What's Across the Border? Re-Evaluating the Cross-Border Evidence on Minimum Wage Effects*

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Abstract

Dube, Lester and Reich (2010) argue that state-level minimum wage variation correlated with economic shocks generates spurious evidence that higher minimum wages reduce employment. Using minimum wage variation within contiguous county pairs that share a state border, they find no relationship between minimum wages and employment in the U.S. restaurant industry. We show that this result is overturned if we use instead multi-state commuting zones, which provide superior definitions of local economic areas. Using the same within-local area research design—but within cross-border commuting zones—we find a robust negative relationship between minimum wages and employment. We demonstrate that a positive bias emerges in the county-pair specification when using pairs formed by counties from different commuting zones, hence explaining the contrasting results between county-pair and cross-border commuting zone analyses.

JEL Classification: J23, J38

Keywords: minimum wage, employment, commuting zones

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1 Introduction

In an influential paper on the employment effects of the minimum wage in the United States, Dube, Lester and Reich (2010)—DLR hereafter—find a small, non-negative, and insignificant relationship between minimum wages and employment in the U.S. restaurant industry. The core evidence in DLR is that the negative relationship between employment and minimum wages in that industry disappears—flipping to a positive, small, and not significant estimate—when using a specification that is intended to control for time-varying local economic conditions by using minimum wage variation within cross-border county pairs.

DLR's study is cited frequently (1,645 Google scholar cites as of November 15, 2023). More significantly, it is often cited in both policy debate and research summaries as one of the key papers overturning the prediction from the competitive labor market model—and from a great deal of evidence (see, for example, Neumark and Wascher, 2008)—that a higher minimum wage reduces employment among lower-skilled workers.[1]

In short, the key contention of DLR is that one needs local controls to credibly identify the employment effects of minimum wages. Their general strategy is to study minimum wage variation *within* local economic areas to control for economic shocks that may be correlated with minimum wages. DLR define these local economic areas as contiguous county pairs sharing a state border. They estimate minimum wage effects using variation within cross-border county pairs, and doing this changes the answer from a negative employment effect near -0.2 (an elasticity) to close to zero.

In this paper, however, we show that DLR's conclusion relies critically on defining the local

[1] For example, in a recent survey, Alan Manning mentions: "Dube, Lester, and Reich (2010) argue that a better way to control for other economic conditions is to use counties that border each other but are in different states and sometimes have different minimum wages as a result (an approach first used in Card and Krueger 1994). They find clear evidence of wage effects from the minimum wage but not evidence of disemployment effects" (Manning, 2021, p. 12). Michael Reich, one of the authors of DLR, testified before the U.S. House of Representatives that: "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 do provide a credible guide to the likely employment effects of a $15 floor. They indicate that the Act will have minimal to no adverse effects on employment and that they will have substantial positive dynamic effects on the lowest-wage areas of the U.S." (IRLE link). And Dube, in dismissing the results of more conventional state-by-year panel data estimates, and citing DLR, writes: "a 'fourth generation' of ... papers ... have shown that the variation over the past two decades in minimum wages has been highly selective spatially, and employment trends for low-wage workers vary substantially across states..." and that "approaches such as comparing contiguous counties across policy boundaries ... produce employment effects close to zero" (Dube, 2011, p. 764). Section B in the Appendix includes more quotes from these authors that place DLR as the source of claims that newer, more credible evidence shows that minimum wages do not reduce employment.

Prominent labor economists have described DLR as very convincing. For example, David Autor says that "The paper presents a fairly irrefutable case that state minimum wage laws do raise earnings in low wage jobs but do not reduce employment to any meaningful degree," and, similarly, Lawrence Katz says that "This is one of the best and most convincing minimum wage papers in recent years" (National Employment Law Project, 2010).

economic areas used to capture spatial economic shocks as pairs of contiguous counties across state lines. If local economic areas are defined instead as commuting zones, which is far more natural—but otherwise we follow DLR's approach, using as identifying information minimum wage variation within multi-state commuting zones—we find a negative and significant employment elasticity.[2]

There is compelling reason to believe that commuting zones are better able to net out other economic shocks and hence to capture employment variation driven by cross-state minimum wage differentials than are any or all contiguous county pairs sharing a state border. As described by Tolbert and Sizer (1996), commuting zones are defined as "groups of counties with strong commuting ties" based on Census's *journey-to-work* data, and specifically, "commuting zones are intended for use as spatial measures of local labor markets." This is not necessarily the case for county pairs: even if they are contiguous, two isolated U.S. counties may share little or no commuting and economic activity.[3] Hence, DLR's main finding follows from their contiguous-county-pair approach not meeting a definition of "local economic area" that effectively captures common local shocks.

Indeed, in another (unpublished) paper written at about the same time as DLR, two of the three authors of DLR made the same argument. There, Allegretto, Dube and Reich (2009) write "But while DLR uses county pairs straddling state borders we use a more economically-motivated definition of local labor markets: commuting zones."[4] They also mention that 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."[5]

[2]Each county in the U.S. maps to only one commuting zone. Approximately 52 percent of state-border counties do not belong to a multi-state commuting zone. (A multi-state commuting zone may also include non-border counties.)

[3]For example, there are ten county pairs between Colorado and Kansas in DLR's list, even though there is not any defined commuting zone between them. By definition, commuting ties between two counties in different commuting zones are weak (Tolbert and Sizer, 1996).

[4]Allegretto, Dube and Reich (2009) analyze teen employment in the U.S. by exploiting minimum wage variation within multi-state commuting zones and find no disemployment effects. Importantly, their close-controls multi-state CZ specifications only use four years of data (1990 and 2000 from the Decennial Census, and 2005 and 2006 from the American Community Survey) and these are individual-level specifications where the dependent variable in their employment regression is whether a teen is working or not. There is not a direct comparison, thus, between their estimates, and those in this paper or DLR. Allegretto et al. (2009) note this explicitly: "the elasticities are not directly comparable, since DLR focuses on jobs, while we focus on individuals" (p. 34). Having mentioned this, estimates of minimum-wage employment effects for different groups of workers do tend to differ. As Neumark and Shirley (2022) show, estimates for low-skilled groups (like teenagers) tend to produce elasticities around -0.15, while estimates for low-wage industries—although there are far fewer—are clustered nearer to zero. One might rationalize this by greater possibilities for labor-labor substitution within an industry, since even low-wage industries also have higher-wage/higher-skilled workers. As evidence that labor-labor substitution could matter, Neumark and Shirley show that estimates for directly-affected workers (where there is really no possibility of labor-labor substitution) are closer to -1. The bottom line, though, is that there is no reason to necessarily expect estimates for teens and restaurant workers to be similar. However, as our results show, the leading evidence for restaurant workers—DLR—mistakenly gets elasticities near zero, rather than in the -0.2 range.

[5]For much the same reason, Liu, Hyclak and Regmi (2016) estimate minimum wage effects using Bureau of Economic Analysis Economic Areas—which are micropolitan or metropolitan statistical areas and the surrounding

Our empirical approach proceeds as follows. In section 2 we use DLR's replication package (1990-2006 quarterly data and programs) and re-estimate their contiguous-pair specifications, and compare results to those obtained using pairs from multi-state commuting zones. We find that the estimated minimum wage elasticity changes from a non-significant value of 0.016 in DLR, to a significant value of -0.141 when using pairs from multi-state commuting zones, which is very similar to the estimate without the time-varying spatial heterogeneity controls. *This is the crux of our paper: a simple alteration in the definition of local areas to commuting zones, using DLR's own data, overturns DLR's widely-touted result.*

We then validate the robustness of this result by using more complete data. Additionally, we examine the long-term minimum wage elasticity and disentangle the sources of discrepancies between cross-border county-pair and multi-state commuting zone specifications.

First, in section 3 we estimate specifications similar to DLR but using yearly U.S. County Business Patterns (CBP) data at both the county and commuting zone-by-state levels for the 1990-2016 period. This dataset, and extension of the CBP commuting-zone datasets of Autor, Dorn and Hanson (2013) and Acemoglu, Autor, Dorn, Hanson and Price (2016), has far superior geographical coverage than DLR's dataset and spans a longer period. Our CBP data yield a non-significant elasticity of -0.081 when using contiguous-county pairs that share a state border, whereas they yield a significant elasticity of -0.242 when using pairs from multi-state commuting zones. This latter result remains robust to changes in the end-year of the sample, the exclusion of data before 1993 (as suggested by Cengiz, Dube, Lindner and Zipperer, 2019), and weighting by employment or population.

Second, following DLR, in section 4 we explore the long-term effects of minimum wages and the possibility of pre-existing trends affecting our specifications. While the results using county-pairs show a non-significant long-term effect of minimum wages, echoing the findings in DLR, using pairs from multi-state commuting zones we find large and significant negative effects of minimum wages in the medium and long terms, with an estimated cumulative minimum wage elasticity of -0.689 after four years. Additionally, we perform an event study to assess the robustness of our long-term results, and find that the negative effect of minimum wages on restaurant employment only levels off after six years. We also present other evidence indicating that commuting zones provide better controls for local shocks.

counties that are economically related—to control for spatial heterogeneity. On the use of metropolitan statistical areas or commuting zones, Allegretto et al. (2009) prefer commuting zones because these are "defined for all counties in the U.S., not just metro or urban counties," which then allows for "a fuller range of local variation than is possible with MSA-based units."

Finally, in an attempt to understand what drives the different results in contiguous county pairs compared to pairs from multi-state commuting zones, in section 5 we show that if we use all possible cross-border county pairs—contiguous and non-contiguous—from multi-state commuting zones, we obtain a significant estimate of -0.244 for the minimum wage elasticity of employment, which is very close to our main estimate of -0.242. In contrast, using only pairs of contiguous counties that belong to different commuting zones, we obtain a small and non-significant estimate of -0.047. Explaining the latter finding, we document the presence of a positive bias when relying on cross-border county pairs not in the same commuting zone. This indicates that counties across the border, but not in the same commuting zone, do not provide good controls for common shocks potentially correlated with minimum wage changes; just because two areas are "close" does not imply that the shocks are common.

2 Re-Analysis of the Dube, Lester and Reich (2010) Approach

Using DLR's data and programs (Dube, Lester and Reich, 2011), this section focuses on the replication and re-analysis of DLR's preferred specification (their specification 6), which is given by

$$\ln e_{ipt} = \alpha + \beta \ln MW_{it} + \zeta Z_{it} + \eta_i + \tau_{pt} + \nu_{ipt}, \tag{1}$$

where for entity i from pair p in period t, e_{ipt} denotes employment, MW_{it} is the minimum wage, Z_{it} is a vector of time-variant geographic entity level controls (which include population and total private sector employment), η_i is an entity i fixed effect, τ_{pt} denotes pair-time fixed effects, which are intended to control for spatial heterogeneity at the local level, and ν_{ipt} is the error term.[6] We also show results for DLR's specification 5, which is a restricted version of (1) with $\tau_{pt} = \tau_t$ for every pair p—the more standard two-way fixed effects model that DLR claim gives spurious evidence of negative employment effects. In DLR an entity is a county that shares a state border, whereas we look at multi-state commuting zones, with an entity defined as a commuting zone-state (*i.e.*, the part of a multi-state commuting zone that is in a single state).

DLR use quarterly data from the Quarterly Census of Employment and Wages (QCEW) from 1990 to 2006. They identify 1,139 counties sharing a state border and 1,181 border-county pairs.[7] Of these, DLR only use counties that report information for the restaurant industry for the 66 quarters in their 1990-2006 sample period (due to confidentiality practices, the QCEW suppresses information for many counties), which restricts the sample to 504 border counties. These 504

[6] DLR's replication package is available at this link.

[7] These include three pairs from California that involve the County of San Francisco, which imposed a different minimum wage than neighboring California counties.

border counties appear in 754 county pairs, but only 316 pairs are "complete" in the sense that there is information for both counties in the pair (*i.e.*, 438 of the 754 pairs are "incomplete" because they have information for only one county). From these 754 pairs, DLR build a sample—which they refer to as the contiguous border county-pair (CBCP) sample—with 1,070 observations each period ($1,070 \times 66 = 70,620$ observations in total), composed of 438 observations in each period from incomplete pairs, and $316 \times 2 = 632$ observations in each period from complete pairs. Given that a county can appear in multiple contiguous pairs, there are many repeated observations in the CBCP sample, with some counties appearing up to seven times each period.[8]

We create a similar dataset with multi-state commuting zones. From Tolbert and Sizer (1996), there are 137 multi-state commuting zones in the U.S. (out of 741 total commuting zones): 129 two-state zones and 8 three-state zones. These 137 zones yield 282 commuting zone-state entities and 153 pairs.[9] All counties are assigned to a commuting zone, but as these numbers indicate, the lion's share of commuting zones do not extend across state borders. To aggregate DLR's county-level QCEW data to the commuting zone-state level, we use the county-to-commuting zone crosswalk of Acemoglu, Autor, Dorn, Hanson and Price (2016)—AADHP hereafter. Following DLR (for now), we only use counties that report restaurant-industry information in all 66 quarters, which restricts the sample to 184 commuting zone-state entities. These 184 entities appear in 128 pairs, of which 73 are complete and 55 are incomplete. Similar to DLR, from these 128 pairs we build a sample—which we refer to as the multi-state commuting zone-pair (MCZP) sample—with 201 observations each period ($201 \times 66 = 13,266$ observations in total), composed of 55 observations from incomplete pairs, and $73 \times 2 = 146$ observations from complete pairs. Given that a commuting zone-state can appear in at most two pairs (if it belongs to a three-state zone), the fraction of repeated observations is smaller in our MCZP sample than in DLR's CBCP sample (each period, 167 commuting zone-state entities appear once, and 17 appear twice).

Table 1 shows the estimation results for specifications 5 and 6 in DLR's Table 2. As in DLR, in addition to estimates of the minimum wage elasticity of employment, Table 1 also shows estimates of the elasticity of average earnings. Panel A shows DLR's estimated coefficients, which uses the CBCP sample, and panel B re-estimates those specifications but using instead the MCZP sample. Following Cameron, Gelbach and Miller (2011), DLR report two-way clustered standard errors at the state and border segment levels, where a border segment is defined as a pair of states sharing

[8]For example, county 32031 (Washoe County, NV) appears seven times each period: two times from complete pairs with California counties 6057 and 6061, five times from three incomplete pairs with California counties (6035, 6049, and 6091) and two times from incomplete pairs with Oregon counties (41025 and 41037).

[9]There are 129 pairs from the two-state zones, and 24 pairs from the three-state zones.

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

	DLR Specification 5		**DLR 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

Notes: This table replicates and re-analyzes the estimation results of specifications 5 and 6 of Table 2 in DLR. It uses DLR's replication package to obtain their exact reported coefficient estimates in panel A. After aggregating DLR's data at the commuting zone-state level, panel B shows the results from the re-estimation of DLR's specifications using instead cross-border pairs from multi-state commuting zones. Standard errors (in parentheses) are two-way clustered at the state and border segment levels. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

contiguous counties.[10]

Table 1 shows that the coefficients from the earnings regressions are very stable across specifications and across samples. But that is not the case for the employment regressions. DLR find

[10] DLR's standard errors for their specification 6 presented in their Table 2 are not accurate because they use data from incomplete pairs in their calculation. Specifically, controlling for pair-period effects, τ_{pt}, in equation (1) requires complete pairs, and thus, only complete pairs are effectively used in its estimation. Therefore, columns 3 and 4 in Table 1 estimate their specification 6 using only complete pairs (the size of the CBCP sample declines to $316 \times 2 \times 66 = 41,712$ observations, and the size of the MCZP sample declines to $73 \times 2 \times 66 = 9,636$ observations), and report the estimated coefficients of DLR (in panel A) but corrected standard errors. This standard error correction makes a small but not material difference in the results.

that the estimate of the minimum wage elasticity of employment changes from a significant -0.137 in the specification without pair-period effects and no total private sector employment (panel A, column 1), to a non-significant 0.016 when using the specification that controls for pair-period effects and total private sector employment (panel A, column 4). And the qualitative difference in the results stems from the inclusion of the county pair-period fixed effects.

On the other hand, when using instead pairs from multi-state commuting zones, panel B shows that the estimate remains negative, sizable, and significant in DLR's preferred specification, only declining from -0.212 in column 1 to -0.141 in column 4. This is the central point of the paper; had DLR estimated their preferred specification using multi-state commuting zones, as Allegretto, Dube and Reich (2009) advocated in their contemporaneous paper, they would not have concluded that using close controls establishes that minimum wages do not affect employment.

Therefore, the definition of local economic area—whether a pair of contiguous counties or a commuting zone—is crucial for the sign, size, and significance of the minimum wage elasticity of employment. In particular, when we control for spatial heterogeneity by using minimum wage variation within commuting zones—which actually are defined to capture common economic shocks to the labor market—we continue to find evidence that minimum wages reduce employment, with elasticity estimates in the traditional or earlier "consensus" range of -0.1 to -0.2. Hence, using DLR's QCEW data, there is little or only modest evidence of bias in the standard panel data estimator.

A possible interpretation of the previous results is that, even if negative and significant, elasticities around -0.2 are small. Neumark and Wascher (2008) address the issue of whether we should think of an employment elasticity in this range as "small," in the sense that if it is far from -1, the income elasticity with respect to the minimum wage must be positive and sizable. This argument is incorrect for two reasons. First, the estimated elasticity is with respect to minimum wage changes. But many restaurant workers earn well above the minimum wage, so in some sense we should be more concerned with the elasticity of employment for those whose earnings are affected by the minimum wage. Whatever proportion of restaurant workers this is, dividing the elasticity by this proportion blows up the relevant elasticity for thinking about effects on incomes of affected workers. Second, the elasticities are calculated with respect to the change in the legislated minimum wage. But because many workers earn between the "old" and "new" minimum wage, the induced wage change is smaller than the legislated minimum wage change. Again, one would divide by the ratio of these quantities, further blowing up the relevant elasticity. Neumark and Wascher (2008) show that it is not implausible that the relevant employment elasticity for workers whose wages are directly increased by higher minimum wages could be closer to -1 after these two adjustments. Indeed, recent evidence from Clemens and Wither (2019) implements an approach to estimate the employment elasticity for these workers, and finds an elasticity of -0.97.

3 Estimation Using the County Business Patterns Database

To examine the evidence from more complete data that is thus more generalizable, in this section we analyze the relationship between minimum wages and employment in the restaurant industry using yearly Census CBP data from 1990 to 2016. The CBP data not only includes ten more years of data than the contiguous border county-pair (CBCP) and multi-state commuting zone-pair (MCZP) samples, but also has much better geographic coverage. For example, whereas the CBCP and MCZP only include 316 and 73 complete pairs, respectively, the CBP data yields 1,165 complete pairs (out of 1,181) in the county approach and 151 complete pairs (out of 153) in the commuting-zone approach: our pair coverage goes from 26.75 percent to 98.65 percent in the county approach, and from 47.71 percent to 98.69 percent in the commuting-zone approach.

