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MINIMUM WAGE EFFECTS ACROSS STATE BORDERS: ESTIMATES USING CONTIGUOUS COUNTIES

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Abstract

Most local case studies of minimum wages do not find significant employment effects, while studies using national data find some negative effects for teenagers. We develop and apply two local estimators that compare all contiguous counties or metro areas in the U.S. that straddle a state-based minimum wage differential. The local estimators show no adverse employment effects. Moreover, we reconcile the local and national level estimates by showing that the negative elasticities in national panel models are generated by unobserved spatial heterogeneities in employment trends that are unrelated to minimum wage policies. We can also rule out other explanations for the difference between local and national level estimates, such as lagged or long-term effects of minimum wages.

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1. Introduction

The primary source of the controversy in the minimum wage literature can be traced to the divergent findings in the national level studies as compared to local case studies. National level studies typically draw upon household-based data, and use variation in the minimum wage across states over time to estimate effects on a high impact group, such as teenagers. Local studies typically employ before-after estimates using establishments in a single industry—restaurants—to compare local areas with different minimum wages (e.g., New Jersey vs. Pennsylvania).

Since the incidence of minimum wage laws and the proportional share of the minimum wage workers are similar for teenagers and the restaurant workforce, one might expect the estimated elasticities in the national and the local studies to be similar.[1] But that is not the case. Studies using national-level data typically obtain point estimates of -0.1 to -0.3, while most local case studies fail to find significant negative employment effects. The proliferation of state and local minimum wage laws over the past ten years has provided researchers with additional dimensions of variation to study minimum wage effects. But this additional variation has not resolved scholarly controversies regarding the effects of minimum wages on employment. For example, Neumark and Wascher (2007) use national panel data and report statistically significant negative employment effects for some groups and specifications. In contrast, in their study of the 2004 citywide minimum wage in San Francisco, Dube, Naidu and Reich (2007) find the minimum wage employment elasticity is indistinguishable from zero for restaurant workers.

This paper shows that both approaches may generate misleading results by failing to account for unobserved heterogeneity in employment growth that are correlated with the introduction of minimum wages. To advance this literature, in this paper we present and apply

[1] In 2006, 26.4 percent of sixteen to nineteen year old workers, and 18.8 percent of restaurant workers, earned within 10 percent of the relevant state or federal minimum wage in the United States (authors' calculations using the 2006 Current Population Survey). Of course, the elasticity of substitution may be different for restaurant workers as opposed to teenagers.

two variants of a local identification method that generates improved estimates of minimum wage effects. Specifically, we compare a) all contiguous county pairs in the U.S. that are located on opposite sides of a state border with different minimum wages; and b) all multi-state metropolitan statistical areas (MSAs) that exhibited a difference in minimum wage across their component counties. We use county-level data on restaurant earnings and employment from the Quarterly Census of Employment and Wages (QCEW) for 1990 to 2006.

A number of studies have used border counties to identify local treatment effects in other contexts (e.g., Holmes (1998), or Huang (forthcoming)). Individual case studies in the minimum wage literature have also considered border areas in specific cases (e.g., NJ versus PA, San Francisco versus surrounding areas). This paper generalizes the case study approach by utilizing all local differences in minimum wages in the US over sixteen and a half years, and it also permits comparisons with national-level panel studies within a common framework. Our identification strategy accounts for unobserved heterogeneity in employment growth, an issue that is not adequately addressed by existing national studies. We show that this heterogeneity is spatial in nature. We also show that in presence of such spatial heterogeneity, precision of the individual case study estimates are 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 three specific ways. First, we present improved minimum wage effects using the new local identification estimates based on contiguous country pairs and cross-state MSAs, and compare them to national level estimates using panel and county fixed effects. Both local and national estimates show strong and similar positive effects of minimum wages on restaurant earnings. *The local estimators produce employment effects that are indistinguishable from zero. The estimate using contiguous counties rules out, at the 90 percent confidence level, minimum wage elasticities greater in magnitude than -0.08.* Unlike individual case studies to date, we can show that our results are robust to cross-border spillovers, which could occur if restaurant wages in border counties respond to minimum wage hikes across the border. We also show that besides serial correlation in the panel, spatial (cross-border) autocorrelation is a serious issue for local estimates, and may bias the standard errors by as much as 50%.

In contrast to the local estimates, the national estimates using only panel and time period fixed effects produce negative employment elasticities of -0.18 or greater in magnitude. The difference between these two sets of findings has serious welfare implications. The national estimates imply a labor demand elasticity of around -1, which suggest that minimum wage hikes do not raise aggregate earnings of affected workers. In contrast, our estimate using contiguous county rules out, at the 90 percent level, labor demand elasticities greater in magnitude than -0.46, suggesting that the minimum wage increases substantially raise total earnings for these workers.

Second, we provide a reconciliation between the conflicting results. Our results show that the negative employment effects in national level studies reflect an omitted variables bias. A novel falsification test provides evidence of this spatial heterogeneity—i.e., that regional employment shocks are correlated with the timing of minimum wage increases. We consider only counties that *never* had any state or local minimum wage policies (and hence always had *identical* minimum wages), and show that minimum wage increases in neighboring states are associated with spurious employment declines. Consistent with this finding, inclusion of state-level linear employment trends as added controls cause the national-level employment elasticities to change signs and become positive (and insignificant); in contrast, the local estimators are shown to be robust to their inclusion.

Third, we consider and reject several other explanations for the divergent findings. We rule out the possibility of an anticipation effect or a lagged effect of minimum wage increases. Differential response to high versus low frequency components in the minimum wage variation cannot explain the difference between the local and national estimates. This finding calls into question the reconciliation proposed by Baker et al (1999), who argue that local case study methods suffer by only considering high frequency component of variation. We also address whether the differences between the local estimates and the national panel estimates are due to sample differences or differences in specification—i.e., allowing local areas to have heterogeneous time effects. To do so we partition counties into geo-coded local areas (geographical cells) and consider progressively finer partitions. We find that allowing for local area (or cell) specific time effects largely eliminates the disemployment finding, even for relatively coarse partitioning. Finally, we show that the divergent findings are not driven by tip credits that can be applied to the minimum wage in many states for some full service restaurant workers. 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 in the local estimators proposed in this paper.

The rest of the paper is organized as follows. Section 2 describes the chronology of minimum wages across the United States since the mid1980s. Section 3 briefly reviews the extant literature, with a focus on identification assumptions. Section 4 presents our research design, including our regression specifications, and Section 5 presents results for our local estimators. Section 6 presents results using national panel data, while Section 7 addresses why the local and national estimates differ. Section 8 provides our conclusions.

2. Recent Minimum Wage Developments

In 1985, Alaska, Connecticut and Massachusetts became the first states to institute a higher minimum wage; by 1989, fifteen states (and the District of Columbia) had raised their minimum wage floors above the federal level. In this period the highest state wage exceeded the federal level by 27 percent. After the federal minimum wage increased to $4.25 per hour in April 1991, only three states (and DC) had higher floors. (See Figure 1A.) By September 1996, ten states (and DC) had floors above the federal level and the highest state wage exceeded the federal level by 24 percent. After the federal level increased to \$4.75 in October 1996, four states (and DC) had higher floors. By August of 1997 an additional six states raised their floor, in most cases to the \$5.15 federal level that was scheduled to go into effect in September 1997.

The chronology since 1997 follows a different pattern. Many more states began to increase their minimum wages than before—32 states plus DC, by May 2007. The size of the state-federal differential has also grown– reaching 45 to 48 percent in some states. As a result, the proportion of the workforce covered by higher state minimum wage laws now exceeds 50 percent (Figure 1B) and the states with higher minimum wage are no longer concentrated in a few regions of the country. The federal increase to \$5.85 in July 2007 left a large number of states—27 plus DC— with higher floors. Moreover, already-enacted state policies, some involving indexation, imply that *at least* 12 states (plus DC, San Francisco and Santa Fe) will have floors higher than the federal level of \$7.25 that is scheduled for July 2009.

Overall, the pattern of levels and changes in state minimum wage differentials in the past fifteen years provide us with a rich sample of local wage differences. Changes in minimum wages at the city (county) level, such as in San Francisco and Washington, DC, add to this variation.

3. Related Literature

Previous local minimum wage studies typically used 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 teenage employment, as the incidence of minimum wage workers is similar among both groups, and many of the teenagers earning 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 contiguous states (NJ and PA) to obtain local comparisons. Using administrative payroll data from Unemployment Insurance (ES202) records, Card and Krueger

(2000) do not detect any significant effects of the 1992 NJ statewide minimum wage increase on restaurant employment. Moreover, they obtain similar findings when the 1996-97 federal increases eliminated the NJ-PA differential.[2] Neumark and Wascher (2000) find a negative effect using payroll data provided by restaurants in those two states. However, there is uncertainty about the quality of their data, especially in light of the results using ES202 data over the same period and states by Card and Krueger (2000).

A more recent study (Dube, Naidu and Reich 2007) compares restaurants in San Francisco and the adjacent East Bay before and after the 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, Naidu and Reich do not find any significant effects of the minimum wage increase upon employment or hours, but do find an increase in job tenure.[3]

However, as with the other case studies, their data contain a limited before-after window. Consequently, they cannot address whether minimum wage effects occur with a longer lag. This critique of before-after studies is made most forcefully by Baker et al. (1999), who use Canadian data to argue that local case studies mainly utilize the high frequency component of variation in the minimum wage, while the putative disemployment effect is associated the low frequency component (i.e., disemployment occurs with a lag). Equally important, case studies that compare a small number of neighboring locations are susceptible to overstating the precision of the estimates of the minimum wage effect. The case studies in the literature suffer from treating individual firm level observations as being independent (i.e., they do not account for spatial autocorrelation). Employment changes in firms in a particular geographic area are likely to face

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[2] Other previous local case studies of fast food restaurants include (Katz and Krueger 1992 for fast food chains in Texas; Spriggs and Klein 1994 for Jackson, MI and Greensboro, NC 2000 for NJ-PA. Except for Neumark and Wascher, these studies also fail to find significant disemployment effects. A study by Powers et al of fast-food chains near the Indiana-Illinois border is still in the data-analysis stage.
[3] They do find, among fast-food restaurants only, a small price effect, a shift from part-time jobs to full-time jobs, and a large increase in worker tenure.

correlated shocks. As a consequence, the precision of the estimates may be overstated, especially when combined with the homogeneity of minimum wages within the treatment and control areas.

Aaronson (2001) measure the impact of minimum wages on prices charged by restaurants using a panel of metropolitan areas, and find a substantial pass through in prices. In a later paper, Aaronson and French perform a calibration exercise using these price responses and infer a minimum wage elasticity of -0.1 to -0.3 for restaurant employment (Aaronson and French 2007). As we show, this range derived from calibration is outside the statistical bounds we can place around our local estimates on restaurant employment, which are centered round zero, raising questions about the validity of their calibration exercise.

