

# RIETI BBL Seminar Handout

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**“ What Can We Conclude from the Evidence  
on Minimum Wages and Employment ?  
- Recent Progress ”**

**November 9, 2023  
David Neumark**

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

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**David Neumark**

# Question of the employment effects of minimum wages appears 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)

# **Dispute persists in the research literature, in two different ways**

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- **Conflict over the best or most compelling way to identify MW-employment effects**
- **Conflict over what overall literature concludes**

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

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- 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

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- “...[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, economists can't even agree on what we disagree about

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- 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

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- 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)
  - “... 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

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- 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)
  - “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)

# A puzzling disagreement

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- 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 summaries of what these studies show

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

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- 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?”**

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- **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**

# Database of studies

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- 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

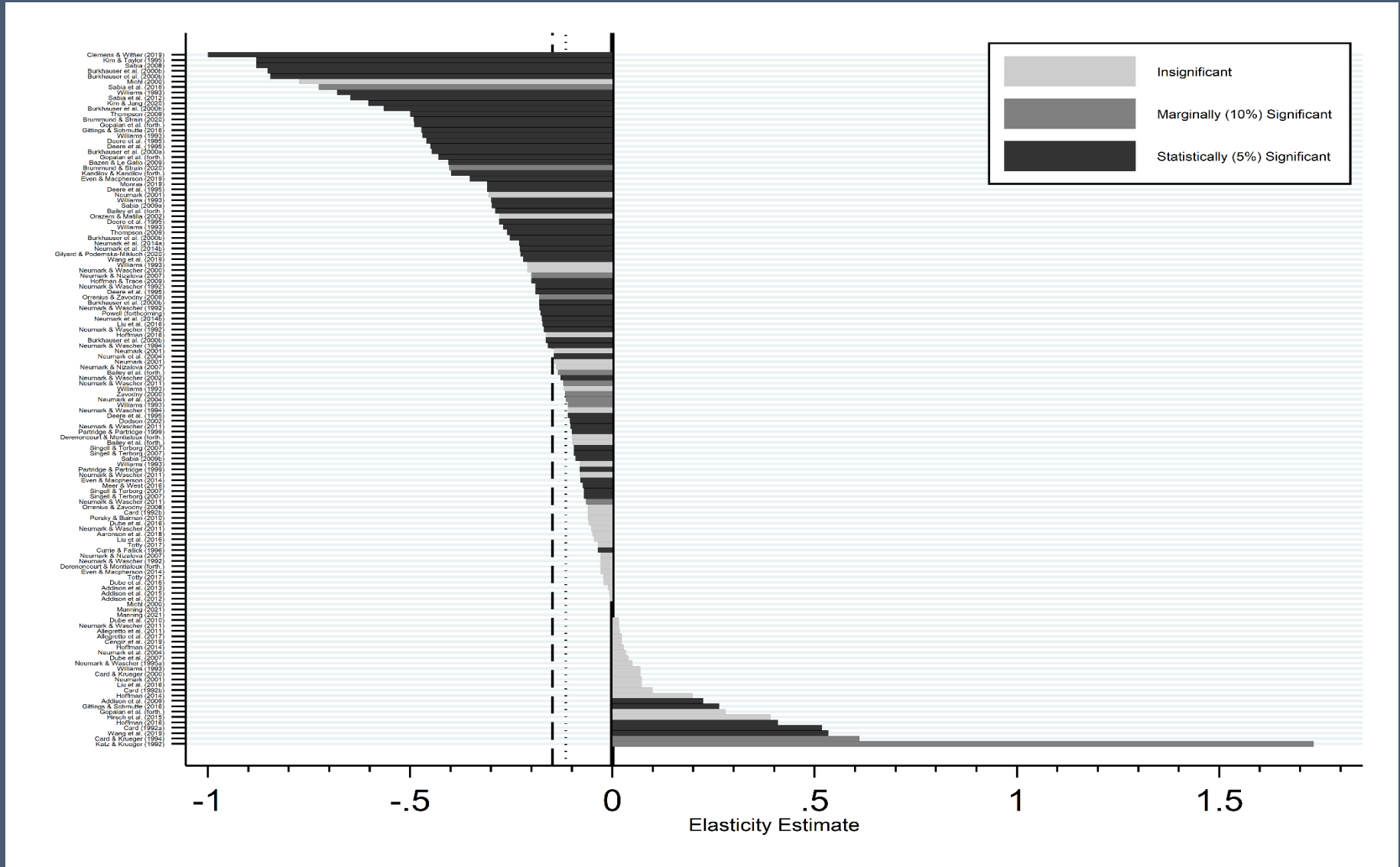
# Focused on authors' conclusions

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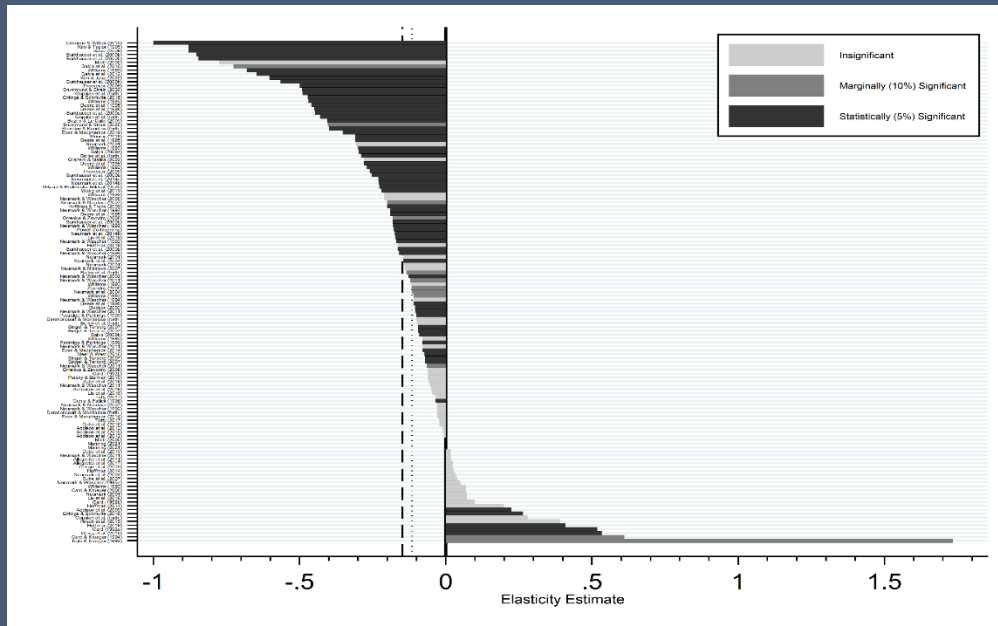
- Entire set of estimates likely to fail to convey conclusions of paper
  - E.g., authors often report estimates they don't find as credible before reporting their preferred estimates
- Create database of each study's "preferred estimates," which could number more than one (e.g., teens and young adults)
  - In order of priority:
    - 1. Summary statements in conclusions
    - 2. Descriptions of results in tables
- Supplemented by survey of authors
  - High response rate
  - Compared our coding and survey responses – no bias one way or the other, so we use our coded responses for the non-responses

# All preferred estimates

“a picture is worth > 1000 words”