DLR also use CBP data for a robustness check (still using cross-border county pairs) and obtain a non-significant minimum wage elasticity of employment of -0.034. However, DLR are skeptical about their CBP data because of changes in industry classification (from SIC to NAICS) and the fact that, due to confidentiality reasons, the CBP reports many county-industry cells as an employment range. These problems are minimal in our CBP data, which was processed using the sophisticated fixed-point imputation and industry-classification method of Autor, Dorn and Hanson (2013) and AADHP.[11]

3.1 Data Description

From the CBP we obtain yearly employment counts and annual pay from 1990 to 2016.[12] We follow AADHP—and make extensive use of their detailed programs—to process the CBP data into 479 industries and 722 commuting zones, with the difference that in our data commuting zones are

[11] AADHP's employment-imputation algorithm makes use of three pieces of information in the CBP: *(i)* through 2016, when not exactly reported in a county-industry cell, employment is given as one of 12 ranges, starting from "0-19 employees" and up to "more than 100,000 employees"; *(ii)* through 2016, the CBP always reports the exact number of establishments in each county-industry cell and splits the total into 12 establishment-size categories, starting from the number of establishments with 0-4 employees up to the number of establishments with more than 5000 employees; and *(iii)* even if exact employment is not reported for a county-industry cell, the CBP reports employment at higher levels of industry aggregation.

[12] Although CBP data can be obtained through 2021, we do not use the 2017-2021 period, because starting with the 2017 release, Census's changes in confidentiality practices no longer allow the implementation of AADHP's employment-imputation algorithm. In particular, since 2017 the CBP fully omits county-industry cells with less than three establishments, and thus, the CBP no longer shows full establishment counts nor employment ranges for these cells (see previous footnote).

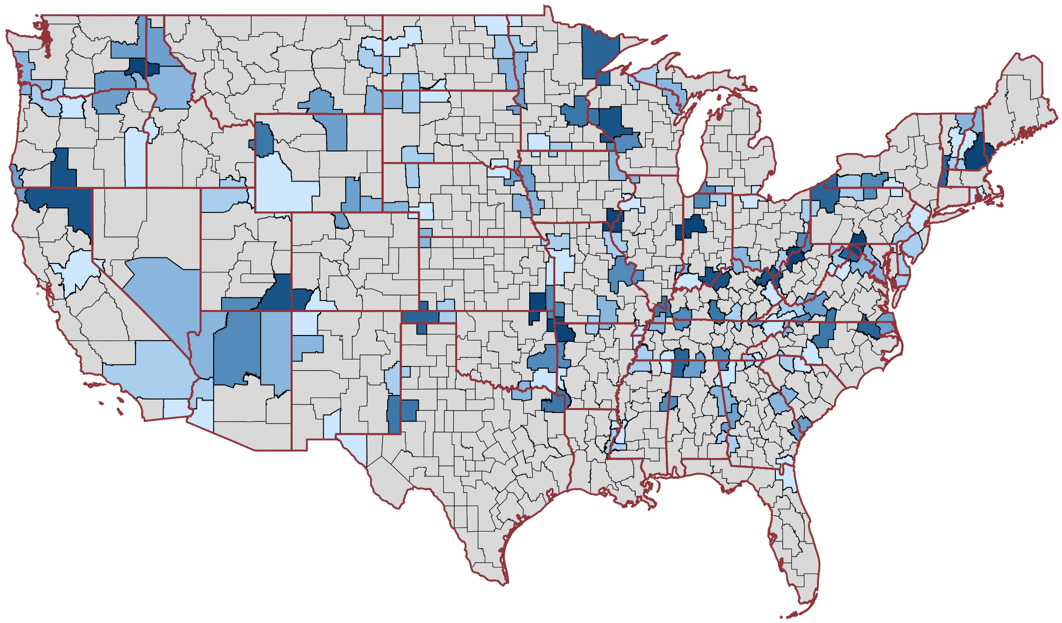


Figure 1: The 137 multi-state commuting zones in the CBP-MCZP sample

also split by state. At the commuting zone-state level there are 585 single-state commuting zones, 129 two-state commuting zones, and 8 three-state commuting zones.[13] We also follow AADHP to obtain yearly working-age population at the commuting zone-state level from the Census Bureau's Population Estimates Program. Last, yearly minimum wage data at the state level—defined as the largest of the federal minimum wage and the state minimum wage—are obtained from Vaghul and Zipperer (2016).[14]

From these sources we construct two datasets. The first dataset includes all available 866 commuting zone-state entities, while the second dataset includes only the 137 multi-state commuting zones (corresponding to 281 commuting zone-state entities). The 866 commuting zone-state entities come from 585 single-state commuting zones, 129×2 two-state commuting zones, and $(8 \times 3) - 1$ three-state commuting zones.[15] The second dataset—which we label as the CBP-MCZP sample—is analogous to the MCZP sample from section 2, and its purpose is to exploit local differences in minimum wages to control for spatial heterogeneity at the local level. As shown in Figure 1, the 137 multi-state commuting zones are widely distributed across the continental United States.

The multi-state commuting zone data are representative of the U.S. economy as a whole. Throughout the 1990-2016 period, the multi-state commuting zone group accounts on average

[13] As in AADHP, we exclude Alaska and Hawaii from our analysis.

[14] The minimum wage data are available at this link.

[15] We subtract one entity because the District of Columbia, which appears in the CBP starting in 2004 and is not included in our analysis, is part of a three-state commuting zone. That is, in our analysis with CBP data, the DC-VA-MD commuting zone is treated as a two-state commuting zone between Virginia and Maryland.

for 29.8 percent of U.S. employment, 29 percent of U.S. establishments, and 29.4 percent of the U.S. working-age population.[16] Moreover, all the variables of interest in this exercise are very similar in the multi-state commuting zones (137 commuting zones) and in the rest of the country (585 single-state commuting zones). To show this, Figure 2 presents a comparison between the groups of establishment and employment counts, employment-to-populations ratios, earnings per worker, and average minimum wages.

Figures 2a–2c show that both groups follow the same patterns for establishment counts, employment counts, and employment-to-population ratios. The only noticeable difference is that employment-to-population ratios are slightly higher in the multi-state group—the average throughout the period is 55.5 percent for the multi-state group and 54.4 percent for the single-state group. Figure 2d shows similar values and patterns for earnings per worker, calculated for each group as the total annual pay divided by total employment. Finally, Figure 2e shows that the average minimum wage—weighted by commuting zone-state working-age population—has a similar evolution in both groups, increasing from about \$3.86–\$3.93 in 1990 to about \$8.12–\$8.45 in 2016.[17]

3.2 Econometric Specifications

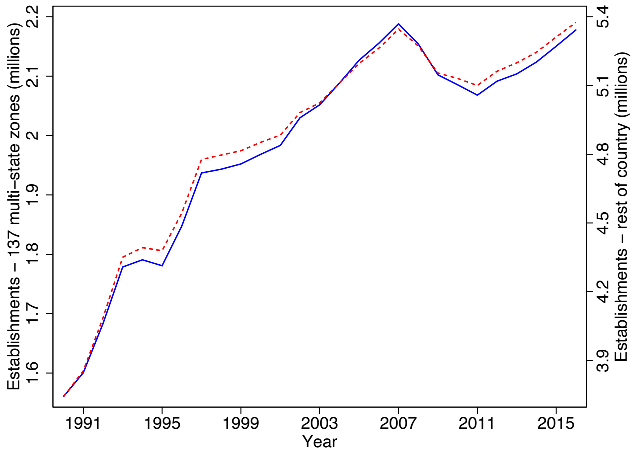
Although in section 2 we focused on DLR's preferred specification 6 to replicate and re-analyze their findings, here we take advantage of our CBP data. Because the application of these new data to this issue is new, we provide a more complete analysis that covers a larger set of the specifications that DLR report—building up to the key specifications using within-commuting zone minimum wage variation. As in DLR, we start with a simple specification à la Neumark and Wascher (1992) that uses the full-country dataset and controls for some levels of time-varying spatial heterogeneity. We then move to our multi-state commuting zone sample and estimate a specification similar to DLR's specification 6, which controls for time-varying local economic conditions.

Using the full-country dataset, we estimate equation

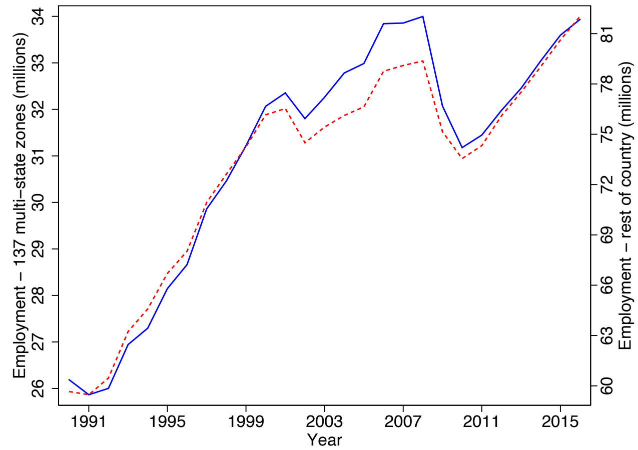
$$\ln e_{it} = \alpha + \beta \ln MW_{it} + \gamma \ln E_{it}^{-} + \delta \ln P_{it} + \eta_i + \tau_{ct} + \kappa_S \mathbb{1}_S \cdot T + \varepsilon_{it}, \qquad (2)$$

[16] These shares are very stable over time. They range between 29.3 and 30.5 percent for employment, 28.8 and 29.4 percent for establishments, and 29.1 and 29.7 percent for the working-age population.

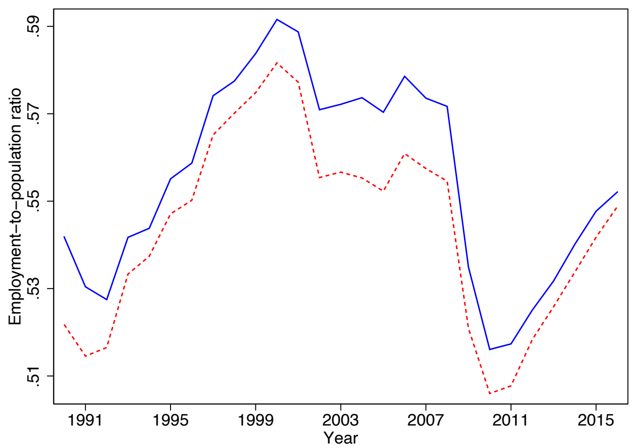
[17] The restaurant industry has code 5812 (Eating and drinking places) in the Standard Industrial Classification used by AADHP. After aggregating the remaining 478 AADHP industries into 19 industries, Table A-1 in the Appendix shows industry-level earnings per worker and earnings rankings in 1990 and 2016. In both years the restaurant industry has the lowest earnings per worker. Moreover, the earnings gap with the retail-trade industry (the second lowest-earnings industry) is large, with the retail-trade industry paying 76 percent more in 1990 and 56 percent more in 2016. Table A-1 also presents industry employment shares, showing that restaurants accounted for 7.21 percent of U.S. employment in 1990 and for 9.37 percent in 2016. Finally, Figure A-1 in the Appendix shows that establishment counts, employment, and earnings in the restaurant industry follow the same patterns in the multi-state commuting zone group and in the rest of the country.



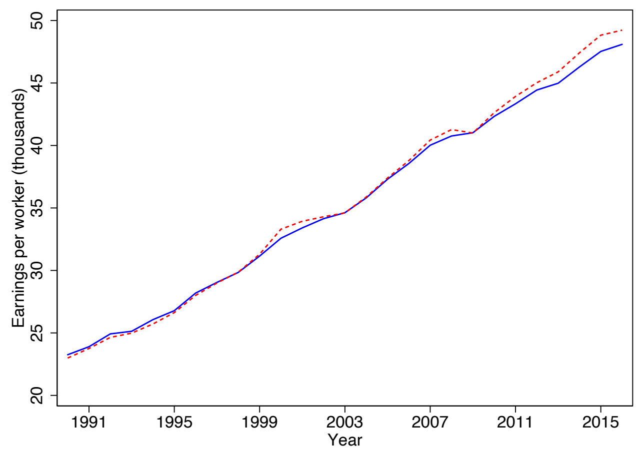
(a) Number of establishments



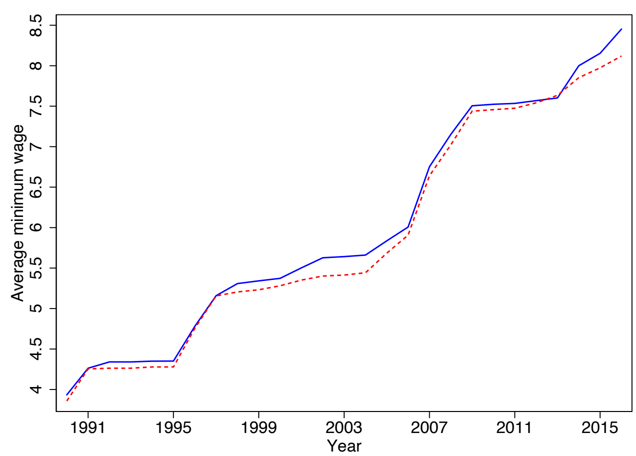
(b) Total employment



(c) Employment-to-population ratio



(d) Earnings per worker (U.S. dollars)



(e) Average minimum wage (U.S. dollars)

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

where for commuting zone-state i in year t, e_{it} is total employment in the restaurant industry, MW_{it} is the minimum wage, E_{it}^{-} is total employment in commuting zone-state i in all other industries, P_{it} is the working-age population, η_i is a commuting zone-state i fixed effect, τ_{ct} accounts for time fixed effects for each of the nine Census regional divisions, and $\mathbb{1}_S \cdot T$ represents state-level trends ($\mathbb{1}_S$ is a dummy variable taking the value of 1 if entity i belongs to state S and T denotes a time trend).

Although equation (2) attempts to account for spatial heterogeneity by including Census-division time fixed effects and state-level trends, DLR argue that this is not enough to account for local economic conditions and introduce their novel cross-border county-pair approach. Along these lines, we exploit minimum wage variation within commuting zones by using the 137 multi-state commuting zone dataset and the econometric model

$$\ln e_{ipt} = \alpha + \beta \ln MW_{it} + \gamma \ln E_{it}^{-} + \delta \ln P_{it} + \eta_i + \tau_{pt} + \nu_{ipt}, \tag{3}$$

where subscript p identifies a pair for commuting zone-state i, τ_{pt} denotes pair-period fixed effects, which are intended to control for spatial heterogeneity at the local level, and ν_{ipt} is the error term. The only difference between (3) and (1) is that DLR use total private sector employment as a control (which we assume includes employment in restaurants), whereas we use instead total employment in the rest of the industries.[18]

Among our 137 multi-state commuting zones there are 151 complete pairs: one pair for each of the 129 two-state commuting zones, three pairs for each of the 7 three-state commuting zones, and one more pair corresponding to Virginia and Maryland in the DC-VA-MD commuting zone (recall that DC is excluded from our data). Notice that the estimation of (3) requires complete pairs: if entities i and j belong to pair p, we control for τ_{pt} by subtracting the equation for $\ln e_{jpt}$ from (3)

[18]Using total employment as a control amounts to including our dependent variable on the right-hand side of equation (3), which would introduce a bias in the estimation of β. Recall that β is the effect of our variable of interest on the dependent variable holding the controls constant, and the latter condition cannot be satisfied if one of the controls includes the dependent variable. In particular, if the minimum wage reduces restaurant employment, and restaurant employment is in the total employment control (restaurant employment accounts for between 7.21% and 9.37% of total U.S. employment during the 1990-2016 period), there is attenuation of the minimum wage effect towards zero (because we effectively have a "bad control," or we "overcontrol"). If we use instead total employment as the control in our commuting-zone specifications below, the estimated minimum wage elasticity of employment is between 75% and 94% the size of the estimated elasticity when using as control total employment in the rest of the industries. For example, it declines in size from -0.242 to -0.188 and from -0.255 to -0.227 in our results from Table 3 (below), and from -0.689 to -0.580 in the four-year elasticity (the p-value increases to 0.13 in the first case, but remains below 0.01 in the other two cases). In principle, one could construct a control that leaves out other low-wage industries as well, so that we are more likely picking up a cyclical control and not effects of minimum wages on employment in other industries; removing more of this overcontrol would only strengthen the minimum wage effect.

to obtain

$$\ln e_{ipt} - \ln e_{jpt} = \beta \left(\ln MW_{it} - \ln MW_{jt}\right) + \gamma \left(\ln E_{it}^{-} - \ln E_{jt}^{-}\right) + \delta \left(\ln P_{it} - \ln P_{jt}\right) + \eta_p + \nu_{pt}, \quad (4)$$

where $\eta_p \equiv \eta_i - \eta_j$ is the pair (i, j) fixed effect, and $\nu_{pt} = \nu_{ipt} - \nu_{jpt}$ is the error term. We can then estimate (4) as a panel with 151 pairs and 27 years, or we can directly estimate (3) by using the multi-way fixed effects estimator of Correia (2016) in our CBP-MCZP sample.[19] We follow the latter approach, as Correia (2016) provides a Stata package (`reghdfe`) that allows for multi-way clustering of standard errors as in Cameron, Gelbach and Miller (2011).

3.3 Main Results

Using the full sample, which is a panel of 866 commuting zone-state entities and 27 years, Table 2 presents our results from the estimation of different versions of equation (2). Panel A presents the employment results, and panel B shows the estimation results of similar specifications for earnings per worker. All regressions include commuting zone-state effects and report standard errors that are clustered at the state level.

Column 1 in Table 2 starts with a version of (2) that only includes commuting zone-state fixed effects and year fixed effects, so that it abstracts from controlling for time-varying spatial heterogeneity. The estimated minimum wage elasticity of employment is -0.338. Columns 2 and 3 show that as we add time-varying spatial heterogeneity controls, first with Census region-year effects in column 2 and then state trends in column 3, the minimum wage elasticity declines in magnitude, but keeps its negative sign. However, the estimated elasticity loses its statistical significance when including state trends. Although in that case the elasticity is small (-0.049), Meer and West (2016) point out that the model with state trends can lead to attenuation, especially when the treatment effect grows over time. Indeed, our evidence below in section 4.1 points to minimum wage effects that increase over time.[20]

In columns 4-6 we re-estimate the specifications from columns 1-3, but restricting the sample to the 281 commuting zone-state entities of the 137 multi-state commuting zones. The story is similar as with the full sample, though the elasticity in the specification with state trends in column 6 (-0.128) is more than twice the size of the elasticity in column 3 and is statistically significant.

[19] Our CBP-MCZP sample includes 151×2 observations each year, for a maximum of $8,154$ observations in the 1990-2016 period. Of these, our main regression below uses $8,134$ observations, as we have 20 observations with missing data.

[20] Questions about the reliability of controlling for spatial heterogeneity by including state-specific linear trends have been addressed by Neumark, Salas and Wascher (2014), and discussed further in Allegretto, Dube, Reich and Zipperer (2017) and Neumark and Wascher (2017). It is clear to us that the main and more influential contribution of DLR is the cross-border county research design, and hence that is our focus.

