Minimum Wages and Aggregate Job Growth: Causal Effect or Statistical Artifact?

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Abstract

Consistent with recent work by Meer and West, I find a negative association between minimum wages and state-level aggregate employment growth in both the Business Dynamics Statistics and the Quarterly Census of Employment and Wages data, and it is sizable for some time periods. However, I show that this negative association is present in exactly the wrong sectors. It is particularly strong in manufacturing which hires very few minimum wage workers. At the same time, there is no such association in retail, or in accommodation and food services—which together hire nearly 2/3 of all minimum wage workers. These results indicate that the negative association between minimum wages and aggregate employment growth does not represent a causal relationship. Rather the association stems from an inability to account for differences between high and low minimum wage states and the timing of minimum wage increases. Consistent with that interpretation, when I use bordering counties to construct more credible control groups, I find no such negative correlation between minimum wages and overall employment growth.

Introduction

In a recent paper, Jonathan Meer and Jeremy West investigate the relationship between minimum wages and employment growth (Meer and West 2013). They argue that employment levels take time to adjust after a minimum wage increase, making it more difficult to detect an impact of the policy. In contrast, they argue, impact is easier to discern when considering employment growth.

The possibility of a delayed effect of minimum wages on employment has long been recognized. For example, Neumark and Wascher (1992) argued that such lagged effects are important, before eschewing the inclusion of lags in their recent work (Neumark and Wascher 2011; Neumark, Salas and Wascher 2013).[1] In Allegretto, Dube and Reich (2011) and Dube, Lester and Reich (2010),

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[1] In their 2013 paper with Ian Salas, Neumark and Wascher appear to go to the other extreme, and consider only a *single quarter* of post-intervention data in their preferred matching estimator. This makes it the shortest run estimate of minimum wage impact that I am aware of.

we explicitly consider distributed lags in minimum wages covering up to 4 years or more. Baker, Benjamin and Stanger (1999) also evaluate the "long run" impact by using filtered minimum wage variation—which for practical purposes boils down to using weighted lags of minimum wages. However, different from the existing body of work, Meer and West refrain from an explicit estimation of lagged effects, and instead re-formulate the outcome to be employment growth which they expect to perform better when the impact occurs slowly over time.

While this argument makes some intuitive sense, there are some unattractive aspects of using employment growth as an outcome. In particular, their specification suggests that even a minute change in minimum wages has a permanent effect on the rate of employment growth, which is difficult to derive from existing theories or intuition. However, the technical issues surrounding this specification are far less important than the specific manner in which it is implemented by Meer and West. Namely, they look at employment growth for the workforce overall, even though their analysis suggests that only 3.25 percent of workers were earning the minimum wage during the period. Such a small coverage rate makes the search for employment effect among all workers akin to looking for a needle in the haystack. Indeed, it is customary in the literature to *control for* overall employment, either in levels or changes, in order to avoid spuriously attributing to minimum wages the impact of other factors such as the business cycle, sectoral shocks, etc. Instead, Meer and West choose to use aggregate employment growth as the key outcome.

And yet, surprisingly, they find a strong partial correlation between minimum wages and aggregate employment growth. Their findings suggest a 10 percent increase in minimum wage is associated with 0.4 to 0.5 percentage point lower employment growth rates depending on the specification. The authors state that this translates into a 1.2 percent lower employment in five years' time after a minimum wage increase, which is far greater in magnitude than other existing estimates. For example, in their most recent work, Neumark, Salas and Wascher suggest that a 10 percent increase in minimum wage would reduce employment for *teens* by around 1-2 percent, even though teens are around five times more likely to be earning minimum wages than the workforce as a whole.[2]

As Meer and West acknowledge, a serious limitation of their work is the data they choose to use: the Business Dynamics Statistics (BDS) dataset, which reports employment for the workforce as a whole for the 1977-2011 period. Most importantly, it does not break down state-level employment counts by industry or demographic groups. Secondarily, it only reports employment at the state level, and not at a more disaggregated geography. While the authors are correct that other datasets do not provide the same data as the BDS over as long a time period, their dismissal of other data sources is unwarranted. Their key measure is "net job growth," which is the sum of job creation and