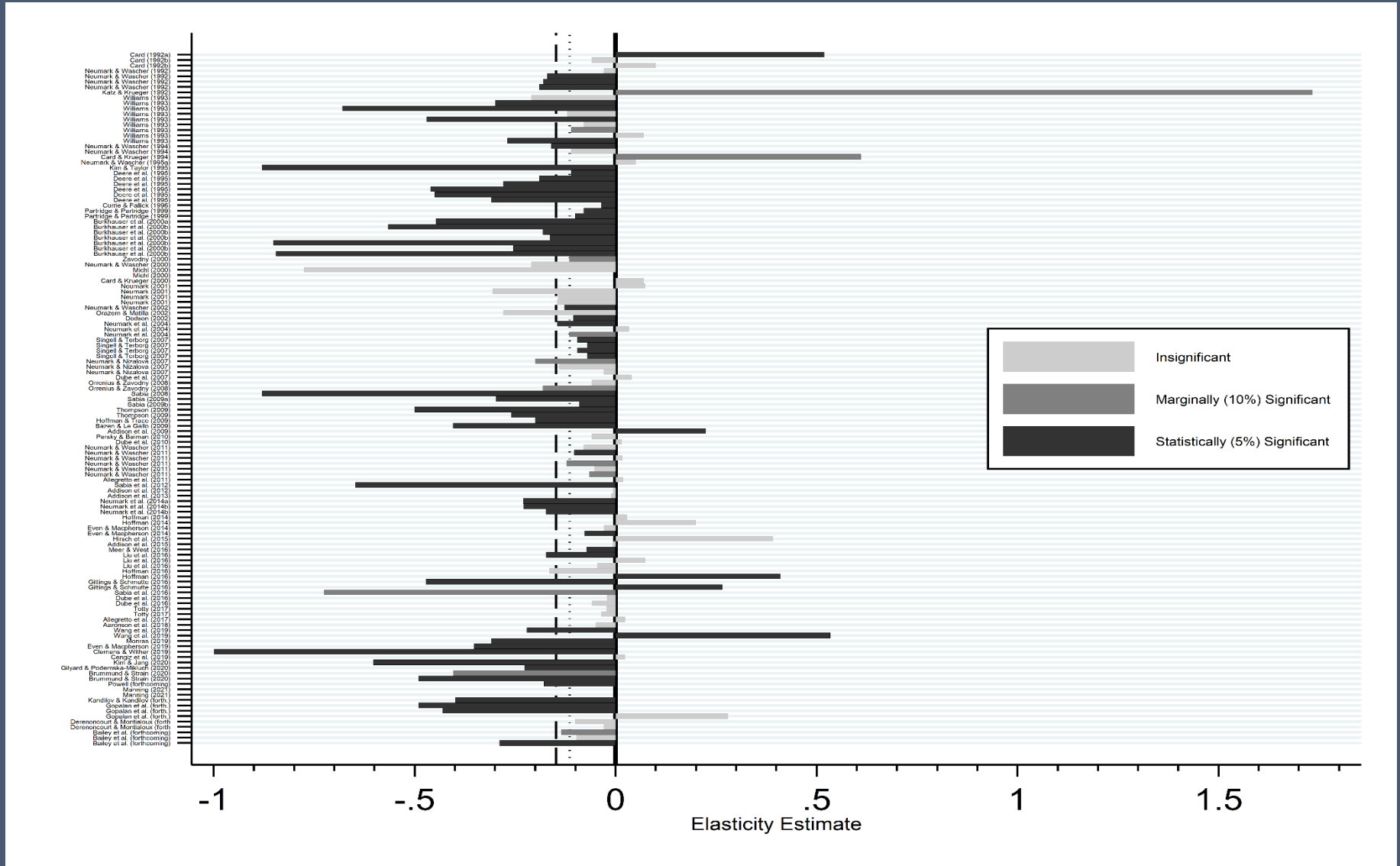
# All preferred estimates



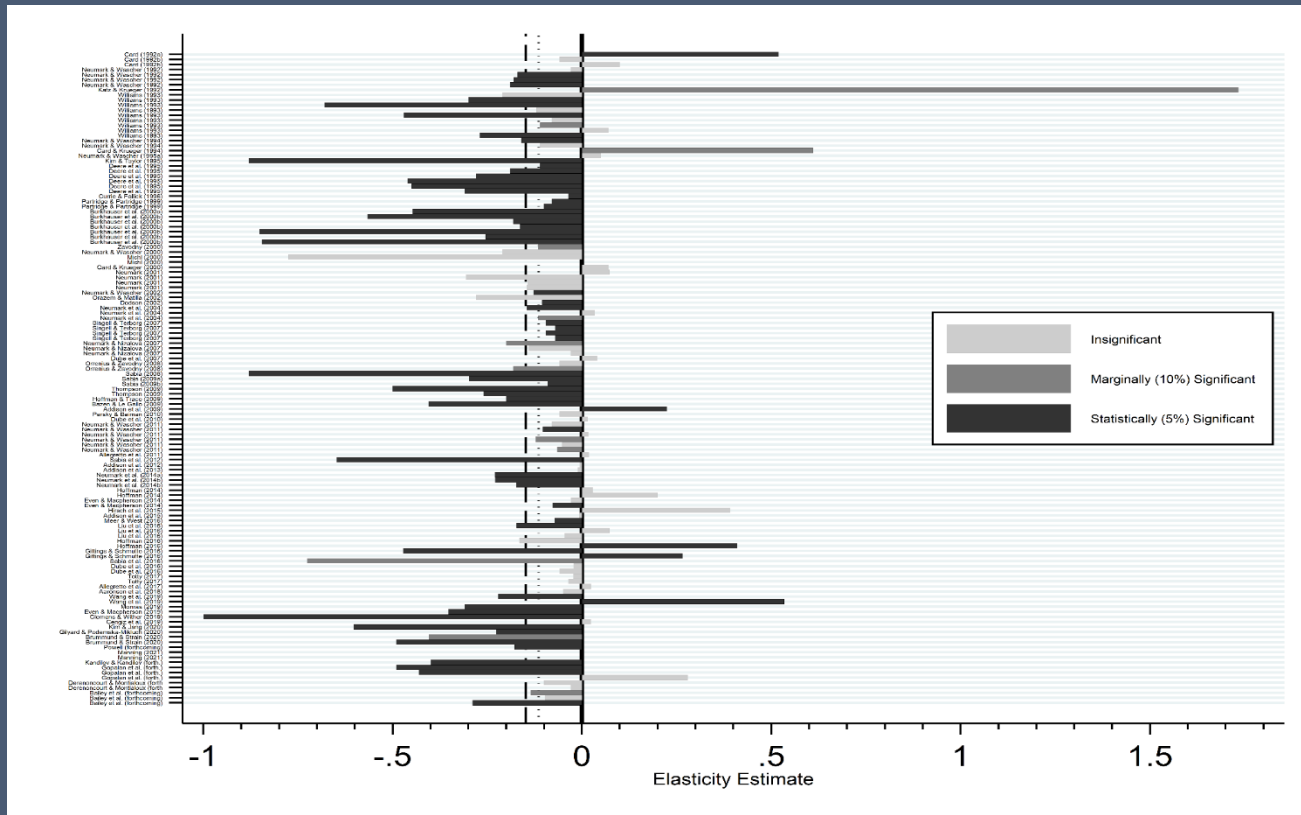
- Mean elas:  $-0.148$ ; median elas.:  $-0.115$
- 79.1% negative; 54.3% negative with  $p < .1$ ; 46.5% negative with  $p < .05$
- Of the large positive estimates, weaker statistical significance (so these estimates are less informative)
- Simplistic binomial treatment for null (true  $P(\text{neg. effect}) = .5$ ): z-stat = 8.01