Table 2: Conventional estimation of minimum wage responses with CBP 1990-2016 data

	Full sample			**Multi-state zones**		
	(1)	(2)	(3)	(4)	(5)	(6)
A. ln(employment)						
ln(minimum wage)	-0.338***	-0.126**	-0.049	-0.299***	-0.161*	-0.128**
	(0.089)	(0.051)	(0.038)	(0.101)	(0.091)	(0.062)
ln(employment^{-})	0.000	0.115**	0.122**	-0.095	0.006	0.039
	(0.072)	(0.056)	(0.057)	(0.070)	(0.066)	(0.080)
ln(population)	1.029***	0.866***	0.876***	1.155***	1.077***	1.025***
	(0.088)	(0.071)	(0.074)	(0.125)	(0.124)	(0.116)
B. ln(earnings)						
ln(minimum wage)	0.215***	0.170***	0.168***	0.178***	0.169***	0.154***
	(0.037)	(0.033)	(0.035)	(0.050)	(0.037)	(0.050)
ln(earnings^{-})	0.255***	0.221***	0.185***	0.287*	0.248*	0.203
	(0.069)	(0.067)	(0.060)	(0.143)	(0.142)	(0.148)
ln(population)	0.091**	0.111***	0.120**	0.017	0.004	-0.014
	(0.039)	(0.036)	(0.045)	(0.079)	(0.076)	(0.095)
Zone-state effects	Y	Y	Y	Y	Y	Y
Year effects	Y			Y		
Region-year effects		Y	Y		Y	Y
State trends			Y			Y
Observations	23,361	23,361	23,361	7,577	7,577	7,577

Notes: This table reports $\hat{\beta}$, $\hat{\gamma}$, and $\hat{\delta}$ from the estimation of specification (2) for the restaurant industry using yearly data from 1990 to 2016. In panel A, the dependent variable is log employment. Panel B uses instead log earnings per worker. Columns 1-3 use the full sample with 866 commuting zone-state entities, and columns 4-6 use the 281 multi-state commuting zone-state entities. Standard errors (in parentheses) are clustered at the state level. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

In panel B in Table 2, we estimate equation (2) using earnings per worker in the restaurant industry (and earnings per worker in the rest of the industries on the right-hand side of the specifications). All columns show a positive and highly significant elasticity that ranges between 0.154 and 0.215.

Using our multi-state zones dataset (the CBP-MCZP sample), we now turn to the estimation of equation (3), which controls for time-varying spatial heterogeneity at the local level, as DLR advocate (and which they claim eliminates evidence of adverse employment effects of the minimum wage). Table 3 presents the estimates, with column 1 using the 151 pairs available in our CBP data, and column 2 restricting the CBP-MCZP sample to 71 of the 73 complete pairs available in the MCZP sample from DLR's QCEW data. As in DLR, standard errors are two-way clustered at the state and border segment levels. The elasticity of employment is negative, large, and highly

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

Notes: This table reports $\hat{\beta}$, $\hat{\gamma}$, and $\hat{\delta}$ from the estimation of specification (3) for the restaurant industry from CBP 1990-2016 yearly data. Columns 1-2 use pairs within multi-state commuting zones, and columns 3-4 use contiguous county pairs. The dependent variable in panel A is log employment, whereas in panel B it is log earnings per worker. Column 1 uses the 151 multi-state commuting zone pairs, and column 2 uses 71 of the 73 DLR complete pairs used in columns 3-4 of panel B in Table 1 (we lose the two DC pairs). Column 3 uses all county pairs available in our dataset, and column 4 uses the complete pairs of DLR that are available in the CBP data (309 out of 316—we lose four pairs involving DC and three pairs involving the County of San Francisco). Standard errors (in parentheses) are two-way clustered at the state and border segment levels. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

significant when estimating the local specification exploiting minimum wage differentials within commuting zones: columns 1 and 2 in panel A show elasticities of -0.242 and -0.255. Thus, there is evidence of a negative and statistically significant relationship between minimum wages and restaurant industry employment in the United States, even after controlling for time-varying local economic conditions.

Having established that the pair-approach estimation with multi-state commuting zones yields negative and significant minimum wage elasticities of employment with both the CBP-MCZP sample (in columns 1-2 of Table 3) and the MCZP sample from DLR's QCEW data (in Table 1), we now re-estimate equation (3) but using contiguous county-pairs that share a state border. Out of the 1,181 possible county pairs, our CBP data contains 1,165 for the restaurant industry. Columns 3 and 4 in Table 3 present the county-pair estimation results.

Column 3 estimates equation (3) using the 1,165 complete county pairs in the CBP data, and shows a non-significant elasticity of -0.081, which is about one third of the -0.242 elasticity in column 1. The large reduction in the magnitude of the elasticity indicates, again, that how we define local economic areas matters. For a comparison with DLR's county-pair coverage, column 4 presents the estimation of equation (3) using 309 of the 316 complete county pairs of DLR, and reports a non-significant minimum wage elasticity of employment of -0.023 (about one ninth of the elasticity in column 2). Hence, columns 3 and 4 show that for the within-county-pair approach using the CBP data, the sample that uses 309 of the 316 DLR's complete pairs (26.16 percent of all possible pairs) underestimates the minimum wage elasticity of employment when compared to a sample that uses 98.65 percent of contiguous county pairs that share a state border, although, as we have seen, the much more substantive impact—the comparison with columns 1 and 2—comes from the definition of local economic areas used to control for local shocks.

Finally, all columns in panel B of Table 3 show statistically significant point estimates for the minimum wage elasticity of earnings in the restaurant industry, with values ranging between 0.156 and 0.211. Taking our preferred specifications in columns 1 and 2, a 10 percent increase in the minimum wage is associated with an average earnings increase in restaurants between 1.63 and 1.98 percent. The fact that the elasticities for earnings in the restaurant industry are not much affected when using the county-pair approach simply reflects that no matter the location, there is always a sizable fraction of restaurant employees paid the minimum wage. Hence, an increase in the minimum wage in one county will be met by an increase in the average earnings gap between this county and the control counties (irrespective of whether a county pair has any joint commuting or economic activity). In addition, elastic labor supply to the restaurant industry will also imply little impact on the earnings estimates from alternative attempts to control for unobserved demand shocks.

Comparing the Table 2 and Table 3 estimates using the CBP data, which are preferable to the QCEW data, leads to a similar conclusion. Including pair-period fixed effects for commuting zones rather than simply cross-border county pairs points to little or only modest evidence of bias in the standard panel data estimator. Equivalently, using within commuting zone pair-period fixed effects to control for time-varying spatial heterogeneity, as advocated by DLR—but using more compelling local economic areas to do so—we still find evidence that higher minimum wages reduce restaurant employment, with our best estimate indicating an elasticity of -0.242.

3.4 Evolution of the Minimum Wage Elasticity of Employment

Our analysis of the CBP data includes ten more years of available data than DLR's QCEW data analysis. In this section we study the evolution of the minimum wage elasticity of employment between 2006 (the last year in DLR) and 2016 (the last year of our CBP data). We do this by estimating with our CBP data the four versions of equation (3) in Table 3 for different periods, starting with DLR's 1990-2006 period and ending with 1990-2016. Table 4 shows the estimated elasticities.

Our preferred specification in column 1, which uses all available pairs in our multi-state commuting zone data, consistently shows a negative, large, and significant elasticity. It follows a bit of a U pattern, decreasing from -0.291 in 1990-2006 to -0.341 in 1990-2012, and then rising to our last value of -0.242.[21] We observe similar evidence in column 2 when we restrict the sample to the DLR pairs, with the elasticity ranging between -0.214 and -0.285. Importantly, the 1990-2006 estimate of -0.214 is not statistically different from the -0.141 estimate obtained in Table 1 with DLR's QCEW data.

Columns 3 and 4, which use contiguous-county pairs, show three important results. First, the estimated non-significant elasticity of -0.059 from using DLR pairs with CBP data for the period 1990-2006 is not statistically different from DLR's estimated elasticity of 0.016. Second, the DLR-pairs specification in column 4 consistently underestimates the size of the elasticity when compared to the sample using all available pairs in column 3, being on average less than half the size, indicating an important sample-selection bias in DLR's very limited county-pair sample—recall that DLR pairs in column 4 only cover about 26.16 percent of all potential county pairs, whereas column 3 covers 98.65 percent.[22] And third, upon comparing column 1 with column 3 and column 2 with column 4, it is evident that the county-pair approach underestimates the employment elasticity when compared to using pairs within multi-state commuting zones. Section 5 below shows that the latter finding is a consequence of the county-pair sample including pairs

[21] As a potential explanation of the observed U pattern, Clemens and Wither (2019) document particularly strong minimum wage effects on employment as a consequence of the large hike in the federal minimum wage (from \$5.15 to \$7.25) between July 2007 and July 2009, whereas the effects are negative but weaker for minimum wage changes implemented in post-Great Recession years (Clemens and Strain, 2018, 2021).

[22] On the other hand, when using multi-state commuting zones, the coefficients in column 2 are on average about 87 percent of the size of the column 1 coefficients, and are larger in the last three periods.

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

Notes: This table reports $\hat{\beta}$ from the estimation of specification (3) for the restaurant industry using CBP yearly data for different periods. Columns 1-2 use pairs within multi-state commuting zones, and columns 3-4 use contiguous county pairs. The dependent variable is log employment, the main regressor is the log minimum wage, and the controls (not reported) are log employment in the rest of the industries and log working age population. Column 1 uses the 151 multi-state commuting zone pairs, and column 2 uses 71 of the 73 DLR complete pairs used in columns 3-4 of panel B in Table 1. Column 3 uses all county pairs available in our dataset, and column 4 uses the complete pairs of DLR that are available in the CBP data (309 out of 316). Standard errors (in parentheses) are two-way clustered at the state and border segment levels. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

formed by counties from different commuting zones—these specific pairs fail to control for spatial heterogeneity because they are composed of counties that are unlikely to share common shocks.

3.5 Weighted Estimates and Post-1992 Period

Although minimum wage elasticities in DLR are estimated using unweighted regressions, this section looks at the robustness of estimated elasticities to employment or population weighting. Moreover, given that Cengiz, Dube, Lindner and Zipperer (2019)—CDLZ hereafter—strongly argue that using data before 1993 is the main driver of negative and significant elasticities for employment in conventional two-way fixed effects studies (see their Appendix G), here we also look at unweighted and weighted estimated elasticities for the 1993-2016 period. We focus on elasticity estimates from equation (3)—with pair-period effects—for employment and earnings per worker using all available pairs in our multi-state commuting zone data.

For various end years, as presented in Table 4, Table 5 shows the evolution of the minimum wage elasticity estimates for employment in columns 1-3, and for earnings per worker in columns 4-6. Columns 1 and 4 report the unweighted estimates (so that column 1 is the same as column 1 in Table 4), columns 2 and 5 report the estimates weighted by initial employment, and columns 3 and 6 report the estimates weighted by initial working-age population.

Comparing columns 2 and 3 to column 1, note that all the estimates of the minimum wage elasticity of employment remain negative and significant whether we weight by employment or population. However, the magnitude of the estimated elasticities declines, ranging between -0.204 and -0.163 when weighting by employment (on average about 63 percent the size of the unweighted elasticities), and ranging between -0.187 and -0.149 when weighting by population (on average about 57 percent the size of the unweighted elasticities). Though smaller, these estimated elasticities are well in the range of conventional TWFE estimates.[23]

When comparing columns 5 and 6 to column 4, it is noteworthy that the estimated elasticities for earnings exhibit a high sensitivity to weighting, particularly for periods ending before 2011. For instance, for DLR's 1990-2006 period, the weighted elasticity estimates are non-significant and only represent between 17 and 18 percent of the size of the unweighted estimate. For the longer periods, the weighted elasticities are larger and again significant; for example, for the 1990-2016 period, they are about 83 percent the size of the unweighted estimate.[24]

[23]Table A-2 in the Appendix redoes Table 5, but weighting instead by log employment and log population, and shows extremely similar weighted and unweighted elasticities for employment. This indicates that the changes in the coefficient sizes are likely driven by a few very large commuting zone-states (weights in logs reduce the importance of outliers).

[24]As with the employment elasticities, Table A-2 in the Appendix shows that the weighted estimates for the

Table 5: Pair-approach minimum wage elasticities of employment and earnings using multi-state commuting zones: Unweighted/weighted estimation with different end years

	Employment			**Earnings**		
Period	(1)	(2)	(3)	(4)	(5)	(6)
1990–2006	-0.291***	-0.177**	-0.157**	0.275***	0.046	0.050
	(0.095)	(0.070)	(0.061)	(0.088)	(0.077)	(0.079)
1990–2007	-0.301***	-0.191***	-0.171***	0.243***	0.034	0.039
	(0.092)	(0.068)	(0.061)	(0.071)	(0.071)	(0.071)
1990–2008	-0.327***	-0.202***	-0.183***	0.239***	0.057	0.060
	(0.093)	(0.070)	(0.064)	(0.069)	(0.064)	(0.063)
1990–2009	-0.337***	-0.204***	-0.186***	0.243***	0.070	0.073
	(0.092)	(0.072)	(0.066)	(0.069)	(0.058)	(0.056)
1990–2010	-0.338***	-0.202**	-0.185**	0.244***	0.084	0.088*
	(0.094)	(0.075)	(0.070)	(0.069)	(0.051)	(0.048)
1990–2011	-0.333***	-0.201**	-0.185**	0.238***	0.098**	0.102**
	(0.094)	(0.079)	(0.073)	(0.069)	(0.046)	(0.043)
1990–2012	-0.341***	-0.202**	-0.187**	0.228***	0.103**	0.105**
	(0.095)	(0.080)	(0.075)	(0.068)	(0.047)	(0.044)
1990–2013	-0.323***	-0.200**	-0.185**	0.212***	0.120**	0.122***
	(0.099)	(0.080)	(0.074)	(0.070)	(0.045)	(0.041)
1990–2014	-0.275**	-0.185**	-0.170**	0.176***	0.116**	0.117**
	(0.112)	(0.080)	(0.074)	(0.065)	(0.050)	(0.047)
1990–2015	-0.258**	-0.172**	-0.157**	0.185***	0.127***	0.128***
	(0.117)	(0.075)	(0.070)	(0.063)	(0.047)	(0.044)
1990–2016	-0.242**	-0.163**	-0.149**	0.163***	0.133***	0.133***
	(0.120)	(0.069)	(0.065)	(0.055)	(0.044)	(0.042)
Zone-state effects	Y	Y	Y	Y	Y	Y
Pair–period effects	Y	Y	Y	Y	Y	Y
Number of pairs	151	151	151	151	151	151
Weighted by:		emp	pop		emp	pop

Notes: This table reports $\hat{\beta}$ from the unweighted and weighted estimation of specification (3) for the restaurant industry using CBP yearly data for different end-year periods. All regressions in the table use the 151 multi-state commuting zone pairs. The dependent variable in columns 1-3 is log employment, whereas in columns 4-6 it is log earnings per worker. The main regressor is the log minimum wage, and the controls (not reported) are log employment in the rest of the industries in columns 1-3, log earnings per worker in the rest of the industries in columns 4-6, and log working age population. Columns 1 and 4 show the unweighted estimation results, columns 2 and 5 use initial employment weights, and columns 3 and 6 use initial working age population weights. Standard errors (in parentheses) are two-way clustered at the state and border segment levels. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Table 6: Pair-approach minimum wage elasticities of employment and earnings using multi-state commuting zones: Unweighted/weighted estimation with CBP 1993-2016 data

	Employment			**Earnings**		
Period	(1)	(2)	(3)	(4)	(5)	(6)
1990–2016	-0.242**	-0.163**	-0.149**	0.163***	0.133***	0.133***
	(0.120)	(0.069)	(0.065)	(0.055)	(0.044)	(0.042)
1993–2016	-0.237*	-0.147**	-0.133**	0.160***	0.146***	0.146***
	(0.122)	(0.061)	(0.057)	(0.056)	(0.044)	(0.041)
Zone-state effects	Y	Y	Y	Y	Y	Y
Pair–period effects	Y	Y	Y	Y	Y	Y
Number of pairs	151	151	151	151	151	151
Weighted by:		emp	pop		emp	pop

Notes: This table reports $\hat{\beta}$ from the unweighted and weighted estimation of specification (3) for the restaurant industry using CBP yearly data for periods 1990-2016 and 1993-2016. All regressions in the table use the 151 multi-state commuting zone pairs. The dependent variable in columns 1-3 is log employment, whereas in columns 4-6 it is log earnings per worker. The main regressor is the log minimum wage, and the controls (not reported) are log employment in the rest of the industries in columns 1-3, log earnings per worker in the rest of the industries in columns 4-6, and log working age population. Columns 1 and 4 show the unweighted estimation results, columns 2 and 5 use initial employment weights, and columns 3 and 6 use initial working age population weights. Standard errors (in parentheses) are two-way clustered at the state and border segment levels. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Turning to a different issue regarding the sensitivity of the estimates, CDLZ argue that pre-1993 data generates a spurious negative correlation between employment and minimum wages in conventional TWFE estimates.[25] Although our 1990-2016 period only includes three years before 1993, we address this point directly by estimating unweighted and weighted versions of equation (3) for the 1993-2016 period. Table 6 presents the estimates for the 1993-2016 period alongside the estimates for the 1990-2016 period. Note that the unweighted and weighted elasticities for employment and earnings for the 1993-2016 period—considered by CDLZ as a period not affected by pre-trends generating a negative spurious correlation between employment and minimum wages—barely change in comparison to those for the 1990-2016 period.[26]

earnings elasticity are very close to the unweighted estimates (*i.e.*, positive and significant) when using log employment or log population as weights.

[25]In particular, CDLZ mention on p. 67 of their online Appendix G: "The TWFE-logMW specification is sensitive to shocks to upper tail employment in the 1980s and early 1990s in Democratic-leaning states, which contaminate the TWFE-logMW estimates using the full 1979-2016 sample. Even though most minimum wage variation comes from after 1992, these shocks affect the estimation of the fixed effects. In contrast, the TWFE-logMW specification in the 1993-2016 sample is not affected by these shocks."

[26]In section 4.2, we delve deeper into the issue of pre-existing trends in the data.

4 Time Paths, Pre-Trends, and Common Shocks

DLR explore both the long-term effects of minimum wages and the possibility of pre-existing trends that may affect their specifications. For their preferred specification 6 for contiguous county pairs sharing a state border, DLR find *(i)* a minimum wage elasticity of employment that is stable around zero with no delayed effects after four years of the minimum wage change, and *(ii)* no evidence of existing pre-trends. This section revisits these findings using instead pairs from multi-state commuting zones. Moreover, we also look at differences between the two types of geographical aggregation in capturing common shocks, and discuss potential spillover effects when using the within-local area research design.

4.1 Long-Term Employment Effects of Minimum Wages

The core motivation for DLR's border-county design is to control for spatial shocks that might be correlated with minimum wage variation. They argue that the more conventional panel data estimator using state variation (with fixed state and year effects) is biased—towards evidence of job loss—from such shocks. Although there is good reason to believe that commuting zones capture local economic shocks, and do this better than cross-border counties, it is at least in principle possible that commuting zones fail to do this. We thus replicate the analysis DLR present to try to argue that their cross-border county design controls for time-varying spatial heterogeneity. We find that, on the criterion that DLR use, commuting zones capture local economic shocks, and appear to do so better than do cross-border county pairs.

DLR estimate a distributed-lag model with eight quarters of leads and sixteen quarters of lags. In particular, the distributed-lag version of DLR's preferred specification 6 is

$$\ln e_{ipt} = \alpha + \sum_{k=-4}^{7} \beta_{2k} \Delta_2 \ln MW_{i,t-2k} + \beta_{16} \ln MW_{i,t-16} + \zeta Z_{it} + \eta_i + \tau_{pt} + \nu_{ipt}, \quad (5)$$

where Δ_2 is a two-quarter difference operator, and the rest of the variables are defined as in equation (1). The model estimates thirteen β parameters that indicate the cumulative effect of minimum wages, with β_{-8} denoting the lead effect eight quarters before the minimum wage change, and up to β_{16}, which denotes the cumulative effect sixteen quarters after the minimum wage change.