[2] In 2012, teens were 5.7 times as likely to earn at the minimum wage, and 4.5 times as likely to earn at or below the minimum wage, as the workforce overall. See here: http://www.bls.gov/cps/minwage2012tbls.htm#1. As we show in Allegretto, Dube, Reich and Zipperer (2013), even this estimate by Neumark et al. is too large in magnitude, and driven by their insufficient attention to differences between high and low minimum wage states. Using four different datasets and six different credible research designs accounting for time-varying heterogeneity, we show that the minimum wage elasticity for teens is close to zero. For a discussion of recent estimates of minimum wage impact on employment, see Schmitt (2013).

destruction at date t divided by the average employment level during dates t and $t-1$. But this is virtually identical to employment growth between times $t-1$ and t, which can be computed using many datasets. The correlation between these two job growth rates in the BDS dataset is 0.95, and nothing is lost by focusing on this measure.[3]

Two datasets are particularly relevant for estimating the impact on job growth: the Quarterly Census of Employment and Wages (QCEW) and County Business Patterns (CBP). The QCEW data is available from 1975 and the CBP since 1977. Both of these datasets provide employment counts by industry; the QCEW provides data by NAICS industry classification since 1990. Both provide the data at the county level, which allows for a more refined causal identification, such as comparing counties straddling state borders as in Dube, Lester and Reich (2010; 2013). In addition, the Quarterly Workforce Indicators (QWI) includes job creation and destruction rates as well as turnover rates for states and counties by detailed industrial sector, demographic group and geography; data for most states are available from 2000 onwards, although earlier data is available for a smaller group of states.

Extant research offers several reasons for us to be skeptical of the causal import of Meer and West's findings.[4] First, in Allegretto, Dube and Reich (2011) and Dube, Lester, and Reich (2010), we specifically consider distributed lags in minimum wages covering up to 4 years or more but did not find these to be important. Second, in Dube Lester and Reich (2013), we use data from QWI and explicitly looked at employment flows find that both hires and separations of low-wage workers (teens, restaurant workers) fall in response to minimum wage increase, but employment levels do not change noticeably. Finally, and most directly, in Allegretto, Dube, Reich and Zipperer (2013), one of the specifications we estimate is very similar to that used by Meer and West.[5] Using state-level data for teens from the Current Population Survey, and county-level data for restaurant workers from the QCEW (both since 1990), we found that minimum wage was not associated with lower employment growth for either group.

These findings suggest the likelihood that Meer and West's findings of a negative association are spurious, and do not reflect impact of the policy on employment growth for actual minimum wage workers. As we show in Allegretto, Dube, Reich and Zipperer (2013), states that have raised the minimum wage more over the past several decades are systematically different from other states. These states have experienced more severe economic downturns; they have experienced greater job polarization in the form of sharper reduction in jobs involving routine tasks; and they have seen faster growth in upper-half wage inequality. Inability to account for these and other differences can

[3] Meer and West also study the association between minimum wage and job creation and destruction rates. However, by far their most important finding is on employment growth. Since net job increase is simply the sum of the job creation and destruction, those added results only help us interpret the key finding on employment growth and do not shed any further light on its validity.

[4] Some of these reasons are also discussed in a blog post by John Schmitt available here: http://www.cepr.net/index.php/blogs/cepr-blog/meer-and-west-on-minimum-wage

[5] As we discussed in Allegretto, Dube, Reich and Zipperer (2013), the Meer and West specification is a special case of a AR-1 lagged dependent variable model where the autoregressive coefficient is fixed at 1. In that paper, we consider a range of specifications with lagged employment controls, and in one specification set the AR coefficient equal to 1, which is the Meer and West case.

easily produce spurious results. We show this to be true when it comes to employment levels in low-wage sectors, and it is certainly possible in the case of aggregate job growth.

In this note, I use data from the Quarterly Census of Employment and Wages between 1990 and 2011 to directly assess the findings in Meer and West. Using the same econometric specifications used by the authors, I find that: (1) The negative association between aggregate employment growth and minimum wages can also be found using the QCEW data, especially since mid-1990s. (2) This negative association is particularly strong in manufacturing, a sector with virtually no minimum wage workers. (3) The negative association is completely absent in both retail and accommodation and food services, the two main sectors hiring minimum wage workers. (4) The negative association between aggregate employment growth and minimum wages disappears when I use a reliable control group: bordering counties with different minimum wages.

Together, these four findings indicate that the statistical association reported in Meer and West does not represent a causal effect of the policy. Rather, the correlation reflects the kind of heterogeneity between high and low minimum wage areas that I have documented in a number of papers coauthored with Sylvia Allegretto, T. William Lester, Michael Reich and Ben Zipperer. The findings here also provide added external validity for our argument that a credible research design like comparing bordering counties can filter out such artifacts, and produce reliable estimates.

