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MINIMUM WAGE EFFECTS ACROSS STATE BORDERS: ESTIMATES USING CONTIGUOUS COUNTIES

Arindrajit Dube, T. William Lester, and Michael Reich*

Abstract—We use policy discontinuities at state borders to identify the effects of minimum wages on earnings and employment in restaurants and other low-wage sectors. Our approach generalizes the case study method by considering all local differences in minimum wage policies between 1990 and 2006. We compare all contiguous county-pairs in the United States that straddle a state border and find no adverse employment effects. We show that traditional approaches that do not account for local economic conditions tend to produce spurious negative effects due to spatial heterogeneities in employment trends that are unrelated to minimum wage policies. Our findings are robust to allowing for long-term effects of minimum wage changes.

I. Introduction

THE minimum wage literature in the United States can be characterized by two different methodological approaches. Traditional national-level studies use all cross-state variation in minimum wages over time to estimate effects (Neumark & Wascher, 1992, 2007). In contrast, case studies typically compare adjoining local areas with different minimum wages around the time of a policy change. Examples of such case studies include comparisons of New Jersey and Pennsylvania (Card & Krueger, 1994, 2000) and San Francisco and neighboring areas (Dube, Naidu, & Reich, 2007). On balance, case studies have tended to find small or no disemployment effects. Traditional national-level studies, however, have produced a more mixed verdict, with a greater propensity to find negative results.

This paper assesses the differing identifying assumptions of the two approaches within a common framework and shows that both approaches may generate misleading results: each approach fails to account for unobserved heterogeneity in employment growth, but for different reasons. Similar to individual case studies, we use policy discontinuities at state borders to identify the effect of minimum wages, using only variation in minimum wages within each of these cross-state pairs. In particular, we compare all contiguous county-pairs in the United States that are located on opposite sides of a state border.[1] By considering all such pairs, this paper generalizes the case study approach by using all local differences in minimum wages in the United States over sixteen and a half years. Our primary focus is on restaurants, since they are the most intensive users of minimum wage workers, but we also examine other low-wage industries, and we use county-level data on earnings and employment from the Quarterly Census of Employment and Wages (QCEW) between 1990 and 2006.

We also estimate traditional specifications with only panel and time period fixed effects, which use all cross-state variations in minimum wages over time. We find that traditional fixed-effects specifications in most national studies exhibit a strong downward bias resulting from the presence of unobserved heterogeneity in employment growth for less skilled workers. We show that this heterogeneity is spatial in nature. We also show that in the presence of such spatial heterogeneity, the precision of the individual case study estimates is overstated. By essentially pooling all such local comparisons and allowing for spatial autocorrelation, we address the dual problems of omitted variables bias and bias in the estimated standard errors.

This research advances the current literature in four ways. First, we present improved estimates of minimum wage effects using local identification based on contiguous country pairs and compare these estimates to national-level estimates using traditional fixed-effects specifications. Both local and traditional estimates show strong and similar positive effects of minimum wages on restaurant earnings, but the local estimates of employment effects are indistinguishable from 0 and rule out minimum wage elasticities more negative than -0.147 at the 90% level or -0.178 at the 95% level. Unlike individual case studies to date, we show that our results are robust to cross-border spillovers, which could occur if restaurant wages and employment in border counties respond to minimum wage hikes across the border.

In contrast to the local estimates, traditional estimates using only panel and time period fixed effects produce negative employment elasticities of -0.176 or greater in magnitude. The difference between these two sets of findings has important welfare implications. The traditional fixed-effects estimates imply a labor demand elasticity close to -1 (around -0.787), which suggests that minimum wage increases do not raise the aggregate earnings of affected workers very much. In contrast, our local estimate using contiguous county rules out, at the 95% level, labor demand elasticities more negative than -0.482, suggesting that the minimum wage increases substantially raise total earnings at these jobs.

Second, we provide a way to reconcile the conflicting results. Our results indicate that the negative employment effects in national-level studies reflect spatial heterogeneity

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[1] State border discontinuities have also been used in other contexts, for example, by Holmes (1998) and Huang (2008).

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and improper construction of control groups. We find that in the traditional fixed-effects specification, employment levels and trends are negative prior to the minimum wage increase. In contrast, the levels and trends are close to 0 for our local specification, which provides evidence that contiguous counties are valid controls. Consistent with this finding, when we include state-level linear trends or use only within–census division or within–metropolitan area variation in the minimum wage, the national-level employment elasticities come close to 0 or even positive.

Third, we consider and reject several other explanations for the divergent findings. We rule out the possibility of anticipation or lagged effects of minimum wage increases—a concern raised by the typically short window used in case studies. We use distributed lags covering a 6-year window around the minimum wage change and find that for our local specification, employment is stable both prior to and after the minimum wage increase. We obtain similar results when we extend our analysis to accommodation and food services, and retail. Our local estimates for the broader low-wage industry categories of accommodation and food services and retail also show no disemployment effects. Hence, the lack of an employment effect is not a phenomenon restricted to restaurants. Overall, the weight of the evidence clearly points to an omitted variables bias in national-level estimates due to spatial heterogeneity, which is effectively controlled for by our local estimates.

Finally, in the presence of spatial autocorrelation, the reported standard errors from the individual case studies usually overstate their precision. As we show in this paper, the odds of obtaining a large positive or negative elasticity from a single case study is nontrivial. This result establishes the importance of pooling across individual case studies to obtain more reliable inference, a point made in earlier papers.

The rest of the paper is organized as follows. Section II briefly reviews the literature, with a focus on identifying assumptions. Section III describes our data and how we construct our samples, while section IV presents our empirical strategy and main results. Section V examines the robustness of our findings and extends our results to other low-wage industries, Section VI provides our conclusions.

II. Related Literature

The vast U.S. minimum wage literature was thoroughly reviewed by Brown (1999). On the most contentious issue of employment effects, studies since Brown's review article continue to obtain conflicting findings (for example, Neumark & Wascher, 2007; Dube et al., 2007). In discussing this literature, we highlight what to us is the most critical aspect of prior research: the key divide in the minimum wage literature is along methodological lines—between local case studies and traditional national-level approaches that use all cross-state variations. Our reading of the literature suggests that this difference in methods may account for much of the difference in results.

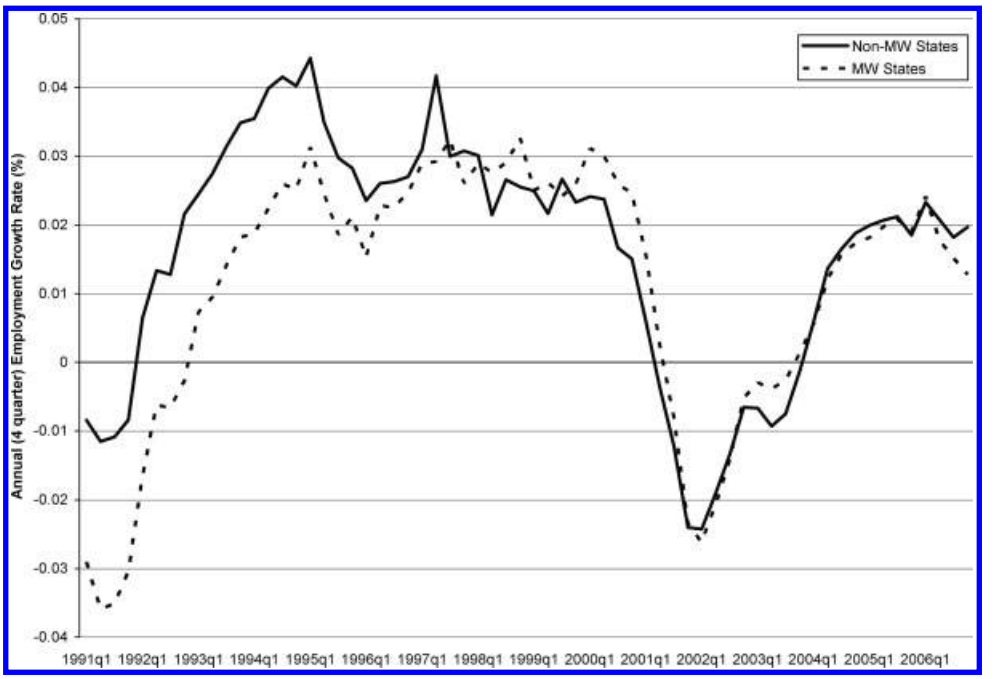
Local case studies typically use fast food chain restaurant data obtained from employers. The restaurant industry is of special interest because it is both the largest and the most intensive user of minimum wage workers. Studies focusing on the restaurant industry are arguably comparable to studies of teen employment, as the incidence of minimum wage workers is similar among both groups, and many of the teens earning the minimum wage are employed in this sector. Card and Krueger (1994, 2000) and Neumark and Wascher (2000) use case studies of fast food restaurant chains in New Jersey and Pennsylvania to construct local comparisons. Card and Krueger (1994) find a positive effect of the minimum wage on employment. However, using administrative payroll data from Unemployment Insurance (ES202) records, Card and Krueger (2000) do not detect any significant effects of the 1992 New Jersey statewide minimum wage increase on restaurant employment. Moreover, they obtain similar findings when the 1996–1997 federal increases eliminated the New Jersey–Pennsylvania differential. Neumark and Wascher (2000) find a negative effect using payroll data provided by restaurants in those two states.

A more recent study (Dube et al., 2007) compares restaurants in San Francisco and the adjacent East Bay before and after implementation of a citywide San Francisco minimum wage in 2004 that raised the minimum from $6.75 to $8.50, with further increases indexed annually to local inflation. Considering both full-service and fast food restaurants, Dube et al. do not find any significant effects of the minimum wage increase on employment or hours.[2] As with the other case studies, however, their data contain a limited before-and-after window. Consequently they cannot address whether minimum wage effects occur with a longer lag. Equally important, individual case studies are susceptible to overstating the precision of the estimates of the minimum wage effect, as they treat individual firm-level observations as being independent (they do not account for spatial autocorrelation). The bias in the reported standard errors is exacerbated by the homogeneity of minimum wages within the treatment and control areas (a point made in Donald & Lang, 2007, and more generally in Moulton, 1990).

Most traditional national-level panel studies use data from the CPS and cross-state variation in minimum wages to identify employment effects. These studies tend to focus on employment effects among teens. Neumark and Wascher (1992) obtain significant negative effects of minimum wages on employment of teenagers, with an estimated elasticity of -0.14. Neumark and Wascher (2007) extend their previous analysis, focusing on the post-1996 period and including state-level linear trends as controls, which their

[2] They do find a shift from part-time to full-time jobs, and a large increase in worker tenure, and an increase in price among fast food restaurants.

FIGURE 1.—ANNUAL EMPLOYMENT GROWTH RATE, MINIMUM WAGE STATES VERSUS NON–MINIMUM WAGE STATES



Source: QCEW.

Annual private sector employment growth rates calculated on a four-quarter basis (for example, 1991Q1 is compared to 1990Q1). Minimum wage states are the seventeen states plus the District of Columbia that had a minimum wage above the federal level in 2005. These states are Alaska, California, Connecticut, Delaware, Florida, Hawaii, Illinois, Maine, Massachusetts, Minnesota, New Jersey, New York, Oregon, Rhode Island, Vermont, Washington, and Wisconsin.

specification tests find cannot be excluded. They obtain mixed results, with negative effects only for minority teenagers, with results varying substantially depending on groups and specifications.[3]

In our view, traditional panel studies do not control adequately for heterogeneity in employment growth. A state fixed effect will control for level differences between states, but both minimum wages and overall employment growth vary substantially over time and space (see figure 1). As recently as 2004, no state in the South had a state minimum wage. Yet the South has been growing faster than the rest of the nation, for reasons entirely unrelated to the absence of state-based minimum wages. Figure 1 illustrates this point more generally by displaying year-over-year employment growth rates for the seventeen states with a minimum wage higher than the federal level in 2005 and for all the other states.

Figure 1 also shows that spatial heterogeneity has a time-varying component. Considering the seventeen states (plus Washington, D.C.) that had a minimum wage above the federal level in 2005, average employment growth in these states was consistently lower than employment growth in the rest of the country between 1991 and 1996. These two groups then had virtually identical growth between 1996 and 2006. Since overall employment growth is not plausibly affected by minimum wage variation, we are observing time-varying differences in the underlying characteristics of the states.