Using DLR's QCEW data and programs, Figure 3 shows the estimates of the β parameters from the estimation of (5)—along with 90 percent confidence intervals—using either contiguous county pairs (CBCP sample) or pairs from multi-state commuting zones (MCZP sample). (Table A-3 in the Appendix shows the estimation results.) We assume that the minimum wage change occurs at

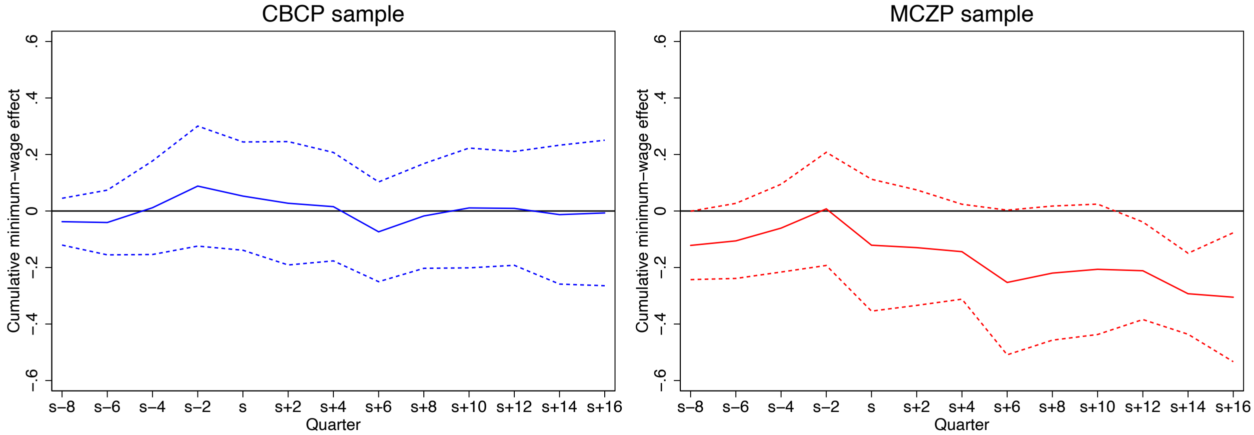


Figure 3: Time paths of minimum wage effects on employment with 90% confidence intervals using DLR's QCEW data: Contiguous county pairs (left–blue) and pairs from multi-state commuting zones (right–red)

time s, and hence, each plot in Figure 3 starts at $s-8$ (with $\hat{\beta}_{-8}$) and ends at $s+16$ (with $\hat{\beta}_{16}$). The plot that uses the CBCP sample is reported by DLR, with the only difference being that the 90 percent interval was calculated using corrected standard errors, based only on the complete-pair observations used in the estimation.

Notice that although the cumulative effect of a minimum wage change on restaurant employment is stable around zero when using county pairs, the story is very different when using pairs from multi-state commuting zones. In the latter case, the cumulative effect is near zero two quarters before the minimum wage change, but then it gets into a solid negative trend, reaching a significant elasticity of -0.305 after four years—this cumulative effect is more than twice as large as the -0.141 contemporaneous elasticity reported in Table 1. Therefore, a simple switch of the definition of local economic area—from pairs of contiguous counties sharing a state border, to multi-state commuting zones that are actually defined as local economic areas—dramatically overturns DLR's finding of a near zero long-term effect of minimum wages on employment in the restaurant industry.

To assess the robustness of this result, we also look at time paths for the estimated minimum wage elasticity of employment using the more comprehensive CBP yearly data. The distributed-lags version of equation (3) is

$$\ln e_{ipt} = \alpha + \sum_{k=-2}^{3} \beta_k \Delta \ln MW_{i,t-k} + \beta_4 \ln MW_{i,t-4} + \gamma \ln E_{it}^{-} + \delta \ln P_{it} + \eta_i + \tau_{pt} + \nu_{ipt}, \qquad (6)$$

where Δ is a one-year difference operator, and the other variables are as described in section 3.2. Thus, similar to equation (5), equation (6) includes two years of leads and four years of lags, with the difference that we use one-year rather than two-quarter changes. In total, from equation (6) we estimate seven β parameters, starting with β_{-2} for the lead effect two years before the minimum wage change, and up to β_4, which captures the cumulative effect up to four years after the minimum wage change.

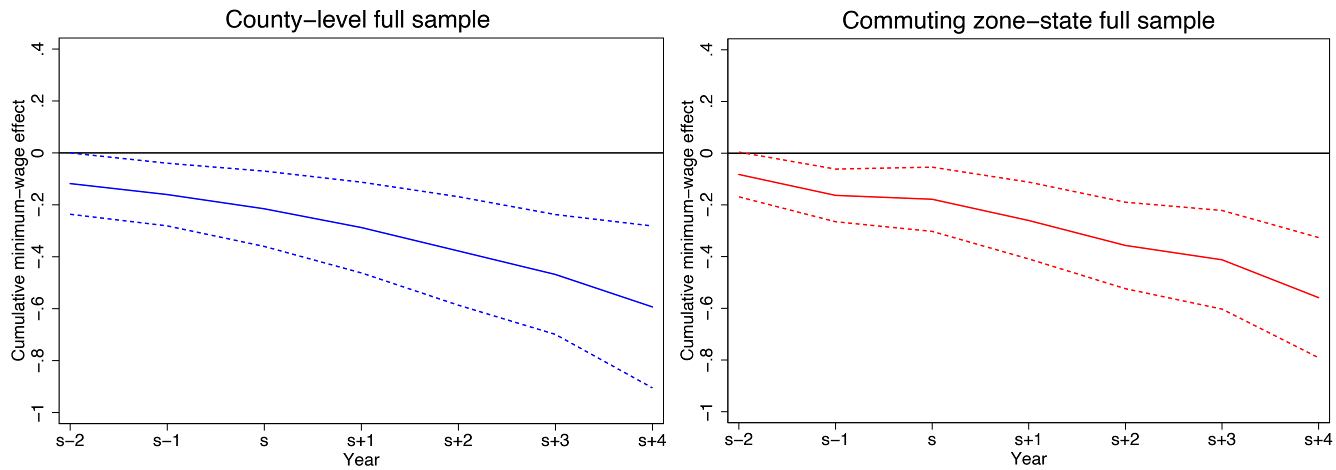
Using both contiguous county pairs and pairs from multi-state commuting zones, we start with a leads-lags version of the conventional two-way fixed effects estimation using the full-country dataset (as in column 1 of Table 2), and then we estimate equation (6) using either all complete pairs or only DLR's complete pairs. For all these cases, Figure 4 shows plots of the estimated β coefficients along with 90 percent confidence intervals. (Table A-4 in the Appendix shows the full estimation results.) Figure 4a shows the cumulative effects for the two-way fixed effects estimation, Figure 4b presents the cumulative effects from the estimation of equation (6) when using all available complete pairs, and Figure 4c shows the effects when we restrict the CBP sample to DLR's complete pairs.

Whether we use the county-level sample or the commuting zone-state level sample, Figure 4a shows that the conventional two-way fixed effects estimation yields similar cumulative minimum wage responses. The cumulative minimum wage elasticity after four years is -0.593 when using the county-level sample, and is -0.559 when using the commuting zone-state sample. Even though there are negative leading effects, implying that minimum wages were rising more where employment had declined, there is still a noticeable change in the slope of the cumulative effect after the treatment; this is very clear in the right-hand panel of Figure 4a, but even in the left-hand panel, the relationship is not linear. Hence, even if there are some leading correlations in the two-way fixed effects estimation that, unaccounted for, would lead to negative bias, the plots in Figure 4a show that there is an employment decline attributable to the minimum wage increase.

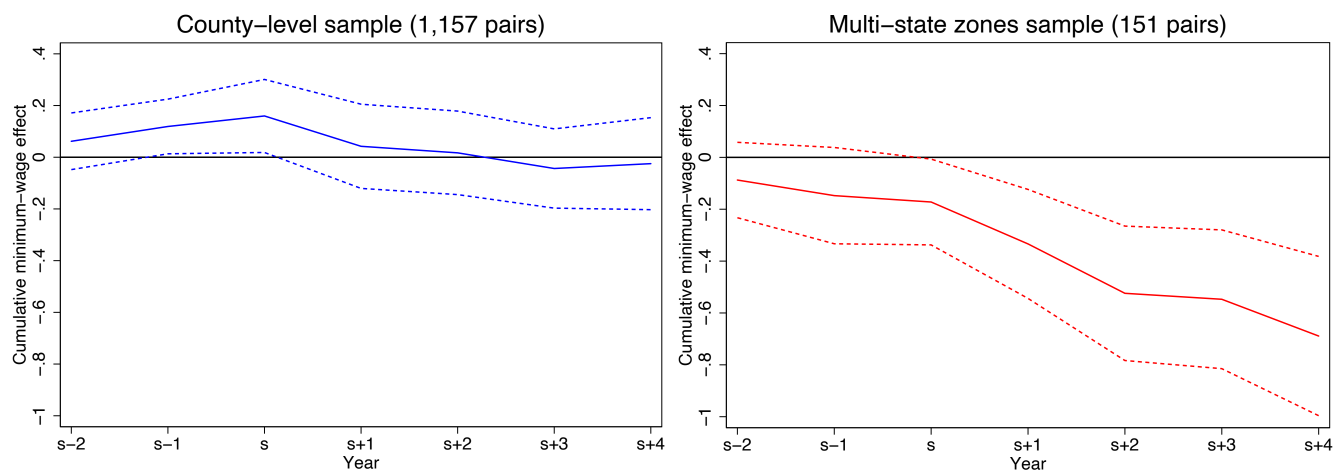
From the estimation of equation (6), Figures 4b and 4c reinforce our findings from Figure 3. Whereas the county-level plots show a non-significant long-term effect of minimum wages, the plots using pairs from multi-state commuting zones show large and significant negative effects of minimum wages in the medium and long terms, with stark changes in the slope of the cumulative effect occurring around time s. The cumulative minimum wage elasticity after four years is -0.689 in the estimation that uses the 151 complete pairs, and -0.512 in the estimation that uses the 71 complete pairs in DLR's QCEW data. In the first case, the cumulative four-year elasticity is 2.8 times larger than the contemporaneous elasticity in column 1 of Table 3 (-0.242), and in the second case, the four-year elasticity is two times larger than the elasticity in column 2 (-0.255).[27]

Whereas when using commuting-zone pairs we find that the better-identified local control ap-

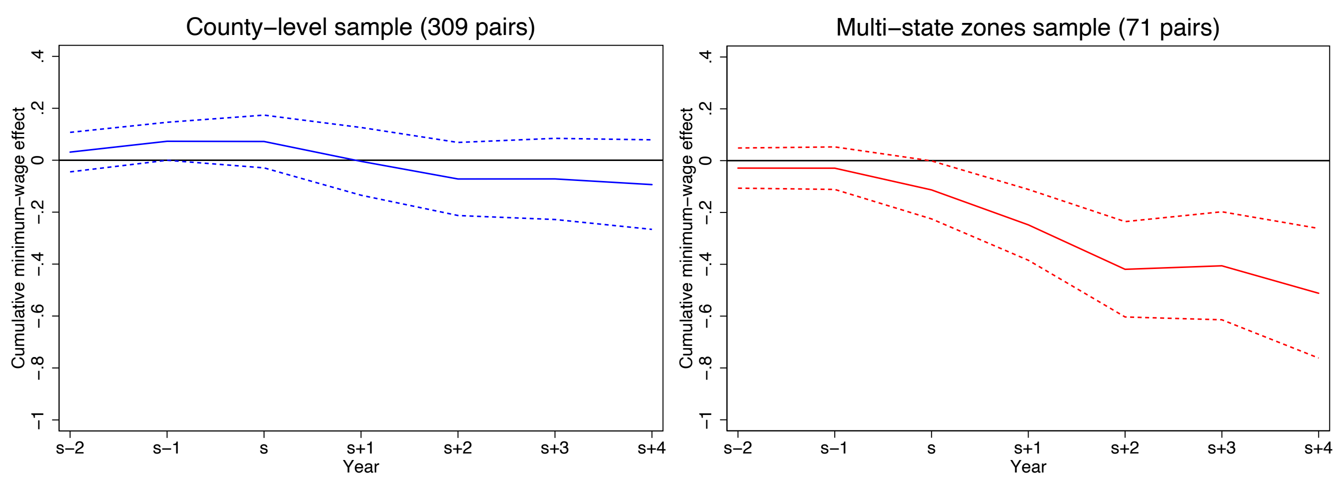
[27] The contrast between the estimated elasticities in the long-run and short-run underscores the potentially slow adjustment of capital to now-more-expensive labor. This is consistent with a budding literature spurred by Sorkin (2015) and Aaronson, French, Sorkin and To (2018).



(a) Conventional two-way fixed effects leads-lags estimation using full sample



(b) Estimation of equation (6) using all complete pairs



(c) Estimation of equation (6) using DLR's complete pairs

Figure 4: Time paths of minimum wage effects on employment with 90% confidence intervals using CBP data: County-level samples (left–blue) and commuting zone-state samples (right–red)

proach yields a larger estimated cumulative effect than the the two-way fixed effects approach, the opposite happens when using the county-level pairs. The preceding results—both short-term and longer-term—suggest that there may be upward bias in the estimation of the minimum wage elasticity of employment when using cross-border county pairs as local economic areas. We return to this question more fully in section 5.

4.2 Pre-existing Trends

To formally test for pre-existing trends, DLR estimate a leads-only version of their specification 6 similar to

$$\begin{aligned}\ln e_{ipt} =& \alpha + \beta_{-12}(\ln MW_{i,t+12} - \ln MW_{i,t+4}) + \beta_{-4}(\ln MW_{i,t+4} - \ln MW_{it}) + \beta_0 \ln MW_{it} \\ & + \zeta Z_{it} + \eta_i + \tau_{pt} + \nu_{ipt},\end{aligned} \tag{7}$$

where β_{-12} is the lead effect twelve quarters before the minimum wage change and β_{-4} is the cumulative effect four quarters before the change, so that if the minimum wage change occurs in quarter s, $\beta_{-4} - \beta_{-12}$ captures the pre-existing trend between $s - 12$ and $s - 4$.

Panel A in Table 7 shows $\hat{\beta}_{-12}$, $\hat{\beta}_{-4}$, $\hat{\beta}_0$ and $\hat{\beta}_{-4} - \hat{\beta}_{-12}$ from the estimation of equation (7) when using complete pairs from the CBCP and MCZP samples. As in DLR, in addition to the analysis of pre-trends in restaurant employment, we also estimate a version of equation (7) that uses total private sector employment as the dependent variable. The county-pair results for restaurant employment pre-trends are similar but not identical to those reported in DLR's Table 3, as we keep total private sector employment as a control in equation (7) (in addition to the population control) whereas DLR remove it—we also corrected the standard errors so that only complete-pair observations are used in their calculation. The results show no evidence of pre-trends for restaurant employment or total private sector employment for either the contiguous-county and multi-state commuting zones samples.

To test for pre-trends in the CBP data, we estimate a yearly version of equation (7) with β_{-3} denoting the lead effect three years before the change in the minimum wage, β_{-1} denoting the cumulative effect up to one year before the change, and $\beta_{-1} - \beta_{-3}$ capturing the trend between year $s - 3$ and year $s - 1$. Besides restaurant employment, we also test for pre-trends in total employment in the rest of the industries. Panel B in Table 7 shows $\hat{\beta}_{-3}$, $\hat{\beta}_{-1}$, $\hat{\beta}_0$ and $\hat{\beta}_{-1} - \hat{\beta}_{-3}$ when using all complete pairs in the county-level and multi-state zones samples. Whereas the county-level sample yields a positive trend for restaurant employment, the multi-state zones sample shows a negative and significant trend driven by $\hat{\beta}_{-1} = -0.166$. The large negative $\hat{\beta}_{-1}$ in the multi-state zones

Table 7: Testing for pre-trends in the pair-approach estimation

	County-level sample		Multi-state zones sample	
	Restaurant Employment	Aggregate Employment	Restaurant Employment	Aggregate Employment
	(1)	(2)	(3)	(4)
A. DLR's Quarterly Data (1990-2006)				
$\hat{\beta}_{-12}$	0.000	0.025	-0.098	0.025
	(0.053)	(0.053)	(0.062)	(0.054)
$\hat{\beta}_{-4}$	0.022	0.084	-0.074	0.056
	(0.129)	(0.112)	(0.111)	(0.100)
$\hat{\beta}_{0}$	0.002	0.172	-0.206*	0.101
	(0.115)	(0.174)	(0.115)	(0.121)
Trend ($\hat{\beta}_{-4} - \hat{\beta}_{-12}$)	0.022	0.058	0.025	0.031
	(0.095)	(0.073)	(0.074)	(0.063)
Number of pairs	316	316	73	73
Observations	37,896	37,896	8,758	8,758
B. CBP Yearly Data (1990-2016) — All Complete Pairs				
$\hat{\beta}_{-3}$	0.023	-0.034	-0.074	-0.065
	(0.062)	(0.037)	(0.066)	(0.058)
$\hat{\beta}_{-1}$	0.104	-0.019	-0.166**	-0.075
	(0.082)	(0.059)	(0.075)	(0.086)
$\hat{\beta}_{0}$	-0.042	-0.082	-0.420***	-0.215
	(0.097)	(0.089)	(0.125)	(0.146)
Trend ($\hat{\beta}_{-1} - \hat{\beta}_{-3}$)	0.080	0.015	-0.092*	-0.009
	(0.049)	(0.030)	(0.048)	(0.041)
Number of pairs	1,163	1,163	151	151
Observations	54,766	54,766	7,204	7,204
C. CBP Yearly Data (1990-2016) — DLR's Complete Pairs				
$\hat{\beta}_{-3}$	0.041	-0.018	-0.053	-0.065
	(0.050)	(0.040)	(0.051)	(0.063)
$\hat{\beta}_{-1}$	0.068	0.014	-0.105*	-0.076
	(0.063)	(0.057)	(0.053)	(0.090)
$\hat{\beta}_{0}$	-0.027	-0.006	-0.358***	-0.117
	(0.087)	(0.079)	(0.103)	(0.140)
Trend ($\hat{\beta}_{-1} - \hat{\beta}_{-3}$)	0.027	0.033	-0.052	-0.011
	(0.029)	(0.029)	(0.033)	(0.036)
Number of pairs	309	309	71	71
Observations	14,784	14,784	3,396	3,396

Notes: Panel A in this table reports $\hat{\beta}_{-12}$, $\hat{\beta}_{-4}$, $\hat{\beta}_{0}$, and $\hat{\beta}_{-4} - \hat{\beta}_{-12}$ from the estimation of specification (7) using DLR's QCEW data for contiguous county pairs and pairs from multi-state commuting zones. Panels B and C report similar coefficients but using yearly CBP data, with panel C restricting the sample to DLR's complete pairs. Aggregate employment is total private sector employment in panel A, and employment in all other industries in panels B and C. Columns 1 and 3 include controls for private employment (as in the earlier tables) and population. Standard errors (in parentheses) are two-way clustered at the state and border segment levels. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

sample, however, is not surprising, as minimum wage changes are typically announced months before implementation, which allows firms to prepare and adjust employment before the change goes into effect; see Karabarbounis, Lise and Nath (2022), Kudlyak, Tasci and Tuzemen (2023), and Leung (2021) for evidence pointing to leading adverse effects in anticipation of minimum wage increases.[28] Moreover, the size of the trend (-0.092) is about one third of the change that occurs between $s-1$ and s (-0.254), with the cumulative effect increasing in size from -0.166 to -0.420.