- Clearly at odds with conclusion that there is no evidence of negative effects, or negative vs. positive effects equally likely



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



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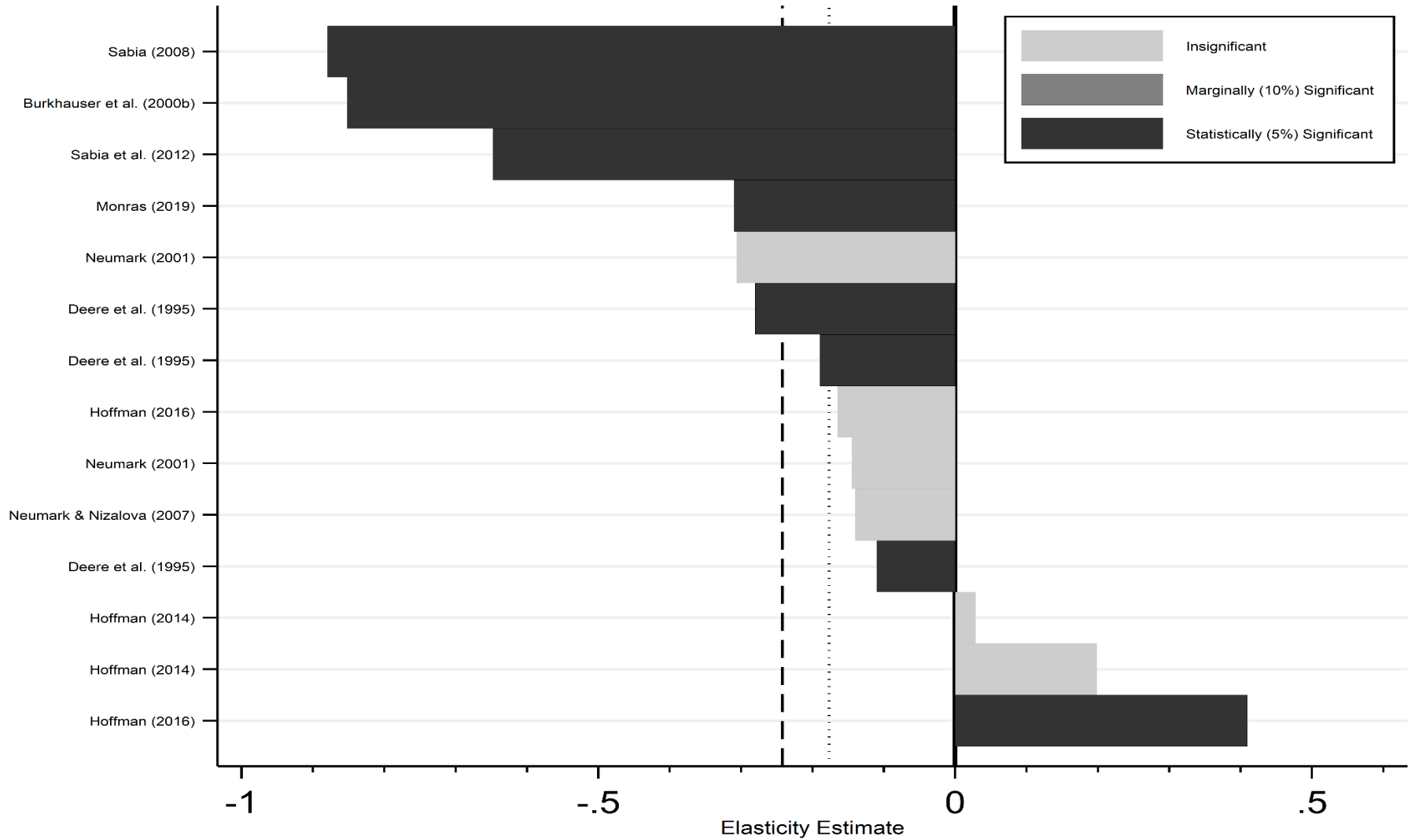
- Regression of elasticity on publication year: coef. (p-value) = -.001 (.774)
- Largest positive estimates are from three of the earliest studies in the NMWR (Katz and Krueger, *ILRR*, 1992; Card, *ILRR*, 1992; Card and Krueger, *AER*, 1994)