By itself, heterogeneity in overall employment growth may not appear to be a problem, since most estimates control for overall employment trends. Nonetheless, using states with very different overall employment growth as controls is problematic. The presence of such heterogeneity in overall employment suggests that controls for low-wage employment using extrapolation, as is the case using traditional fixed-effects estimates, may be inadequate. Our results indicate that this is indeed the case.[4]

Including state-level linear trends (as in Neumark & Wascher, 2007) does not adequately address the problem, since the estimated trends may themselves be affected by minimum wages. Whether inclusion of these linear trends corrects for unobserved heterogeneity in employment prospects, or whether they absorb low-frequency variation in the minimum wage cannot be answered within such a framework.[5] While we report estimates with state-level trends as additional specifications, our local estimates do not rely on such parametric assumptions.

To summarize, a major question for the recent minimum wage literature concerns whether the differing findings result

[3] Orrenius and Zavodny (2008) use the CPS and also find negative effects on teens.

[4] Other heterogeneities may arise from correlations of minimum wage changes with differential costs of living, regulatory effects on local housing markets, and variations in regional and local business cycle patterns and adjustments.

[5] Indeed, in Neumark and Wascher (2007), the measured disemployment effects for teenagers as a whole become insignificant once state-level linear trends are included.

from a lack of adequate controls for unobserved heterogeneity in most national panel estimates, the lack of sufficient lag time in the case studies, or the overstatement of precision of estimates in the local case studies. As we show in this paper, the key factor is the first: unobserved heterogeneity contaminates the existing estimates that use national variation. And this heterogeneity has a distinct spatial component.

III. Data Sources and Construction of Samples

In this section we discuss why we chose restaurants as the primary industry to study minimum wage effects and a description of our data set and sample construction.

A. Choice of Industry

Restaurants employ a large fraction of all minimum wage workers. In 2006, they employed 29.9% of all workers paid within 10% of the state or federal minimum wage, making restaurants the single largest employer of minimum wage workers at the three-digit industry level (authors' analysis of the Current Population Survey from 2006). Restaurants are also the most intensive users of minimum wage workers, with 33% of restaurant workers earning within 10% of minimum wage at the three-digit level. No other industry has such high intensity of use of minimum wage workers. Given the prevalence of low-wage workers in this sector, changes in minimum wage laws will have more bite for restaurants than for businesses in other industries.

Given our focus on comparing neighboring counties, a focus on restaurants allows us to consider a much larger set of counties than if we considered other industries employing minimum wage workers, as many of these counties do not have firms in these industries.

Finally, studying restaurants also has the advantage of comparability to studies using the CPS that are focused on teens. The proportion of workers near or at the minimum wage is similar among all restaurant workers and all teenage workers, and many teenage minimum wage workers are employed in restaurants. The similarity of coverage rates makes the minimum wage elasticities for the two groups comparable, with the caveat that the elasticities of substitution for these two groups may vary. At the same time, focusing on restaurants allows us to better compare our results with previous case study research, which also were limited to restaurants.[6]

[6] By including all restaurants, both limited service and full service, we incorporate any substitution that might occur among differentially affected components of the industry. Neumark (2006) suggests that take-out stores, such as pizza parlors, might be most affected by a minimum wage increase, thereby buffering effects on fast food restaurants, for which demand may rise relative to take-out shops. By including all restaurants, our analysis accounts for any such intra-restaurant substitution. Moreover, the closest substitute to restaurants consists of food (prepared or unprepared) purchased in supermarkets; this industry has a much lower incidence of minimum wage workers, ruling out such substitution effects.

Although our primary focus is on restaurants, we also present results for the accommodation and food services sector (a broader category than restaurants) and for the retail sector. Finally, as a counterfactual exercise, we present results for manufacturing, an industry whose workforce includes very few minimum wage workers. This industry's wages and employment should not be affected by minimum wage changes.

B. Data Sources

Our research design is built on the importance of making comparisons among local economic areas that are contiguous and similar, except for having different minimum wages. The Current Population Survey (CPS) is not well suited for this purpose due to small sample size and the lack of local identifiers. The best data set with employment and earnings information at the county-level is the Quarterly Census of Employment and Wages (QCEW), which provides quarterly county-level payroll data by detailed industry.[7] The data set is based on ES-202 filings that every establishment is required to submit quarterly for the purpose of calculating payroll taxes related to unemployment insurance. Since 98% of workers are covered by unemployment insurance, the QCEW constitutes a near-census of employment and earnings.[8] We construct a panel of quarterly observations of county-level employment and earnings for Full Service Restaurants (NAICS 7221) and Limited Service Restaurants (NAICS 7222). The full sample frame consists of data from the first quarter of 1990 through the second quarter of 2006 (66 quarters).[9] BLS releases employment and wage data for restaurants for all 66 quarters (the balanced panel) for 1,380 of the 3,109 counties in our 48 states (we exclude Alaska and Hawaii, as they do not border other states).[10]

Our two primary outcome measures are average earnings and total employment of restaurant workers. Our earnings measure is the average rate of pay for restaurant workers. BLS divides the total restaurant payroll in each county in a given quarter by the total restaurant employment level in each county for that quarter, and then reports the average weekly earnings on a quarterly basis. The QCEW does not measure hours worked. In section IVD, we partly address the possibility of hours reduction by comparing the magnitude of our estimates on weekly earnings to what would be expected given the proportion of workers earning minimum wage in the absence of any hours adjustments.

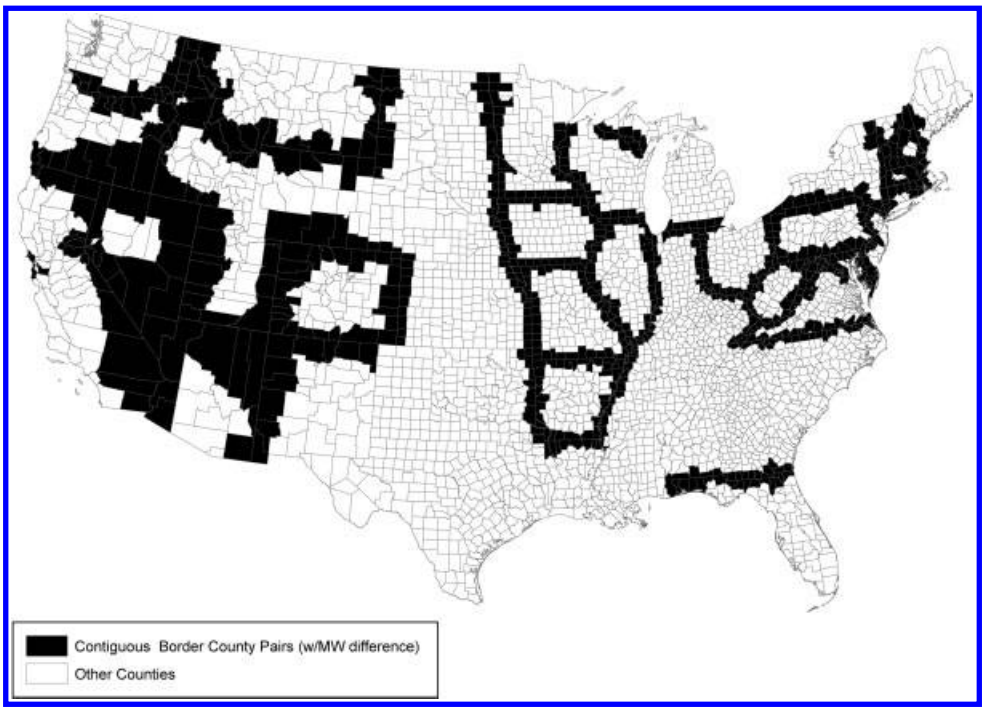
[7] County Business Patterns (CBP) constitutes an alternative data source. In section VB, we discuss the shortcomings of the CBP data set for our purposes and also provide estimates using this data set as a robustness check on our key results.

[8] The 2% who are not covered are primarily certain agricultural, domestic, railroad, and religious workers.

[9] BLS began using the NAICS-based industry classification system in 2001; data are available on a reconstructed NAICS basis (rather than SIC) back to 1990.

[10] Section VC reports the results including counties with partial reporting. Results for this unbalanced panel were virtually the same.

FIGURE 2.—CONTIGUOUS BORDER COUNTY-PAIRS IN THE UNITED STATES WITH A MINIMUM WAGE DIFFERENTIAL, 1990–2006Q2



We merge information on the state (or local) and federal minimum wage in effect in each quarter from 1990q1 to 2006q2 into our quarterly panel of county-level employment and earnings. During the sample period, the federal minimum wage changed in 1991–1992 and again in 1996–1997. The number of states with a minimum wage above the federal level ranged from 3 in 1990 to 32 in 2006.

C. *Sample Construction*

Our analysis uses two distinct samples: a sample of all counties and a sample of contiguous border county-pairs. In section IVB, where we present our empirical specification comparing contiguous border counties, we explain the need for the latter sample in greater detail. Our replication of more traditional specifications uses the full set of counties with balanced panels. This all counties (AC) sample consists of 1,381 out of the 3,081 counties in the United States. The number of counties with a balanced panel of reported data yields a national sample of 91,080 observations.

The second sample consists of all the contiguous county-pairs that straddle a state boundary and have continuous data available for all 66 quarters.[11] We refer to this sample as the contiguous border county-pair (CBCP) sample. The QCEW provides data by detailed industry only for counties with enough establishments in that industry to protect confidentiality. Among the 3,108 counties in the mainland United States, 1,139 lie along a state border. We have a full (66 quarters) set of restaurant data for 504 border counties. This yields 316 distinct county-pairs, although we keep unpaired border counties with full information in our border sample as well. Among these, 337 counties and 288 county-pairs had a minimum wage differential at some point in our sample period.[12] Figure 2 displays the location of these counties on a map of the United States. Since we consider all contiguous county-pairs, an individual county will have p replicates in our data set if it is part of p cross-state pairs.[13]

Table 1 provides descriptive statistics for the two samples. Comparing the AC sample (column 1) to the CBCP sample (column 2), we find that they are quite similar in terms of population, density, employment levels, and average earnings.

D. *Contiguous Border Counties as Controls*

Contiguous border counties represent good control groups for estimating minimum wage effects if there are substantial differences in treatment intensity within cross-state county-pairs, and a county is more similar to its cross-state counterpart than to a randomly chosen county. In contrast, panel and period fixed-effects models used in the national-

[11] As we report below, this exclusion has virtually no impact on our results.

[12] We also use variation in minimum wage levels within metropolitan statistical areas, which occur when the official boundaries of a metropolitan area span two or more states. We use the OMB's 2003 definition of metropolitan areas. Of the 361 core-based statistical areas defined as metropolitan, 24 cross state lines. See note 16 for a full list of cross-state metropolitan areas.

[13] The issue of multiple observations per county is addressed by the way we construct our standard errors. See section IVC.

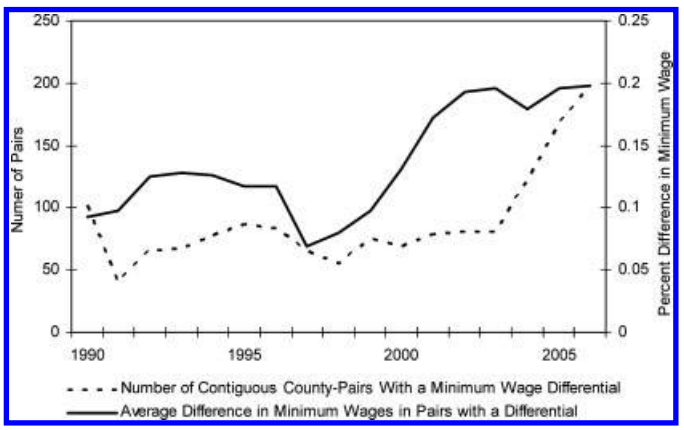
TABLE 1.—DESCRIPTIVE STATISTICS

	(1) All-County Sample		(2) Contiguous Border County-Pair Sample	
	Mean	s.d.	Mean	s.d.
Population, 2000	180,982	423,425	167,956	297,750
Population density, 2000	465	2,553	556	3,335
Land area (square miles)	1,107	1,761	1,380	2,470
Overall private employment	32,179	119,363	32,185	101,318
Restaurant employment	4,508	10,521	4,185	7,809
Restaurant average weekly earnings ($)	171	44	172	46
Accommodation and food services employment	13,226	32,334	12,865	26,862
Accommodation and food services average weekly earnings ($)	273	64	273	67
Retail employment	4,703	14,642	4,543	11,545
Retail average weekly earnings ($)	306	77	304	77
Manufacturing employment	6,608	20,323	6,312	14,100
Manufacturing average weekly earnings ($)	573	202	576	204
Minimum wage	4.84	0.66	4.84	0.67
Number of counties	1,380		504	
Number of county-pairs	NA		318	
Number of states	48		48	

Sample means are reported for all counties in the United States and for all contiguous border county-pairs with a full balanced panel of observations. Standard deviations are reported next to each mean. Weekly earnings and minimum wages are in nominal dollars.