Thus, there is a clear change in the relationship between employment and minimum wages around time s. The positive pre-trend in the county-level sample is harder to explain as an anticipatory effect of minimum wages, unless one believes that higher minimum wages increase employment, and may instead point to a problematic positive pre-trend for the counties in cross-border pairs where the minimum wage increased; we return to this issue in section 5. For employment in the rest of the industries, panel B shows no significant evidence of pre-trends.

Panel C in Table 7 restricts the CBP sample to complete pairs used in DLR's estimation. Similar to panel B, $\hat{\beta}_{-1}$ is statistically significant in the multi-state commuting zone sample, indicating that restaurants may start reducing employment before the minimum wage change goes into effect. However, all of the evidence indicates that using commuting zones as local economic areas points to large negative effects of minimum wages on restaurant employment.

4.3 Event Study

An alternative approach to analyze the long-term effects of minimum wages on employment is by performing an event study. The most influential minimum wage paper following an event-study design is CDLZ, who use Current Population Survey (CPS) data to study the effects of minimum wages on employment across 25-cent wage bins.[29] Of course, our paper's objective is to assess DLR's influential pair approach for the restaurant industry using a better definition of local economic areas, and thus, a replication and re-analysis of CDLZ is very far from this objective; moreover, our CBP data does not allow us to split employment into different wage bins. Nevertheless, in this section we present an event-study analysis for overall restaurant employment, comparing it with the long-term effects of minimum wages using the pair approach outlined in section 4.1.

[28] It is perfectly natural in a dynamic labor demand model to expect employment (or the underlying flows on one side—vacancies) to respond prior to implementation. For example, Kudlyak et al. (2023) document leading vacancy responses. It is reasonable to expect a vacancy response early, as that seems an easy margin for employers to adjust if they expect a lower optimal employment level in the near future—rather than increases in firing, which can hurt morale and raise unemployment insurance costs. This is especially true in low-wage labor markets, where turnover is typically very high, so slowing hiring can reduce employment fairly quickly.

[29] Focusing on all workers (not just restaurant workers), Cengiz et al. (2019) use a stacked event-study approach and find no disemployment effects of minimum wages.

Starting from our equation (3)—with pair-period effects and multi-state commuting zone data—we follow the linear panel event-study design of Freyaldenhoven, Hansen, Pérez Pérez and Shapiro (2021) and estimate the following specification

$$\ln e_{ipt} = \alpha + \sum_{k=-M}^{M} \beta_k \Delta I_{i,t-k} + \beta_{M+1} I_{i,t-M-1} + \beta_{-M-1}(1 - I_{i,t+M}) + \gamma \ln E^{-}_{it} + \delta \ln P_{it} + \eta_i + \tau_{pt} + \nu_{ipt}, \tag{8}$$

where $I_{i,t}$ is an indicator function taking the value of 1 if the minimum wage increased for commuting zone-state i at time t, M denotes number of years over which a minimum wage event is thought to affect employment, and as is common in event studies, β_{-1} is normalized to zero. Following the suggestions of Freyaldenhoven et al. (2021), we estimate a symmetric model (so that the pre-trends horizon is as long as the assumed policy effect horizon) and also estimate the policy effects outside this range. Thus, we estimate $2(M+1)$ β parameters $(\beta_{-M-1}, \beta_{-M}, ..., \beta_{-2}, \beta_0, \beta_1, ..., \beta_M, \beta_{M+1})$, with β_k denoting the cumulative effect of a minimum wage event k periods from time t, for $k \in [-M, M]$, and β_{-M-1} and β_{M+1} denoting the effects of the policy more than M periods ahead or after the change, respectively.

Using the `xtevent` package developed by Freyaldenhoven et al. (2021) to estimate equation (8), in Figure 5 we present event-study plots that not only show the estimated cumulative effects along with 95% confidence intervals, but also include Wald tests for pre-trends and for the leveling-off of the cumulative coefficient—the null of no pre-trends is $H_0 : \beta_{-M-1} = \beta_{-M} = ... = \beta_{-2} = 0$, and the null of leveling-off dynamics is $\beta_M = \beta_{M+1}$.

Similar to section 4.1, we start by using $M = 4$, so that we assume that minimum wage events affect restaurant employment for four years. The first plot of Figure 5 shows similar results to those obtained above when using pair-period effects with multi-state commuting zones: a minimum wage event reduces restaurant employment, with the effect becoming stronger over time. Although the plot shows a mild positive pre-trend, none of the pre-trend coefficients is significant and we cannot reject the null of no pre-trends. On the other hand, the cumulative effect does not show signs of leveling off after four years, with the leveling-off null being rejected at conventional levels. To look further into this issue, Figure 5 also shows event-study plots for $M = \{5, 6, 7\}$. For $M = 5$, the second plot shows that the negative cumulative effect of minimum wages on restaurant employment continues to increase after five years. However, as shown by the plots for $M = \{6, 7\}$, the effect levels off after six years.[30] These event-study estimates, using multi-state commuting zones to construct close controls, show clear evidence that minimum wage increases reduce restaurant employment.

[30]Note that the second and fourth plots show small p-values for the pre-trend test. However, all pre-trend coefficients are non-significant and the two plots show no obvious pre-trend.

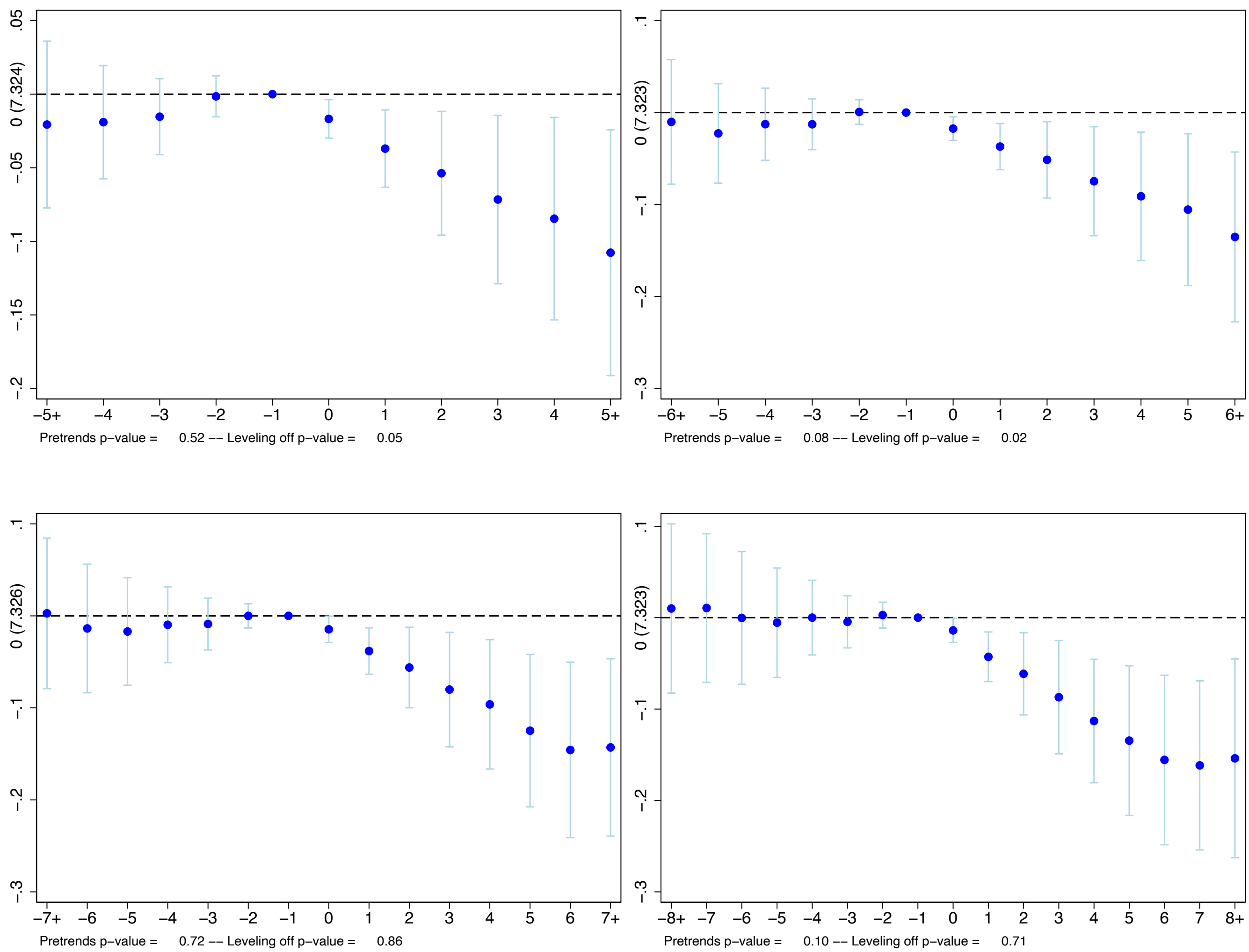


Figure 5: Event-study plots: Cumulative effects of minimum wage events on restaurant employment for different horizons

4.4 Common Shocks in Local Economic Areas

DLR's main argument is that their specification 6, with county-border pairs, controls for spatial heterogeneity by accounting for local economic shocks. This paper shows that the definition of local economic area as two counties sharing a state border is crucial for DLR's result of a near-zero effect of minimum wages on employment in the U.S. restaurant industry. By instead using multi-state commuting zones to define local economic areas—consistent with the actual definition of commuting zones—we find statistically significant, negative, and persistent effects of minimum wages on restaurant employment. Hence, to help interpret the evidence, it is important to look at how each definition of local economic area captures common shocks.

Using complete pairs from both DLR's QCEW data and the CBP data, Table 8 presents within-

Table 8: 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
C. CBP Yearly Data (1990-2016) — DLR's Complete Pairs								
Employment^{-}	0.21	0.33	0.35	0.41	0.35	0.48	0.53	0.58
Population	0.46	0.49	0.48	0.50	0.62	0.66	0.66	0.67
Bartik shock	0.87	0.88	0.87	0.88	0.91	0.92	0.90	0.91

pair correlations for one-, two-, three-, and four-year log changes in the controls used by DLR (total private sector employment and population) and in the controls used in our CBP-data estimations (total employment in the rest of the industries and population), as well as for Bartik shocks that capture predicted changes in labor demand in each commuting zone-state—as a consequence of national industry-level employment changes—while accounting for local specialization patterns.[31] In Table 8, we use Δ_λ to denote log changes of λ years. Panel A shows correlations using DLR's QCEW complete pairs, panel B uses all complete pairs in the CBP data, and panel C restricts the CBP sample to DLR's complete pairs. For all samples and all types of shocks, Table 8 shows that within-pair correlations are always larger for pairs within multi-state commuting zones than for contiguous county pairs, by an average (across all 32 comparisons) of 36%, bolstering the use of multi-state commuting zones as local economic areas to control for time-varying spatial heterogeneity.

However, the previous correlations characterize the pattern of aggregate shocks. They do not measure shocks to restaurant employment, nor do they measure contemporaneous correlations between shocks and minimum wage changes. And there is still an open question of what causes the

[31]We define Bartik shocks at the commuting zone-state level using AADHP's 479 industries. Similar to Autor, Dorn, Hanson and Majlesi (2020), the Bartik shock for commuting zone-state i from t_{start} to t_{end} is given by $\sum_j \left(\frac{e_{ijt_{\text{start}}}}{e_{it_{\text{start}}}}\right)\left[\ln e^{-i}_{jt_{\text{end}}} - \ln e^{-i}_{jt_{\text{start}}}\right]$, where e_{ijs} is the commuting zone-state i's employment in industry j at time s, $e_{it_{\text{start}}}$ is total employment in commuting zone-state i's at time s, and e^{-i}_{js} is industry j's employment across all U.S. commuting zone-state entities with the exception of commuting zone-state i at time s.

apparent positive bias in the cross-border county pair estimates. A possible explanation is that, because shocks are not common to the cross-border counties in these pairs (at least compared to commuting zones), the minimum wage tends to be increased when there is a positive unobserved shock to restaurant/low-skilled employment (Neumark, 2019, pp. 308-309). Before getting into a detailed discussion of potential positive bias in the DLR estimates in section 5, we turn next to the issue of potential spillovers.

4.5 Potential Spillover Effects

DLR estimate specifications to test for spillover effects within county pairs, across state borders. In the competitive model, border counties may be more likely to lose jobs because they can move across the border, compared to interior counties, leading to "amplification." In a search model the opposite can happen ("attenuation"), because the higher minimum wage may induce more search in border counties from workers across the border, leading to more job openings. DLR's spillover analysis rests on comparing estimates in border and interior counties. But DLR's key contention is that there is spatial heterogeneity such that shocks at the border that might be correlated with minimum wage changes differ from shocks in the interior. If so, then their tests are invalid, since we cannot distinguish between different minimum wage effects in border and interior counties and different shocks correlated with minimum wages. If there is this kind of spatial heterogeneity, then an alternative approach to testing for spillovers is needed. If there is not—and the shocks are the same in border and interior areas—then state-level panel data estimates are valid and we do not need DLR's border-county design. (Equivalently—as we find above using commuting zones—the estimates should be quite similar with or without the cross-border pair-period fixed effects.)

The recent literature on the effects of minimum wages on cross-state commuting patterns of low-wage workers does not point conclusively to either amplification or attenuation evidence. Whereas Kuehn (2016) and Shirley (2018) find an increase in commuting of low-wage workers towards states with minimum wage increases (*i.e.*, attenuation), McKinnish (2017) finds the opposite using similar American Community Survey (ACS) data, as does Pérez Pérez (2022) using Local Origin and Destination Employment Statistics (LODES) data from the U.S. Census. Although not a cross-state analysis, Jardim et al. (2018) find that Seattle's 2015 and 2016 minimum wage increases reduced hours worked in the city, but that the more experienced low-wage workers increased their hours outside the city to make up for some of these losses (amplification). Nevertheless, Jardim et al. (2022b) find that these minimum wage increases also reduced hours worked outside Seattle, which leads to attenuation.

Importantly, spillovers would not invalidate the negative employment effect we find. If there is amplification, we may overstate the effect in the affected area alone, but it still must be negative (and imply that a higher minimum wage reduces employment in the affected area). And if there is attenuation, then our negative estimate is understated. That said, we think spillovers are unlikely to be quantitatively important in the restaurant sector, because restaurants serve very local customers (Liu, Han and Cohen, 2015), in contrast to, for example, business-to-business services that could much more easily relocate across a border in the same commuting zone. Relatedly, there is evidence that restaurants are more likely to increase prices rather than relocate after a minimum wage hike (see Romich et al., 2020, and Allegretto and Reich, 2018); and whereas Dharmasankar and Yoo (2022) find evidence of spillovers after a minimum wage increase in Seattle, they find that the effects are concentrated in retail, not hospitality, consistent with the view that people do not travel much to eat, but do to shop.

Given that spillovers driven by changes in cross-border commuting patterns mostly occur in areas located very close to the state border, Kuehn (2016) applies the control-ring approach of Neumark and Kolko (2010) to obtain a spillover-free estimate of the minimum wage elasticity of employment. The idea is to exclude the area of the control region that is the closest to the state border so that the likelihood of spillover effects is minimized while still keeping a local economic area subject to common shocks. For the same reason, in their analysis of the minimum wage effects on Seattle's employment, Jardim et al. (2022a) exclude King County areas outside the city from their control region. Along these lines, below we exploit our commuting-zone definition of local economic area and obtain a "spillover-free" estimate of the minimum wage elasticity by constructing a sample of pairs of *non-contiguous* cross-border counties within the same commuting zone—that is, there is a buffer zone between the counties in each pair while the counties still belonging to the same local economic area. Our spillover-free estimate suggests a small attenuation bias in our main minimum wage elasticity estimate. We show this result in the next section because this analysis integrates naturally with our analysis of how the results depend on which cross-border counties are used as close controls.

5 Disentangling Sources of Discrepancies Across Estimates

One key evidence is that when we use the CBP dataset through 2016 and identify the minimum wage employment effect only from the variation within multi-state commuting zones and across state borders, the size of the estimated employment elasticity only modestly declines relative to the standard two-way fixed effects (TWFE) estimator, remaining negative and statistically significant. The TWFE estimated elasticities are -0.338 when using the full sample and -0.299 when restricting the sample to multi-state commuting zones, both significant at the 1% level (Table 2, columns 1 and 4). The cross-border estimate using multi-states commuting zones and including pair-period effects is -0.242, significant at the 5% level (see Table 3, column 1). In contrast, when we implement the DLR cross-border county approach with these data—not restricting the close controls to cross-border counties in the same commuting zone—the estimated elasticity shrinks considerably towards zero. The TWFE estimates are -0.362 when using the full county-level sample and -0.309 when restricting the sample to border counties, both significant at the 1% level (Table A-5 in the Appendix, columns 1 and 2); and the estimated elasticity using cross-border counties with pair-period effects is -0.081, not significant (Table 3, column 3). The latter estimates parallel DLR's results, although with much more complete data.

Thus, the cross-border design using multi-state commuting zones barely changes anything, while the cross-border design using county pairs leads to a much different conclusion—that higher minimum wages do not reduce restaurant employment. This raises the obvious question of whether we can reconcile the different results and arrive at an understanding of whether one estimate or the other is more convincing.

After describing potential biases in estimated minimum wage employment effects, this section shows that the contiguous-county-pair estimate of -0.081 is biased upward, and that restricting the county-pair sample to pairs within multi-state commuting zones yields an estimate that is closer to -0.242. That is, cross-border pairs whose counties belong to different commuting zones bias the estimate of the minimum wage elasticity of employment toward zero.

5.1 Potential Biases in Estimated Minimum Wage Effects

The cross-border research design is intended to control for shocks to state economies that are correlated with minimum wage changes. If one is to believe that the DLR cross-border county research design captures these shocks, then the fact that the estimated minimum wage elasticity moves from strongly negative to near zero would imply that these shocks (conditional on the controls) are negatively correlated with minimum wage variation, so that the cross-border design removes this source of negative bias in the TWFE estimates of the employment effect of minimum wages. This of course is possible, but there are two potential problems with this interpretation.

First, it is not clear that this is the direction of bias. One might speculate that the correlation could go the other way—with policymakers raising minimum wages in concert with positive shocks.

Dube, Lester and Reich (2010) have put forward conflicting arguments, evidence, and interpretations. In Allegretto, Dube and Reich (2011), two of the three co-authors of DLR suggest that minimum wage increases "are often enacted when the economy is expanding and unemployment is low. But, by the time of implementation, the economy may be contracting ... leading to a spurious [negative] time series correlation between minimum wages and employment" (p. 212), and they cite evidence in Reich (2009). In fact, Reich argues the opposite: "minimum wage increases are voted, almost without exception, and are mostly implemented, in times of growing employment. The pattern holds both for federal and state increases" (p. 13). After first showing that minimum wage increases were "much more likely to occur in times of stronger employment growth" (p. 12), he then notes that "The picture is similar for the implementation years... Of the sixteen implementation events, employment grew at above average rates in nine, grew below average but positive in five, and fell in two" (p. 12). This is also in contrast to DLR's interpretation of their evidence.