Most 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 teenagers, who disproportionately are minimum wage workers. Prominent examples include Burkhauser et al (2000), who use state fixed effects but not year effects, Neumark and Wascher (1992) who include both state fixed effects and year effects, and Neumark and Wascher (2007), who include state effects, year effects and state linear trends. Burkhauser et al argue that year effects should not be included in order to identify federal minimum wage impacts; using the CPS they estimate that the minimum wage elasticity for teens is -0.59. Neumark and Wascher (1992) find that year effects should be included; they 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. In contrast to earlier work, however, they include state-level linear trends as controls, which their specification tests find cannot be excluded. They obtain mixed results, with negative effects only for minority teenagers, and results varying substantially depending on groups and specifications.

Do national panel studies control adequately for heterogeneity in employment growth? A state fixed effect will control for level differences between states, but as Table 1 shows, there is substantial regional variation in overall employment growth over time and space. 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 2 illustrates this point more generally by displaying year over year employment growth rates for the 17 states with a minimum wage higher than the federal level in 2005, and for all the other states. As Figure 2 shows, spatial heterogeneity has a time-varying component as well. Considering the 17 states (plus DC) that had a minimum wage above the federal level in 2005, we find that the average employment growth in these states was consistently lower than employment growth in rest of the country between 1991 and 1996. However, these two groups had virtually identical growth between 1996 and 2006. Since the overall employment growth cannot plausibly be affected by variation in the minimum wage, this reflects time-varying differences in the underlying characteristics of the states. By itself, heterogeneity in overall employment growth is not a problem, since our estimates (and most in the literature) control for overall employment trends. However, if low-wage employment also exhibits heterogeneity, then this may impart an omitted variable bias in estimates using national panel data with only panel and time period fixed effects. Our results indicate that this is indeed the case.[4]

Including state-level linear trends (as in Neumark and Wascher 2007), does not adequately address the problem, since the estimated trends may themselves be affected by minimum wages. If disemployment results mainly from low frequency, or "long term," variation in minimum wages-- as suggested by Baker et al (1999) -- the inclusion of linear trends may be inappropriate. Whether inclusion of these linear trends corrects for unobserved heterogeneity in

[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.

employment prospects, or whether they absorb the low-frequency variation in the minimum wage cannot be answered within such a framework.[5]

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

4. Research Design for Estimating Local Treatment Effects

In this section we discuss why we chose restaurants as the industry to study minimum wage effects, and a description of our dataset and sample construction. We then present our identification strategy and specifications for our local estimators, and discuss the construction of the correct standard errors. Finally, we present some tests of the validity of our research design by showing that contiguous counties are indeed better controls.

4.1 Choice of Industry

We focus our analysis of minimum wage changes on workers in the restaurant industry for a number of reasons. Most importantly, restaurants employ a large fraction of all minimum wage workers. According to the Bureau of Labor Statistics, the hospitality industry employed 37% of workers at the federal minimum wage in 2006, and the vast majority of these workers are in the restaurant sector. Using the Current Population Survey, the Economic Policy Institute reports that 29% of the workers directly affected by the most recent federal minimum wage

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[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.

increase are in the hospitality industry.[6] Furthermore, minimum wage workers constitute a large part of the restaurant workforce (19% according to our calculations using the CPS). 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 firms in other industries.

There is also a data component to this issue. Given our focus on comparing neighboring counties, a focus on restaurants allow us to consider a much larger set of counties than if we considered other industries employing of 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 teenagers. The proportion of workers near or at the minimum wage is similar among all restaurant workers and all teenage workers, and many teenaged minimum wage workers are employed in restaurants. 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. [7]

4.2 Data and Sample Construction

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 due to the fact that the most local identifier available is the metro area. The best dataset with employment and earnings information at the county level is the Quarterly Census of Employment

[6] Aaronson and French (2007) note around 20% of minimum wage workers were employed by Eating and Drinking establishments in the mid 1990s. No other 3-digit level industry employs more than 7% of all minimum wage workers.

[7]By including all restaurants, both limited service and full service, in our study 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 hit particularly hard because of a minimum wage increase, thereby buffering effects on fast-food restaurants who may see their relative demand rise vis a vis take-out stores. By including all restaurants, our analysis accounts for any intra-restaurant substitution. Moreover, the closest substitute to restaurants consists of food (prepared or unprepared) purchased in supermarkets; this industry has a much low incidence of minimum-wage workers, ruling out the such substitution effects.

and Wages (QCEW), which provides quarterly county-level payroll data by detailed industry.[8] The dataset 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 percent of workers are covered by unemployment insurance, the QCEW constitutes a near-census of employment and earnings.[9] 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).[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, and is derived by dividing 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 adjusting this measure to a weekly basis. Unfortunately, the QCEW does not distinguish between part-time and full time-employment (i.e. it does not measure hours worked). Conceivably, employers may switch from full-time to part-time workers in response to a higher minimum wage or reduce the hours of all workers instead of curtailing the number of jobs. We partly address the possibility of hour 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 absence of any hours adjustments.

The QCEW provides data by detailed industry only for counties with enough establishments in that industry to protect confidentiality. For restaurants, our sample therefore

[8] The County Business Pattern (CBP) would be an alternative source of data. However, there are two reasons why the QCEW is the better choice in our case. First, the CBP is annual and not quarterly. Second, the employment and earnings information in the CBP is based on the Census Bureau's synthesis of an array of surveys and administrative record. In contrast, the QCEW is based on actual employment and earnings as reported by employers for each worker in each quarter, and is more reliable for our purposes.
[9] The 2 percent who are not covered are primarily certain agricultural, domestic, railroad and religious workers.
[10] 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.

consists of 1,381 out of the 3,081 counties in the U.S. To our quarterly panel of county-level employment and earnings we merge information on the state (or local) and federal minimum wage in effect in each quarter from 1990q1 to 2006q2. During the sample period the federal minimum wage changed in 1991-92 and again in 1996-97. The number of states with a minimum wage above the federal level ranged from three in 1991 to 32 in 2006.

Our contiguous county sample consists of all the contiguous county pairs that straddle a state boundary, that have a minimum wage differential sometime over the time period, and that have continuous data for all 66 quarters. Following Huang (forthcoming) and Holmes (1998) who have also used contiguous counties to study effects of other policies, we omit very large counties as they are less likely to have common economic characteristics. The 20 counties we exclude (with area of more than 2000 square miles) are all in Western States.[11] This leaves us with 211 counties and 178 county pairs in 28 states (plus DC). We consider all contiguous county pairs, and this means that an individual county will have *p* replicates in our dataset if it is part of *p* cross state pairs. The issue of multiple observations per county is addressed in the way in which we construct our standard errors, as we discuss below.

We also construct a second sample of all cross-state metropolitan statistical areas (MSAs). The Census Bureau defines an MSA as a collection of counties with sufficient population density, and sufficient amount of inter-county commuting for employment. The cross-state metro sample, which consists of counties in MSAs that cross a state border and which had a minimum wage differential at some point in time over the sample period (and with full reporting in the QCEW), contains 24 MSAs and 166 counties in 24 states (including DC).[12] The cross-state metro sample and the contiguous county sample are not nested sets. The former contains counties that are part of a cross-state MSA but do not straddle a state border (e.g., Kings County in the New York MSA containing Brooklyn); and the latter contains all border counties including ones

[11] However, as we report below, this exclusion has virtually no impact on our results.
[12] Only 137 of the total 166 counties contain a full balanced panel of observations for the entire sample period. We include the San Francisco Bay Area in both samples.

that are not part of a cross-state MSA (e.g., Spokane county in Washington state neighboring Idaho). Appendix A shows the maps with both the contiguous county sample and the cross-state metro sample; Appendix B lists individual counties in the sample.

In both the contiguous county sample, and the cross-state MSA sample, the same county can experience positive and negative treatments at different times during the sample period. This feature of our research design provides an added benefit, as on average it tends to smooth out the influence of potential unobservable effects.

Table 2 provides descriptive statistics. Comparing all counties nationally (column 3) to counties that are part of cross-state pairs (column 1), we find that they are quite similar in terms of population, density, employment levels and average earnings. Cross-state metro counties (column 2) tend to be somewhat larger, more dense and have higher earnings than metro counties overall (column 4), reflecting the greater incidence of state minimum wages in more urbanized states. Table 2 also shows that our two local samples—cross-state metros and cross-state county pairs—are somewhat different, with the cross-state metro sample having a greater incidence of urban counties and greater population density. Consequently, finding similar effects of minimum wages in these two local subsamples would suggest that these effects are not substantially different if we consider more urban versus rural areas.

4.3 Identification Strategy and Specifications

Our identification strategy exploits the fact that a state-level minimum wage treatment is applied to only one part of a common labor market area. Economic activity is continuous across a state border, but policies change discontinuously. As we discuss below, unobserved heterogeneity in economic activity is likely to be reduced when we consider a contiguous county pair or a metropolitan area, relative to randomly-selected county pairs. Additionally, by pooling across such pairs or metropolitan areas, we can also account for spatial and temporal autocorrelation that affect local case-study estimates.

An ideal region would be sufficiently homogeneous in labor market conditions, but where minimum wages are set differentially. The finest such set allowable by our data consists of all contiguous county pairs that straddle state borders and that at some point had different minimum wages. Observations are at the county (*i)*, time period (*t*) level, where time is measured in quarters. Although our observations are at the county-quarter level, a county may have multiple observations for each quarter, since it can be part of several cross-state pairs. We estimate:

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

Here y_{ipt} refers either to average earnings or employment in restaurants in county *i* at time *t*, w_{it}^{M} is the minimum wage in county *i* at time *t*, and *p* subscripts each county pair. The ϕ_i term represents a county fixed effect, and τ_{pt} represents a county-pair specific time period effect. The y_{it}^{TOT} term refers to average private sector earnings, or total private sector employment, for the earnings and employment regressions, respectively. The log of annual county-wide population is also included as a covariate for the employment regression.[13] This specification only utilizes the variation in minimum wages within each county pair, as the τ_{pt} term sweeps out the variation between pairs.[14] Our identifying assumption is $\mathrm{cov}(\ln(w_t^M), \varepsilon_{it}) = 0$, i.e., the log minimum wage within the pair is uncorrelated with the residual employment (or earnings) in either county.

As a second approach to identifying minimum wage effects, we use Metropolitan Statistical Areas (MSAs) that cross state boundaries. The equation we estimate is

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

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[13] The Census Bureau does not report county-level population estimates at the quarterly level.
[14] In principle, the presence of the ln(y^{TOT}) and the ln(pop) terms means that county pairs without a minimum wage differential could be used in the regression to these coefficients; however, including these counties does not add to identifying the parameter of interest, η. Therefore, we restrict our sample to county pairs that had some differences in minimum wages over this period.

Here τ_{mt} is a metro-specific time effect that captures local labor market conditions that may vary over time and place and may be correlated with the minimum wage. Analogous to the county-pair case, τ_{mt} sweeps out between-MSA variation.