Sources: *QCEW*; U.S Department of Labor, Employment Standards Administration, Wage and Hour Division. U.S. Bureau of the Census, *2000 Census*.

FIGURE 3.—NUMBER OF COUNTY-PAIRS WITH MINIMUM WAGE DIFFERENTIAL AND AVERAGE MINIMUM WAGE DIFFERENTIAL



level estimates implicitly assume that one county in the United States is as good a control as any other.

Figure 3 displays for each year the number of counties that are part of a contiguous county-pair that exhibits a minimum wage differential, as well as the average minimum wage gap in each year. The number of counties that provide the variation to identify a minimum wage effect is sizable, with an increase after 2003. Moreover, there is a substantial pay gap among these counties, and this gap increases in later years in the sample. Between 1990 and 2006, the minimum wage gap between contiguous pairs was between 7% and 20%, and the gap was greater in the later years. In other words, contiguous counties display substantial variation in minimum wages over this period, which allows us to identify minimum wage effects within contiguous county-pairs.

Second, contiguous counties are relatively similar, and hence form better controls, especially with respect to underlying employment trends. We provide more direct evidence on the importance of comparability in section IVE, where we estimate the dynamic response of employment to changes in the minimum wage. We show there that the lead terms capturing employment levels and trends prior to minimum wage increases are much better behaved when we use contiguous county-pairs as controls.

IV. Empirical Strategy and Main Results

A. *Specifications Using the All Counties Sample*

To replicate findings from traditional approaches in the literature, we first estimate earnings and employment effects using the all-counties (AC) sample, including county and period fixed effects. Although the analysis takes place at the county rather than the state level, the specifications are analogous to those in Neumark and Wascher (1992):

$$\ln y_{it} = \alpha + \eta \ln(MW_{it}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_t + \varepsilon_{it}. \quad (1)$$

This specification controls for the log of total private sector employment (or average private sector earnings) denoted as $\ln(y_{it}^{TOT})$, and the log of county-level population $\ln(pop_{it})$ when we estimate employment effects.[14] The ϕ_i term represents a county fixed effect. Crucially, the time period fixed effects (τ_t) are assumed to be constant across counties, which rules out possibly heterogeneous trends.

As two intermediate specifications that control for heterogeneous time trends at a coarse level, we also present estimates that allow the period fixed effects to vary across the

[14] We use county-level Census Bureau population data, which are reported on an annual basis.

nine census divisions and additionally include state-level linear time trends:

$$\ln y_{it} = \alpha + \eta \ln(w_{it}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_{ct} + \varepsilon_{it} \quad (2)$$

$$\ln y_{it} = \alpha + \eta \ln(w_{it}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_{ct} + \xi_s I_s \cdot t + \varepsilon_{it}. \quad (3)$$

The term τ_{ct} sweeps out the between-census division variation, and estimates are based on only the variation within each census division. In equation (3), I_s is a dummy for state s, and ξ_s is a state-specific trend.

Finally, we include a specification with MSA-specific time effects:

$$\ln y_{it} = \alpha + \eta \ln(w_{it}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_{mt} + \varepsilon_{it}. \quad (4)$$

The term τ_{mt} in equation (4) sweeps out the variation between metropolitan statistical areas across the United States. In this case, η is identified on the basis of minimum wage differences within individual metropolitan areas.[15] Within-MSA variation occurs when a given metropolitan definition includes counties from two or more states whose minimum wage levels differ at least once during the sample period.[16] The cross-MSA specification, equation (4), is similar to our local county-pair specification presented above. The main difference is the relatively smaller set of counties providing identifying variation, as the number of cross-state metropolitan areas is much smaller than the number of state border segments.

Together, equations (2), (3), and (4) allow us to characterize the nature of bias in the traditional fixed-effects estimates by considering progressively finer controls for spatial heterogeneity; they constitute intermediate specifications as compared to our contiguous county-pair specification below.

[15] For the San Francisco–Oakland–Fremont MSA, variation in the minimum wage results from San Francisco's 2004 minimum wage increase, which is indexed annually.

[16] Cross-state metropolitan areas include: Allentown-Bethlehem-Easton, PA–NJ; Boston-Cambridge-Quincy, MA–NH; Chicago-Naperville-Joliet, IL–IN–WI; Cumberland, MD–WV; Davenport-Moline-Rock Island, IA–IL; Duluth, MN–WI; Fargo, ND–MN; Grand Forks, ND–MN; Hagerstown-Martinsburg, MD–WV; La Crosse, WI–MN; Lewiston, ID–WA; Minneapolis-St. Paul-Bloomington, MN–WI; New York-Northern New Jersey-Long Island, NJ–NY; Omaha-Council Bluffs, NE–IA; Philadelphia-Camden-Wilmington, PA–NJ–DE; Portland-Vancouver-Beaverton, OR–WA; Providence-New Bedford-Fall River, RI–MA; San Francisco-Oakland-Fremont, CA; Sioux City, IA–NE–SD; South Bend-Mishawaka, IN–MI; St. Louis, MO–IL; Washington-Arlington-Alexandria, DC–VA–MD; Weirton-Steubenville, WV–OH; Youngstown-Warren-Boardman, OH–PA.

B. *Identification Using the Contiguous Border County-Pair Sample*

Our preferred identification strategy exploits variation between contiguous counties straddling a common state boundary and uses the sample with all such contiguous border county-pairs. Since this strategy involves a change in samples (going from the AC to CBCP sample) as well a change in specification, we also estimate an analog to equation (1) with common time period fixed effects in the CBCP sample, where y_{ipt} and e_{ipt} denote that counties may be repeated for all pairs they are part of:

$$\ln y_{ipt} = \alpha + \eta \ln(MW_{it}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_t + \varepsilon_{ipt}. \quad (5)$$

Finally, for our preferred specification, we allow for pair-specific time effects (τ_{pt}), which use only variation in minimum wages within each contiguous border county-pair:

$$\ln y_{ipt} = \alpha + \eta \ln(w_{it}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_{pt} + \varepsilon_{ipt}. \quad (6)$$

Our identifying assumption for this local specification is $E\left(\ln(w_{it}^{M}), \varepsilon_{ipt}\right) = 0$, that is, minimum wage differences within the pair are uncorrelated with the differences in residual employment (or earnings) in either county.

An important observation is that equation (6) is not identified using the AC sample and including pair period effects for all contiguous county-pairs. At first blush, this may seem odd, as we could identify within-MSA effects by including a set of MSA-period dummies as in equation (4). However, county-pairs do not form a unique partitioning (unlike an MSA). Each observation would have many pair-period dummies, and we would need to include a vector of such pair-period effects τ_{pt}. But the number of all contiguous county-pairs far exceeds the number of counties in the United States. Therefore, if we were to use the AC sample and include pair-period dummies for all contiguous pairs, the number of variables that we would need to estimate would far exceed the number of observations. Even for the set of border counties and cross-border pairs, the model is under identified if we try to jointly estimate all the pair-identified coefficients, since we have 754 pairs and 504 border counties. Given this problem, we use the CBCP sample to identify equation (6).

What allows us to identify equation (6) using the CBCP? Note that the CBCP sample stacks each border county-pair, so that a particular county will be in the sample as many times as it can be paired with a neighbor across the border. Here τ_{pt} is the coefficient for each pair-period dummy for each of the 754 pairs. Given our sample construction, each observation has a nonzero entry only for a single pair-period dummy. This property allows us to mean difference all the variables within each pair-period group, treating τ_{pt} as a

nuisance parameter. Equation (6) is identified using the CBCP sample because we do not try to estimate each pair-period coefficient taking into account the cross-correlations of all pairs. We do not need to do this, as each pair provides a consistent estimate of the treatment effect based on our identifying assumption that $E\left(\ln(w_{it}^{M}), \varepsilon_{ipt}\right) = 0$. Hence, equation (6) uses the within-pair variation across all pairs and effectively pools the estimates.

C. Standard Errors

The OLS standard errors are subject to three distinct sources of possible bias. For all specifications, there is positive serial correlation in employment at the county level, and the treatment variable (minimum wage) is constant within each state. Both of these factors cause the standard errors to be biased downward (see Moulton, 1990; Kedzi, 2004; and Bertrand, Duflo, & Mullainathan, 2004). For estimates using the all-county sample, we cluster the standard errors at the state level to account for these biases.

For our sample of all contiguous border county-pairs, the presence of a single county in multiple pairs along a border segment induces a mechanical correlation across county-pairs, and potentially along an entire border segment.[17] Formally, this implies that $E(e_{ipt}, e_{i'p't'}) \neq 0$ if $i, i' \in S$, or if $p, p' \in B$. The residuals are not independent if the counties are within the same state S or if the two pairs are within the same border segment B.

To account for all these sources of correlation in the residuals, standard errors for estimates based on the contiguous border county-pair sample are clustered on the state and border segment separately.[18] The variance-covariance matrix with this two-dimensional clustering can be written as $VC_{S,B} = VC_S + VC_B\text{-}VC_{S\cap B}$. Finally, our standard errors also correct for arbitrary forms of heteroskedasticity.

D. Main Findings

Table 2 reports the earnings and employment effects for all six specifications—each one with or without including the log of average private sector earnings (or total private sector employment) as controls.

The earnings elasticities all range between 0.149 and 0.232.[19] All of these coefficients are significant at the 1% level. It is reassuring that the impact of the minimum wage in the traditional specification 1 (0.217) is quite similar to the impact in our local specification 6 (0.188) that compares contiguous counties. This result rules out the possibility that

TABLE 2.—MINIMUM WAGE EFFECTS ON EARNINGS AND EMPLOYMENT

	All-County Sample						Contiguous Border County-Pair Sample			
	(1)		(2)		(3)	(4)	(5)		(6)	
ln Earnings										
lnMWt	0.224***	0.217***	0.204***	0.195***	0.219***	0.210*** 0.153***	0.149***	0.232***	0.221***	0.200*** 0.188***
	(0.033)	(0.028)	(0.038)	(0.034)	(0.037)	(0.034) (0.030)	(0.028)	(0.032)	(0.032)	(0.065) (0.060)
Ln Employment										
lnMWt	−0.211**	−0.176*	−0.028	−0.023	0.054	0.039 0.052	0.032	−0.137*	−0.112	0.057 0.016
	(0.095)	(0.096)	(0.066)	(0.068)	(0.055)	(0.050) (0.084)	(0.078)	(0.072)	(0.076)	(0.118) (0.098)
lnpop or lnpop+lntotprivatesector	1.04***	1.05***	1.04***	1.05***	1.07***	1.05*** 1.30***	1.21***	0.95***	0.97***	1.12*** 1.11***
	(0.060)	(0.058)	(0.048)	(0.043)	(0.045)	(0.039) (0.065)	(0.048)	(0.073)	(0.073)	(0.190) (0.189)
P values for H0:										
bs = b1 for s=2,3,4, bs = b4 for s=6				0.022		0.066	0.011			0.056
Labor demand elasticity		−0.787*		−0.114		0.183	0.211		−0.482**	0.079
		(0.427)		(0.332)		(0.219)	(0.507)		(0.235)	(0.286)
Controls										
Census division × period dummies			Y	Y	Y	Y				
State linear trends					Y	Y				
MSA × period dummies							Y	Y		
County-pair × period dummies									Y	Y
Total private sector		Y		Y		Y		Y		Y

Sample size equals 91,080 for specifications 1, 2, and 3 of the all-county sample and 48,348 for specification 4 (which is limited to MSA counties) and 70,620 for the border county-pair sample. All of the employment regressions control for the log of annual county-level population. Total private sector controls refer to log of average total private sector earnings or log of employment. All samples and specifications include county fixed effects. Specifications 1, 3, and 5 include period fixed effects. Specification 3 also includes state-level linear trends. For specifications 2, 4, and 6, period fixed effects are interacted with each census division, metropolitan area, and county-pair, respectively. Robust standard errors, in parentheses, are clustered at the state level for the all-county samples (specifications 1–4) and on the state *and* border segment levels for the border pair sample (specifications 5 and 6). Probability values are reported for tests under the null hypothesis that the minimum wage coefficients are equal across specification 1 and specifications 2, 3, and 4 and between specifications 5 and 6. For the labor demand elasticity, we jointly estimate the earnings and employment equations using seemingly unrelated regression, and the labor demand elasticity is computed as the ratio of the employment effect divided by the earnings effect. The standard errors for the SUR are clustered at the same level as indicated before. Significance levels: *10%, **5%, ***1%.