Second, and more substantively important, if employment shocks negatively correlated with minimum wage increases bias the TWFE estimates, then why do we not see the same change in the estimated employment elasticity from negative to zero in the cross-border research design using multi-state commuting zones? This is even more puzzling if, as Allegretto, Dube and Reich (2009) argue (an argument with which we agree), multi-state commuting zones are better able to control for common economic shocks on opposite sides of the border.

These considerations suggest that we need an alternative explanation—and in particular, one that can account for the substantial change in the estimated elasticity when using cross-border counties, but not when using cross-border areas of multi-state commuting zones. Of course, one possibility is that the cross-border research design using bordering counties does not eliminate negative bias, but introduces positive bias. As already noted, if minimum wages tend to be increased in concert with positive employment shocks, but cross-border counties do not control well for shocks, then the DLR research design using cross-border counties could introduce upward bias. Or, as originally suggested in Neumark, Salas and Wascher (2014), in a case like this, minimum wage increases within similar geographic areas could be more endogenous with respect to economic shocks, rather than less. In contrast, if cross-border areas of multi-state commuting zones (MSCZs hereafter) do control well for shocks that are common on both sides of the border, then the absence of much change in the estimated elasticity from the cross-border research design applied to MSCZs would imply that there is not much bias in the MSCZ estimator.

The argument, developed in Neumark (2019), harkens back to the literature on within-family estimates of the returns to schooling, where Griliches showed that whether or not bias is reduced when we difference between observations with similar unobservables (like identical twins) depends on what generates the variation within versus across units. To see this in a simple setting, suppose we have only two years of data, form the first differences between treated states (s) and bordering states (s'), and estimate

$$\Delta \ln e_s - \Delta \ln e_{s'} = \beta \Delta \ln MW_s + \gamma(\Delta Z_s - \Delta Z_{s'}) + (\Delta\varepsilon_s - \Delta\varepsilon_{s'}). \tag{9}$$

(Note that $\Delta \ln MW_{s'} = 0$, since s' denotes the untreated states.)

Suppose there is a shock, $\Delta\theta_s$, correlated with $\Delta \ln MW_s$. If we assume the shock for state s' ($\Delta\theta_{s'}$) is the same, then it drops out of equation (9) and we obtain an unbiased estimate of β. In contrast, if we use control states further away, the shocks are less likely to be the same, and estimators that do not rely solely on close controls will be biased. This is the rationale for close-controls estimators.

But the assumption that the shock is identical in the treatment and close-control states is likely not strictly true. That is, there is an omitted variable in equation (9) equal to $(\Delta\theta_s - \Delta\theta_{s'})$. The simple intuition that might still rationalize the close-controls estimator is that the difference in shocks must be a good deal smaller than the difference in shocks between a treatment state and some other (not close) state or set of states. However, this does not necessarily imply less bias. The omitted variable bias in equation (9), ignoring the Z terms, is

$$\frac{\mathrm{cov}(\Delta\theta_s - \Delta\theta_{s'}, \Delta \ln MW_s)}{\mathrm{var}(\Delta \ln MW_s)}. \tag{10}$$

(Formally, this is the inconsistency, derived from taking probability limits.) The only assertion about the shocks in different sets of states that is compelling a priori is that $\mathrm{var}(\Delta\theta_s - \Delta\theta_{s'})$ is smaller for nearby states than more-distant states. But equation (10) shows that two different magnitudes form the bias in the close-controls estimator.

First, is $\mathrm{cov}(\Delta\theta_s - \Delta\theta_{s'}, \Delta \ln MW_s)$ necessarily lower for close states? This takes us back to the question of what drives minimum wage variation between nearby states. Here is one possibility in which the covariance would be higher for nearby states: Suppose policymakers respond to changes in low-skill labor markets in setting minimum wages, but they also respond to other factors. In two distant states, because they differ on many dimensions, the other factors (or, more precisely, changes in those factors), vary more. In contrast, in bordering states, because of their assumed homogeneity, the other factors do not differ. In that case, even though $\mathrm{var}(\Delta\theta_s - \Delta\theta_{s'})$ is higher for the more-distant state pairs, $\mathrm{cov}(\Delta\theta_s - \Delta\theta_{s'}, \Delta \ln MW_s)$ is higher for the bordering states.

Second, the denominator in equation (10), $\mathrm{var}(\Delta \ln MW_s)$, is generally lower for nearby states, because of a strong regional component to minimum wages; for example, New England states are more likely to border other New England states that tend to have higher minimum wages. This, in itself, will exacerbate the bias in the close-controls estimator.[32]

Finally, depending on the geographic units used as close controls, $\mathrm{cov}(\Delta\theta_s - \Delta\theta_{s'}, \Delta \ln MW_s)$ can vary. In particular, we have suggested that this covariance may be higher for cross-border county pairs than for cross-border areas of MSCZs, precisely because shocks are more common on the two sides of the border in MSCZs.

Is there evidence consistent with this? At first blush, this explanation might seem unlikely, because many cross-border counties are in MSCZs, and we would not expect any difference between the extent to which cross-border counties in MSCZs capture common shocks associated with minimum wage changes than do other counties in MSCZs that are on opposite sides of the border but are not across the border from each other. However, the set of cross-border counties used includes many county pairs that are not within the same MSCZ, and these county pairs may not provide good controls for common shocks. As it turns out, the evidence below is completely consistent with this explanation.

5.2 Estimated Minimum Wage Effects in Different County-Pair Subsamples

Our CBP county-pair sample used in the estimation of column 3 in Table 3, which reports an estimated minimum wage elasticity of employment of -0.081, contains 1,165 cross-border pairs of contiguous counties. To verify if it matters whether or not contiguous-county pairs belong to the same commuting zone, we start by splitting this sample into two subsamples: the first includes the 843 pairs whose counties in each pair belong to different commuting zones (*subsample 1*), and the second includes the remaining 322 pairs whose counties in each pair belong to the same commuting zone (*subsample 2*).[33] Based on the argument that commuting zones better define local economic areas, it makes sense that two cross-border counties within the same commuting zone face common shocks even if they are not contiguous. Hence, we can create a sample that includes all cross-border county pairs (contiguous and non-contiguous) that span from multi-state

[32] Based on the full sample of contiguous county pairs used in the estimation of column 3 of Table 3, the variance of the ln(minimum wage) conditional on county, pair-period effects, and the employment and population controls is 0.00134, whereas the conditional variance is 0.00165 (*i.e.*, 23 percent higher) if we include period effects rather than pair-period effects.

[33] Whereas all pairs in the second subsample belong to multi-state commuting zones, each pair in the first subsample can be the result of either two counties that belong to different single-state commuting zones, or two counties that belong to different multi-state commuting zones, or one county from a single-state commuting zone and the other from a multi-state commuting zone. (Recall that all counties in the U.S. are assigned to a commuting zone.)

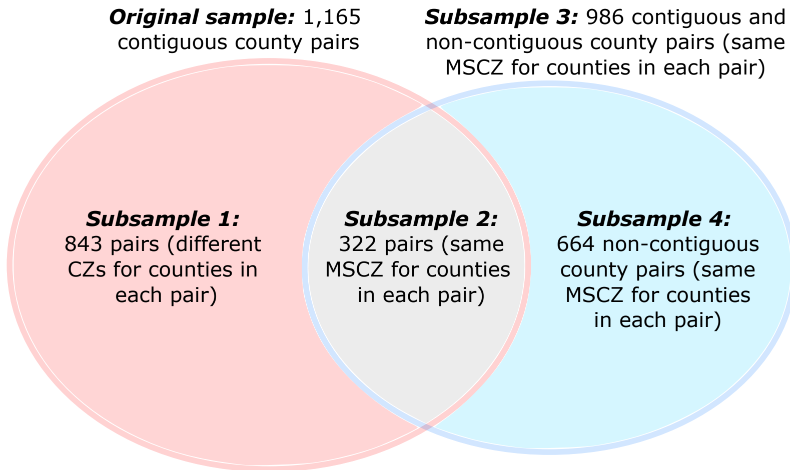


Figure 6: The cross-border county pair subsamples

commuting zones. This gives us *subsample 3*, which includes 986 pairs—322 pairs from subsample 2 plus 664 cross-border pairs where the two non-contiguous counties in each pair belong to the same commuting zone. Finally, *subsample 4* is a subset of subsample 3 that only includes the 664 pairs from non-contiguous counties. Given potential spillover effects of minimum wages—likely to be more important among contiguous counties—the objective of the last subsample is to obtain an elasticity estimate that is the closest to a spillover-free estimate (see section 4.5). Figure 6 presents a summary of our fours cross-border county pair subsamples.

For log restaurant employment, panel A in Table 9 reports conventional TWFE estimates—with county and year effects—using the county-level panel datasets corresponding to each subsample (as indicated in each column).[34] Note that the estimated minimum wage elasticity of employment is significant at the 1% level across subsamples, with values ranging from −0.293 to −0.414.

However, panel B in Table 9 shows very different results across subsamples when we use DLR's cross-border research design that includes pair-period fixed effects. When we use pairs of contiguous counties that belong to different commuting zones (subsample 1), the estimated elasticity falls to −0.047 (much like DLR's estimates), whereas for subsample 2—which uses cross-border pairs of contiguous counties that belong to the same commuting zone—the estimated elasticity falls only to −0.160 (with a p-value of 0.141). The results for subsample 3 show that when we add pairs of non-contiguous counties to the sample of same-commuting-zone pairs, the estimated elasticity is significant at the 10% level and equal to −0.244 (very close to the −0.242 estimate in column 1

[34] Alternatively, we could also directly use the county-pair subsamples and estimate equation (3) with the restriction $\tau_{pt} = \tau_t$ (this is DLR's specification 5). Although these regressions would include multiple repeated observations (as a county may appear in several pairs), the estimated coefficients are very similar to those reported in panel A of Table 9.

Table 9: Estimation of restaurant employment responses to minimum wage changes for different county-level samples using CBP 1990-2016 data

	Pairs formed by contiguous counties not in same CZ	*Pairs formed by contiguous counties in same MSCZ*	*Pairs formed by contig. and non-contig. counties in same MSCZ*	*Pairs formed by non-contig. counties in same MSCZ*
	Subsample 1	**Subsample 2**	**Subsample 3**	**Subsample 4**
A. Conventional TWFE				
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)
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
B. With pair-period effects				
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)
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

Notes: In panel A, this table reports $\hat{\beta}$, $\hat{\gamma}$, and $\hat{\delta}$ from the estimation of a county-level version of specification (2) with $\tau_{ct} = \tau_t$ and no time trends using yearly data from 1990 to 2016. Panel B reports $\hat{\beta}$, $\hat{\gamma}$, and $\hat{\delta}$ from the estimation of specification (3) using yearly county-pair data from 1990 to 2016. Each column indicates the subsample used. Standard errors (in parentheses) are clustered at the state level in panel A, and are two-way clustered at the state and border segment levels in panel B. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

of Table 3). Moreover, the spillover-free estimate from subsample 4—which uses only cross-border pairs of non-contiguous counties within the same commuting zone—is -0.286 (with a p-value of 0.137), consistent with a small attenuation bias in the estimate from subsample 3. Given that subsample 3 uses the maximum number of pairs formed by counties in the same commuting zone, we consider the estimated value of -0.244, which is prone to underestimation (due to attenuation), as the most persuasive evidence suggesting a reduction in restaurant employment following minimum wage increases.

These results lead to two conclusions. First, there is nothing fundamentally different about using counties versus using commuting zones. Whether we use cross-border areas of MSCZs, or cross-border counties that are in the same MSCZ, we still get a sizable negative employment elasticity using the cross-border research design.[35] Second, and more important, it is only when the cross-border county research design uses pairs formed by counties that are not in the same commuting zone (as in subsample 1 and the original 1,165-pair sample) that this research design undermines the conclusion that a higher minimum wage reduces restaurant employment.

We obtain further evidence bolstering this conclusion from the same test that DLR use—namely estimating whether there are pre-trends associated with minimum wage increases. And recall what kind of evidence is most interesting. As noted earlier, evidence that employment declines somewhat before the minimum wage increases is not evidence of a spurious relationship. This is because minimum wage hikes are enacted before they are implemented, and there is evidence indicating leading adverse effects associated with announced minimum wage increases (see Karabarbounis et al., 2022 and Kudlyak et al., 2023). But evidence that employment is increasing where minimum wages are increased cannot be viewed as an "anticipation" effect. More importantly, if we find evidence that employment was increasing where minimum wages are increased when the pair-period effects are introduced, that would imply that positive bias in estimated employment effects is not controlled for by these pair-period fixed effects—despite the intention of DLR's approach to use these fixed effects to control for bias from employment shocks that are correlated with minimum wage changes.

In Table 10, we show that it is precisely when identification of minimum wage effects focuses

[35] Note that the estimation is more efficient when using cross-border areas of multi-state commuting zones (as in column 1 of Table 3) rather than cross-border county pairs. One might point out that the elasticity estimates of -0.160 and -0.286 in columns 2 and 4 of panel B in Table 9 are not significant at conventional levels. However, considering the long-prevailing consensus on the employment elasticity of the minimum wage being about -0.1 to -0.2 for low-skill workers at the time DLR was written, an estimate of -0.160 or -0.286 from their cross-border county analysis would not have been viewed as evidence contradicting this consensus, even while accounting for potential sensitivity in the strength of the statistical evidence.

Table 10: Testing for restaurant employment pre-trends for different samples

	Pairs formed by contiguous counties not in same CZ	*Pairs formed by contiguous counties in same MSCZ*	*Pairs formed by contig. and non-contig. counties in same MSCZ*	*Pairs formed by non-contig. counties in same MSCZ*
	Subsample 1	**Subsample 2**	**Subsample 3**	**Subsample 4**
A. Conventional TWFE				
$\hat{\beta}_{-3}$	-0.155*	-0.055	-0.103	-0.130
	(0.091)	(0.096)	(0.082)	(0.089)
$\hat{\beta}_{-1}$	-0.207*	-0.106	-0.208**	-0.314***
	(0.120)	(0.106)	(0.098)	(0.110)
$\hat{\beta}_{0}$	-0.426***	-0.258*	-0.465***	-0.592***
	(0.153)	(0.142)	(0.167)	(0.190)
Trend ($\hat{\beta}_{-1} - \hat{\beta}_{-3}$)	-0.052	-0.051	-0.105	-0.184**
	(0.072)	(0.069)	(0.066)	(0.076)
County effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Number of counties	928	458	742	557
Observations	22,035	10,926	17,676	13,260
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

Notes: In panel A, this table reports $\hat{\beta}_{-3}$, $\hat{\beta}_{-1}$, $\hat{\beta}_{0}$, and $\hat{\beta}_{-1} - \hat{\beta}_{-3}$ from the estimation of a county-level version of specification (2) expanded to account for pre-trends, with $\tau_{ct} = \tau_t$ and using yearly CBP data from 1990 to 2016. Panel B reports $\hat{\beta}_{-3}$, $\hat{\beta}_{-1}$, $\hat{\beta}_{0}$, and $\hat{\beta}_{-1} - \hat{\beta}_{-3}$ from the estimation of a yearly version of specification (7) using 1990-2016 CBP data for different cross-border county-pair samples. Each column indicates the subsample used. Standard errors (in parentheses) are clustered at the state level in panel A, and are two-way clustered at the state and border segment levels in panel B. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

on variation within pairs whose counties belong to different commuting zones that this positive bias shows up. Panel A of Table 10 shows the TWFE estimates for the four subsamples. Note that the trend coefficient is negative in all TWFE estimates, being significant only for subsample 4. However, in panel B—when the pair-period fixed effects are added—we see evidence of a positive and significant pre-trend of 0.114 when using cross-border pairs formed by counties from different commuting zones (subsample 1), whereas for the other cases the estimates are near zero or negative.

This evidence confirms that the cross-border research design produces a positively biased estimate of the minimum wage employment elasticity when focusing on pairs of counties not in the same commuting zone. In contrast, whether one applies the cross-border research design to MSCZs themselves (which we think is most compelling as they better capture common shocks—see section 4.4—and yield more efficient estimates), or to pairs of counties in the same MSCZs, the cross-border research design confirms that higher minimum wages reduce restaurant employment.[36]

We think there is one more type of evidence that reinforces the conclusion that DLR's estimates are biased towards finding no employment effect. In particular, other work has recognized the potential correlation between minimum wages and unobserved shocks, but using different matching approaches to such bias—such as Powell (2022), Karabarbounis, Lise and Nath (2022), and Clemens and Wither (2019) (who match on similar workers)—continues to find disemployment effects. Although these "matching" estimators can control for prior changes in outcomes correlated with minimum wage changes, they do not control for contemporaneous correlations. The close controls can, as can IV estimates (see, e.g., Baskaya and Rubinstein, 2015). (These studies and the comparisons of results are discussed in Neumark, 2019.) Thus, rather than there being a general result that considering correlations between minimum wages and unobserved shocks leads to no detectable job loss from minimum wages, this conclusion appears to be largely unique to the DLR approach—and, as we have shown, to using cross-border counties that are not in the same commuting zone.

5.3 Modeling Shocks: Simulated Data and Estimation Bias Analysis

To delve deeper into the characteristics of the data generating process that we interpret as driving the results of Table 9, we use artificially generated data to examine the possible combination of

[36] If we ignore the evidence of a positive pre-trend when applying the cross-border design using pairs formed by counties from different commuting zones, one may argue that our findings in panel B of Table 9 simply reflect heterogeneous effects of minimum wages, with less adverse employment effects in the more rural counties that are not in multi-state commuting zones (the intuition being that more rural counties have less competitive labor markets). Along these lines, section C in the Appendix makes two important points. First, it shows that a greater fraction of the population lives in counties where the impact of minimum wages on employment is negative. Second, it shows that the differential results in different subsamples cannot be explained by varying degrees of monopsony power, as captured by employment concentration.

shocks that could lead to these results. Assuming a negative elasticity of restaurant employment to minimum wages, these shocks should meet the following conditions:

(i) There should be some negative bias in TWFE elasticity estimates in all subsamples (as shown in panel A of Table 9).

(ii) In specifications with pair-period effects, the inclusion of pairs consisting of counties from different commuting zones should yield an estimated elasticity biased toward zero. This was observed with an estimate of -0.047 for subsample 1 in panel B of Table 9, and an estimate of -0.081 in column 3 of Table 3 using the original 1,165-pair sample.

First, to address condition *(i)*, it is necessary to introduce a shock that is negatively correlated with minimum wage changes. In line with this, the original argument of DLR was that TWFE estimates are downward biased due to employment shocks that are negatively correlated with minimum wage changes, and that these shocks can be controlled for by pair-period effects. Therefore, for this type of shock our simulation exercise considers state-pair shocks, so that employment changes within neighboring state pairs are negatively correlated with minimum wage changes. If we only incorporate this shock, our TWFE estimates would exhibit a downward bias, while our pair-period estimates would be unbiased across all subsamples.