4.4 Standard Errors

The OLS standard errors are subject to three distinct sources of possible bias. First, there is serial correlation in employment at the county level. (Bertrand Duflo and Mullainathan (2004) shows how serial correlation can bias standard errors for difference in difference estimates.) Second, the minimum wage variable is (almost always, except in San Francisco) constant across counties within a state; intra-group correlation in the treatment variable across counties often causes standard errors to be biased downward (Moulton 1990).[15] Finally, employment is very likely to be spatially correlated across state boundaries. Indeed, our identification is based on the presumption that local comparisons are good ones because they take out a common employment trend; in such cases, the residuals are also likely to be correlated across cross-state county pairs, and potentially among the entire set of counties on both sides of a state border.

Since the point estimates from our local estimators are very small, our main goal is to provide statistical bounds on the minimum wage elasticities. For this reason, we pursue a conservative approach and 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.[16] The third option utilizes multi-dimensional clustering, where the variance covariance matrix with multi-dimensional clustering, VC_3 , is equal to $VC_1 + VC_2.-VC_{1\cap 2}$. (For more details, see Cameron, Gelbach and Miller 2006.)[17] In practice, the maximal standard error is almost always

[15] This problem is amplified in our county pair specification, since some of our counties have multiple cross-state contiguous counties and therefore become part of more than one observation.
[16] A "border segment" is defined as the set of all counties on both sides of a border between two states.
[17] The number of clusters on both these dimensions exceeds 30, which is large enough to allow reliable inference using clustered standard errors. Block-bootstrapped estimates (where block refers to either state or border segment) on each of these two dimensions produced standard errors and bounds nearly identical to what we report here.

(2), although it is quantitatively close to (3). The only exception occurs when we consider the possibility of cross-border spillover from state minimum wage mandates (discussed in the next section). In that case, (3) is chosen as it is the largest. Finally, our standard errors also correct for arbitrary forms of heteroscedasticity.

4.5 Validity of the Research Design

Contiguous counties will make good control groups for estimating minimum wage effects if (1) there are substantial and durable differences in treatment intensity in the set of cross-state counties, and (2) the set of contiguous cross-state county pairs are more similar to each other than to a set of randomly-chosen counties. In contrast, panel and period fixed effects models used in the national level estimates implicitly assume that one county in the U.S. is as good a control as any other.

Figures 3A and 3B display for each year the number of counties that are part of a contiguous county pair or a cross-state metro or that exhibits a minimum wage differential. The figures also display the average “minimum wage gap” in each year for our subsamples. The number of counties that provide the variation to identify a minimum wage effect stays relatively steady for most of the time-period, with an increase after 2003. However, the pay gaps within these counties exhibit substantial swings over time, consistent with the pattern of increases in state and federal minimum wages, thereby providing the basis for identifying the treatment effect. In other words, contiguous counties display the substantial variation we want for the treatment variable.

Moreover, as Figures 3A and 3B show, in our local samples the minimum wage differences in nearby areas are *durable*. Between 1997 and 2002, the minimum wage gap between contiguous pairs with a wage differential increased from about 7 percent to about 20 percent, and then stayed near that level until 2006. Since the variation in minimum wages in nearby areas has not been transitory, the measured business response represents adjustments to enduring changes.

Second, contiguous counties are relatively similar, and hence form better controls, especially with respect to underlying employment trends. To measure this similarity, we take each of the border counties in our sample and assign a randomly-chosen county to each one as its pair. We then calculate the mean absolute value of the differences in employment growth—for a single year, 3 years and for the full period—for contiguous county pairs and for the randomly-assigned pairs. This difference provides a measure of "dissimilarity."

As Table 3 shows, in every case the mean absolute difference between a county and its pair is greater when a randomly chosen county is the pair than when the contiguous county is the pair; and the difference is statistically significant.[18] This is true for both overall employment growth and for restaurant employment growth. We also present in Table 3 the mean absolute differences in *levels* of employment and earnings between randomly assigned pair as compared to the contiguous county pairs. The results again clearly indicate that contiguous counties are more similar.

Could the greater correlation in restaurant employment or earnings simply result from contiguous counties being more similar in their minimum wage laws? To address this concern, we also present the correlation between restaurant employment growth and levels of employment and earnings among cross-state counties *that do not have any differences in minimum wages during this period.* The results confirm that contiguous counties are, indeed, more similar—irrespective of the similarity in minimum wage laws. For instance, during the full period of study, the mean absolute difference in restaurant employment growth between the actual sample of border county pairs and random pairs was about 8.5 percent, while the corresponding figure for out -of-sample border county pairs without any minimum wage variation was 8.9 percent.

5. Results using Local Estimators

[18]Although we report results from only one round of random matching, the results are not sensitive to the particular random draw.

In this section we first report our main findings on minimum wage effects. We find strong earnings effects, but employment effects that are close to zero. We then visually examine the dynamic response of employment and earnings to minimum wage changes using a distributed lag specification. We also test whether cross-border spillovers might be affecting our local comparisons. Finally, we consider the effects of our sample restrictions.

5.1 Main Findings

Table 4 displays our estimates of the minimum wage effect on earnings and employment. The first row reports the effects on restaurant earnings, while the second row reports the effects on restaurant employment. Columns 2 and 5 provide our preferred specifications, as they control for average private sector earnings/employment (and for log population in the case of employment regressions), and they cluster the standard errors at the highest level (between state, border segment, and both).[19]

Row 1 shows that the minimum wage effect on earnings is positive and relatively similar in both samples (contiguous county pairs, as well as cross-state-MSAs), with elasticities of 0.18 in the contiguous county and 0.15 in the within-MSA subsample. When the standard errors are clustered at the highest level (columns 2 and 5), the earnings effects remain significant at the 1 percent level in both cases. This results shows that we are capturing a true wage effect generated by changes in the minimum wage (i.e., we find clear evidence of treatment).

The results in Table 4 (row 2) also indicate that minimum wage employment elasticities are very close to zero. The point estimate of the minimum wage elasticity for the contiguous county sample is 0.007 (column 2), and 0.024 for the cross-state metro sample. Even after correcting for serial correlation and spatial autocorrelation, we can rule out minimum wage

[19] In all the cases, this procedure picks clustering at the border segment.

elasticities smaller than *–0.081* in our contiguous county sample, as shown by our 90 percent confidence intervals. This result is similar to the findings of previous local case studies. [20]

What difference does correcting for serial and spatial autocorrelation make? For employment (Table 4, row 2), correcting for all three forms of autocorrelation (which in both cases selects clustering just on the border-segment) produces standard errors that are roughly 3 to 4 times larger than the OLS standard errors. Although not reported here, when we just cluster on state, standard errors are about 2 to 3 times larger than the unclustered standard errors, indicating the importance of clustering on the border segment in local case studies. Comparing neighboring places across the state border can generate an overstatement of precision by up to 50% if cross-state spatial autocorrelation is not taken into account.

We can also provide estimates and bounds on the labor demand elasticity (as opposed to the minimum wage elasticity). The labor demand elasticity is the employment effect divided by the earnings effect. To compute bounds on this ratio, we estimate the earnings and employment effects jointly 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 residual within clusters). As Table 5 shows, in the contiguous county case the point estimate for the labor demand elasticity is 0.035, and the 90 percent confidence interval is (-0.46, 0.53). The lower bound is substantially smaller in absolute value than -1. This is important as a labor demand elasticity of -1 implies that an increase in wage is exactly offset by a decrease in employment to keep aggregate earnings of (affected) workers constant. Previous minimum wage studies have often found labor demand elasticities greater in magnitude than -1. As before, the smaller cross-state MSA sample produces a wider bound, of (-0.84, 1.23).

[20] We examined whether the county-level restaurant employment series is not stationary. We conduct a Levin-Lin-Chu test, which is a pooled adjusted Dickey-Fuller test for panel data. For our full sample of counties, this test strongly rejects the null hypothesis that all the panels exhibit non-stationarity, with t* statistics of -12.48, allowing up to 2 lags, and -4.75,allowing up to 8 lags. (Under the null hypothesis, t* is distributed as a standard normal random variable.) We conclude that our results are not affected by non-stationarity.

One caveat is that our dataset does not measure the extent to which restaurants adjust to a minimum wage by reducing hours of work per employee. An indirect assessment is possible, however. The strong effects we obtain for average earnings per worker (as opposed to per hour) provide a bound on the size of any hours effects. Indeed, if the full-time equivalent (FTE) labor demand elasticity (as opposed to headcount labor demand elasticity discussed above) were -1 or more (in magnitude), the wage bill (earnings-per-worker X employment) would have to fall. Instead, the wage bill increases in our local specifications, with elasticities between 0.15 and 0.17.[21] Since about a fifth of the restaurant workforce is paid at or near the minimum wage in our sample period (18.8 percent in 2006), our estimated wage bill elasticity is inconsistent with any significant reductions in hours.[22]

5.2 Dynamic Responses to Minimum Wage Increases

We examine possible lagged or anticipated effects of minimum wages using a distributed lag specification of four leads and four lags. Here time is incremented in 6 month intervals, so *t+4* is two years after the minimum wage increase. The regression specifications are identical to equations (4) and (5), except that in addition to the contemporaneous minimum wage, we also include leads and lags of the same Panels A and B in Figure 4 display the cumulative response of minimum wage increases. The cumulative response plots consistently show sharp increases in the earnings centered around *t*, i.e., the time of the minimum wage increase. The maximal effects occur typically at around *t*+2 or *t*+3, i.e., a year to year and half after the increase, and range from 0.25 to 0.32 depending on the specification.

In contrast, we find no response of employment in the estimation window. The cumulative responses for both the contiguous county and cross-state metro samples essentially show a flat time profile. Overall, we do not observe any evidence of an "anticipation effect,"

[21] To obtain the wage bill elasticity implied by Table 4, we add the employment and earnings elasticities.
[22] Dube, Naidu and Reich (2007) collected data on hours for part-time and full-time workers. Consistent with these results, they find no effects on scheduled hours or a shift from full-time to part-time employment in fast food restaurants.

whereby employers downwardly adjust their employment prior to the time of actual increase in minimum wage, nor do we observe any evidence of a "lagged effect" of the increase outside the typical "case study" windows. Moreover, allowing for longer lag time (3 years) does not change this result.

5.3 Spillover Effects

Although we find positive earnings effects and insignificant employment effects in Table 4 and Figure 4, spillovers between the treatment and control counties may be affecting our results. Spillovers may occur because the labor or product markets in each county pair are 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 double-differenced specification:

(3)

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

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. Similarly $\overline{y_{st}^{TOT}}$ is the average employment (or earnings) of all private sector workers in the interior counties. The coefficient η measures the *differential* effect of a minimum wage in a border county vis-à-vis interior counties in the same state, as compared to its cross-state border counterpart. In terms of employment, a significant negative coefficient for η indicates that there is 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.[23]

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

When we difference our county level outcome from the state interior, as in equation 3, we are introducing a mechanical correlation in the dependent and control variables across counties within the same state even when they are not on the same border segment. This correlation is not a concern *per se* for the calculation of standard errors, as we allow two-dimensional clustering by state and by each border segment. As we indicated before, however, in this case our maximal standard errors turn out to be those with two-dimensional clustering, which makes sense since we are looking at outcomes in border counties differenced from a (common) state interior. In this example, multi-dimensional clustering makes a real difference, as it controls for spatial autocorrelation on different non-nested dimensions in a manner that accords with our expectation.