# Variation by type of worker consistent with expectations of competitive model

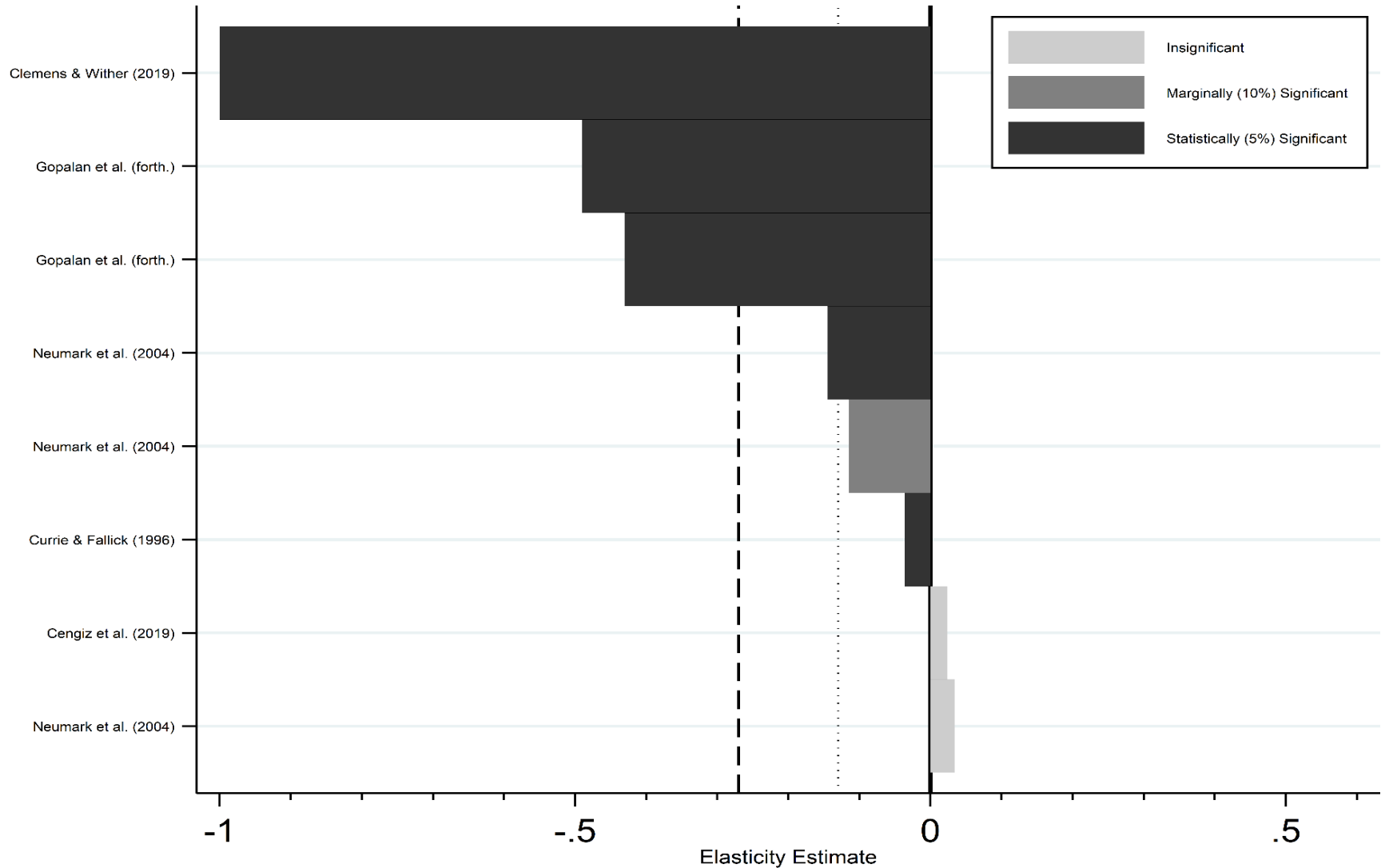
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- **Effects similar for teens and young adults**
  - Mean elas:  $-.17$  to  $-.19$ ; median elas.:  $-.12$  to  $-.16$
  - 80-83% negative; 57-58% negative with  $p < .1$ ; 42-46% negative with  $p < .05$
- **Effects more strongly negative for less-educated, and more so for those directly affected (based on low wages)**