Second, to address condition *(ii)*, a shock at the commuting zone level that is positively correlated with minimum wage changes is sufficient. This shock is controlled for in pair-period fixed effects regressions when pairs consist of cross-border counties in the same commuting zone (*i.e.*, when both counties in each pair belong to the same local labor market), but not when pairs consist of cross-border counties from different commuting zones, causing a positive bias and pre-trend in the latter case. The idea here is that states also worry about losing jobs to cross-border areas in the same commuting zone, since that is where workers can go look for jobs. So they are, all else the same, still more likely to raise the minimum wage when their low-skilled labor market is doing well relative to the cross-border commuting zone areas (or the cross-border area is doing badly), since then the flow of jobs across the border (and attendant loss of income tax revenue) is viewed as less likely to happen.

We assume that the true minimum wage elasticity of restaurant employment is -0.2. Using actual minimum wage data, the simulated log employment for county i in pair p in year t is defined as follows:

$$\widetilde{\ln e_{ipt}} = \eta_i - 0.2 \ln MW_{it} + \varphi_{zt} + \psi_{\mathcal{P}t} + \nu_{ipt}, \tag{11}$$

with county i belonging to commuting zone z, and pair p belonging to state-pair $\mathcal{P}$. In (11), η_i denotes a county fixed effect, φ_{zt} represents the shock at the commuting-zone level (positively correlated with minimum wage changes), $\psi_{\mathcal{P}t}$ is the state-pair shock (negatively correlated with minimum wage changes), and ν_{ipt} is a normally distributed random term that is independent of minimum wage changes. The shocks φ_{zt}, $\psi_{\mathcal{P}t}$, and ν_{ipt} have mean zero and standard deviations denoted by σ_φ, σ_ψ, and σ_ν, respectively. Additionally, we use ρ_+ to denote the correlation coefficient between φ_{zt} and minimum wage changes, and ρ_- to denote the correlation between $\psi_{\mathcal{P}t}$ and minimum wage changes.[37]

Using (11), we simulate data for the $1,829$ county pairs that are used across the four subsamples in Table 9 (see also Figure 6). We assume that η_i is uniformly distributed in the interval $[0.3, 12.14]$ to match the 6.22 average log employment across counties.[38] After noticing that the root mean squared errors (RMSEs) of the regressions in Table 9 range between 0.24 and 0.28 in the TWFE regressions, and between 0.22 and 0.27 in the pair-approach regressions, we assume $\sigma_\varphi = 0.17$, $\sigma_\psi = 0.13$, and $\sigma_\nu = 0.2$ to get close to those RMSEs. After this, we look for the values of ρ_+ and ρ_- that yield average estimated elasticities that are close to the -0.081 from column 3 in Table 3—for the specification that uses the original 1,165 contiguous-county-pair sample—and to the -0.047 from subsample 1 in panel B of Table 9.

Using $\rho_+ = 0.20$ and $\rho_- = -0.45$, Figure 7 displays the distributions of the TWFE (left) and pair-approach (right) elasticity estimates from regressing $\widetilde{\ln e_{ipt}}$ on $\ln MW_{it}$ across 500 simulations. The figure presents distributions for the four subsamples in Table 9, in addition to the sample containing all 1,165 contiguous-county pairs. Notice that the assumed combination of shocks offers a plausible explanation for our findings from Table 9.[39] On one hand, the TWFE specification fails to control for either of the shocks. While these shocks counteract each other, the negative shock has a stronger impact, resulting in an overall net negative bias across all samples in the TWFE

[37] This is the process we follow to create commuting zone shocks positively correlated with minimum wage changes. First, for each commuting zone z we create a variable y_{zt} that is normally distributed over time, with mean zero, standard deviation σ_y, and independent of minimum wage changes. Letting x_{zt} denote the minimum wage change in commuting zone z from $t-1$ to t, with μ_x and σ_x representing its mean and standard deviation, we define φ_{zt} as $\varphi_{zt} = \rho_+ \left({}^{\sigma_y}/_{\sigma_x}\right)(x - \mu_x) + y\sqrt{1-\rho_+^2}$. Note that $E(\varphi_{zt}) = 0$ and $\sigma_\varphi = \sigma_y$. We follow a similar process to create $\psi_{\mathcal{P}t}$.

[38] In the data, across-years average log employment ranges between 0.40 and 12.48 for the 1,328 counties that make our 1,829 pairs.

[39] Starting from the sample with all contiguous pairs up to subsample 4, the average estimated elasticities over the 500 simulations are -0.254, -0.245, -0.287, -0.281, and -0.279 for the TWFE regressions, and -0.086, -0.037, -0.199, -0.200, and -0.200 for the pair-approach regressions. Average RMSEs range between 0.296 and 0.297 for the TWFE regressions, and between 0.201 and 0.261 for the pair-approach regressions. It can also be demonstrated that the values of σ_φ and σ_ψ amplify the impact of the correlation coefficients. Allowing for higher variances for φ_{zt} and $\psi_{\mathcal{P}t}$ can yield results similar to those shown in Figure 7, even when using correlation levels closer to zero. For example, if we assume instead that $\sigma_\varphi = 0.35$ and $\sigma_\psi = 0.3$, but with $\rho_+ = 0.1$ and $\rho_- = -0.2$, we obtain average estimated elasticities that are very similar to those described above.

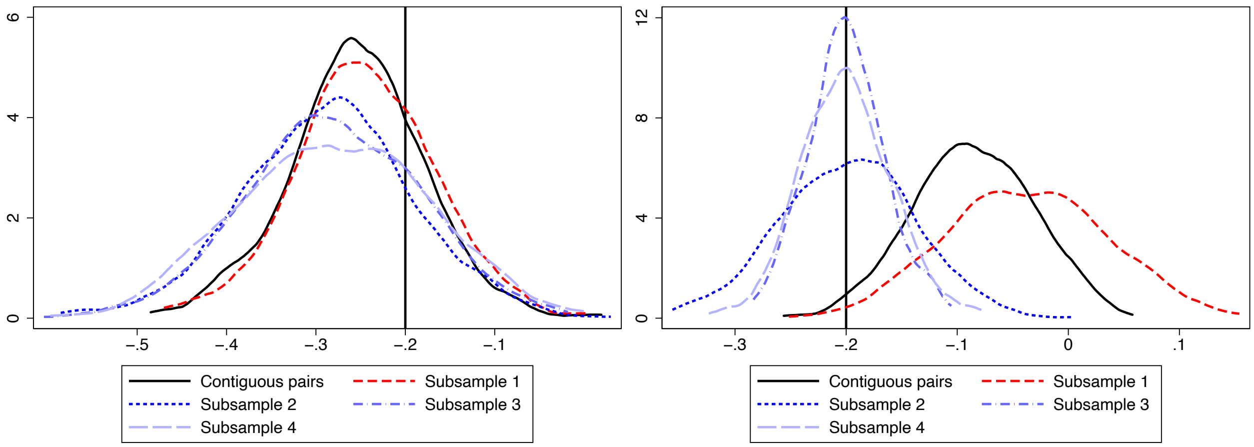


Figure 7: Distribution of estimated minimum wage elasticities of employment from 500 simulations: TWFE (left) and pair approach (right)

distribution in Figure 7. On the other hand, the pair approach is able to control for the negative shock across all samples, but cannot control for the CZ-level positive shock if the sample includes pairs formed by counties from different commuting zones. As a consequence, the pair-approach distribution shows unbiased estimates for subsamples 2, 3, and 4 (which only include cross-border pairs formed by counties in the same commuting zone), but shows positively biased estimates when using subsample 1 and the 1,165 contiguous-county-pair sample (recall that the latter includes subsamples 1 and 2).

6 Conclusion

This paper shows that a simple change in the local level of aggregation—from pairs of contiguous border counties to pairs within multi-state commuting zones—overturns DLR's finding of no relationship between minimum wages and employment in the U.S. restaurant industry. We explain this result by showing that the cross-border research design is subject to positive bias in the estimated minimum wage employment elasticity when the identifying variation is not restricted to pairs formed by counties from the same commuting zone. Allegretto, Dube and Reich (2009) were right: cross-border regions within the same commuting zone provide better controls for estimating the effects of minimum wages on employment. In contrast, cross-border counties fail to provide good controls when they are not in the same commuting zone.

Given that multi-state commuting zones are defined to capture common economic influences, our evidence implies that accounting for time-varying spatial heterogeneity in estimating the effects of state minimum wage variation does not eliminate the evidence that minimum wage increases reduce restaurant employment. Rather, the potentially more-rigorous approach of isolating state minimum wage variation from correlated economic shocks by looking within local economic areas generates evidence that reinforces the conclusion that a higher minimum wage reduces restaurant employment.

An alternative interpretation of the evidence—though not our preferred interpretation—is that there is a range of estimates one gets from using cross-border controls. However, most of these are much more negative than what DLR reported, and if that range of estimates had been entertained (or noted as preferred), then DLR would not have served as the basis for numerous claims that newer, more credible evidence shows that minimum wages do not reduce employment.

Finally, our analysis demonstrates an important general issue for cross-border research designs to estimate policy effects. These have been used widely not only in minimum wage studies, but also in applications to, for example, right to work laws (Holmes, 1998), school desegregation (Boustan, 2012), fiscal policy and growth (Peltzman, 2016), income tax manipulation (Buhlmann, Elsner and Peichl, 2018), effects of the EITC on local economies (Stokan, 2019), as well as many more policy questions.[40] Our analysis and results suggest that researchers have to be cautious in assuming that such research designs necessarily produce unbiased or less biased estimates than alternative approaches. Furthermore, we have outlined some empirical approaches that—in at least some contexts—may be useful in testing the validity of cross-border research designs.

[40] Cross-border designs continue to be widely used in minimum wage studies. For a non-exhaustive list of post-DLR minimum wage cross-border studies, refer to Aaronson et al. (2018), Coviello, Deserranno and Persico (2022), Dube, Lester and Reich (2016), Kong, Qin and Xiang (2021), Li, Shi and Zhou (2023), McVicar, Park and McGuinness (2019), Rohlin (2011) and Taylor and West (2023). The approach has been strongly defended by two of the DLR authors, who argue that "... the border discontinuity design provides more reliable estimates by using more similar comparisons" (Allegretto et al., 2017, p.589).

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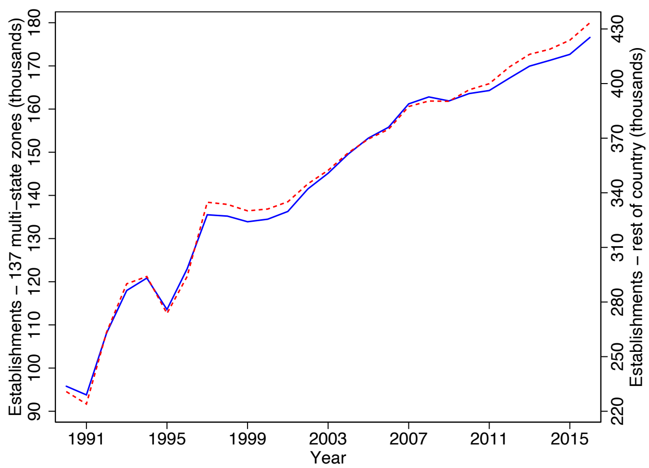
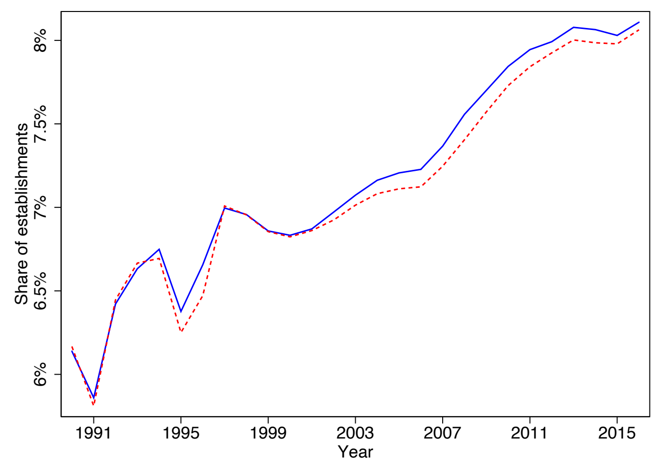
Appendix

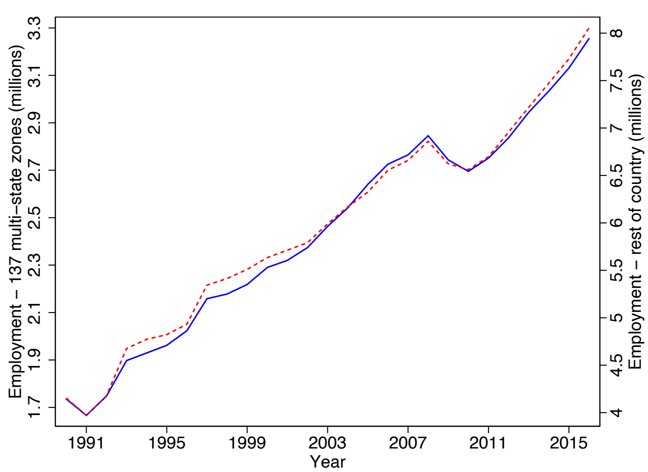
A Supporting Figures and Tables



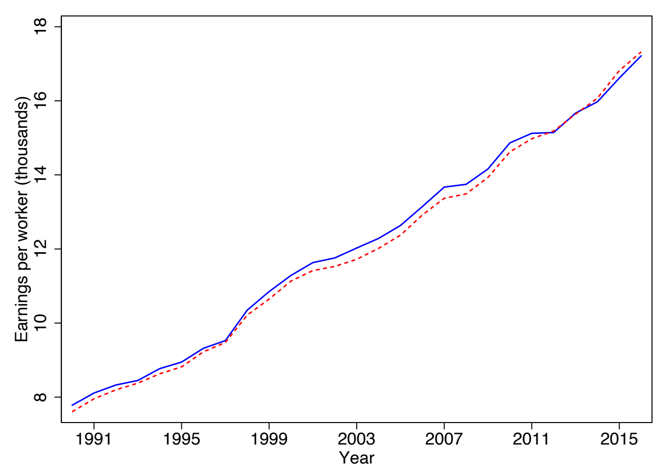
(a) Number of establishments



(b) Fraction of total establishments



(c) Employment



(d) Earnings per worker (U.S. dollars)

Figure A-1: Comparison between commuting-zone groups for restaurant industry: 137 multi-state commuting zones (solid blue) and rest of the country (dashed red)

Notes: This figure shows that in both groups, the restaurant industry accounted for 6.1 percent of all establishments in 1990 and that this share increased to about 8.1 percent by 2016. From Figure A-1d, note that nominal earnings per worker increased from \$7.6-\$7.8 thousand in 1990 to \$17.2-\$17.4 thousand in 2016. The annual payroll variable (from the CBP database) that we use to calculate earnings per worker includes reported tips.

Table A-1: Employment shares and earnings ranking of industries, 1990 and 2016

	1990			2016		
Industry	*Employment share*	*Worker earnings (thousands US$)*	*Earnings ranking (lowest=1)*	*Employment share*	*Worker earnings (thousands US$)*	*Earnings ranking (lowest=1)*
Eating and drinking places	7.21%	7.68	1	9.37%	17.36	1
Retail trade	13.94%	13.47	2	12.81%	27.08	2
Services	16.40%	14.06	3	22.92%	33.02	3
Textiles/apparel	1.99%	15.92	4	0.31%	36.20	4
Wood/furniture	1.36%	19.73	5	0.68%	41.76	5
Other manufacturing	0.44%	21.36	6	0.19%	47.40	7
Food/tobacco	1.62%	23.98	7	1.31%	45.28	6
Health services	9.83%	24.27	8	12.78%	53.87	10
Plastics, clay, stone	1.57%	24.72	9	0.91%	49.80	8
Agriculture, forestry, fishing, and mining	1.29%	25.10	10	1.52%	52.35	9
Construction	5.94%	25.22	11	5.13%	58.69	12
Paper/printing	2.44%	26.94	12	1.04%	63.76	14
Wholesale trade	6.69%	27.97	13	5.50%	66.68	16
Finance, insurance, and real estate	7.60%	28.00	14	7.05%	86.09	19
Metals	2.46%	28.31	15	1.36%	54.19	11
Transp., comm., elec., gas, and sanitary	5.88%	28.98	16	5.50%	60.16	13
Equipment	4.94%	30.33	17	2.21%	66.18	15
Legal, consulting, and computing services	5.27%	34.43	18	7.67%	91.09	20
Transportation manufacturing	2.03%	35.02	19	1.04%	69.44	17
Chemicals/petroleum	1.11%	35.94	20	0.70%	84.35	18

Table A-2: Pair-approach minimum wage elasticities of employment and earnings using multi-state commuting zones: Unweighted/log weighted estimation with different end years

	Employment			Earnings		
Period	(1)	(2)	(3)	(4)	(5)	(6)
1990–2006	-0.291***	-0.281***	-0.279***	0.275***	0.236***	0.251***
	(0.095)	(0.093)	(0.094)	(0.088)	(0.070)	(0.078)
1990–2007	-0.301***	-0.300***	-0.292***	0.243***	0.213***	0.223***
	(0.092)	(0.087)	(0.089)	(0.071)	(0.058)	(0.064)
1990–2008	-0.327***	-0.323***	-0.315***	0.239***	0.212***	0.221***
	(0.093)	(0.085)	(0.090)	(0.069)	(0.055)	(0.061)
1990–2009	-0.337***	-0.332***	-0.325***	0.243***	0.216***	0.224***
	(0.092)	(0.084)	(0.089)	(0.069)	(0.055)	(0.061)
1990–2010	-0.338***	-0.332***	-0.326***	0.244***	0.217***	0.226***
	(0.094)	(0.085)	(0.090)	(0.069)	(0.054)	(0.060)
1990–2011	-0.333***	-0.328***	-0.322***	0.238***	0.216***	0.223***
	(0.094)	(0.085)	(0.090)	(0.069)	(0.054)	(0.060)
1990–2012	-0.341***	-0.334***	-0.330***	0.228***	0.215***	0.216***
	(0.095)	(0.085)	(0.091)	(0.068)	(0.053)	(0.059)
1990–2013	-0.323***	-0.325***	-0.317***	0.212***	0.211***	0.207***
	(0.099)	(0.086)	(0.092)	(0.070)	(0.053)	(0.060)
1990–2014	-0.275**	-0.291***	-0.278***	0.176***	0.191***	0.180***
	(0.112)	(0.092)	(0.100)	(0.065)	(0.047)	(0.055)
1990–2015	-0.258**	-0.264***	-0.259**	0.185***	0.187***	0.184***
	(0.117)	(0.097)	(0.104)	(0.063)	(0.042)	(0.051)
1990–2016	-0.242**	-0.242**	-0.241**	0.163***	0.165***	0.163***
	(0.120)	(0.098)	(0.106)	(0.055)	(0.040)	(0.047)
Zone-state effects	Y	Y	Y	Y	Y	Y
Pair–period effects	Y	Y	Y	Y	Y	Y
Number of pairs	151	151	151	151	151	151
Weighted by log		emp	pop		emp	pop