[23] Here the unit of observation is still county by period, so there are duplicated observations (as the state-wide 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 (3).

Table 5 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 full sample (column 1) as well as for the subsample for which we can match counties with state interiors (column 2).

The earnings effect is somewhat smaller when we restrict our sample to counties in states that have an "interior" (column 2). The effects are similar, however, in border and interior counties when we examine the two sets of counties separately, 0.147 and 0.133, respectively. The spillover measure is small (0.012) and not significant.

We also do not find any statistically significant spillover effects on employment. When we compare interior counties only, the measured effect is a very small positive (0.036), while when we consider the border counties (column 1 or 2), the effect is essentially zero. The magnitude of the spillover is small (-0.053) and statistically insignificant.

Since we find a near zero employment effect for our full sample of contiguous counties, the relevant concern is whether a rising minimum wage in a border county pushed up wages and hence also reduced employment in the neighboring county. We do not find any evidence of such positive wage spillover or negative employment spillovers. To sum up, the results of this analysis indicate that spillovers turn out not to be important and so comparing contiguous cross-state county pairs does not generate misleading estimates of the true average treatment effects.

5.4 Sample Robustness

Our sample consists of a balanced panel of 211 counties where restaurant employment is reported for all 66 quarters. However, sometimes there may be too few to restaurants to satisfy the non-disclosure requirements. To check for the possibility that excluding these 197 counties with partial information is affecting our results, we estimate the minimum wage elasticity keeping them in the sample. We do not report these results in the tables for space consideration , but we find that the two set of estimates are virtually identical. Whereas the elasticity (standard error) from the balanced panel regression is 0.007 (0.053), the elasticity from the unbalanced panel is 0.001 (0.055).[24]

We also exclude very border counties covering more than 2,000 square miles. However, our estimates are not sensitive to whether we exclude these 20 counties. When we include these large counties in our sample, the employment elasticity (standard error) changes from 0.007 (0.053) to 0.006 (0.059). (These results are also not included in the tables for consideration of space.)

6. Estimating Minimum Wage Effects with National Panel Data

To compare our results to more conventional approaches in the literature, we estimate earnings and employment effects using the national sample of counties and including county and period fixed effects. Although the analysis takes place at the county rather than the state level, the specifications are in the mode of Neumark and Wascher (1992). The number of counties with a balanced panel of reported data yields a national sample of 91,146 observations. We also estimate the national level regression just on counties that par part of a MSA. Among the 866 counties that are located in the 359 MSAs defined by the Census Bureau, the QCEW reports a full panel of restaurant industry data on 733 counties.

We estimate:

$$\ln y_{it} = \alpha + \eta \ln(w_{it}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_t + \varepsilon_{it} \tag{5}$$

The inclusion of log population makes the results qualitatively similar to examining the employment/population ratio, as is done in the CPS-based teenage employment studies. The county and time dummies control for local differences and overall labor market conditions. Crucially, however, unlike the local estimates, here the time effects are assumed to be common

[24] One may 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 and insignificant.

across counties, ruling out possibly heterogeneous trends. We estimate the equation for all counties, as well as for metro counties only. We thereby facilitate comparisons between our two types of local estimators (contiguous county pairs, and cross-state MSAs).

As an intermediate specification, we consider a coarser control for spatial heterogeneity. Here we estimate the minimum wage effect within the nine census divisions, denoted by *c*

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

The term τ_{ct} sweeps out the between-census division variation and estimates are based only on the variation within each census division. Although this specification is less likely than the local estimators to control for all relevant unobserved trends, it provides a sense of the level of geographical aggregation at which the unobserved trends may contaminate the estimated elasticity.[25] Standard errors for these estimates are clustered at the state level.

In Table 6 the earnings effects for the preferred specifications are presented in columns 2, 5, and 8; these include all controls and standard errors are clustered at the state level. The earnings effects from the national panel are quite similar to what we find in our local estimates: they range between 0.18 and 0.22. Again, these effects are all significant at the 1 percent level.

In contrast, the employment effects are quite different from the local estimates. For the estimates without division-specific time effects (columns 2, 5), the minimum wage employment effects (elasticities) range between -0.18 and -0.21, and are significant at the 5 percent level after clustering at the state level. In other words, the national panel specifications for restaurant workers generate negative elasticities that are similar in magnitude to previous CPS- based panel studies that focus on teenagers.[26]

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[25] We only report the estimates that include census division specific time period effects for metro counties. However, the estimates for all counties are quantitatively similar.

[26] 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 zero, the true elasticity is exp(η); in this case exp(0.22) = 0.25.

When we allow for division-specific time effects (column 8), the employment effect diminishes greatly in magnitude, to -0.07, and it is no longer statistically significant. The minimum wage elasticity and the labor demand elasticity are each about a third as large in magnitude when we allow for division specific time effects (column 8 versus column 5). This result is consistent with the hypothesis that the national-panel estimators suffer from serious omitted variables bias arising from spatial heterogeneity.

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. Our results raise concerns that the implied precision of the estimates in the literature may have been overstated because of inattention to correcting for correlated error terms. Since, however, the data sets in question are different, further research is needed to confirm this hypothesis.

We turn next to the cumulative responses from the distributed lag model. The national panel estimates (Figures 5C and 5D) show negative employment effects, especially through a year and half after the minimum wage increase. There is the appearance of a rebound in the last lag, which captures the "long run" effects when the regression is in levels. Comparing t+4 to t-1, we find a cumulative decline in employment of around -0.18, similar to the effects from just including the contemporaneous minimum wage.

Figure 4E shows the cumulative response when division-specific time effects are allowed. The negative effect is much more muted, and unlike Figures 4C and 4D, Figure 4E does not provide visually compelling evidence of job loss starting around the time of the wage increase. The patterns in the cumulative response graphs thus mirror our previous regression results.

We draw two conclusions from these results. First, the bias introduced in the national panel estimates does not arise simply from long-run growth differences between places with higher minimum wages and places with lower ones. The apparent employment reductions in these time paths are concentrated about a year and half after the minimum wage increases; but this reduction is completely erased once we allow for division-specific time effects. The evidence indicates that region-specific shocks (or region-specific responses to common shocks) are correlated with minimum wage changes. Lack of controls for such shocks can produce highly misleading estimates of minimum wage effects.[27]

7. Reconciling the Local and National Estimates

In this section, we consider some factors that might generate the differences between the local and national estimates. The evidence thus far suggests that national estimates insufficiently control for heterogeneity in employment growth, and that this heterogeneity is spatial in nature. We test this hypothesis first by including state-level linear trends in all our specifications. To show the spatial nature of the heterogeneity, we also devise a falsification test which estimates the effects of a spatially-correlated placebo minimum wage increase on the sample of counties that had the same minimum wage over the whole period of study. We then consider the possibility that sample differences between the local and national estimators, as opposed to specification differences (i.e., including local time effects) are behind the different findings. We also consider the possibility that the differences arise from different relative weights of low frequency and high frequency components in minimum wage variation in the two samples. We finally examine minimum wage effects by type of restaurant (full service or limited service) and by whether state minimum wage laws allow for tip credits.

7.1 State-level Linear Trends

We compare our local estimators to the national estimators by testing whether the resulting elasticities are robust to the inclusion of state-specific linear trends as added controls. Comparing how the different estimators respond to the inclusion of such trends provides a one-sided test of their internal validity. If the inclusion of state-level linear trends does not change the

[27] We also estimated our local and national level regressions using populations weights. These results were very similar to the unweighted results reported here.

minimum wage coefficient, we have strong evidence that the model controls for unobserved long term trends. This result will obtain if the true effect is small in magnitude, and if we have sufficient controls for heterogeneity in latent employment growth.

However, if an estimated coefficient changes substantially with the inclusion of linear trends, we will not be able to distinguish between the following two interpretations: (1) there are unobserved trends correlated with minimum wage changes that the linear trend is controlling for, and (2) there are no such unobserved trends, but including the linear trend attenuates the minimum wage coefficient. The inclusion of linear trends may understate the true effect of the minimum wage if the policy causally affects the trend itself. The state-level trend is jointly estimated with the minimum wage effect. Since there is no variation in minimum wages within a state in a time period, we cannot identify the effect of minimum wages without assumptions about the functional form of the trend.

Table 7 reports results for specifications that include state-level linear trends. As before, we find strong earnings effects that are similar across specifications. For employment, in our local estimators (columns 1 and 2) we cannot reject the hypothesis that coefficients are stable to the inclusion of linear trends, as the probability value for the Hausman test is around 0. For contiguous county pairs, for example, the coefficient when including linear trends is 0.056, as opposed to 0.007 without.[28]

In the national panel estimates, however, including linear trends reverses the signs and significance of the employment estimates (columns 3, 4 and 5). For all counties (column 3), the coefficient changes from -0.18 to 0.04, and it is not significant at conventional levels. The Hausman tests indicate that we can reject the null hypothesis of coefficient stability from the inclusion of trends in these three specifications, at least at the 10% level.

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[28] We test for the cross-equation stability of the coefficients by jointly estimating the equations with and without a state-level linear trend using seemingly unrelated regression (SUR), allowing for the standard errors to be clustered at the appropriate levels.

The sensitivity of the national panel estimates to the inclusion of state linear trends does not *necessarily* imply that the national panel estimator is biased; inclusion of trends may "overcontrol" if minimum wages themselves reduce the employment trends of minimum wage workers. However, the local estimators are not sensitive to the inclusion of linear trends. These two pieces of evidence together thus provide internal validity of the local estimators and further support for the contention that the fixed-effects estimator is biased downwards due to insufficient controls.

7.2 Falsification Tests using Spatially-Correlated Placebo Laws

To provide a direct assessment of how the national estimates are affected by spatial heterogeneity, we estimate 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.* In other words, we consider only states that have exactly the same minimum wage profiles, but that *happen to be located in a "neighborhood" with higher minimum wages.* To give an example, we are examining whether the pattern of minimum wages in Massachusetts predicts an employment loss for neighboring New Hampshire, compared to the pattern of minimum wages in Texas applied to Oklahoma. New Hampshire, Texas and Oklahoma all had not instituted state-wide mandates and had identical minimum wage profiles over this period. If there is no confounding spatial correlation between minimum wage increases and employment growth, the estimated elasticity from the fictitious minimum wage should be zero.

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)

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

$$\text{(7)} \qquad \ln y_{it} = \alpha + \eta_n \ln(w_{it}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_t + \varepsilon_{it}$$

This is identical to equation (5) with only county and time fixed effects, but 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 only have a federal minimum wage.