[17] A border segment is defined as the set of all counties on both sides of a border between two states.

[18] For more details, see Cameron, Gelbach, and Miller (2006). The number of clusters on both these dimensions exceeds forty, which is large enough to allow reliable inference using clustered standard errors.

[19] Given the double-log specification, throughout the paper we refer to the treatment coefficient η as the elasticity. However, for values that are not close to 0, the true elasticity is $\exp(\eta)$—in this case, $\exp(0.22) = 0.25$.

TABLE 3.—PREEXISTING TRENDS IN EMPLOYMENT AND EARNINGS AND VALIDITY OF CONTROLS

	Specification 1		Specification 4		Specification 6	
	Restaurants	All Private Sector	Restaurants	All Private Sector	Restaurants	All Private Sector
			ln Earnings			
η_{t-12}	0.002	−0.013	−0.042	−0.005	0.029	0.025
	(0.019)	(0.016)	(0.036)	(0.044)	(0.048)	(0.043)
η_{t-4}	0.001	−0.001	0.051	0.007	0.068	0.051
	(0.042)	(0.036)	(0.061)	(0.053)	(0.080)	(0.081)
Trend	−0.001	0.012	0.093***	0.012	0.039	0.026
$(\eta_{t-4}-\eta_{t-12})$	(0.029)	(0.024)	(0.034)	(0.024)	(0.059)	(0.053)
N	82,800	82,787	43,980	43,969	64,200	64,174
			ln Employment			
η_{t-12}	−0.071	−0.037	0.025	0.005	0.009	0.025
	(0.057)	(0.027)	(0.069)	(0.034)	(0.067)	(0.068)
η_{t-4}	−0.194*	−0.076	−0.016	0.004	0.050	0.084
	(0.115)	0.061	(0.127)	(0.051)	(0.172)	(0.145)
Trend	−0.124*	−0.039	−0.041	−0.002	0.041	0.058
$(\eta_{t-4}-\eta_{t-12})$	(0.070)	(0.035)	(0.077)	(0.033)	(0.134)	(0.095)
N	82,800	82,787	43,980	43,969	64,200	64,174
Controls						
MSA × period dummies			Y	Y		
County-pair × period dummies					Y	Y

Here $t-j$ denotes j quarters prior to the minimum wage change. η_{t-12} is the coefficient associated with $(\ln(MW_{t-4}) - \ln(MW_{t-12})$ term in the regression; η_{t-4} is the coefficient associated with $(\ln(MW_t) - \ln(MW_{t-4})$ term; and all specifications also include contemporaneous minimum wage $\ln(MW_t)$ as a regressor in levels. All specifications include county fixed effects, and all the employment specifications include log of county-level population. Specification 1 includes common time dummies; specification 4 includes MSA-specific time dummies; and specification 6, county–pair specific time dummies. Robust standard errors in parentheses are clustered at the state level (for specifications 1 and 3), and at the state and border segment level for specification 6. Significance levels: *10%, **5%, ***1%.

the employment effects may be different in the local specification because minimum wages may be differentially binding.

In contrast, the employment effects vary substantially among specifications. The employment effects in the traditional specification in the AC sample (specification 1) range between −0.211 and −0.176, depending on whether controls for overall private sector employment are included and between −0.137 and −0.112 in the CBCP sample (specification 4). We also report the implied labor demand elasticities by jointly estimating the earnings and employment effects using seemingly unrelated regression where the residuals from the earnings and employment equations are allowed to be correlated across equations (while also accounting for correlation of the residuals within clusters). The implied labor demand elasticities for the traditional fixed-effects specifications are −0.787 and −0.482 in the AC and CBCP samples (specifications 1 and 5) and are significant at the 10% and 5% level, respectively. Overall, the traditional specifications generate negative minimum wage and labor demand elasticities that are similar in magnitude to previous CPS-based panel studies that focus on teenagers.

In contrast, even intermediate forms of control for spatial heterogeneity through the inclusion of either census division–specific time period fixed effects (specification 2), division–specific time fixed effects and state-level linear time trends (specification 3), or metropolitan area–specific time fixed effects (specification 4) leads the coefficient to be close to 0 or positive. In our preferred specification 6, we find that comparing only within contiguous border county-pairs, the employment elasticity is 0.016 when we also control for overall private sector employment. Bounds for this estimate rule out elasticities more negative than −0.147 at the 90% confidence level and −0.178 at the 95% confidence level.[20] The implied labor demand elasticities are also, as expected, close to 0 and insignificant at conventional levels.[21]

The results are consistent with the hypothesis that the traditional approach with common time period fixed effects suffer from serious omitted variables bias arising from spatial heterogeneity. Table 3 reports probability tests for the equality of the employment elasticity estimates across specifications. In the AC sample, we test coefficients from specifications 2, 3, and 4 to the coefficient in specification 1, and in the CBPC sample, we test the coefficients from specification 6 to specification 5.[22] The p-values are 0.022, 0.066, 0.011, and 0.056, respectively—showing that in all cases, we can reject the null that the controls for spatial heterogeneity do not affect the minimum wage estimates at least at the 10% level.

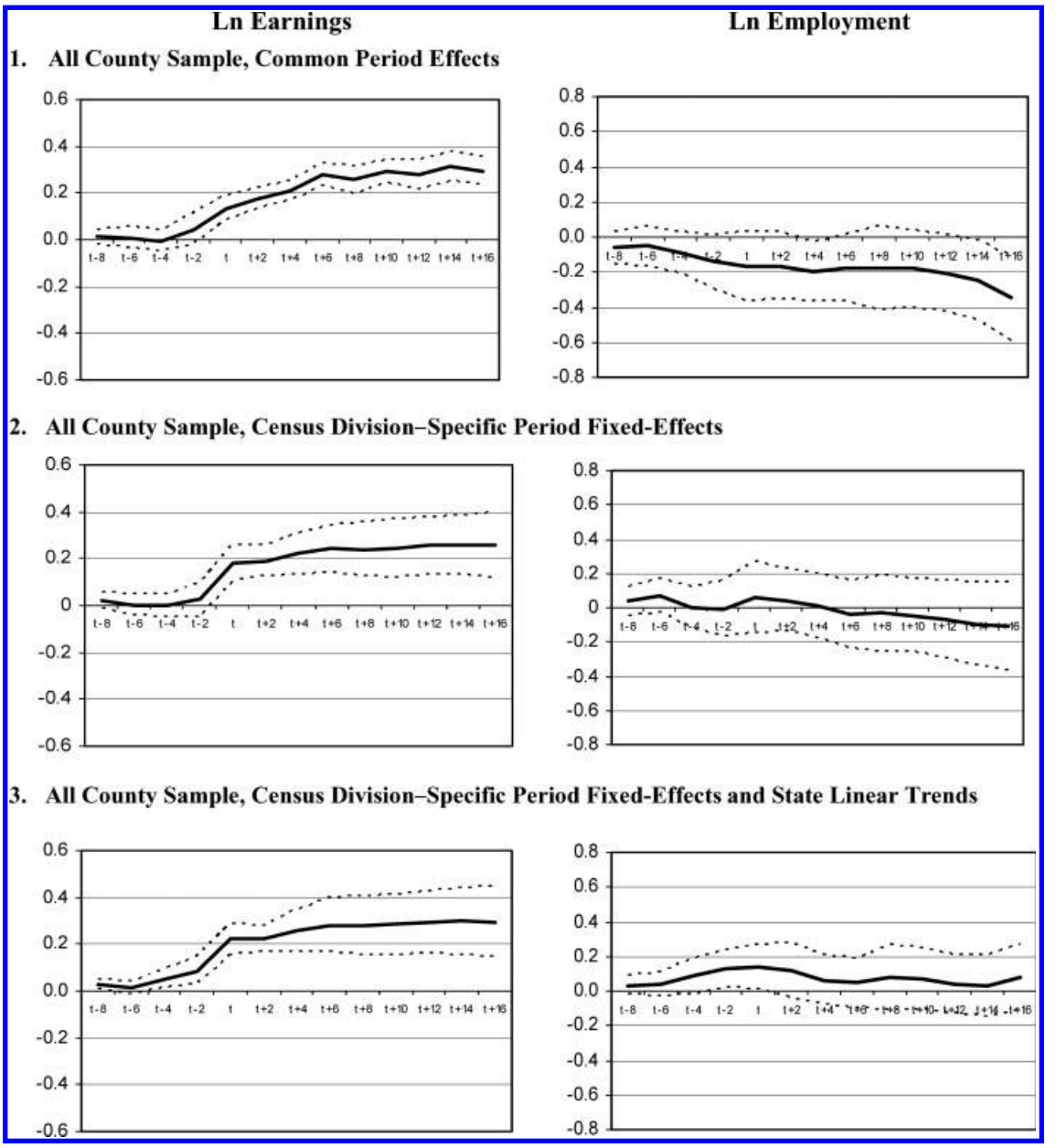
In table A1 in Appendix A, we also report estimates for each of the five primary specifications (1, 2, 4, 5, and 6)

[20] A comparison of the standard errors with and without clustering shows that the unclustered standard errors are understated by a factor between five and twelve, suggesting that the implied precision of some of the estimates in the literature may have been overstated because of inattention to correcting for correlated error terms. But since the data sets in question are different, further research is needed to confirm this hypothesis.

[21] The estimated coefficients for log population reported in table 2 are around unity across the relevant specifications. When both log population and log of private sector employment are included, the sum of the coefficients is always close to unity. This result suggests that results would be virtually identical if we had normalized all employment by population; we corroborate this in section VB for our preferred specification.

[22] We test for the cross-equation stability of the coefficients by jointly estimating the equations using seemingly unrelated regression (SUR), allowing for the standard errors to be clustered at the appropriate levels.