Data and Empirical Design

The primary data source used here is the QCEW, which is an employer-based dataset that provides payroll head counts and monthly earnings based on Unemployment Insurance filings. The universe includes 98 percent of private sector employees; the 2 percent who are not covered are primarily certain agricultural, domestic, railroad, and religious workers. The QCEW data are available for all states beginning in 1990 by the NAICS industry classification I construct a panel of state as well as county-level observations of employment. For the purpose of comparison, I also utilize aggregate employment growth from the BDS dataset between 1977 and 2011.[6]

Besides aggregate employment, I also consider employment in select NAICS sectors: Accommodation and Food Services (AFS), Retail, and Manufacturing. AFS (which includes restaurants) employs around half of all minimum wage workers in the United States, while retail employs another 16 percent of the minimum wage workforce.[7] For this reason, these two sectors (or components thereof) have often been studied by scholars analyzing minimum wage policies (e.g., Card and Krueger 1994, 2000; Dube, Lester and Reich 2010, 2013; Addison, Blackburn and Cotti 2009, 2012; Giuliano 2013). If the minimum wage has a large negative effect on aggregate employment growth,

[6] I use the version of the dataset made available at Jonathan Meer's website: http://econweb.tamu.edu/jwest/files/Meer_West_MinimumWage_2013_Code.zip. I have not further assessed the construction of this dataset.

[7] See here for sectoral composition of minimum wage workers in 2012: http://www.bls.gov/cps/minwage2012tbls.htm#5. The table reports estimates for the Leisure and Hospitality supersector, 88 percent of whose employment is in Accommodation and Food Services sector, as shown here: http://research.stlouisfed.org/fred2/categories/32323.

it almost certainly has to involve *much stronger* reductions in these two sectors that employ nearly 2/3 of all minimum wage workers. In contrast, manufacturing is a relatively high wage sector which hires very few minimum wage workers. Only 1 percent of manufacturing workers earn minimum wages, and the sector accounts for around 3 percent of all minimum wage workers. Therefore, we should not expect to see a sizable reaction of employment growth in this sector to minimum wage changes. However, it is also a sector which is highly procyclical and has experienced a secular decline. If spurious trends are contaminating minimum wage estimates, manufacturing could play an important role in that process.

The two key specifications I utilize are those presented as specifications 1 and 2 by Meer and West:

$$\Delta \ln(employment_{st}) = \beta \ln(MW_{st}) + \tau_t + \phi_s + \epsilon_{st} \tag{1}$$

$$\Delta \ln(employment_{st}) = \beta \ln(MW_{st}) + \tau_{rt} + \phi_s + \epsilon_{st} \tag{2}$$

Equation (1) is a standard two-way fixed effects model with time dummies τ_t and state fixed effects ϕ_s, while equation (2) allows for time fixed effects to vary by the four census regions. The key outcome is annual change in log employment, which has the usual interpretation as the annual employment growth.[8] Like Meer and West, I first consider aggregate employment growth. However, I supplement the analysis with employment growth in the three specific industries (AFS, retail, manufacturing). In some specifications, I additionally include population growth estimate ($\Delta \ln(pop_{st})$) as an added control.

As shown in Allegretto, Dube, Reich and Zipperer (2013), nearby counties are more similar and a border discontinuity design produces much more reliable estimates that are less contaminated by pre-existing trends and spatial heterogeneity. For this reason, to assess the validity of the findings from (1) and (2), I also estimate a county-level specification with county-pair-by-year fixed effects:

$$\Delta \ln(employment_{ct}) = \beta \ln(MW_{ct}) + \tau_{pt} + \phi_c + \epsilon_{ct} \tag{3}$$

The inclusion of the county-pair-by-year fixed effect sweeps out all the variation between pairs, and only uses variation within pairs surrounding a policy border. This border discontinuity specification allows us to control for time-varying heterogeneity in the outcomes across local areas. Unbiased estimates using the canonical model (1) require the strong assumption that minimum wage differences between *any* locations j are uncorrelated with residual outcomes. In contrast, the spatial controls τ_{pt} in model (3) significantly weaken this assumption, only requiring it to hold for any locations within a given pair, p around the state border. Since a single county can be a part of multiple cross-border pairs, the data is stacked by pairs; the standard errors are clustered by state and by

[8]When using the BDS data, I use Meer and West's job growth measure and not $\Delta \ln(employment)$ for consistency with their paper. However, this makes little difference quantitatively and the correlation between these two measures is 0.95. Additionally, I follow Meer and West's sample selection rules and discard four observations that are considered "outliers."

border pair to account for multiple instances of counties in the dataset. For details, see Dube Lester and Reich (2010, 2013).