We then take our placebo sample of counties that only had 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:

$$\text{(8)} \qquad \ln y_{it} = \alpha + \eta_n \ln(w_{nt}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_t + \varepsilon_{it}$$

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 zero, as this set of counties has identical minimum wage profiles. If it is instead similar to the η from equation (7), we have evidence that the national level estimates (using only time and county fixed effects) are biased because of the presence of spatial heterogeneity. To use our earlier example, equation (7) estimates the effect of an increase in the Massachusetts minimum wage on employment in Massachusetts as compared to Texas. In contrast, equation (8) documents the effect of an increase in the Massachusetts minimum wage on employment in New Hampshire as compared to Oklahoma, both of whom have the same (federal) minimum wage over this period. As before, we restrict our analysis to balanced panels with full reporting of data.

The first two rows of Table 8 show the effects from equation (7), or the actual sample. As expected, we find results similar to the national estimates (in Table 6), with employment effects of -0.21 and -0.33 in the all counties and metro counties specifications, respectively. The standard errors are larger due to the smaller sample size, but the metro coefficient is significant at the 5 percent level. 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 (equation (8)), we do not find significant earnings effects. This is as 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 elasticities of -0.12 (all counties) and -0.30 (metro counties). For the metro counties, the results remain significant at the 10 percent level after clustering the standard errors at the state level.

In other words, the measured disemployment effect when we compare Massachusetts and Texas is similar to what we obtain by applying Massachusetts' minimum wage to New Hampshire and Texas' minimum wage to Oklahoma -- even though New Hampshire and Oklahoma have had the same minimum wage over this whole period. As we discussed in section 5, we do not find actual (causal) cross-border spillovers in earnings or employment. Therefore, the estimates from placebo laws provide strong evidence that spatial heterogeneity in low-wage employment prospects are correlated with minimum wages, and these trends seriously confound minimum wage effects in models using national-level variation.

7.3 Moving from National to Local Estimates – Sample versus Specification

The evidence from both the placebo minimum wages and the state trends point to a omitted variables bias in the national-level panel estimates. However, there are two differences between the local estimates and the national estimates: 1) local estimates have controls for local heterogeneity by allowing time period effects to vary by local areas; and 2) local estimates are based on a smaller subsample of counties with local differences in minimum wages. For instance, by looking at county pairs with a minimum wage differential, we exclude a large number of counties, especially those in the South and the Mountain states, that consistently only have the (lower) federal minimum wage. Therefore, our contiguous county sample is already more homogeneous in its attributes.

To evaluate the effects of sample differences versus specification differences as we move from national to local data, we utilize the following approach. We partition the counties into geographic cells, and consider progressively finer partitions. For each partition, we estimate the minimum wage effects with and without cell specific time effects (i.e., different specification) holding the estimation sample constant.

The landmass of the continental United States can be exactly fitted into a rectangle of width 57 and height 24, measured in decimal degrees. We partition this rectangle into K^2 potential cells by dividing the height and width into K equal segments. We consider all values of K between 2 to 10 . For example, with $K=2$, we divide the rectangle containing all counties into four cells, roughly corresponding to the four census regions. With $K=3$, we partition the rectangle into nine cells, somewhat corresponding to the nine census divisions. With $K=10$, we are creating 100 such potential cells. Note that this partitioning creates rectangular cells of equal sizes for a given level of K. A county is considered to be part of a given cell if the latitude and longitude of its geographic centroid falls with the cell.

For each K, we identify cells that have some internal variation in minimum wages, which we need to identify minimum wage effects once we allow cell-specific time effects. In Table 9, we report both the potential number of cells (K^2), as well as the actual number of cells that have some within-cell variation in minimum wages over this period. As we partition the rectangle into finer sets, the actual number of usable cells rises more slowly than the number of potential cells.

For each *K*, we estimate two regressions:

$$\ln y_{it} = \alpha + \eta \ln(w_{it}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_t + \varepsilon_{it} \tag{9}$$

$$\ln y_{it} = \alpha + \eta \ln(w_{it}^{M}) + \delta \ln(y_{it}^{TOT}) + \gamma \ln(pop_{it}) + \phi_i + \tau_{cell,t} + \varepsilon_{it} \tag{10}$$

Equation (9) has the same specification as equation (4), and only allows for county and (common) time effects.. Equation (10) allows cell-specific time effects and county effects, and hence sweeps out all variation between cells. This estimate is analogous to our local estimates and the census-division estimates, but proceeding progressively from national, then regional, and then to more localized levels of variation in a continuous fashion. The estimation sample (for a given *K*) used for both (9) and (10) is restricted to cells that have some within-cell variation in minimum wages, and hence is identical. Hence, the difference between the two set of estimates results solely from allowing cell-specific (as opposed to common) time effects, and not due to differing samples.

The results, displayed in Table 9, exhibit two distinct patterns. First, the effect of allowing for cell-specific time effects is stark. With 4 cells (*K*=2), when we control for spatial heterogeneity, the coefficients falls (in magnitude) from -0.17 (column 3) to -0.05 (column 4). With 11 cells (*K*=4), the coefficient falls (in magnitude) from -0.17 to -0.02. With 16 or more cells, the estimate with cell specific time effects (column 4) essentially hovers around zero.

Second, as we consider finer cells, the sample becomes smaller, as there are fewer cells with minimum wage variation. Moreover, the measured disemployment effect without including local controls becomes smaller in magnitude. This pattern indicates the effect of sample homogenization when making local comparisons. Yet for every partitioning, including cell-specific time effects produces smaller disemployment estimates. This pattern indicates the specification effect of making local comparisons. The specification effect becomes smaller as we consider finer partitions, as the sample of cells with minimum wage variation becomes more and more homogeneous. Each of these patterns strongly suggests that spatial heterogeneity contaminates national-level panel/time fixed effects estimates; local estimates are superior both because they utilize better specifications, and because they have a more homogeneous sample.

7.4 High and Low Frequency Components of Minimum Wage Effects

In an often-cited paper, Baker et al (1999) also argue that they can rationalize the divergent findings between the "new" minimum wage literature that focuses on before-after case studies, and the more traditional time series or panel based analysis. Using Canadian provincial data for 1975-1993, they find larger disemployment results are associated with low-frequency (i.e., more permanent) variation in the minimum wage, suggesting that the long-term effects are indeed negative; they find that small or zero employment effects are associated with high frequency (i.e., more transitory) components of variation in the minimum wage. They aregue that case studies using before-after comparisons find small effects because they observe only a short window around the minimum wage changes (i.e., they only utilize the high frequency component of variation).

In our study, with a 16½ year panel of counties and substantial variation in minimum wages, identification of the local and the national level estimates does not rely on the high frequency component of variation. While we find apparent negative effects that are similar to Baker et al for the national sample with panel/period fixed effects, these effects disappear as we consider more local comparisons. The key factor creating the negative employment effects in the national panel estimates does not appear to be the frequency component of variation in minimum wages, but rather an omitted variables bias resulting from heterogeneity in employment growth.

It is possible, nonetheless, that the difference between the national and the local results arises from differences in the *relative weight* of high versus low frequency components in minimum wage variation between the two samples. It is possible that limiting our sample to areas with local differences in minimum wages increases the weight of the high frequency component in our local estimators. In this section, we consider this possibility directly by estimating the impact of high versus low frequency components of minimum wage separately in our national and local samples. Our terminology and definition of high and low frequency components come directly from Baker et al.

The minimum wage series can be exactly decomposed into a high and a low frequency component:

$$\ln(w_{it}^{M}) = \frac{1}{2}\left(\ln(w_{it}^{M}) - \ln(w_{it-4}^{M})\right) + \frac{1}{2}\left(\ln(w_{it}^{M}) + \ln(w_{it-4}^{M})\right)$$

Here , $\frac{1}{2}\left(\ln(w_{it}^{M}) - \ln(w_{it-4}^{M})\right)$ or the high frequency component, is the 4 quarter difference in log minimum wage, while $\frac{1}{2}\left(\ln(w_{it}^{M}) + \ln(w_{it-4}^{M})\right)$ or the low frequency component is a moving average. Since we use quarterly data, the annual difference subtracts the fourth lag in minimum wage. Baker et al use annual data and hence utilize a first-differenced series for the high frequency component and a first order moving average for the low frequency one.

Now consider the following two regressions, where "time effects" refer to possibly geography specific time effects:

$$\begin{aligned} \ln y_{ipt} &= a + \theta^{H}\frac{1}{2}\left(\ln(w_{it}^{M}) - \ln(w_{it-4}^{M})\right) + \theta^{L}\frac{1}{2}\left(\ln(w_{it}^{M}) + \ln(w_{it-4}^{M})\right) + b\ln(y_{it}^{TOT}) \\ &\quad + c\ln(pop_{it}) + \phi_i + [\mathit{time\ effects}] + e_{it} \end{aligned} \tag{11}$$

$$\begin{aligned} \ln y_{ipt} &= a + \eta_0 \ln(w_{it}^{M}) + \eta_4 \ln(w_{it-4}^{M}) + b\ln(y_{it}^{TOT}) + c\ln(pop_{it}) + \phi_i \\ &\quad + [\mathit{time\ effects}] + e_{it} \end{aligned} \tag{12}$$

Equation (11) regresses employment on both the high and low frequency components of minimum wage. It can be shown that the resulting coefficients θ^{H} and θ^{L} can also be defined using the elasticity coefficients from the contemporaneous minimum wage η_0, and the one-year lagged minimum wage η_4 obtained by estimating equation (12). Namely, $\theta^{H} = \eta_0 - \eta_4$, and

$\theta^L = \eta_0 + \eta_4$. There are two key points to note here. First, the effect of the low frequency component of the minimum wage is simply the sum of the elasticities from contemporaneous and lagged minimum wage, i.e., $\theta^L = \eta_0 + \eta_4$. It is this component that Baker et al find to be negative and large in magnitude, which suggests that lower frequency variation (in this case of *cycles longer than 4 years*) in the minimum wage is associated with reduced employment. In contrast, they find the high frequency component is often zero or even positive, and this, they argue is what case studies identify.

In Table 10, we report these four parameters, $\eta_0, \eta_4, \theta^L, \theta^H$, for each of the five specifications. Our local estimates show that the low frequency component θ^H reflecting the "long term" effect (column 3) is close to zero. For contiguous counties, this long term effect is -0.015. Moreover, the standard error of 0.07 suggests we can rule out long term minimum wage elasticities of greater than -0.127 at the 90 percent confidence level. In contrast, for the national specification (rows C and D), *both* the high and low frequency components are negative, and similar in magnitude. These results show that the employment effects in both the local and the national estimates are not associated with any particular frequency components of the minimum wage variation. Put more simply, the divergent findings from the national and local estimates we report are *not* due to differences in long versus short run response to minimum wage hikes.[29]

7.5 Minimum wage effects by type of restaurant and the impact of tip credits

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 the full service and limited service restaurants. We also explore the impact of tip credits.

[29] We also estimated the models with a 12 quarter (3 year) lag in minimum wage instead of 4 quarter lag to allow for even lower frequency variation. The results are identical for the contiguous counties specification, where the sum (std. error) is *still* equal to -0.02 (0.07). In other words, allowing for even longer lags does not change our conclusions on the point estimate or the statistical bounds.