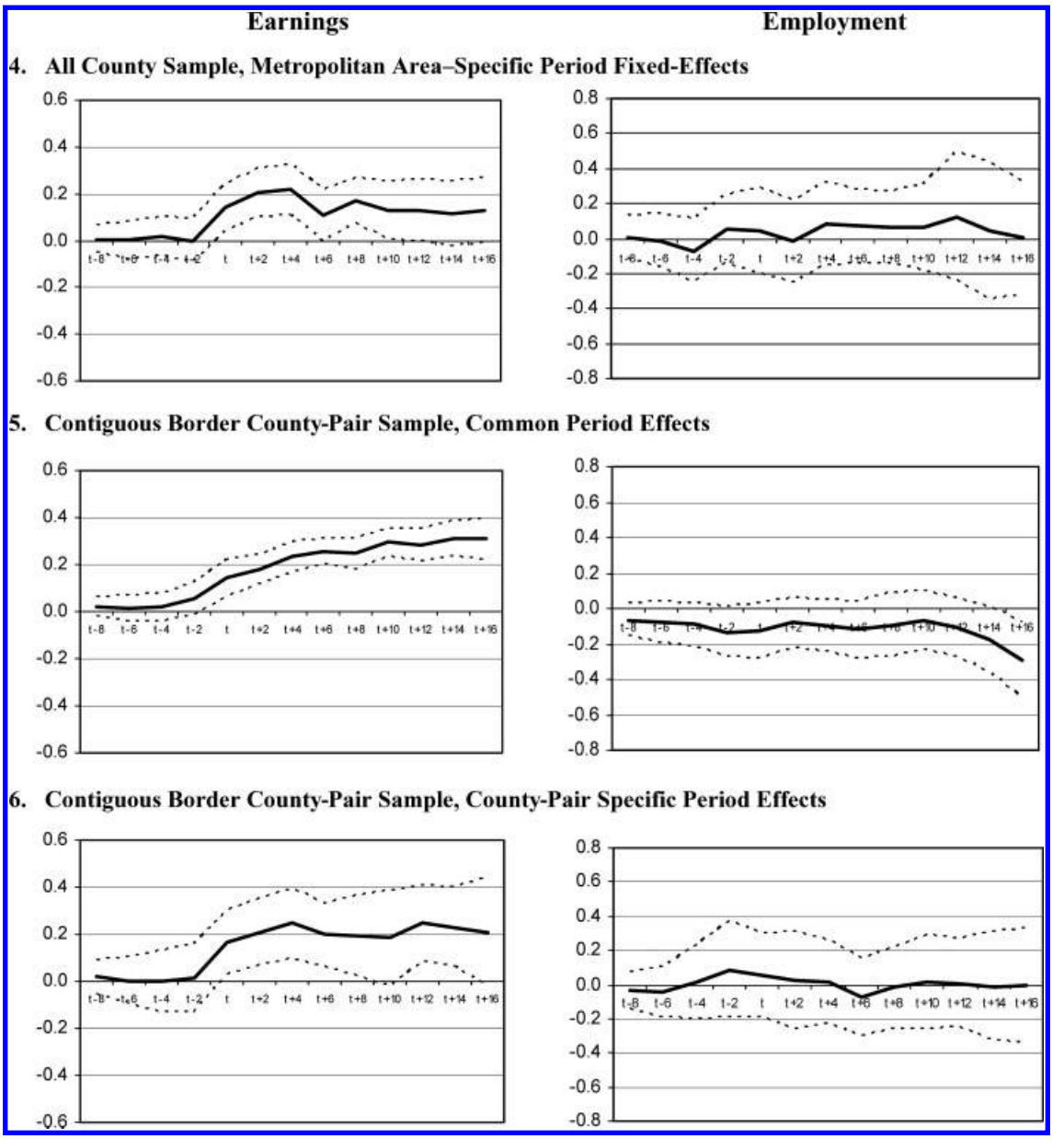
FIGURE 4.—TIME PATHS OF MINIMUM WAGE EFFECTS, BY SAMPLE AND SPECIFICATION



with and without the inclusion of a state-level time trend (specification 3 is just specification 2 with such a trend and has been reported in table 2). We find that the traditional specifications with common time effects (1 and 5) are particularly sensitive to the inclusion of such a linear trend. The sensitivity of the estimates from the traditional specification (1) to the inclusion of a linear time trend does not necessarily imply that it is biased. Inclusion of parametric trends may "overcontrol" if minimum wages themselves reduce the employment trends of minimum wage workers, as the two coefficients are estimated jointly under functional form assumptions. However, the estimates from including such linear time trends in our local specification (6) are virtually identical with respect to both the point estimate and the standard error. This combination of evidence provides further internal validity to our local specification using discontinuity at the policy borders.

One limitation of the QCEW data is that we do not observe hours of work. Therefore, although the effect of minimum wages on head count employment is around 0 in our local specification, it is possible that there is some reduction in hours. Here we provide some rough calculations that place bounds on the hours effect. To begin, note that the minimum wage elasticity of weekly earnings is 0.188. This elasticity reflects the combined effect on hourly wages and weekly hours. If we can use auxiliary estimates

FIGURE 4.—(*CONTINUED*)



The cumulative response of minimum wage increases using a distributed lag specification of four leads and sixteen lags based on quarterly observations. All specifications include county fixed effects and control for the log of annual county-level population. Specifications 1 and 4 (panels 1 and 4) include period fixed effects. Specification 3 includes state-level linear trends. Specification 2 includes census division–specific period fixed effects, and specification 5 includes county-pair–specific period fixed effects. For all specifications, we display the 90% confidence interval around the estimates in dotted lines. The confidence intervals were calculated using robust standard errors clustered at the state level for specifications 1, 2, and 4 (panels 1, 2, and 4) and at both the state level and the border segment level for our local estimators (panels 3, 5, and 6).

on how much earnings "should" rise absent an hours effect, we can approximate the effect on hours.

Using the 2006 CPS, we find that 23.0% of restaurant workers (at the three-digit NAICS level) earn no more than the minimum wage. The difference between our earnings elasticity of 0.188 and this 0.230 figure suggests a −0.042 elasticity for hours. It is likely, however, that some workers below the minimum wage do not get a full increase because of tip credits in some states, that some additional workers above the old minimum wage but below the new minimum get a raise, and that some workers even above the new minimum wage get a raise because of wage spillovers.

While a full accounting of these effects is beyond the scope of this paper, we can provide a very approximate bound for a 10% increase in the minimum wage. About 32.5% of restaurant workers nationally are paid no more than 10% above the minimum wage.[23] Assuming a uniform distribution of wages between the new and old minimum suggests a minimum wage elasticity for hours of −0.090. However, this estimate is likely to be an upper bound, as not all of those below the minimum will get a full increase. We conclude that the elasticity of weekly earnings is relatively

[23] Authors' calculations based on the current population survey.

close to the percentage of workers earning the minimum wage and that the fall in hours is unlikely to be large.

E. Dynamic Responses to Minimum Wage Increases

Changes in outcomes around the actual times of minimum wage changes provide additional evidence on the long-term effects of minimum wages, as well on the credibility of a research design by evaluating trends prior to the minimum wage change. Since we have numerous and overlapping minimum wage events in our sample, we do not employ a pure event study methodology using specific minimum wage changes. Instead, we estimate all the five specifications with distributed lags spanning 25 quarters, where the window ranges from $t + 8$ (eight quarters of leads) to $t - 16$ (sixteen quarters of lags) in increments of two quarters:

$$\begin{aligned}\ln y_{it} = \alpha + \sum_{j=-4}^{7}(\eta_{-2j}\Delta_2 \ln(w^M_{i,t+2j})) + \eta_{-16} \\ \times \ln(w^M_{i,t-16}) + \delta \ln(y^{TOT}_{it}) + \gamma \ln(pop_{it}) + \phi_i \\ + (Time\ Controls) + \varepsilon_{it}.\end{aligned} \tag{7}$$

Here Δ_2 represents a two-quarter difference operator. Specifying all but the last (the sixteenth) lag in two-quarter differences produces coefficients representing cumulative as opposed to contemporaneous changes to each of the leads and lags in minimum wage.[24] *Time controls* refer to either common time effects (with and without state-time trends), or division, MSA, or county-pair–specific time effects, depending on the specification.

Figure 4 reports the estimated cumulative response of minimum wage increases. The full set of coefficients and standard errors underlying the figure is reported in table A2 in the Appendix A. The cumulative response plots consistently show sharp increases in earnings centered around time t— the time of the minimum wage increase. The maximal effects range from 0.215 to 0.316, depending on the specification, and most of the increase occurs within a few quarters after the minimum wage change.

With regard to employment, the estimates from the traditional fixed-effects specification (1) show that restaurant employment is both unusually low and falling during the two years prior to the minimum wage increase, and it continues to fall subsequently. This general pattern obtains when the same specification is estimated using the border county-pair sample (specification 5) with common time effects. In contrast, the cumulative responses for the local estimates (specification 6) using variation within contiguous county-pairs is quite different. First, we see relatively stable coefficients for the leads centered around 0. Second, we do not detect any delayed effect from the increase in the minimum wage with sixteen quarters of lags, though the precision of the estimates is lower for longer lags. Intermediate specifications (2, 3, and 4) with coarser controls for heterogeneity in employment show similar results to the local specification (6).

Baker, Benjamin, and Stanger (1999) proposed a reconciliation for divergent findings in the minimum wage literature by suggesting that short-term effects of minimum wages (those associated with high-frequency variation in minimum wage) are close to 0, while the longer-run effects (associated with low-frequency variation) are negative. We do not find any evidence in our data to support this conclusion. Long-run estimates in our local specification are very similar to shorter-run estimates, and both are close to 0. In contrast, the measured long-term effects in specifications that do not account for heterogeneous trends are more biased downward than are short-run estimates in those models.

We also formally test for the presence of preexisting trends that seem to contaminate the traditional fixed-effects specification and whether contiguous counties are more valid controls. To do so, we now employ somewhat longer leads in the minimum wage and estimate the following equation:

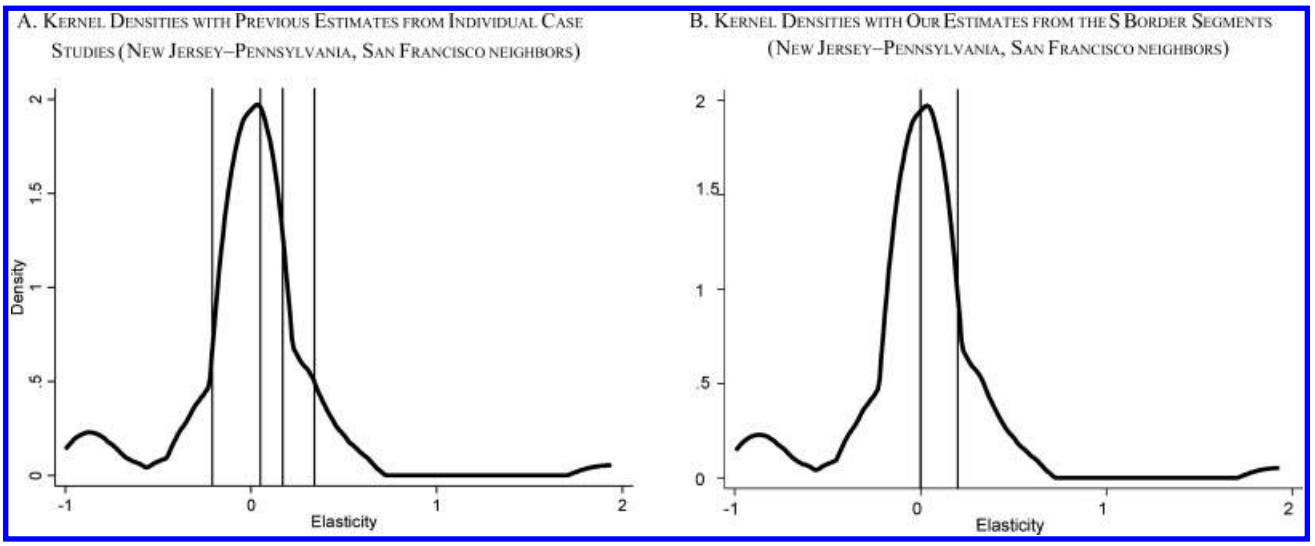
$$\begin{aligned}\ln y_{it} = \alpha + \eta_{12}(\ln(w^M_{i,t+12} - w^M_{i,t+4})) \\ + \eta_4(\ln(w^M_{i,t+4} - w^M_{i,t})) + \eta_0 \ln(w^M_{i,t}) \\ + \gamma \ln(pop_{it}) + \phi_i + (Time\ Controls) + \varepsilon_{it}.\end{aligned} \tag{8}$$

This specification is of the same structure as equation (7) in terms of using differences and levels to produce a cumulative response to a minimum wage shock, but is focused only on the leading terms. Here η_{12} captures the level of $\ln(y)$ 12 quarters (3 years) prior to a log point minimum wage shock, and η_4 captures the level 4 quarters (1 year) prior to the shock. We report point estimates and standard errors for these two terms, as well as $(\eta_4 - \eta_{12})$, which captures the trend between $(t - 12)$ and $(t - 4)$, where t is the year of the minimum wage change. We do so for the traditional fixed-effects specification (1) with common time dummies, specification 4 with MSA-specific dummies, and our preferred contiguous border county-pair specification (6) with pair-specific time dummies. Table 3 reports the results for restaurant employment, total private sector employment, average restaurant earnings, and average private sector earnings.