I show the results for three different time periods: 1991-2011, 1995-2011, and 2000-2011. The 1991-2011 sample is the maximal one; since the employment data by NAICS goes back to 1990, the earliest growth estimate is from 1991. However, since 1991 was a recessionary year, I use 1995 as an alternative start date. Finally, I show results using 2000-2011 sample for additional sensitivity check (I note that Dube, Lester and Reich's (2013) analysis of employment flows using the QWI also uses the 2000-2011 sample, which makes this exercise useful for comparability.)

Results

In Table 1, I attempt to replicate the Meer and West findings using the QCEW datasets for different time periods. First, I find that the QCEW data and the BDS data produce similar results—the minimum wage estimates across the two datasets match quite closely for each of the samples. In their specification with region-by-year effects, for example, the 1991-2011 sample using the BDS produces a coefficient of -0.0247, while the QCEW produces a coefficient of -0.0273.

Second, I find that contrary to what Meer and West seem to suggest, their results do not require the use of a long panel stretching into the 1970s. Across columns 1-4, the 1977-2011 estimates range between -0.0197 and -0.0393; in contrast, when using the BDS data for 2000-2011, estimates range between -0.0271 and -0.0434. In other words, if anything the sample since 2000 shows stronger negative correlation between aggregate employment growth and minimum wages than the authors' original sample. Moreover, the relatively small cross sectional variation in minimum wages during the 1970's and 1980s means not much statistical power is lost from limiting attention to the post-1990 sample: the standard errors increase only modestly between rows 1 and 2. I note, in passing, that the inclusion of population growth as a control (which the authors did not do) tends to lower the magnitude of the estimates across datasets and time periods.[9] Across all samples in Table 1, controlling for population growth reduces the magnitude of the minimum wage coefficient by an average of 38 percent. In later tables, I always include population growth as a control since it is difficult to find a good reason to exclude it.

These facts together call into question the authors' claim that their use of a long panel (and hence their choice of BDS data) is important for their findings. The results suggest that using the more commonly used QCEW is not problematic and, in fact, beneficial, since the QCEW allows us to focus on employment by sector. If the association between employment growth and minimum wages is causal, it should be primarily in low wage industries, and not in high wage ones.

Table 2 uses QCEW data report estimates for the 3 sectors separately. Here we find that for time periods that exhibit sizable negative association between employment growth and minimum wage, the association is the strongest in manufacturing, a sector with almost no minimum wage

[9] Meer and West include the population *level* in some specifications. But their outcome is employment growth, which strongly suggests the need to include the population growth rate as a control.

workers. For example, in the 1995-2011 period, a 10 percent higher minimum wage is associated with a 0.3 percentage point lower employment growth in the aggregate, and a 0.4 percentage point lower growth in manufacturing, which hires almost no minimum wage workers. (The coefficient is statistically significant at the 10 percent level.) A similar result holds for the 2000-2011 sample as well. If a research design is to be believed, it should not suggest that minimum wages sharply reduce manufacturing employment in the U.S. labor market. However, this spurious negative association is consistent with the greater pace of job polarization in high minimum wage states, as documented in Allegretto, Dube, Reich and Zipperer 2013.

In contrast, there is no discernible association between minimum wage and employment growth in retail and AFS, which together account for the majority of minimum wage workers. In these two low wage sectors, the point estimates are small (ranging between -0.0213 and 0.0035), and are never statistically significant. In other words, the negative association between aggregate employment and minimum wage is in exactly the "wrong" place when it comes to the sectoral composition of employment.