These results are reported in Table 11. The estimated earnings effects, reported in column (1), are positive and significant for both full-service and limited-service restaurants, and for both the local and national estimators. For the local estimators, the earnings effect is considerably greater among limited-service restaurants than among full-service restaurants (0.218 versus 0.140 in row 1), which is to be expected since limited-service restaurants have a higher proportion of minimum wage workers and a very small proportion of workers in such restaurants qualify for tip credits. But for the national estimators, the reverse is the case. In row 3, for example, the elasticity for full-service restaurants is .267, compared to .197 for limited service restaurants.

The employment effects are reported in column (2) of Table 11. For the local estimators, the effects are indistinguishable from zero for both restaurant sectors. The results we reported above (in Table 4) for the entire restaurant industry hold when we consider full service restaurants or not. However, the precision of our estimates falls when we disaggregate the results by sector.

The employment effects we obtain using the national estimators, in contrast, differ considerably by restaurant sector. For full-service restaurants, the disemployment effects are substantially larger than they are among limited-service restaurants. This result is surprising, given the higher proportion of minimum wage workers in the limited-service segment.

The magnitude and significance of our earnings effects do not support the hypothesis that tip credits attenuate minimum wage effects on earnings of full-service restaurant workers.[30] Why might this be? First, 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.

[30] Tip credits 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 percent of the minimum wage.

Second, when minimum wages increase, competitive pressures may lead to similar increases in base pay for all workers, whether or not they receive tips.

To examine this question more directly, we report in Table 11 estimates using only the 43 states that have tip credits.[31] Since the non-tip credit states include the entire Pacific division as well as many of the states with higher than federal minimum wages, the resulting sample becomes not only smaller, but also more homogeneous. Nonetheless, each of the estimated effects on earnings, reported in column 3, remains quite close to those in column 1. For the local estimators, the employment effects (column 4) are also quite similar to those in column 2—indistinguishable from zero in both full-service and limited-service restaurants. For the national estimators, the employment effects are now more attenuated than in column 2, and for both types of restaurants, which is consistent with the increased homogeneity of this sample.[32]

Overall, we conclude that the results for the local estimates are not driven by tip credits, as the earnings effects are strong in both full and limited service restaurants, and also when we only consider 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.

8. Discussion and Conclusions

In this paper, we use two new estimators that take advantage of local minimum wage differences, either between pairs of contiguous counties or between groups of counties within metropolitan areas. Our approach addresses the twin concerns that heterogeneous spatial trends can bias the estimated minimum wage effects in studies that rely on national comparisons, and that not accounting for heterogeneous spatial trends overstates the precision in individual case studies.

[31] The seven states that do not have a tip credit are: Alaska, California, Minnesota, Montana, Nevada, Oregon and Washington.
[32] When census division controls are included, the employment effects are greater; this may result from the exclusion of one of the nine census divisions from the sample.

For cross-state metro counties and 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 sets of studies result from insufficient controls for unobserved heterogeneity in employment growth in the national panel models. 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 national panel regressions 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 estimators perform better in a number of tests of internal validity. Unlike national- panel regressions, they are robust to the inclusion of state-level trends.

How should one interpret the magnitude of the difference between the local and national estimators? The national level estimates suggest a labor demand elasticity of around -1. This implies that an increase in the minimum wage has no 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 90% level) around our contiguous county estimates of the labor demand elasticity as identified from a change in the minimum wage rule out anything above -0.46 in magnitude. This suggests that minimum wage increases *do* raise the overall earnings of affected workers, although there may be differential effects by demographic groups due to labor-labor substitution.

Do our findings carry over to other datasets and affected groups? Although we cannot address this question directly, results from our forthcoming companion paper using the CPS suggests an affirmative answer. (See Allegretto, Dube, and Reich 2007.) In that paper, we find that allowing for spatial trends at the census division level reduces the measured disemployment level substantially when we consider the response of teenage employment to minimum wage increases. Additionally, and parallel to our findings here, we find that the measured disemployment effects are not robust to allowing state level trends in the underlying teenage employment. Although the evidence is preliminary, it suggests that the relevance of our findings are broader than just the restaurant industry (which at any rate is the major employer of minimum wage workers).

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 sizeable, our conclusion is limited by the scope of the actual variation in policy. In other words, although we find virtually no disemployment effects from moderate increases in minimum wages, the outcome may be different if we were to consider much larger increases in the minimum wage. A second caveat concerns the distribution of minimum wage differences across the country over time. We find durable local differences in minimum wages in our sample, especially in recent years. However, the estimated effect of a wage increase in a given area is necessarily *conditional on the profile of wage increases elsewhere.* To the extent that regional gaps in minimum wages are not expected to be truly permanent, the employment response may be muted.

These caveats notwithstanding, our results provide an explanation for 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, and these estimates suggest little employment loss from the kind of minimum wage increases we have seen in the US. Our analysis highlights the importance of accounting for such heterogeneity in future work on this topic.

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Table 1 Average Annual Employment Growth Rates, by Census Division

	(1) New England	(2) Middle Atlantic	(3) East North Central	(4) West North Central	(5) South Atlantic	(6) East South Central	(7) West South Central	(8) Moun-tain	(9) Pacific
Restaurant Employment									
1991-1996	2.7	1.6	5.0	2.9	3.6	3.9	5.0	5.5	2.6
1996-2001	1.7	1.9	0.8	1.7	2.0	2.8	3.9	3.2	2.3
2001-2006	1.8	2.2	1.9	2.5	3.5	3.2	2.2	3.0	2.3
1991-2006	1.7	1.9	2.9	2.7	3.2	3.7	4.1	4.1	2.4
Overall Private Sector Employment									
1991-1996	0.9	0.2	2.0	2.5	2.6	2.9	2.7	5.1	0.5
1996-2001	2.1	2.0	1.4	2.1	2.7	1.9	3.3	4.1	3.1
2001-2006	-0.6	-0.1	-0.5	0.2	1.1	0.7	0.4	1.8	0.6
1991-2006	0.4	0.4	0.8	1.5	1.8	1.5	2.0	3.2	1.1

Source: Bureau of Labor Statistics, *Quarterly Census of Employment and Wages* (QCEW).

Table 2 Descriptive Statistics

	(1)		(2)		(3)		(4)	
	Contiguous county pairs		**Cross-state MSA counties**		**All counties**		**All metro counties**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Population, 2000	166,828	(339,525)	425,063	(610,049)	180,945	(423,274)	302,131	(552,758)
Population density, 2000	591	(3,551)	2,575	(7,656)	464	(2,552)	807	(3,463)
Land area (square miles)	1,236	(2,161)	543	(624)	1,107	(1,760)	972	(1,705)
Overall private employment	62,817	(156,618)	165,941	(276,880)	66,668	(172,259)	113,985	(225,984)
Restaurant employment	4,095	(8,340)	9,212	(13,884)	4,509	(10,517)	7,593	(13,693)
Restaurant average weekly earnings ($)	171.50	(34.19)	197.16	(58.72)	170.79	(43.76)	183.18	(45.38)
Minimum wage	4.73	(0.91)	4.92	(0.74)	4.62	(0.80)	4.62	(0.61)
Mean number of federal minimum wage events	3.53	(1.96)	3.13	(1.20)	3.54	(0.98)	3.55	(0.96)
Mean number of state minimum wage events	2.32	(2.99)	2.15	(2.07)	1.32	(2.25)	1.31	(2.16)
Number of counties	211		137		1,381		733	
Number of MSAs	n/a		24		n/a		359	
Number of States	33		24		48		48	

Notes: Sample means are reported for contiguous county pair sample, the cross-state MSA sample, all counties, and all metro counties for counties with full reporting. Standard deviations are reported in parentheses. Mean number of federal minimum wage events is computed as the mean of an indicator variable which takes on the value 1 in a quarter where the federal minimum changes. The mean number of state minimum wage events is computed analogously for statewide minimum wages. 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.*

Table 3 Mean Absolute Differences: Random Versus Contiguous Pairs

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall private Sector			**Restaurants**		
	Difference from Random Pair	Difference from Contiguous Pair	*Difference in Difference (Random - Contiguous)*	Difference from Random Pair	Difference from Contiguous Pair	*Difference in Difference (Random - Contiguous)*
A. Sample of Border Counties with Minimum Wage Variation						
Employment Growth: 1 year	0.048	0.030	0.019***	0.089	0.070	0.019***
	(0.0004)	(0.0002)	(0.0004)	(0.001)	(0.001)	(0.001)
Employment Growth: 3 years	0.099	0.061	0.034***	0.136	0.108	0.028***
	(0.001)	(0.0004)	(0.001)	(0.001)	(0.001)	(0.001)
Employment Growth: Full Period	0.309	0.220	0.090***	0.332	0.246	0.085***
	(0.017)	(0.011)	(0.019)	(0.020)	(0.016)	(0.024)
Log Employment	2.269	1.109	1.160***	1.810	1.117	0.693***
	(0.001)	(0.005)	(0.011)	(0.011)	(0.0070)	(0.012)
Log Earnings	0.308	0.180	0.128***	0.302	0.152	0.150***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
B. Out-of-Sample Border Counties with No Minimum Wage Variation						
Employment Growth: 1 year	0.053	0.041	0.012***	0.099	0.091	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Employment Growth: 3 years	0.102	0.077	0.025***	0.151	0.137	0.014***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Employment Growth: Full Period	0.360	0.248	0.112***	0.319	0.302	0.089***
	(0.045)	(0.016)	(0.026)	(0.030)	(0.022)	(0.034)
Log Employment	1.879	1.274	0.605***	1.676	1.336	0.340***
	(0.012)	(0.008)	(0.013)	(0.013)	(0.010)	(0.015)
Log Earnings	0.234	0.173	0.061***	0.243	0.161	0.082***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)

Notes: Columns (1) and (2) refer to the mean absolute difference between a county and its (a) random pair, and (b) contiguous cross-state pair. Columns (3) refer to the difference-in-difference between the random pair and the contiguous pair. Standard errors are displayed in parentheses below the means. For tests of difference-in-difference between contiguous and random county pairs, significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 4 Minimum Wage Effects on Earnings and Employment

	(1)	(2)	(3)	(4)	(5)	(6)
	Contiguous county pairs			Cross-state MSA counties		
Log Earnings	0.177^{***}	0.177^{***}	0.188^{***}	0.149^{***}	0.149^{***}	0.153^{***}
	(0.011)	(0.033)	(0.035)	(0.014)	(0.045)	(0.046)
Log Employment	0.007	0.007	0.0003	0.024	0.024	-0.0003
	(0.015)	(0.053)	(0.060)	(0.023)	(0.093)	(0.122)
90% CI: minimum wage elasticity		[-0.081, 0.095]			[-0.132, 0.180]	
Labor demand elasticity		0.035			0.210	
		(0.300)			(0.631)	
Controls:						
County Pair x Period	Y	Y	Y			
MSA x Period				Y	Y	Y
Overall private sector	Y	Y		Y	Y	
Population	Y	Y		Y	Y	
Std. errors clustered		Y	Y		Y	Y