Notes: This table reports $\hat{\beta}$ from the unweighted and weighted estimation of specification (3) for the restaurant industry using CBP yearly data for different end-year periods. All regressions in the table use the 151 multi-state commuting zone pairs. The dependent variable in columns 1-3 is log employment, whereas in columns 4-6 it is log earnings per worker. The main regressor is the log minimum wage, and the controls (not reported) are log employment in the rest of the industries in columns 1-3, log earnings per worker in the rest of the industries in columns 4-6, and log working age population. Columns 1 and 4 show the unweighted estimation results, columns 2 and 5 use initial log employment weights, and columns 3 and 6 use initial log working age population weights. Standard errors (in parentheses) are two-way clustered at the state and border segment levels. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Table A-3: Long-term minimum wage responses with DLR's QCEW 1990-2006 data

	CBCP sample	**MCZP sample**
$\hat{\beta}_{-8}$	-0.038	-0.122*
	(0.050)	(0.073)
$\hat{\beta}_{-6}$	-0.041	-0.106
	(0.069)	(0.080)
$\hat{\beta}_{-4}$	0.012	-0.061
	(0.100)	(0.093)
$\hat{\beta}_{-2}$	0.088	0.008
	(0.128)	(0.121)
$\hat{\beta}_{0}$	0.053	-0.121
	(0.116)	(0.140)
$\hat{\beta}_{2}$	0.027	-0.130
	(0.132)	(0.123)
$\hat{\beta}_{4}$	0.015	-0.144
	(0.116)	(0.101)
$\hat{\beta}_{6}$	-0.074	-0.253
	(0.107)	(0.154)
$\hat{\beta}_{8}$	-0.017	-0.220
	(0.112)	(0.143)
$\hat{\beta}_{10}$	0.011	-0.206
	(0.128)	(0.139)
$\hat{\beta}_{12}$	0.009	-0.211**
	(0.122)	(0.104)
$\hat{\beta}_{14}$	-0.013	-0.293***
	(0.149)	(0.086)
$\hat{\beta}_{16}$	-0.007	-0.305**
	(0.156)	(0.137)
ln(priv. employment)	0.384***	0.354***
	(0.091)	(0.125)
ln(population)	0.727***	0.717
	(0.199)	(0.478)
Pair–period effects	Y	Y
Total private sector	Y	Y
Number of pairs	316	73
Observations	40,416	9,342

Notes: This table reports $\hat{\beta}_k$, for $k \in \{-8, -6, -4, -2, 0, 2, 4, 6, 8, 12, 16\}$, from the estimation of specification (5) using either the CBCP sample or the MCZP sample. Standard errors (in parentheses) are two-way clustered at the state and border segment levels. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Table A-4: Long-term minimum wage responses with CBP 1990-2016 data

	County-level sample			**Multi-state zones sample**		
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_{-2}$	-0.118	0.062	0.031	-0.082	-0.087	-0.029
	(0.070)	(0.065)	(0.045)	(0.052)	(0.087)	(0.046)
$\hat{\beta}_{-1}$	-0.160**	0.119*	0.073	-0.163***	-0.148	-0.029
	(0.072)	(0.063)	(0.044)	(0.061)	(0.111)	(0.049)
$\hat{\beta}_{0}$	-0.215**	0.160*	0.072	-0.178**	-0.172*	-0.113*
	(0.086)	(0.084)	(0.061)	(0.074)	(0.099)	(0.066)
$\hat{\beta}_{1}$	-0.287***	0.042	-0.004	-0.260***	-0.334**	-0.248***
	(0.104)	(0.097)	(0.078)	(0.089)	(0.125)	(0.081)
$\hat{\beta}_{2}$	-0.378***	0.017	-0.072	-0.357***	-0.524***	-0.419***
	(0.124)	(0.096)	(0.084)	(0.100)	(0.154)	(0.109)
$\hat{\beta}_{3}$	-0.468***	-0.044	-0.072	-0.412***	-0.547***	-0.406***
	(0.138)	(0.091)	(0.093)	(0.114)	(0.159)	(0.124)
$\hat{\beta}_{4}$	-0.593***	-0.025	-0.094	-0.559***	-0.689***	-0.512***
	(0.186)	(0.106)	(0.103)	(0.139)	(0.183)	(0.149)
ln(employment^{-})	0.163***	0.204***	0.115**	0.019	0.079	0.067
	(0.039)	(0.055)	(0.044)	(0.073)	(0.090)	(0.088)
ln(population)	1.007***	0.929***	1.015***	1.065***	0.806***	1.128***
	(0.072)	(0.121)	(0.079)	(0.091)	(0.182)	(0.212)
Zone-state effects				Y	Y	Y
County effects	Y	Y	Y			
Year effects	Y			Y		
Pair–period effects		Y	Y		Y	Y
DLR data pairs			Y			Y
Number of pairs	–	1,157	309	–	151	71
Observations	64,064	47,268	12,866	18,109	6,262	2,954

Notes: This table reports $\hat{\beta}_k$, for $k \in \{-2,-1,0,1,2,3,4\}$, $\hat{\gamma}$, and $\hat{\delta}$ from the estimation of specification (6) using either the CBP county-level sample or the CBP multi-state commuting zones sample. Columns 3 and 6 restrict the sample to complete pairs in DLR's data. Although column 1 in Table 3 uses 1,165 complete pairs, the leads and lags in specification (6) make us lose eight pairs. We do not lose any pairs in the estimation with multi-state commuting zones. For the sample that uses DLR's complete pairs, recall that we have 309 out of 316 of DLR's complete pairs, and 71 out of 73 for the multi-state zones estimation. Standard errors (in parentheses) are clustered at the state level in columns 1 and 4, and are two-way clustered at the state and border segment levels in columns 2-3 and 5-6. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Table A-5: Conventional TWFE estimation of minimum wage responses at the county level using CBP 1990-2016 data

	(1)	(2)
A. ln(employment)		
ln(minimum wage)	-0.362***	-0.309***
	(0.118)	(0.104)
ln(employment$^-$)	0.150***	0.090
	(0.044)	(0.055)
ln(population)	1.024***	1.047***
	(0.070)	(0.091)
B. ln(earnings)		
ln(minimum wage)	0.216***	0.226***
	(0.037)	(0.051)
ln(earnings$^-$)	0.141***	0.183***
	(0.035)	(0.065)
ln(population)	0.021	0.043
	(0.026)	(0.036)
County effects	Y	Y
Year effects	Y	Y
All counties	Y	
Only border counties		Y
Number of counties	3,103	1,129
Observations	83,160	30,287

Notes: This table reports $\hat{\beta}$, $\hat{\gamma}$, and $\hat{\delta}$ from the estimation of specification (2) for the restaurant industry using yearly county-level data from 1990 to 2016. In panel A, the dependent variable is log employment. Panel B uses instead log earnings per worker. Each column uses a different county-level sample. Standard errors (in parentheses) are clustered at the state level. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

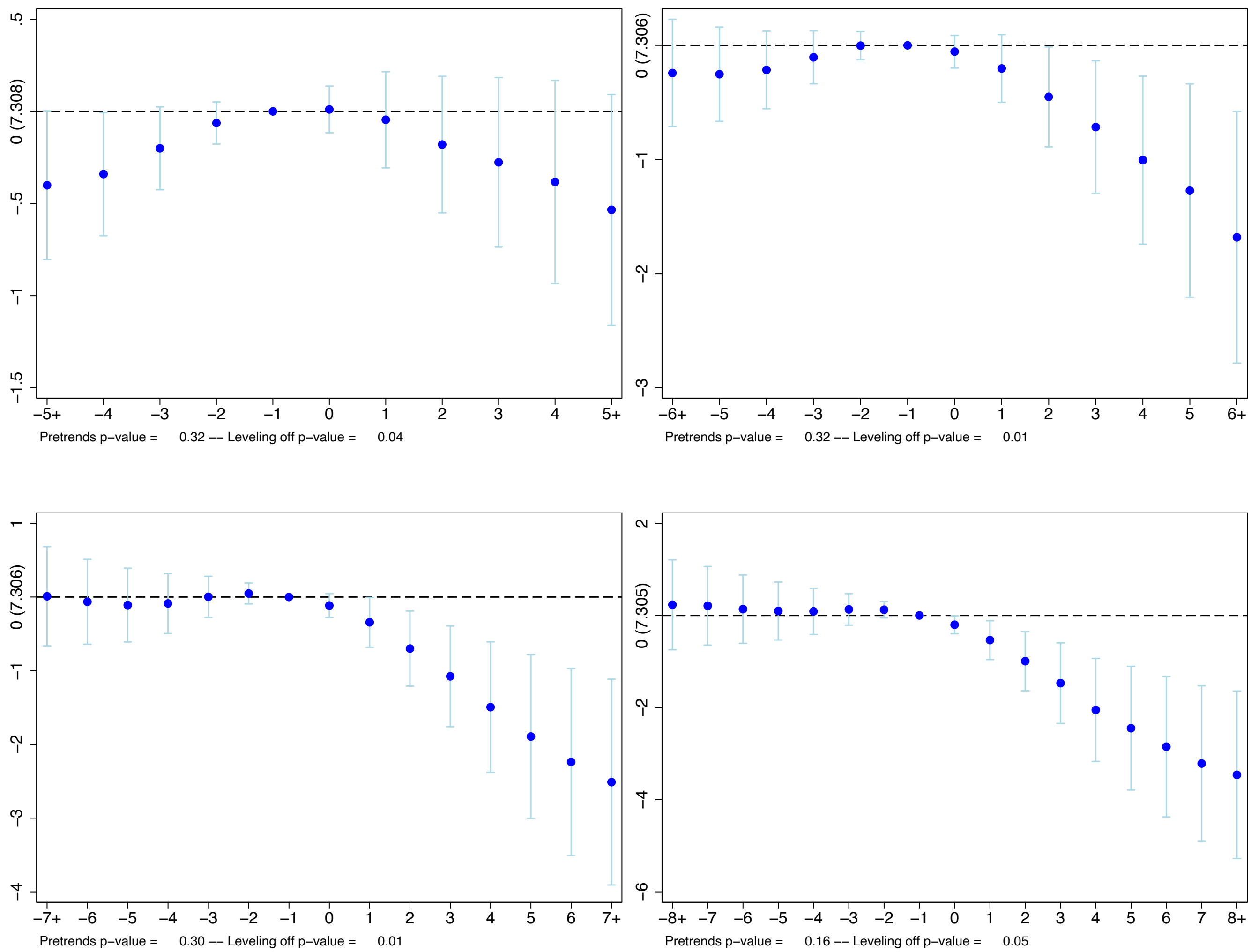


Figure A-2: Event-study plots: Cumulative effects of minimum wage events on restaurant employment for different horizons and continuous treatment

B Quotes on DLR's Relevance in the Minimum Wage Literature

DLR is the source of many claims that newer, more credible evidence shows that minimum wages do not reduce employment. In addition to those mentioned in footnote 1, here are other examples:

"... [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, 2011, p. 763)

"Careful causal studies of the restaurant industry also suggest that a 10 percent increase in the minimum wage affects restaurant employment somewhere between -0.5 percent and zero." [citing DLR] (Reich, 2021)

Moreover, the reliance on the DLR estimates has been used in work advocating for higher minimum wages and criticizing claims that higher minimum wages can lead to job loss. Here are some examples:

"In summary, the best research on minimum wage effects does not find negative employment effects on low-wage workers" [citing DLR extensively] (Reich, 2012, p. 9, arguing for a higher minimum wage in San Jose)

"Dube, Lester, and Reich (2010 and forthcoming) ... find no statistically significant effects of minimum wage increases on either employment or hours worked in restaurants ..." (Jacobs et al., 2015, p. 14, arguing for a higher minimum wage in Contra Costa County)

And the conclusion in the original paper says the same: "The estimates suggest no detectable employment losses from the kind of minimum wage increases we have seen in the United States." (Dube, Lester and Reich, 2010, p. 962)

C Heterogeneous (or Not) Minimum Wage Effects

This section shows that a larger fraction of the population lives in counties where the employment effects of minimum wages are negative, and then explores if the differential results in different subsamples can be explained by varying degrees of monopsony power, as captured by employment concentration.

The 843 pairs of subsample 1, whose contiguous counties in each pair belong to different commuting zones, are formed from 929 counties, whereas the 986 cross-border pairs of subsample 3 span from the 742 counties (excluding DC) within the 137 multi-state commuting zones. In total population size, subsample 3 is between 19.5% and 24.3% larger than subsample 1: the 929 counties

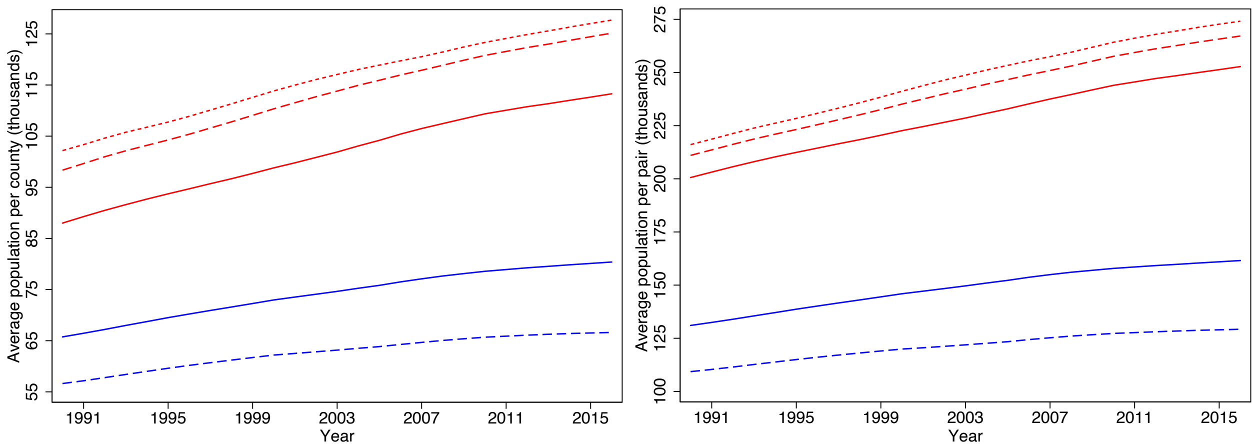


Figure C-1: Average population per subsample: 1 (blue-solid), 2 (red-solid), 3 (red-dash), 4 (red-short dash), 5 (blue-dash)

of subsample 1 had a population of 61.1 million in 1990 and of 74.7 million in 2016, while the 742 counties of subsample 3 had 73 million in 1990 and 92.9 million in 2016. Although these differences may not seem too large, it is important to note that 343 of the 742 MSCZ counties in subsample 3 also appear in subsample 1, and account for about 40% of the population of its 929 counties.

For a rural versus urban comparison, it is more relevant to look at each sample's average population per county and average population per county pair. To obtain a better contrast between MSCZs and non-MSCZs, we create a new subsample—which we refer to as *subsample 5*—which contains the 395 pairs of subsample 1 where each pair is formed by two counties from *single-state* commuting zones (463 counties span these 395 pairs). Figure C-1 shows the evolution of the average population per county (left) and the average population per county pair (right) during the 1990-2016 period for each of our subsamples. Focusing on subsample 3 and subsample 5 (the red and blue dashed lines, respectively), note that the gap between them increases over time for both average population measures: whereas MSCZ counties were on average 73.7% larger than non-MSCZ counties in 1990, they were 87.9% larger by 2016—for county-pairs the average is 93% larger in 1990 and 106.7% larger in 2016. Therefore, MSCZ areas have a much larger population density at the county level than non-MSCZ cross-border areas, and the density difference has only increased over time. As a consequence, one might view the results in panel B of Table 9 as capturing the heterogeneous effects of minimum wages in urban and rural areas, with employment effects being negative in urban areas (where most people live, as captured by multi-state commuting zones) and near zero in rural areas (as captured by low population density county pairs not in MSCZs).

Along the lines of the recent literature on monopsony power and wages (see, for example, Azar, Marinescu and Steinbaum, 2022), a natural exercise is to explore how employment concentration in the restaurant industry affects minimum wage elasticities. In particular, we can verify if more employment concentration—thought to be associated with more monopsony power in the labor market—is related to higher (less negative) minimum wage elasticities of employment.

To calculate employment concentration at the county level, we use the National Establishment Time Series (NETS) data.[41] NETS includes yearly data on employment for the universe of establishments in the U.S., including detailed location information, so we can calculate a Herfindahl–Hirschman index (*HHI*) for each county's restaurant industry using firm-level employment. Given that *HHI* is likely to be endogenous, our employment concentration measure is the *HHI* of 1992, which is the first year of reliable NETS data. Thus, if county i had three restaurants in 1992 with employment shares of 0.25, 0.4, and 0.35, then HHI_i is given by $0.25^2 + 0.4^2 + 0.35^2 = 0.345$.[42]

For each of our subsamples, Table C-1 presents the estimation of a version of equation (3) that includes the interaction term $\ln MW_{it} \times (HHI_i - \overline{HHI})$, where $\overline{HHI}$ is the average *HHI* across all counties in that subsample. Given that our *HHI* measure is from 1992, we restrict our CBP data to the 1992-2016 period. The monopsony argument is that more employment concentration (a higher *HHI*) implies less adverse effects of minimum wages on employment, so that the estimated coefficient of the interaction term should be positive. In contrast, all columns show a negative (though not significant) interaction coefficient—this result appears even when using the most rural subsample 5, which only includes cross-border pairs of counties from single-state commuting zones.[43] Therefore, monopsony power—to the extent that it is captured by employment concentration—does not seem to be a cause of heterogeneity in our estimated minimum wage elasticities of employment.

[41] See Neumark, Zhang and Wall (2007) and Neumark, Wall and Zhang (2011) for a detailed description of the NETS database.

[42] Although the *HHI* is usually presented in a $(0, 10,000]$ range, here we use a $(0, 1]$ normalization. A market is considered moderately concentrated if $HHI \in (0.15, 0.25]$, and highly concentrated if $HHI > 0.25$ (see section 5.3 in the *Horizontal Merger Guidelines* of the U.S. Department of Justice & FTC).

[43] The *HHI* statistics in the bottom of Table C-1 show similar standard deviations, minimums, and maximums across counties used in each subsample, with the mean being higher for the counties of subsample 5. The last result is expected, as rural counties should be more concentrated than urban counties. Note that *HHI* averages are below 0.15 in all subsamples, which indicates that concentration in the restaurant industry is low on average.

Table C-1: Monopsony power in the pair-approach estimation of minimum wage responses for different cross-border county-pair samples

	Pairs formed by contiguous counties not in same CZ	*Pairs formed by contiguous counties in same MSCZ*	*Pairs formed by contig. and non-contig. counties in same MSCZ*	*Pairs formed by non-contig. counties in same MSCZ*	*Pairs formed by contiguous counties not in any MSCZ*
	Subsample 1	**Subsample 2**	**Subsample 3**	**Subsample 4**	**Subsample 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(MW) $\times$ ($HHI - \overline{HHI}$)	-0.605	-0.558	-0.823	-0.904	-0.463
	(0.456)	(0.673)	(0.540)	(0.559)	(0.641)
ln(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 ($\overline{HHI}$)	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

Notes: Using yearly county-pair data from 1992 to 2016, this table reports the estimation from an extension of specification (3) that includes the interaction term $\ln MW_{it} \times (HHI_i - \overline{HHI})$. The dependent variable is log employment in the restaurant industry. HHI_i is calculated at the firm level for each county i in 1992. Each column indicates the subsample used. Standard errors (in parentheses) are two-way clustered at the state and border segment levels. The coefficients are statistically significant at the *10%, **5%, or ***1% level.