As an additional check on these results, I reproduced the same aggregate employment growth results using county level data. As mentioned above, and shown in Allegretto, Dube, Reich and Zipperer (2013), the use of neighboring counties as a control group can substantially diminish spurious correlations by providing a more reliable counterfactual. In Table 3, I report estimates using annual county-level data, both with common and pair-specific time effects, and county fixed effects. Column 1 repeats the state-level estimate from the previous tables. Column 2 estimates an analogous estimate using a panel of all counties, using the otherwise similar specification (with region-year effects) as in column 1. The negative association is clearly present in the county sample with coefficients ranging between -0.0349 and -0.0360 across the samples. Column 3 is the preferred border discontinuity estimate that specializes to a sample of border county pairs, and includes pair-specific year effects to focus on the local area variation. When we account for spatial heterogeneity using the border discontinuity design, the minimum wage estimates are strikingly small in magnitude, with point estimates range between -0.0039 and -0.0145 across the samples. The estimates are never statistically significant at conventional levels, although they are somewhat less precise. Overall, the evidence from the border discontinuity design confirm the likely spurious nature of the negative association uncovered by Meer and West.

Discussion

Over the past two decades, and especially since 2000, the proliferation of state-level policies has created substantial and persistent cross-sectional differences in minimum wages. This is both good and bad news. The good news is that this variation allows us to use a rich set of comparisons to form inference about minimum wages. The bad news is that the variation is not randomly distributed across areas: states engaging in greater increases in the minimum have been systematically different in a time-varying fashion when it comes to growth in upper half inequality, job polarization, depth of recessions, and political economy. The timing of minimum wage increases further complicates the picture, as they are more likely to be raised during the latter part of economic expansions. Insufficient attention to such differences can lead to very misleading inference; and it is important to use credible control groups and identification strategies.

In a set of papers with Sylvia Allegretto, T. William Lester, Michael Reich and Ben Zipperer, I have demonstrated the violation of the parallel trends assumption in the context of narrowly defined low-wage groups (such as teens and restaurant sector), even after controlling for aggregate employment patterns. These concerns are magnified when aggregate employment growth is the outcome itself. In our work, we have found that the most reliable estimates come from border discontinuity specifications which compare changes across proximate areas across borders. For example, the border discontinuity design tends to perform best when it comes to falsification tests on pre-existing trends. And contrary to some recent claims (see Neumark, Salas and Wascher 2013), local areas are indeed more similar in levels and trends of covariates, as shown in Allegretto, Dube, Reich and Zipperer (2013). Moreover, there is sufficient variation in policy such that restricting attention to local comparisons still allows us to draw meaningful inference.

The Meer and West study has inadvertently provided an important and added confirmation of the bias induced by differences between high versus low minimum wage states and the timing of minimum wage increases. As I show in this note, minimum wages are indeed associated with lower employment growth, but exactly in the wrong places—providing a powerful example of how even apparently sensible research designs can go wrong.[10] It is also telling that a border discontinuity design—one that my co-authors and I have advocated—successfully filters out the confounding artifacts, and avoids drawing such an erroneous conclusion.

[10]In many ways, the regression specifications used by Meer and West seem sensible. They use regional trend controls, provide robustness tests using state-specific trends, and provide some evidence of lack of pre-existing trends, which all appear to confirm their results. In our work, we have found that use of these "intermediate" specifications—not as refined as the border discontinuity design but more so than the two-way fixed effects model—often produce more reliable estimates that tend to pass falsification tests (Allegretto, Dube, Reich Zipperer 2013). However, those results are for low-wage sectors once aggregate labor market conditions are accounted for—something missing in Meer and West. This is why we also have strongly advocated for the border discontinuity design, which tends to produce the most reliable estimates.

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Table 1
Minimum Wages and Aggregate Employment Growth Across Datasets and Time Periods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: 1977-2011				
lnMW	-0.0376***	-0.0197**	-0.0393***	-0.0279***				
	(0.0086)	(0.0082)	(0.0125)	(0.0100)				
N	1781	1728	1781	1728				
				Panel B: 1991 - 2011				
lnMW	-0.0116	-0.0020	-0.0247*	-0.0168	-0.00928	0.0007	-0.0273*	-0.0182
	(0.0116)	(0.0092)	(0.0130)	(0.0108)	(0.0119)	(0.0099)	(0.0153)	(0.0136)
N	1069	1067	1069	1067	1071	1071	1071	1071
				Panel C: 1995 - 2011				
lnMW	-0.0249**	-0.0169	-0.0360**	-0.0292**	-0.0339***	-0.0211***	-0.0456***	-0.0337***
	(0.0117)	(0.0101)	(0.0138)	(0.0118)	(0.0102)	(0.0075)	(0.0131)	(0.0111)
N	867	866	867	866	867	867	867	867
				Panel D: 2000 - 2011				
lnMW	-0.0342**	-0.0271*	-0.0434**	-0.0382**	-0.0444***	-0.0315**	-0.0535***	-0.0423***
	(0.0150)	(0.0138)	(0.0174)	(0.0166)	(0.0156)	(0.0130)	(0.0177)	(0.0156)
N	612	612	612	612	612	612	612	612
Dataset:								
BDS	Y	Y	Y	Y				
QCEW					Y	Y	Y	Y
Pop growth control		Y		Y		Y		Y
Region-year effects			Y	Y			Y	Y