In terms of earnings, neither the traditional specification (1) nor our preferred specification (6) shows any pretrends

[24] Using leads and lags for every quarter, as opposed to every other quarter, produces virtually identical results. We choose this specification to reduce the number of reported coefficients while keeping the overall window at 25 quarters. Also, the reason we use only 8 quarters of leads is to keep the estimation sample in the dynamic specification the same as the contemporaneous one, since at the time of writing, we had 2 years of minimum wages after 2006q2, the last period in our estimation sample. When we test preperiod leads below, we use 12 quarters of leads to better identify preexisting trends.

FIGURE 5.—DISTRIBUTION OF ELASTICITIES FROM INDIVIDUAL BORDER SEGMENTS AND SPECIFIC CASE STUDY ESTIMATES



Both graphs show the (same) kernel density estimate of the distribution of elasticities from each of the 64 border segments with a minimum wage differential, using a bandwidth of 0.1. In panel A, estimates from previous individual case studies (New Jersey–Pennsylvania and San Francisco–neighboring counties) are superimposed as vertical lines. These are Neumark and Wascher (2000), −0.21; Dube et al. (2007), 0.03; Card and Krueger (2000), 0.17; and Card and Krueger (1994), 0.34. In panel B, the vertical lines represent specific estimates of the same two borders using our data: New Jersey–Pennsylvania is −0.001; San Francisco–neighboring counties is 0.20.

for either overall earnings or restaurant earnings. The cross-state MSA specification seems to show some positive pretrend for restaurant earnings, though the level coefficients for both $(t - 12)$ and $(t - 4)$ are relatively small.

More importantly, we find evidence of a preexisting negative trend in restaurant employment for the fixed-effects specification. Restaurant employment was clearly low and falling during the $(t - 12)$ to $(t - 4)$ period. The η_4 coefficient and the trend estimate $(\eta_4 - \eta_{12})$ are both negative (−0.194 and −0.124, respectively), and significant at the 10% level. In contrast, none of the employment lead terms are ever significant or sizable in our contiguous county specification or in the cross-state MSA specification. Overall, the findings here provide additional internal validity to our research design and show that contiguous counties provide reliable controls for estimating minimum wage effects on employment. And they demonstrate that the assumption in traditional fixed-effects specification that all counties are equally comparable (conditional of observables) is erroneous due to the presence of spatial heterogeneity.

F. Implications for the Individual Case Study Literature

The local specification comparing contiguous counties can be interpreted as producing a pooled estimate from individual case studies. To facilitate this interpretation, in this section we report estimates of equation (6) separately for each of the 64 border segments that have a minimum wage difference over the period under study. We plot the resulting density of the minimum wage elasticities for employment in figure 5. For illustrative purposes, we also include in figure 5 our estimates for some key individual case studies (New Jersey–Pennsylvania and San Francisco–surrounding areas) that have been the subject of individual case studies. Panel A plots the estimates in the literature as overlaid vertical lines; panel B plots our corresponding estimates for the same border segments.

As figure 5 indicates, the estimated employment elasticities from individual case studies are concentrated around 0. If we construct a pooled estimate by averaging these individual estimates, the estimate (−0.006) is virtually identical to the estimate from specification 6 in table 2, while the standard error (0.049) is somewhat smaller.[25] However, figure 5 also shows that the probability of obtaining an individual estimate that is large—either positive or negative—is nontrivial, which can explain why estimates for individual case studies have sometimes varied. Estimates for individual case studies are less precisely measured than suggested by the reported standard errors based on only the sampling variance, as the latter does not account for spatial autocorrelation. Therefore, while any given case study provides a consistent point estimate accounting for spatial heterogeneity, the pooled estimate is much more informative than an individual case study when it comes to statistical inference.

G. Falsification Tests Using Spatially Correlated Placebo Laws

To provide a direct assessment of how the national estimates are affected by spatial heterogeneity, in Appendix B, we present estimates of the effect of spatially correlated fictitious placebo minimum wages on restaurant employment for counties in states that never had a minimum wage other than the federal one. Our strategy is to consider only states

[25] The findings on the standard error are not surprising, as treating each border segment as a single observation is similar to clustering on the border segment. Our double-clustering also accounts for the additional correlation of error terms across multiple border segments for the same state.

that have exactly the same minimum wage profiles, but that happen to be located in a "neighborhood" with higher minimum wages. If there is no confounding spatial correlation between minimum wage increases and employment growth, the estimated elasticity from the fictitious minimum wage should be 0.

More precisely, we start with the full set of border county-pairs in the United States. We then construct two samples: (1) all border counties in states that have a minimum wage equal to the federal minimum wage during this whole period, and hence have no variation in the minimum wage among them (we call this the *placebo sample*, as the true minimum wage is constant within this group), and (2) all border counties that are contiguous to states that have a minimum wage equal to the federal minimum wage during this whole period. We call this the *actual sample*, as the minimum wage varies within this group. The exact specifications and other details as well as the estimates are presented in Appendix B.

As reported in table B1 in Appendix B, we obtain results similar to the national estimates (in table 2), with an employment effect of −0.21. The standard errors are larger due to the smaller sample size. The earnings effects are strong and essentially the same as before. When we examine the effect of the neighbor's minimum wage on the county in the placebo sample, we do not find significant earnings effects. This is expected, since the minimum wages in these counties are identical and unchanging. However, we find large negative employment effects from these fictitious placebo laws. Although minimum wages never differed among these states, changes in the placebo (or neighboring) minimum wages are associated with large apparent employment losses, with an elasticity of −0.12.

As we discuss in section VA, we do not find actual (causal) cross-border spillovers in earnings or employment. Therefore, the estimates from placebo laws provide additional evidence that spatial heterogeneity in low-wage employment prospects is correlated with minimum wages, and these trends seriously confound minimum wage effects in traditional models using national-level variation.

V. Robustness Tests

A. Cross-Border Spillovers

Although we find positive earnings effects and insignificant employment effects in table 2 and figure 4, spillovers between the treatment and control counties may be affecting our results. Spillovers may occur when either the labor or product market within a county-pair is linked. We have two sets of theoretical spillover possibilities, each associated with a specific labor market model. In the case of a perfectly competitive labor market, the increase in wage rates and the resulting disemployment in county A might reduce earnings and increase employment in county B. This model suggests that the disemployment effects will be stronger in counties across the state border than in the interior counties of the state that raises the minimum wage. We call this the *amplification effect*.

In the case of a labor market model with worker search costs, the possibility of employment at a higher minimum wage in county A across the border pressures employers in county B to partly match the earnings increase. In this case, the rise in wages in A leads to a rise in wages in B. This possibility could also arise in an efficiency wage model, in which the reference point for workers in B changes as they see their counterparts across the border earning more. Either way, the wage increase in A would result in a decrease in employment in A and B. If that is the case, comparing border counties will understate the true effect, and the observed disemployment effect will be larger in the interior counties. We call this the *attenuation effect*.

To test for the possibility of any border spillovers, we compare the effect on border counties to the effect on the counties in the interior of the state, which are less likely to be affected by such spillovers. We estimate the following spatial differenced specification:

$$\left(\ln y_{ipt} - \ln\overline{y_{st}}\right) = \alpha + \eta \ln(w_{it}^{M}) + \delta\left(\ln y_{ipt}^{TOT} - \ln\overline{y_{st}^{TOT}}\right) + \gamma\left(\ln pop_{ipt} - \ln\overline{pop_{st}}\right) + \phi_i + \tau_{pt} + \varepsilon_{it}. \tag{9}$$

Here, $\overline{y_{st}}$ refers to the average employment (or earnings) of restaurant workers in the interior counties of state s in time t and serves as a control for possible spillover effects. We use all counties in the state interior (not adjacent to a county in a different state) that report data for all quarters. Similarly $\overline{y_{st}^{TOT}}$ is the average employment (or earnings) of all private sector workers in the interior counties. The spatial differencing of the state interior means that the coefficient η is the effect of a change in the minimum wage on one side of the border on the outcome relative to the state interior, in relation to the relative outcome on the other side of the border. In terms of employment, a significant negative coefficient for η indicates an amplification effect when we consider contiguous border counties, while a positive coefficient indicates an attenuation effect. We also present results from using just the interior counties while considering the same cross-state pairs:[26]

$$\ln\overline{y_{st}} = \alpha + \eta \ln(w_{it}^{M}) + \delta \ln\overline{y_{st}^{TOT}} + \gamma \ln\overline{pop_{st}} + \phi_i + \tau_{pt} + \varepsilon_{it}. \tag{10}$$

When we difference our county-level outcome from the state interior, as in equation (10), we are introducing a mechanical correlation in the dependent and control variables

[26] Here the unit of observation is still county by period, so there are duplicated observations (as the statewide aggregates are identical for all counties within a state). However, since we cluster on both state and the border counties, the duplication of observations does not bias our standard errors. The reason we follow this strategy is to keep the same number of counties (per state) as in equation (9).

TABLE 4.—TESTS OF CROSS-BORDER SPILLOVER EFFECTS FROM MINIMUM WAGE CHANGES

	(1) Border Counties	(2) Border Counties	(3) Interior Counties	(4) Spillover = (Border − Interior)
		ln Earnings		
$lnMW_t$	0.188***	0.165***	0.164	−0.008
	(0.060)	(0.056)	(0.113)	(0.112)
		ln Employment		
$lnMW_t$	0.016	0.011	0.042	−0.058
	(0.098)	(0.109)	(0.107)	(0.139)
Sample	Baseline CBCP	Spillover	Spillover	Spillover
N	70,620	69,130	69,130	69,130
Controls				
County-pair × period dummies	Y	Y	Y	Y
Total private sector	Y	Y	Y	Y

The spillover sample (columns 2, 3, 4) restricts observations to states with interior counties; Delaware, Rhode Island, Washington, D.C., and San Francisco border segments are dropped from the baseline sample. Population control refers to the log of annual county-level population. Overall private sector controls refer to log of average private sector earnings or log of overall private sector employment depending on the regression. All samples and specifications include county fixed effects and county-pair–specific time effects as noted in the table. Robust standard errors in parentheses. We report the maximum of the standard errors that are clustered on (1) the state only, (2) the border segment only, and (3) the state and border segment separately. In all cases, the largest standard errors resulted from clustering on the state and border segment separately. Significance levels: *10%, **5%, ***1%.

across counties within the same state, even when they are not on the same border segment. This correlation is accounted for, however, in our calculation of standard errors, as we allow two-dimensional clustering by state and by each border segment.

Table 4 presents our spillover estimates for both employment and earnings. Since some border counties do not have an "interior" to be compared to, the sample changes as we look at the interior counties, or when we difference the border county with interior controls. For this reason, we report the coefficient of our baseline county-pair results on the CBCP sample (column 1) as well as for the subsample (column 2) for which we can match counties with state interiors; this subsample excludes Delaware, Rhode Island, Washington, D.C., and San Francisco border segments.

The earnings effect is slightly smaller when we restrict our sample to counties in states that have an "interior" (column 2). When we examine the border and interior sets of counties separately, the effects are virtually identical—0.165 and 0.164, respectively—although the standard error is larger for the interior county specification. The spillover measure is close to 0 (−0.008) and not significant.

We also do not find any statistically significant spillover effects on employment. When we compare interior counties only (column 3), the measured effect is a small positive (0.042), while when we consider the border counties (column 2), the effect is close to 0 (0.011), and it is similar to our baseline results in column 1 (0.016). The magnitude of the spillover from the double-differenced specification is small (−0.058) and not statistically significant.[27] Overall, we do not find any evidence that wage or employment spillovers are contaminating our local estimates.

[27] The results from the spatial differenced specification (column 4) are not expected to be numerically identical to subtracting column 3 from column 2, as each regression is estimated separately, allowing for different coefficients for covariates. But they are numerically close.

B. *Results Using the County Business Patterns Data Set and Employment/Population*

As an additional validation of our findings, we compare estimates from our preferred specifications with the QCEW to identical specifications using the County Business Patterns (CBP) data set. The CBP data are available annually for 1990 to 2005. Several shortcomings of the CBP data led us to use the QCEW as our primary data set. Besides being reported only annually, the actual number of counties disclosing employment levels is less than in the QCEW—1,219 versus 1,380. For other counties, CBP provides an employment range only. While useful for some descriptive purposes, these observations are not usable to estimate changes in employment. Finally, and most important, because of changes in industry classifications, the CBP is available by SIC industries from 1990 to 1997 and by NAICS industries from 1998 to 2005. This break in the series adds further noise to the data, making inference based on the CBP over this period less reliable. To make the data as comparable as possible to the QCEW, we use SIC 5812 (eating places) for 1990–1997 and NAICS 7221 (full-service restaurants) and 7,222 (limited-service restaurants) for 1998–2005. As an additional specification check, we also report results from a regression in which the dependent variable is ln(*employment/population*); in this case, the total private sector employment control is also normalized by population, and we do not include ln(*population*) as an additional control.