Notes: Sample size for contiguous county pairs and cross-state metro counties is equal to 23,460 (211 counties) and 9,042 (137 counties) respectively. 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- or MSA-specific time period 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 border segment only. Confidence intervals for the minimum wage elasticity of employment are reported for specifications 2 and 5; the point estimate of this elasticity is just the regression coefficient in the employment equation reported above. 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 are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 5 Tests of Cross Border Spillover Effects from Minimum Wage Changes

	(1) Border Counties	(2) Border Counties	(3) Interior Counties	(4) Spillover = (Border - Interior)
Log Earnings	0.177***	0.147***	0.133**	0.012
	(0.033)	(0.030)	(0.049)	(0.076)
Log Employment	0.007	-0.025	0.036	-0.053
	(0.053)	(0.065)	(0.059)	(0.102)
Sample	Baseline	Spillover	Spillover	Spillover
Controls:				
Pair x period	Y	Y	Y	Y
Population	Y	Y	Y	Y
Overall private sector	Y	Y	Y	Y

Notes: The spillover sample restricts observations to states with interior counties. Size of the baseline and spillover samples is equal to 23,460 and 22,800 respectively. 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 are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 6 Minimum Wage Effects in National Panel Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All Counties			All Metro Counties			All Metro Counties		
Log Earnings	0.217***	0.217***	0.224***	0.191***	0.191***	0.210***	0.184***	0.184***	0.196***
	(0.004)	(0.028)	(0.033)	(0.005)	(0.024)	(0.031)	(0.008)	(0.032)	(0.038)
Log Employment	-0.176***	-0.176**	-0.147	-0.207***	-0.207***	-0.215**	-0.076***	-0.076	-0.017
	(0.008)	(0.096)	(0.121)	(0.009)	(0.063)	(0.083)	(0.014)	(0.060)	(0.075)
90% CI : Minimum wage elasticity		[-0.338, -0.015]			[-0.312, -0.102]			[-0.176, 0.023]	
Labor demand elasticity		-0.814			-1.081			-0.378	
		(0.448)			(0.273)			(0.420)	
Controls									
Census division x period							Y	Y	Y
Overall private sector	Y	Y		Y	Y		Y	Y	
Population	Y	Y		Y	Y		Y	Y	
Std. errors clustered		Y	Y		Y	Y		Y	Y

Notes: Sample size for contiguous county pairs and cross-state metro counties is equal to 91,061 (1,381 counties) and 48,361 (733 counties) respectively. 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 time period effects or census division-specific period effects as noted in the table. Robust standard errors in parentheses are clustered at the state-level. Confidence intervals for the minimum wage elasticity of employment are reported for specifications 2,5, and 8; the point estimate of this elasticity is just the regression coefficient in the employment equation reported above. For the labor demand elasticity, we jointly estimate the earnings and employment equations using seemingly unrelated regression (SUR), 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 are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 7 Minimum Wage Effects with State-Level Linear Trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Contiguous Pairs		**Cross-state MSA Counties**		**All Counties**		**All Metro Counties**		**All Metro Counties**	
Log Earnings	0.177***	0.139***	0.149***	0.125***	0.217***	0.233***	0.191***	0.217***	0.184***	0.198***
	(0.033)	(0.021)	(0.041)	(0.035)	(0.028)	(0.048)	(0.024)	(0.055)	(0.032)	(0.038)
Log Employment	0.007	0.056	0.024	0.120	-0.176*	0.036	-0.207***	0.055	-0.076	0.06
	(0.053)	(0.059)	(0.093)	(0.069)	(0.096)	-0.039)	(0.063)	(0.042)	(0.060)	(0.041)
Controls:										
Census Div x Period									Y	Y
MSA x Period			Y	Y						
County Pair x Period	Y	Y								
Linear State Trends		Y		Y		Y		Y		Y
Probability value:	0.405		0.373		0.063*		0.004***		0.002***	

Notes: All specifications include county fixed effects. They also include either a general time period effect or one of three geographically-specific time period effects (census division, MSA, or county-pair) as noted in the table. Further state-specific linear trends are also included for indicated specifications. All specifications include population and overall private sector employment controls. Probability values are reported for the null hypothesis that the employment elasticities are the same in the specification with added state-level linear trends as the specification without; specifications are jointly estimated using seemingly unrelated regression (SUR). Robust standard errors in parentheses are clustered at the border segment-level for specifications 1-4 and the state level for 5-8. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 8 Falsification Tests: Placebo Minimum Wages on Earnings and Employment

	(1) **Log Earnings**	(2) **Log Employment**
Actual minimum wage sample		
All Counties	0.265***	-0.208
	(0.045)	(0.149)
All Metro Counties	0.241***	-0.334**
	(0.042)	(0.161)
Placebo minimum wage sample		
All Counties	0.079	-0.123
	(0.056)	(0.158)
All Metro Counties	0.062	-0.297*
	(0.075)	(0.146)
Controls:		
Overall private sector	Y	Y
Population		Y

Notes: Actual minimum wage sample is restricted to only 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) restricts 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 neighbors log minimum wage (i.e. the placebo) on own-county log restaurant earnings and employment. Both panels control for county fixed effects, time period effects. 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. Robust standard errors in parentheses are clustered at the state-level. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 9 Minimum Wage Effects by Varying Size of Geographic Cells, Metro Counties

			(1)	(2)	(3)	(4)
Potential Cells (K^2)	**Actual Cells**	**N**	**Log Earnings**	**Log Earnings**	**Log Employment**	**Log Employment**
4	4	103,868	0.259***	0.242***	-0.171*	-0.049
			(0.030)	(0.033)	(0.096)	(0.069)
9	8	103,340	0.259***	0.196***	-0.171*	-0.062
			(0.030)	(0.024)	(0.096)	(0.055)
16	11	89,568	0.253***	0.195***	-0.166*	-0.017
			(0.030)	(0.029)	(0.098)	(0.072)
25	16	88,185	0.260***	0.195***	-0.170*	-0.064
			(0.028)	(0.024)	(0.097)	(0.043)
36	16	70,763	0.250***	0.206***	-0.150	-0.001
			(0.029)	(0.021)	(0.099)	(0.061)
49	20	72,968	0.246***	0.198***	-0.150	-0.004
			(0.027)	(0.022)	(0.099)	(0.069)
64	23	67,889	0.239***	0.179***	-0.137	-0.003
			(0.027)	(0.021)	(0.101)	(0.055)
81	28	66,051	0.240***	0.183***	-0.097	0.032
			(0.032)	(0.035)	(0.098)	(0.040)
100	32	60,288	0.227***	0.186***	-0.062	0.027
			(0.031)	(0.032)	(0.097)	(0.036)
Controls:						
Overall private sector			Y	Y	Y	Y
Population			Y	Y	Y	Y
Period x Cell				Y		Y

Notes: To create geographic cells we take the smallest rectangle that fits all counties in the continental US. We subdivide this rectangle into K^2 potential cells by dividing the height and width into K equal segments. Each row refers to a different K varying from 1 to 10. Counties are considered part of a given cell if the latitude and longitude of its geographic centroid falls within the cell. Actual cells used in the estimation of all specification (for a given K) are those with a variation in the minimum wage across counties within that cell. Therefore, the sample is the same for specifications for a given K. All specifications include county fixed effects. Common or cell-specific time effects are included as controls as noted in the table. 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. Robust standard errors in parentheses are clustered at the state-level. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 10 Employment Effects for High and Low Frequency Minimum Wage Events

	(1) η_t	(2) η_{t-4}	(3) **Low Freq Component** $\theta^L = (\eta_t + \eta_{t-4})$	(4) **High Freq Component** $\theta^H = (\eta_t - \eta_{t-4})$	***Controls:***
A. Contiguous Counties	0.013	-0.028	-0.015	0.042	County pair x period
	(0.07)	(0.04)	(0.07)	(0.09)	
B. Cross State MSA	0.044	-0.043	0.001	0.087	MSA x period
	(0.10)	(0.05)	(0.10)	(0.12)	
C. All Counties	-0.138	-0.006	-0.145	-0.132	Period
	(0.10)	(0.07)	(0.11)	(0.13)	
D. All Metro Counties	-0.186**	-0.011	-0.197***	-0.175	Period
	(0.08)	(0.06)	(0.06)	(0.13)	
E. All Metro Counties	-0.036	-0.027	-0.063	-0.009	Census division x period
	(0.07)	(0.06)	(0.06)	(0.12)	

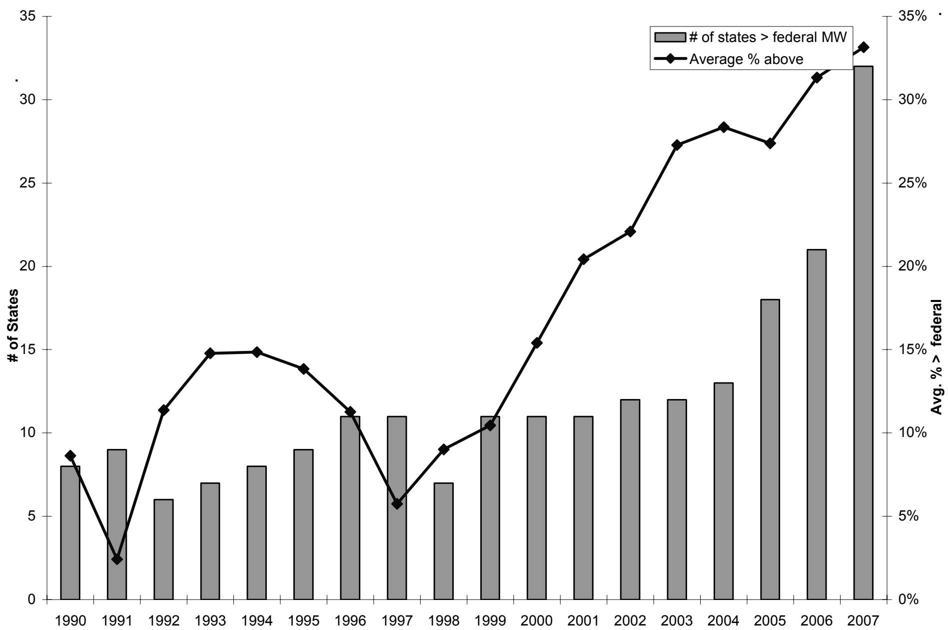
Notes**:** η_t is the contemporaneous employment elasticity and η_{t-4} is the elasticity associated with a one year lagged minimum wage. All specifications include population and overall private sector employment controls. All specifications include county fixed effects. They also include either a general time period effect or one of three geographically-specific time period effects (census division, MSA, or county-pair) as noted in the table. Robust standard errors in parentheses are clustered at the border segment-level for specifications A and B and at the state-level for C-E. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 11 Minimum Wage Effects, States with Tip Credits, Full and Limited Service Restaurants