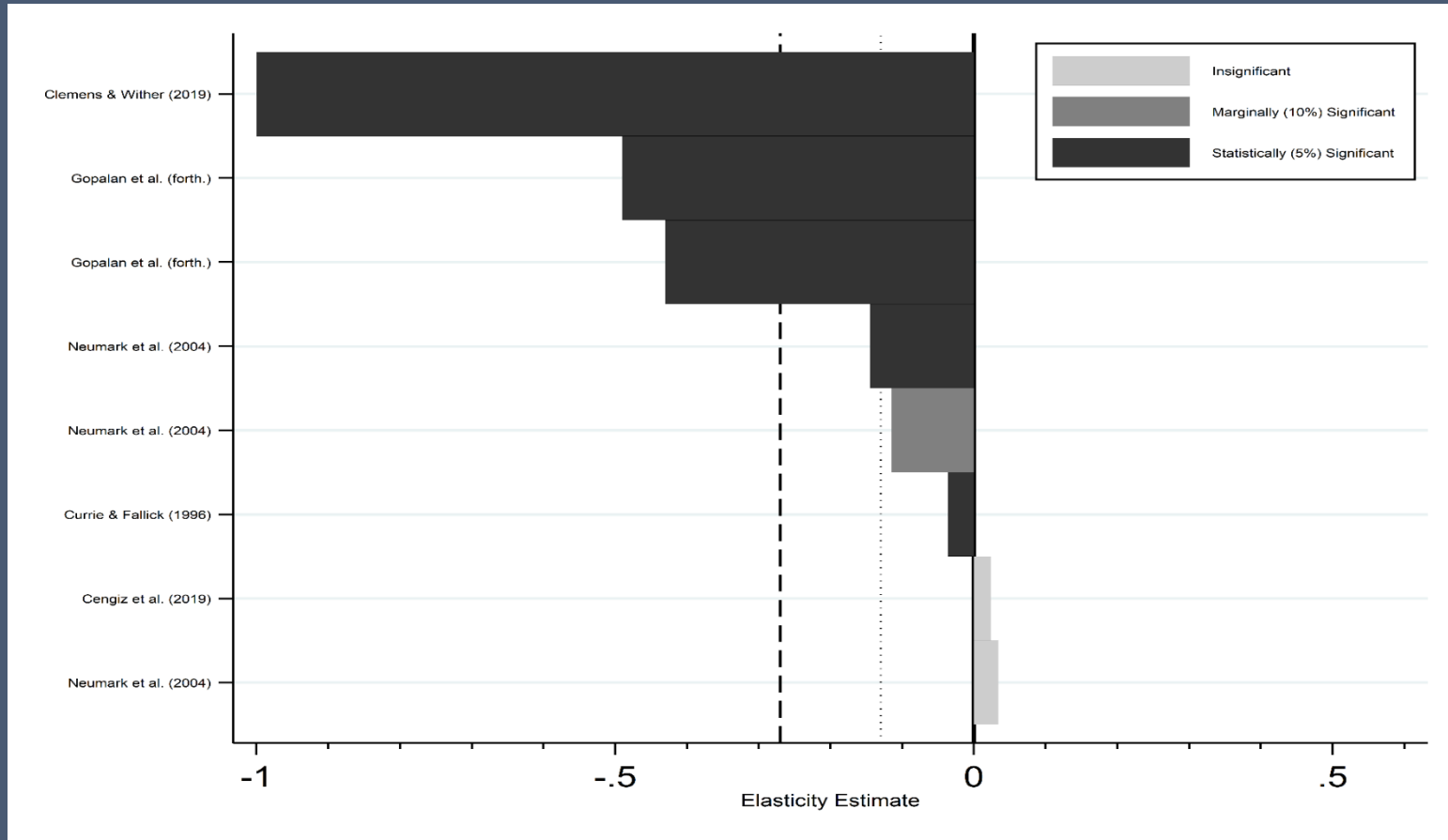
# Less-educated



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

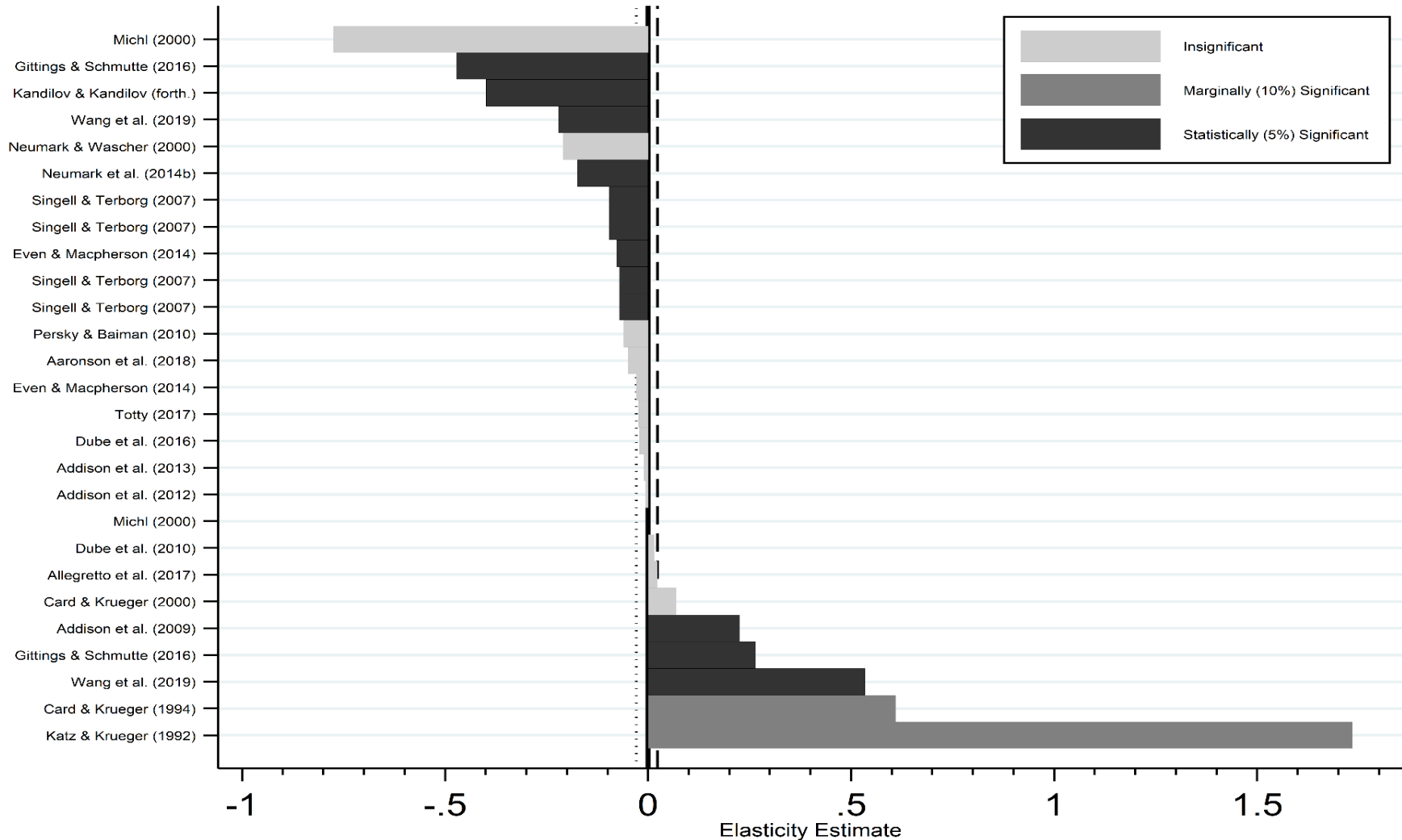


# Not as many studies, but job loss looks worst for directly affected



- 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

# Effects much closer to zero for low-wage industries



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

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- 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?”)



# Evidence shows that minimum wages reduce employment—unless you discard a lot of it

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- *“... 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)**

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- **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**

# Two-way fixed effects models were the norm in earlier “new minimum wage research”

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- Standard two-way FE model (state-by-year obs.) identifies the effect of the minimum wage on employment by comparing how employment of low-skill workers changes in states that raised their minimum, relative to how it changed in states that didn't raise their minimum
  - “Panel data” or “difference-in-differences” or “two-way fixed effects” estimator
- 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

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- Dube et al. (2010, DLR) and Allegretto et al. (2011, ADR) critique/solution
- Approach based on “random assignment” paradigm, but not actually true of policy: policymaker choices about changing the MW may be correlated with economic conditions
- Assertion (important – not a *fact*) that using variation only for very nearby areas solve the problem
- Motivates the “cross-border” research design in DLR, identifying MW effect from changes in low-skill employment in counties on opposite sides of state borders, when MW changes on one side but not the other