Notes. Columns 1-4 are estimated using the annual state-level Business Dynamics Statistics dataset, and columns 5-8 are estimated using the annualized state-level Quarterly Census of Employment and Wages dataset. Sample years are as indicated in each panel. For the BDS dataset, the outcome is aggregate employment growth is defined as in Meer and West. For the QCEW, the outcome is defined as annual difference in log of total private sector employment at the state level. The reported coefficient is for the log of minimum wage. All specifications include state fixed and year fixed effects. As indicated, columns 3,4,7,8 include region-specific year fixed effects. Columns 2,4,6,8 include the annual difference in log population as a control. Robust standard errors in parentheses are clustered by state. Statistical significance is indicated by: *** for 1 percent, ** for 5 percent, * for 10 percent.

Table 2
Minimum Wages and Employment Growth by Industry

	(1)	(2)	(3)	(4)
	Aggregate	*Manufacturing*	*Retail*	*Acc. & Food Services*
	Panel A: 1991 - 2011			
lnMW	-0.0182	-0.0113	0.00174	0.0035
	(0.0136)	(0.0213)	(0.0132)	(0.0146)
N	1071	1071	1071	1071
	Panel B: 1995 - 2011			
lnMW	-0.0337***	-0.0409*	-0.0125	-0.0026
	(0.0111)	(0.0217)	(0.0100)	(0.0099)
N	867	867	867	867
	Panel C: 2000 - 2011			
lnMW	-0.0423***	-0.0384*	-0.0213	-0.0164
	(0.0156)	(0.0215)	(0.0137)	(0.0119)
N	612	612	612	612
Region-year effects	Y	Y	Y	Y

Notes. All regressions are estimated using annualized state-level Quarterly Census of Employment and Wages. Sample years are as indicated in each panel. The outcome is defined as annual difference in log employment for: aggregate private sector employment (column 1), manufacturing sector (column 2), retail sector (column 3) and accommodation and food services sector (column 4). The reported coefficient is for the log of minimum wage. All specifications include as controls state fixed effects and region-specific year fixed effects, as well as the annual difference in log population. Robust standard errors in parentheses are clustered by state. Statistical significance is indicated by: *** for 1 percent, ** for 5 percent, * for 10 percent.

Table 3
Minimum Wages and Aggregate Employment Growth using County-level Data

	(1)	(2)	(3)
	Panel A: 1991 - 2011		
lnMW	-0.0182	-0.0349***	-0.0145
	(0.0136)	(0.0129)	(0.0248)
N	1071	59175	44132
	Panel B: 1995 - 2011		
lnMW	-0.0337***	-0.0347**	-0.0039
	(0.0111)	(0.0148)	(0.0259)
N	867	47890	35706
	Panel C: 2000 - 2011		
lnMW	-0.0423***	-0.0360**	-0.0086
	(0.0156)	(0.0153)	(0.0344)
N	612	33783	25174
Dataset:			
State panel	Y		
All county panel		Y	
Border county panel			Y
Region-year effects	Y	Y	
County-pair-year effects			Y

Notes. All regressions are estimated using the county-level Quarterly Census of Employment and Wages. Column 1 uses state level panel data. Column 2 uses a panel of all counties. For Column 3, the data is stacked by county pairs for the subset of counties straddling a state border. Sample years are as indicated in each panel. The outcome is defined as annual difference in log aggregate private sector employment. The reported coefficient is for the log of minimum wage. All specifications include as control the annual difference in log population at the county level. Column 1 includes state fixed effects, while columns 2 and 3 include county fixed effects. Columns 1 and 2 use region-specific year effects. Column 3 is the border discontinuity design that includes county-pair specific year effects. Robust standard errors in parentheses are clustered by state (column 1) and dual clustered by state and border segment (columns 2 and 3). Statistical significance is indicated by: *** for 1 percent, ** for 5 percent, * for 10 percent.