Table 5 presents results for both the QCEW and CBP data sets, with and without controls for total private sector earnings or employment, depending on the regression. For both the earnings and the employment regressions, the point estimates for both log earnings or log employment are very close in both data sets and for both specifications. In the employment regressions with controls for overall private sector employment, the positive but not significant effect with the QCEW (0.016) becomes a negative but not significant effect with the CBP (−0.034). While the point

TABLE 5.—COMPARING MINIMUM WAGE EFFECTS FOR RESTAURANT INDUSTRY ACROSS DATA SETS AND DEPENDENT VARIABLES

	Contiguous Border County-Pair Sample					
	(1)		(2)		(3)	
	ln Earnings		*ln Employment*		*ln (Emp/Pop)*	
			QCEW			
$lnMW_t$	0.200***	0.188***	0.057	0.016	0.049	0.009
	(0.065)	(0.060)	(0.118)	(0.098)	(0.115)	(.095)
			CBP			
$lnMW_t$	0.247***	0.220**	−0.019	−0.034	−0.052	−0.073
	(0.081)	(0.092)	(0.132)	(0.127)	(0.128)	(0.133)
Controls						
County-pair × period dummies	Y	Y	Y	Y	Y	Y
Total private sector		Y		Y		Y

Sample sizes equal 70,620 (quarterly observations) for QCEW and 14,992 (annual observations) for CBP (County Business Patterns). Specifications for ln Employment include log of annual county-level population. Total private sector controls refer to log of average private sector earnings or log of total private sector employment, depending on the regression. All samples and specifications also include county fixed effects and county-pair–specific period fixed effects. Robust standard errors, in parentheses, are clustered on the state and border segment levels. Significance levels: *10%, **5%, ***1%.

CBP provides data at the four-digit SIC level from 1990 through 1997 and the six-digit NAICS level from 1998 onward. Given the level of detail in the CBP, SIC 5812 "eating places" is the most disaggregated industry that captures restaurants. We make a consistent approximation of the restaurant sector (SIC 5812) after 1997 by combining NAICS 7221 "full-service restaurants" and 7,222 "limited-service restaurants" for 1998–2005.

estimates are quite similar, the standard errors are larger in the CBP data set, which could result from the smaller sample size or added noise due to changes in industry classification. Overall, we conclude that our main findings hold across the two data sets.[28]

Finally, whether we include population as a control or normalize all employment measures by population does not materially affect the findings using the QCEW. The estimates from specification 2 (which controls for log population) and specification 3 (which normalizes employment by population) vary somewhat more when we consider the CBP, but the standard errors for the CBP are also larger, which is consistent with the data problems with the CBP that we noted above.

C. Sample Robustness

Our CBCP sample consists of a balanced panel of 1,070 county replicates (504 counties) for which restaurant employment is reported for all 66 quarters. Some counties contain too few restaurants to satisfy nondisclosure requirements. To check for the possibility that excluding the 452 counties with partial information affects our results, we estimate the minimum wage elasticity keeping those counties in the sample. We do not report these results in the tables for space considerations, but we find that the two sets of estimates are very similar. While the elasticity (standard error) from the balanced panel regression is 0.016 (0.098), the elasticity from the unbalanced panel is −0.023 (0.105).[29]

Some of the border counties in the western part of the country cover large geographic areas, raising the question of whether estimates using such contiguous counties are really local. As another robustness test, we drop border counties that cover more than 2,000 square miles. Our estimates are virtually identical: when we exclude these 59 large counties, the employment elasticity (standard error) changes from 0.016 (0.098) to 0.013 (0.084). (These results are not reported in the tables.)

D. Minimum Wage Effects by Type of Restaurant

Most previous minimum wage studies of restaurants examined only the limited-service (fast food) segment of the restaurant industry. To make our study more comparable to that literature, we present results here separately for limited-service and full-service restaurants. We also explore briefly the impact of tip credit policies.

These results for our preferred specification are reported in table 6. The estimated earnings effects are positive and significant for both limited-service and full-service restaurants. The earnings effect is somewhat greater among limited-service restaurants than among full-service restaurants (0.232 versus 0.187), which is to be expected since limited-service restaurants have a higher proportion of minimum wage workers. The employment effects in table 6 are positive but not significant for both restaurant sectors, as was the case for the restaurant industry as a whole in table 2.[30] In other words, the results we report in table 2 for the entire restaurant industry hold when we consider limited- and full-service restaurants separately.

The magnitude and significance of our earnings effects do not support the hypothesis that tip credits attenuate minimum wage effects on earnings or employment of full-service restaurant workers.[31] Why might this be? First,

[28] Results for other specifications using the CBP are qualitatively similar and are available on request.

[29] One might worry that counties with minimum wage increases may become more likely to drop below the reporting threshold. However, if we estimate equation (1) but replace the dependent variable with a dummy for missing observation, the minimum wage coefficient is negative, small, and insignificant.

[30] The standard errors of the employment coefficients, however, are greater than in table 2.

[31] Tip credits, which apply in 43 states, permit restaurant employers to apply a portion of the earnings that workers receive from tips against the mandated minimum wage. In most tip credit states, employers can pay tipped workers an hourly wage that is less than half of the state or federal minimum wage. Since 1987, the federal tip credit has varied between 40% and 50% of the minimum wage.

TABLE 6.—MINIMUM WAGE EFFECTS, BY TYPE OF RESTAURANT AND IN OTHER LOW-WAGE SECTORS: CONTIGUOUS BORDER COUNTY-PAIR SAMPLE

	ln Earnings	*ln Employment*
Type of restaurant		
Limited service	0.232***	0.019
	(0.080)	(0.151)
Full service	0.187**	0.059
	(0.091)	(0.206)
Other low-wage industries		
Accommodation and food services	0.189**	0.090
	(0.089)	(0.213)
Retail	0.011	−0.063
	(0.051)	(0.066)
Accommodation and food services plus retail (stacked)	0.076**	−0.032
	(0.029)	(0.042)
Manufacturing	−0.019	−0.044
	(0.102)	(0.200)
Controls		
County-pair × period dummies	Y	Y
Total private sector		

Sample sizes are: limited-service restaurants (90,222); full-service restaurants (84,876); accommodation and food services (84,744), retail (150,150), accommodation and food services and retail (84,348); and manufacturing (121,770). All specifications include controls for the log of annual county-level population. All samples and specifications include county fixed effects and county-pair–specific-period fixed effects. The stacked estimate is computed by estimating a common minimum wage effect for the two industries by stacking the data by industry; this specification includes industry-specific county fixed effects, industry-specific population effects, and county-pair X industry X period dummies. Robust standard errors, in parentheses, are clustered at the state and border segment levels. Significance levels: *10%, **5%, ***1%.

some tipped workers are not minimum wage workers, since employers are required to include reported tips in the payroll data that make up the QCEW. Even if tips are not fully reported, it is unclear why the proportion that is reported would change; therefore, an increase in the minimum wage will increase reported earnings. Indeed, this is what we find. Second, when minimum wages increase, competitive pressures may lead to similar increases in base pay for all workers, whether or not they receive tips.[32]

Overall, we conclude that the results are not driven by tip credits, as the earnings effects are strong in both limited- and full-service restaurants, and also when we consider only states with tip credits. Moreover, the employment effects are small for both subsectors and for the full sample, as well as the states with tip credits.

E. Minimum Wage Effects in Other Low-Wage Sectors

Thus far, we have focused on the impact of minimum wages on workers in the restaurant sector, the most intensive user of minimum wage workers. In this section, we extend our analysis to other low-wage sectors. We use the 2006 Current Population Survey to estimate the use of minimum wage workers by sectors. At the two-digit level, the most intensive users of minimum wage workers are accommodation and food services (hotels and other lodging places, restaurants, bars, catering services, mobile food stands, and cafeterias) and retail. Accommodation and food services accounts for 33.0% of all workers paid minimum or near minimum wages (within 10% of the relevant federal or state minimum wage), and 29.4% of workers in this sector are paid minimum or near-minimum wages. Retail accounts for 16.4% of all such minimum or near–minimum wage workers, and these workers make up 8.8% of the retail workforce. Together, the accommodation and food services sector plus the retail sector account for 49.4% of all employees in the United States who are paid within 10% of the federal or state minimum wage.

As the results in table 6 show, we find a positive and significant treatment effect of minimum wages on earnings for the accommodations and food services sector. The magnitude of the effect is quite similar to that for restaurants. Since these broader sectors constitute a sizable share of overall private sector employment in many counties, these estimates do not include a control for total private employment (the results including the control are almost identical). The estimated effect on employment is again positive (0.090) but not statistically significant. The standard error of the employment coefficient for accommodation and food services is somewhat larger, however, than for restaurants in table 2.

For the retail sector, which has higher average wages than accommodation and food services, we do not find a significant treatment effect on earnings; the estimated employment effect is −0.063 but not statistically significant. We also estimate the average effect in accommodation and food services and retail together by stacking the industry data and including industry-pair-period dummies. Here, we find a smaller but significant treatment effect on earnings and a positive but not significant effect on employment. To provide a falsification test, we also estimate the same specifications for manufacturing, since only 2.8% of the manufacturing workforce earns within 10% of the minimum wage. Reassuringly, both the estimated treatment and employment effects are insignificant for this sector.

In summary, the estimated treatment effects are smaller in sectors with higher average wages, and no significant employment effects are discernible in any of these sectors. We conclude that our key findings hold when we examine the low-wage sectors more broadly.

VI. Discussion and Conclusions

In this paper, we use a local identification strategy that takes advantage of all minimum wage differences between pairs of contiguous counties. Our approach addresses the twin concerns that heterogeneous spatial trends can bias the estimated minimum wage effects in traditional approaches using time and place fixed effects, and that not accounting for spatial autocorrelation overstates the precision in individual case studies.

For cross-state contiguous counties, we find strong earnings effects and no employment effects of minimum wage increases. By generalizing the local case studies, we show that the differences in the estimated elasticities in the two

[32] To examine this question more directly, we repeated our estimates using only the 43 states that have tip credits. The earnings effects remain strong, and the employment effects remain indistinguishable from 0 (results available on request).

sets of studies result from insufficient controls for unobserved heterogeneity in employment growth in the national-level studies using a traditional fixed-effects specification. The differences do not arise from other possible factors, such as using short before-after windows in local case studies.

The large negative elasticities in the traditional specification are generated primarily by regional and local differences in employment trends that are unrelated to minimum wage policies. This point is supported by our finding that neighborhood-level placebo minimum wages are negatively associated with employment in counties with identical minimum wage profiles. Our local specification performs better in a number of tests of internal validity. Unlike traditional fixed-effects specification, it does not have spurious negative (or positive) preexisting trends and is robust to the inclusion of state-level time trends as added controls.

How should one interpret the magnitude of the difference between the local and national estimates? The national-level estimates suggest a labor demand elasticity close to -1. This implies that an increase in the minimum wage has a very small impact on the total income earned by affected workers. In other words, these estimates suggest that the policy is not useful for raising the earnings of low-wage workers, as the disemployment effect annuls the wage effect for those who are still working. However, statistical bounds (at the 95% confidence level) around our contiguous county estimates of the labor demand elasticity as identified from a change in the minimum wage rule out anything above -0.48 in magnitude. This result suggests that minimum wage increases do raise the overall earnings at these jobs, although there may be differential effects by demographic groups due to labor-labor substitution.

Do our findings carry over to affected groups other than restaurant workers? Although we cannot address this question directly, the results in a companion paper (Allegretto, Dube, & Reich, 2008) using the CPS suggest an affirmative answer. In that paper, we find that allowing spatial trends at the census division level reduces the measured disemployment level substantially when we consider the response of teen employment to minimum wage increases. Additionally, and parallel to our findings here, we find that the measured disemployment effects disappear once we control for state-level trends in the underlying teenage employment. This evidence suggests that our findings are relevant beyond the restaurant industry.