# “Close-controls” approaches generally find no disemployment effect

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

# Critique has led to 3 other approaches to isolate effects of MW from potentially correlated shocks

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- **“Generalized synthetic control”**: data driven (synthetic control) approach, using all MW increases and their continuous variation (Powell, 2022)
- **IV based on interaction between federal MW and historical probability that federal MW binds in state** (Baskaya and Rubinstein, 2015)
- **Within-state variation to allow for state x year shock**
  - **Thompson (2009)**: effect of federal increases in counties with high share affected
  - **Clemens and Wither (2016)**: compare very low-wage (affected) and low-wage/not affected workers

# Alternative approaches generally find strong evidence of disemployment effects

Authors	Employment elasticity and groups studied	Data/approach
Other approaches		
Powell (2022)	-0.18 (teens)	Generalized SC
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
Baskaya and Rubinstein (2015)	-0.3 to -0.5 for teens	States, using federally-induced variation as instrumental variable

# Different methods, different results

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- Key studies using close controls generally find no evidence of disemployment effect
- Other strategies (SC, IV, DDD) 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 how
- Raises question: Do the studies using close controls get the wrong answer? If so, why?



# Are the “close controls” good controls?

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- One clue: Liu et al. (2016) use BEA Economic Areas
  - Regions that are supposed to have 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 (or especially if they aren't), bad controls for capturing common shocks?

