on effects of MWs on distribution of income – still unresolved but I see little clear evidence of, e.g., poverty reductions
- We do little work that compares effects of different policies
  - Usually limited to comparisons of evaluations of single policies from different studies that aren't fully comparable

Don’t shy away from conflict (or at least conflicting evidence)

- Papers illustrate that we can make progress in sorting out conflicting research findings
- Can be daunting, especially for young scholars
  - Might be “safer” to say “some studies find X, others find Y, I find Z (or X, or Y)”
  - They can’t all be right
- Policymakers need these answers!