Several factors warrant caution in applying these results. First, although the differences in minimum wages across the United States (and in our local subsamples) are sizable, our conclusion is limited by the scope of the actual variation in policy; our results cannot be extrapolated to predict the impact of a minimum wage increase that is much larger than what we have experienced over the period under study. A second caveat concerns the impact on hours. The rough estimates presented here suggest that the impact on hours is not likely to be large; however, our estimates in this regard are only suggestive. Third, our data do not permit us to test whether restaurants respond to minimum wage increases by hiring more skilled workers and fewer less skilled ones. The estimates in this paper are more about the impact of minimum wage on low-wage jobs than low-wage workers.

These caveats notwithstanding, our results explain the sometimes conflicting results in the existing minimum wage literature. For the range of minimum wage increases over the past several decades, methodologies using local comparisons provide more reliable estimates by controlling for heterogeneity in employment growth. These estimates suggest no detectable employment losses from the kind of minimum wage increases we have seen in the United States. Our analysis highlights the importance of accounting for such heterogeneity in future work on this topic.

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APPENDIX A

Additional Specifications with State Linear Trends and Dynamic Response

TABLE A1.—EFFECT OF INCLUDING STATE LINEAR TREND ON MINIMUM WAGE EMPLOYMENT EFFECTS

	All-County Sample						Contiguous Border County-Pair Sample			
	(1)		(2)		(4)		(5)		(6)	
					Ln Employment					
$lnMW_t$	−0.176*	0.035	−0.023	0.039	0.032	0.120**	−0.112	0.031	0.016	−0.002
	(0.096)	(0.038)	(0.068)	(0.050)	(0.078)	(0.058)	(0.076)	(0.056)	(0.098)	(0.119)
Controls										
Census division × period dummies			**Y**	**Y**						
MSA × period dummies					**Y**	**Y**				
County-pair × period dummies									**Y**	**Y**
State linear trends		**Y**		**Y**		**Y**		**Y**		**Y**

Sample size equals 91,080 for specifications 1 and 2 of the all-county sample and 48,348 for specification 3 (which is limited to MSA counties) and 70,620 for the border county-pair sample. All specifications control for the log of annual county-level population and total private sector employment. All samples and specifications include county fixed effects. Specifications 1, 2, and 5 include period fixed effects. For specifications 2, 3, and 5, period fixed effects are interacted with each census division, metropolitan area, and county-pair, respectively. Robust standard errors, in parentheses, are clustered at the state level for the all-county samples (specifications 1–3) and on the state and border segment levels for the border pair sample (specifications 4 and 5). Significance levels: *10%, **5%, ***1%.

TABLE A2.—DYNAMIC RESPONSE TO MINIMUM WAGE CHANGES

	All-County Sample				Contiguous Border County-Pair Sample	
	(1)	(2)	(3)	(4)	(5)	(6)
			ln Earnings			
$\Delta lnMW_{(t-8)}$	0.012	0.022	0.028**	0.006	0.021	0.018
	(0.019)	(0.020)	(0.014)	(0.036)	(0.026)	(0.044)
$\Delta lnMW_{(t-6)}$	0.010	0.003	0.013	0.002	0.012	0.002
	(0.027)	(0.027)	(0.018)	(0.048)	(0.035)	(0.060)
$\Delta lnMW_{(t-4)}$	−0.006	0.000	0.051**	0.017	0.018	0.001
	(0.028)	(0.029)	(0.023)	(0.049)	(0.037)	(0.080)
$\Delta lnMW_{(t-2)}$	0.044	0.025	0.086**	−0.001	0.053	0.014
	(0.041)	(0.043)	(0.037)	(0.056)	(0.041)	(0.086)
$\Delta lnMW_{(t)}$	0.133***	0.183***	0.220***	0.140**	0.142***	0.163**
	(0.032)	(0.046)	(0.038)	(0.061)	(0.049)	(0.083)
$\Delta lnMW_{(t+2)}$	0.177***	0.192***	0.226***	0.204***	0.180***	0.209**
	(0.028)	(0.039)	(0.034)	(0.063)	(0.038)	(0.087)
$\Delta lnMW_{(t+4)}$	0.209***	0.220***	0.257***	0.215***	0.233***	0.247***
	(0.025)	(0.052)	(0.056)	(0.063)	(0.040)	(0.090)
$\Delta lnMW_{(t+6)}$	0.281***	0.241***	0.281***	0.109	0.254***	0.197**
	(0.029)	(0.062)	(0.070)	(0.067)	(0.035)	(0.083)
$\Delta lnMW_{(t+8)}$	0.255***	0.241***	0.281***	0.169***	0.247***	0.192*
	(0.036)	(0.070)	(0.077)	(0.058)	(0.040)	(0.105)
$\Delta lnMW_{(t+10)}$	0.292***	0.243***	0.283***	0.129*	0.295***	0.183
	(0.031)	(0.076)	(0.080)	(0.076)	(0.035)	(0.124)
$\Delta lnMW_{(t+12)}$	0.277***	0.257***	0.294***	0.128	0.283***	0.245**
	(0.038)	(0.074)	(0.080)	(0.081)	(0.042)	(0.098)
$\Delta lnMW_{(t+14)}$	0.316***	0.260***	0.297***	0.116	0.309***	0.230**
	(0.039)	(0.077)	(0.087)	(0.084)	(0.045)	(0.103)
$lnMW_{(t+16)}$	0.294***	0.259***	0.294***	0.128	0.307***	0.210
	(0.035)	(0.083)	(0.090)	(0.083)	(0.053)	(0.139)
Controls						
Census division × period dummies		Y	Y			
State-specific time trends			Y			
MSA × period dummies				Y		
County-pair × period dummies						Y
Total private sector	Y	Y	Y		Y	Y

TABLE A2.—CONTINUED

	All-County Sample				Contiguous Border County-Pair Sample	
	(1)	(2)	(3)	(4)	(5)	(6)
				ln Employment		
$\Delta lnMW_{(t-8)}$	−0.060	0.036	0.034	0.009	−0.065	−0.038
	(0.057)	(0.050)	(0.034)	(0.072)	(0.057)	(0.065)
$\Delta lnMW_{(t-6)}$	−0.051	0.071	0.040	−0.010	−0.081	−0.041
	(0.070)	(0.061)	(0.044)	(0.093)	(0.070)	(0.090)
$\Delta lnMW_{(t-4)}$	−0.084	0.000	0.088	−0.069	−0.090	0.012
	(0.072)	(0.071)	(0.062)	(0.108)	(0.074)	(0.130)
$\Delta lnMW_{(t-2)}$	−0.143	−0.008	0.130*	0.055	−0.133	0.088
	(0.093)	(0.099)	(0.067)	(0.118)	(0.086)	(0.167)
$\Delta lnMW_{(t)}$	−0.168	0.061	0.139*	0.044	−0.126	0.053
	(0.117)	(0.127)	(0.078)	(0.148)	(0.092)	(0.150)
$\Delta lnMW_{(t+2)}$	−0.166	0.043	0.117	−0.016	−0.082	0.027
	(0.117)	(0.109)	(0.094)	(0.142)	(0.086)	(0.171)
$\Delta lnMW_{(t+4)}$	−0.200*	0.009	0.062	0.084	−0.098	0.015
	(0.103)	(0.112)	(0.086)	(0.143)	(0.089)	(0.151)
$\Delta lnMW_{(t+6)}$	−0.180	−0.036	0.047	0.069	−0.122	−0.074
	(0.114)	(0.120)	(0.088)	(0.127)	(0.100)	(0.139)
$\Delta lnMW_{(t+8)}$	−0.175	−0.034	0.077	0.068	−0.094	−0.017
	(0.142)	(0.136)	(0.115)	(0.125)	(0.110)	(0.145)
$\Delta lnMW_{(t+10)}$	−0.180	−0.047	0.072	0.064	−0.065	0.011
	(0.135)	(0.128)	(0.109)	(0.146)	(0.102)	(0.166)
$\Delta lnMW_{(t+12)}$	−0.206	−0.070	0.040	0.124	−0.107	0.009
	(0.131)	(0.138)	(0.100)	(0.223)	(0.100)	(0.158)
$\Delta lnMW_{(t+14)}$	−0.250*	−0.096	0.030	0.043	−0.178	−0.013
	(0.137)	(0.147)	(0.106)	(0.238)	(0.114)	(0.193)
$lnMW_{(t+16)}$	−0.349**	−0.109	0.079	0.003	−0.292**	−0.007
	(0.147)	(0.157)	(0.113)	(0.198)	(0.132)	(0.202)
Controls						
Census division × period dummies		Y				
State-specific time trends			Y			
MAS × period dummies				Y		
County-pair × period dummies						Y
Total private sector	Y	Y	Y	Y	Y	Y

Sample size equals 91,080 for specifications 1, 2, and 4 of the all-county sample and 48,348 for specification 3 (which is limited to MSA counties) and 70,620 for the border-county-pair sample. All specifications control for the log of annual county-level population. Total private sector controls refer to log of average total private sector earnings or log of employment. All samples and specifications include county fixed effects. Specifications 1, 4, and 5 include period fixed effects. Specification 4 also includes state-level linear trends. For specifications 2, 3, and 5 period fixed effects are interacted with each census division, metropolitan area, and county-pair, respectively. Robust standard errors, in parentheses, are clustered at the state level for the all-county samples (specifications 1–4) and on the state and border segment levels for the border pair sample (specifications 5 and 6). Significance levels: *10%, **5%, ***1%.

APPENDIX B

Falsification Test: Specifications and Estimates

First, we estimate a panel and time period fixed effects model using the actual sample:

$$\ln y_{it} = \alpha + \eta \ln(w_{it}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_t + \varepsilon_{it}. \quad \text{(B1)}$$

This is identical to equation (1) with only county and time fixed effects, and reproduced here for clarity. We expect the elasticity η to be similar as before, though the estimation sample is now restricted from all counties to those in the limited sample of border counties next to states that have only a federal minimum wage.

We then take our placebo sample of counties that had only the federal minimum wage throughout the period ($w_t^M = w_t^{M,\,federal}$). We assign to each of these border counties (i) a placebo minimum wage that is equal to the actual minimum wage faced by its cross-state contiguous neighbor (n) that period. We then estimate the "effect" of this fictitious placebo minimum wage on employment for the set of counties in our placebo sample. We include county and time fixed effects as controls, analogous to the national-level panel estimates. Our specification is

$$\ln y_{it} = \alpha + \eta_n \ln(w_{nt}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_t + \varepsilon_{it}. \quad \text{(B2)}$$

The minimum wage variable w_{nt}^M is the minimum wage of the county's cross-state neighbor (denoted again as n). The elasticity η_n with respect to the fictitious minimum wage from one's neighbor should be 0, as this set of counties has identical minimum wage profiles. If it is instead similar to the η from equation (B1), we have evidence that the national-level estimates (using only time and county fixed effects) are biased because of the presence of spatial heterogeneity. As before, we restrict our analysis to balanced panels with full reporting of data.

Panel A in table B1 shows the results from equation (B1) using the actual sample, while panel B shows the results from the placebo sample (equation B2). We find a negative effect in both samples (though imprecise), with elasticities exceeding −0.1 in magnitude, suggesting bias in the canonical specification.

TABLE B1.—FALSIFICATION TESTS: PLACEBO MINIMUM WAGES ON EARNINGS AND EMPLOYMENT

	(1) Ln Earnings	(2) Ln Employment
A. ***Actual minimum wage sample***		
All counties	0.265***	−0.208
	(0.045)	(0.149)
B. ***Placebo minimum wage sample***		
All counties	0.079	−0.123
	(0.056)	(0.158)

Actual minimum wage sample is restricted to those border counties that are next to states that never had a minimum wage higher than the federal level during the sample period. Placebo estimates (B) restrict the sample to border counties in states that never had a minimum wage higher than the federal level. Panel A estimates the effect of the own-county log minimum wage on own-county log restaurant earnings and employment. In contrast, panel B estimates the effect of the neighbor's log minimum wage (the placebo) on own-county log restaurant earnings and employment. Both panels control for county fixed effects and period fixed effects. All specifications include controls for the log of annual county-level population and log of either total private sector earnings (1) or employment (2). Robust standard errors in parentheses are clustered at the state level. Significance levels: *10%, **5%, and ***1%.