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

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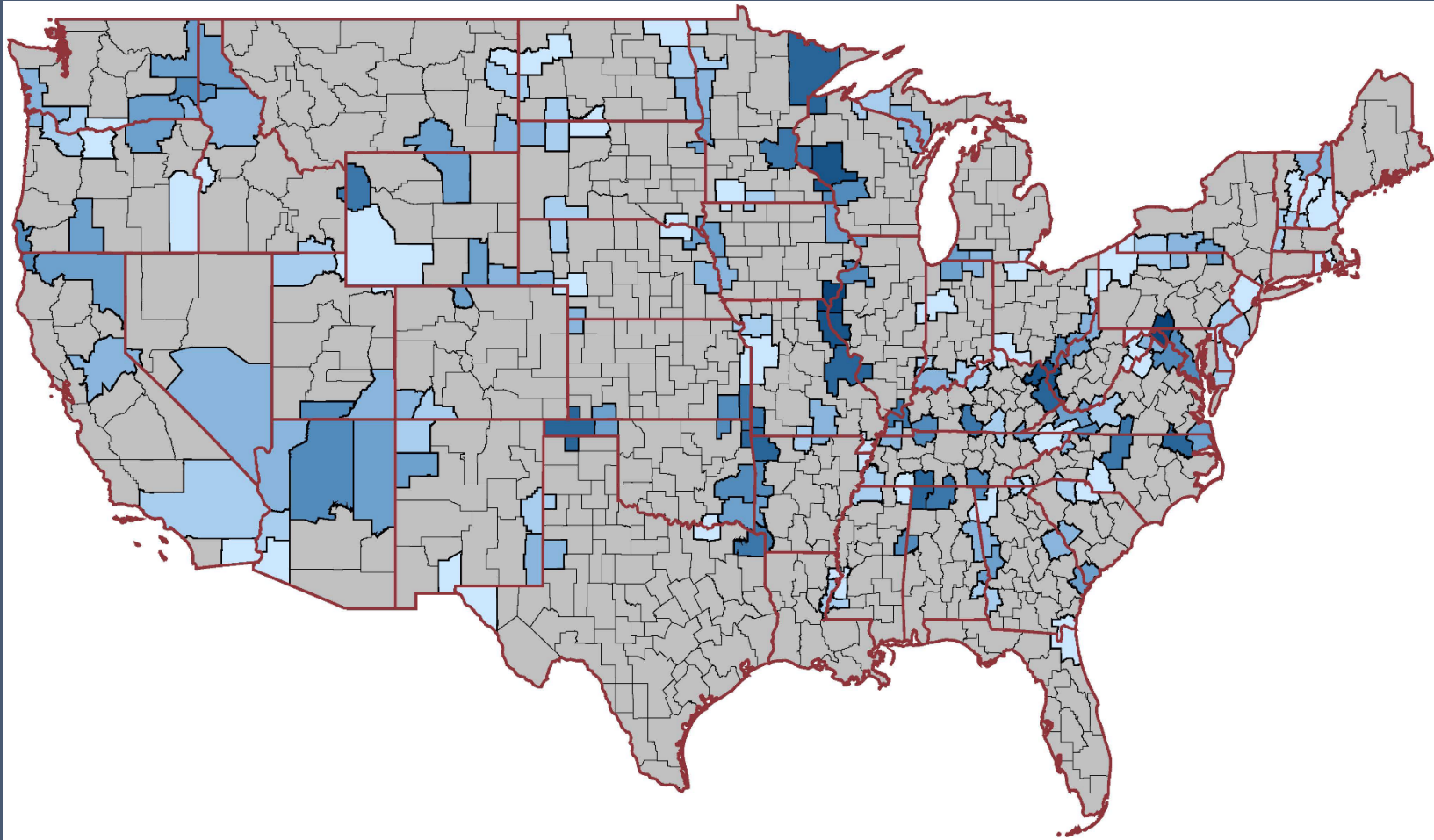
- IRLE working paper by Allegretto, Dube, and Reich (2009), studying teen employment, uses cross-border counties in commuting zones (based on Census's journey-to-work data)
  - Using commuting zones “is appealing because these areas are not only contiguous; they are also demonstrably linked with each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

# Re-evaluation of DLR results

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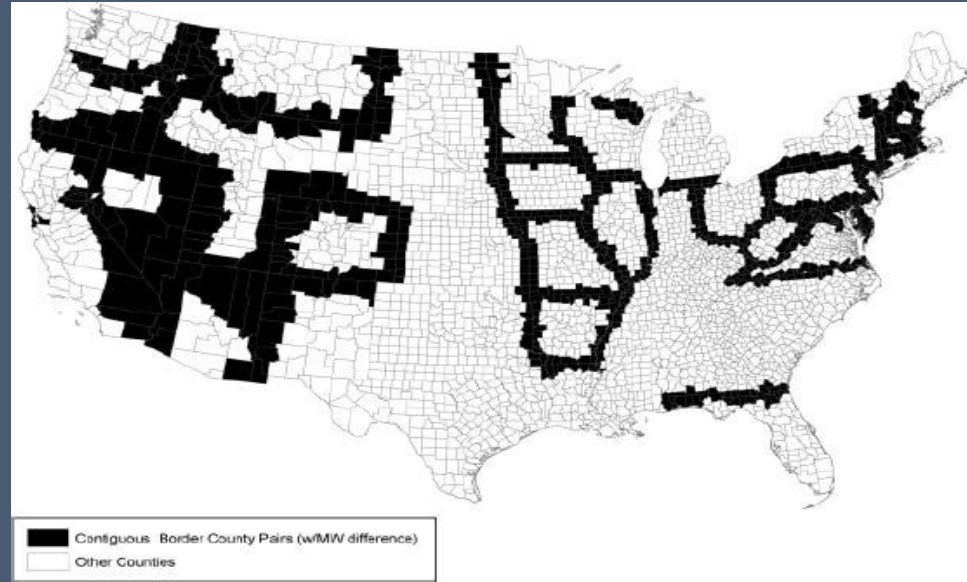
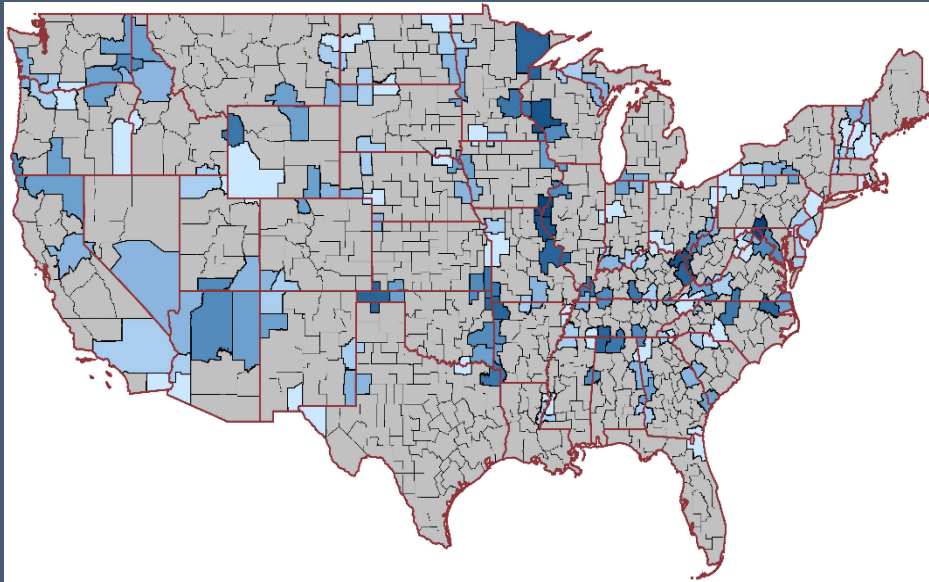
- Use cross-border areas of commuting zones in multi-state commuting zones (MSCZs)
- Like them, use restaurant employment
  - Start with QCEW data (as in DLR), but then turn to CBP data (Autor et al. 2013 & 2016), which has much better coverage because QCEW data is suppressed for many counties (nearly 100% of counties vs. 27%)
    - Also add 10 more years of data (doesn't drive results)

# MSCZ map: geographically dispersed



Note: All counties assigned to CZ; subset are multi-state.

# MSCZ map vs. DLR cross-border county pairs in QCEW data



- Counties in MSCZs have nearly twice the population of counties in cross-border pairs that are not in MSCZs (more urban).
- 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 lots of cross-border county pairs that aren't in the same commuting zone.

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

Estimated MW-employment elasticities	Cross-border design		Cross-border design	
	Two-way FE	Two-way FE	Two-way FE	Two-way FE
	QCEW (1990-2006)	QCEW (1990-2006)	CBP (1990-2016)	CBP (1990-2016)
DLR contiguous border county-pair sample	-.112 (.079)	.016 (.076)	...	...
<b>MSCZ-pair sample</b>	<b>-.186*** (.072)</b>	<b>-.141** (.070)</b>	<b>-.299** (.101)</b>	<b>-.242*** (.120)</b>

# Where are we so far?

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- 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, nearly as large or larger than TWFE estimate
- Why?

# Within-area estimates – bias can be amplified, not reduced (I)

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- There is a simple intuition underlying the close-control approach
  - By comparing very similar areas, economic shocks must be similar
  - So MW changes *can't* be correlated with economic shocks, and bias must be reduced or eliminated
- But the intuition could be wrong, for two reasons



# Within-area estimates – bias can be amplified, not reduced (II)

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- Economic shocks may be more similar across a border, but the MW variation is also smaller (states near each other tend to have closer MWs)
  - So the shocks can be *larger* relative to the MW increases
- The correlation between MWs and shocks can be stronger, because economic conditions, more than other factors (e.g., politics, unions) may be more important in explaining MW variation between close states
  - “If the states are so similar, why do they have different MW changes?”

# Evidence complex, but demonstrates this is the problem with DLR

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- When we look at cross-border counties in the same CZ, the “close-controls” estimates are negative, similar to the standard estimates
  - *This* close-controls approach, based on controls that actually experience the same shock, indicates that negative employment effects are *not* biased
- When we look at cross-border counties *not* in the same CZ, we find two things:
  - The close-controls estimator moves to zero
  - MW increases are associated with *positive* economic shocks, which will generate positive bias (i.e., the estimate moving towards zero)

# Answer very different for close-controls estimates

Estimated MW-employment elasticities	All cross-border contiguous counties <b>not in</b> same CZs	<b>Only</b> cross-border contiguous counties <b>in</b> same CZs	<b>All</b> cross-border contiguous counties <b>in</b> same CZs
CBP (1990-2016)	(1)	(2)	(3)
Two-way FE	-.316*** (.112)	-.293*** (.101)	-.395*** (.1205)
Cross-border control design	-.047 (.075)	-.160 (.107)	-.244* (.145)

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

**(3): Most complete MS CZ close-controls sample**

# Positive prior shock in non-CZ counties across border that raise MW

Leading estimated MW-employment elasticities	All cross-border contiguous counties <b>not in</b> same CZs	<b>Only</b> cross-border contiguous counties <b>in</b> same CZs	<b>All</b> cross-border contiguous counties <b>in</b> same CZs
CBP (1990-2016)	(1)	(2)	(3)
Yr. - 3	.048 (.073)	-.036 (.081)	-.024 (.074)
Yr. - 1	.162* (.089)	-.029 (.120)	-.169 (.143)
Yr. 0	.011 (.100)	-.169 (.160)	-.420* (.214)
Yr. 3 - Yr. 1	<b>.114**</b> <b>(.053)</b>	<b>.007</b> <b>(.068)</b>	<b>-.145</b> <b>(.104)</b>

# Summary of results from re-evaluation

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- **Implementing DLR estimator using cross-border counties in same MSCZ reverses their findings: evidence of disemployment effects is strong**
- **Evidence of positive bias in DLR's analysis from using cross-border counties that aren't in same MSCZ**
- **Core conclusions come from exact same approaches and tests for bias that DLR advocate**
  - ... and geographic controls in the close-controls approach that some of the same co-authors advocated in other work done concurrently or earlier

# Combined evidence reinforces conclusions that higher minimum wages cost jobs

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- 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 many other studies)
- Could be the right approach if close-controls study right and other wrong
- 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

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- Conclusion that higher minimum wage reduces employment does not mean that higher MW is a bad policy or doesn't deliver benefits on net
- It does mean we need to consider tradeoffs, and get more evidence on costs and benefits and their incidence
- This (and lots of other) recent work should shift the debate and research toward these latter questions
  - Effects of MWs on distribution of income – still unresolved but I see little clear evidence of poverty reductions
  - Bring to the fore comparisons with other policies
    - Benefits, but also who pays

# Thank you!

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