	(1)	(2)	(3)	(4)			
	All States		Tip Credit States		*Controls :*		
	Log Earnings	Log Employment	Log Earnings	Log Employment	County Pair x Period	MSA x Period	Census Div x Period
Full Service Restaurants							
A. Contiguous county pairs	0.140**	0.023	0.119**	0.057	Y		
	(0.056)	(0.075)	(0.058)	(0.088)			
B. Cross-state metro counties	0.148**	-0.013	0.121**	-0.047		Y	
	(0.051)	(0.112)	(0.53)	(0.124)			
C. All counties	0.267***	-0.230*	0.213***	-0.038			
	(0.041)	(0.137)	(0.060)	(0.135)			
D .All metro counties	0.226***	-0.324***	0.180***	-0.172**			
	(0.034)	(0.098)	(0.041)	(0.084)			
E. All metro counties	0.190***	-0.149**	0.167***	-0.188***			Y
	(0.044)	(0.061)	(0.055)	(0.064)			
Limited Service Restaurants							
A. Contiguous county pairs	0.218***	-0.022	0.225***	-0.028	Y		
	(0.054)	(0.119)	(0.065)	(0.135)			
B. Cross-state metro counties	0.151***	0.017	0.160*	0.041		Y	
	(0.072)	(0.233)	(0.082)	(0.247)			
C. All counties	0.197***	-0.130	0.167***	-0.014			
	(0.031)	(0.109)	(0.022)	(0.164)			
D .All metro counties	0.175***	-0.112	0.131***	-0.005			
	(0.037)	(0.073)	(0.023)	(0.098)			
E. All metro counties	0.166***	0.003	0.138***	0.020			Y
	(0.028)	(0.098)	(0.018)	(0.128)			

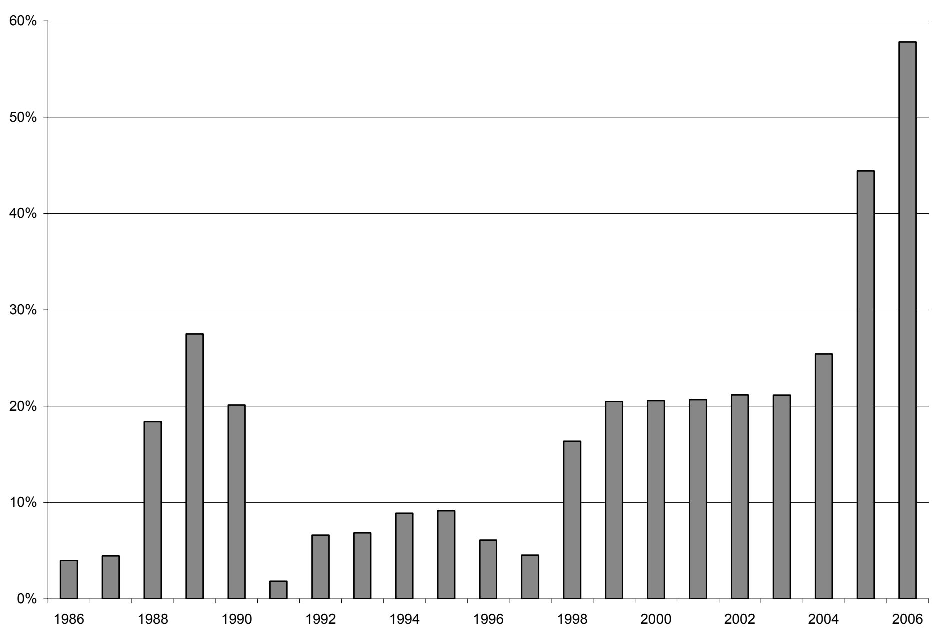
Notes: All specifications include county fixed effects. They also include either a general time period effect or one of three geographically-specific time period effects (census division, MSA, or county-pair) as noted in the table. All specifications include population and overall private sector employment or earnings controls. Sample size for full-service restaurants (specification 1, specification 2 and 3, specification 4, specification 5) = 20,028, 8,052, 93,610, 45,127. Sample size for limited-service restaurants =20,028, 7,524, 105,511, 47,767. 43 states allow employers to pay a sub-minimum wage to tipped employees. States that do not allow a tip credit are: Alaska, California, Minnesota, Montana, Nevada, Oregon, and Washington. Robust standard errors in parentheses are clustered at the border segment-level for specifications A and B and at the state-level for C-E. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Figure 1A **Number of States with Higher Minimum Wages and Average Percent above Federal, 1990-2007**



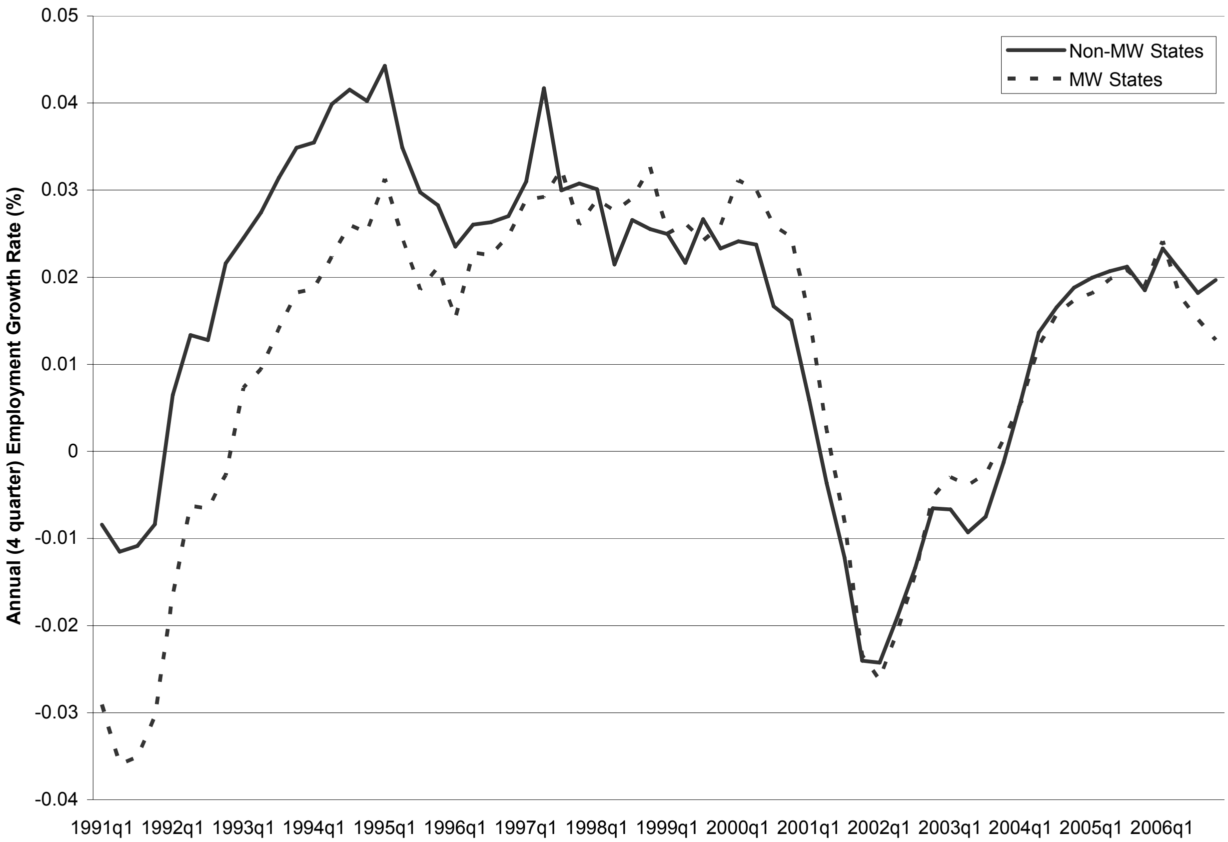
Source: U.S. Department of Labor, Wage and Hours Division.

Figure 1B **Share of the Workforce Residing in States with Higher Minimum Wages, 1986 - 2006.**



Source: U.S. Department of Labor, Wage and Hours Division and Economic Policy Institute.

Figure 2 Annual Employment Growth Rate, Minimum Wage States versus Non-Minimum Wage States



Source: QCEW

Notes: Annual private sector employment growth rates calculated on a four quarter basis (e.g. 1991Q1 is compared to 1990Q1). Minimum wage states are the 17 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.

Figure 3A **Number of County Pairs with Minimum Wage Differential and Average Minimum Wage Differential**

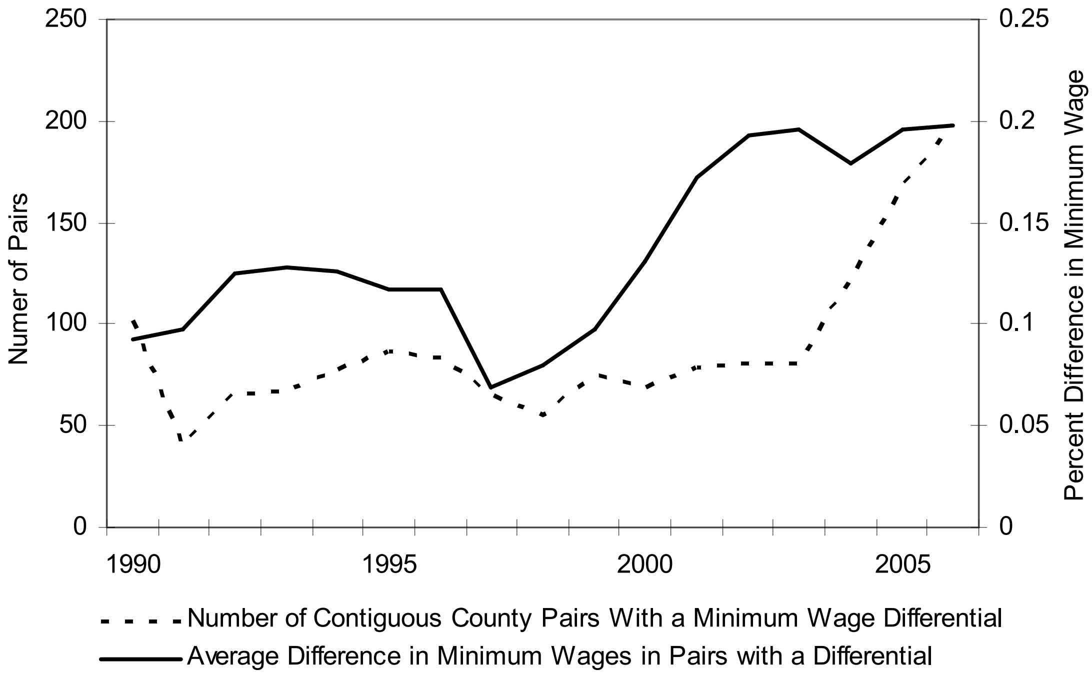


Figure 3B **Number of Counties in Cross-State Metro Areas with Minimum Wage Differential and Average Minimum Wage Differential**

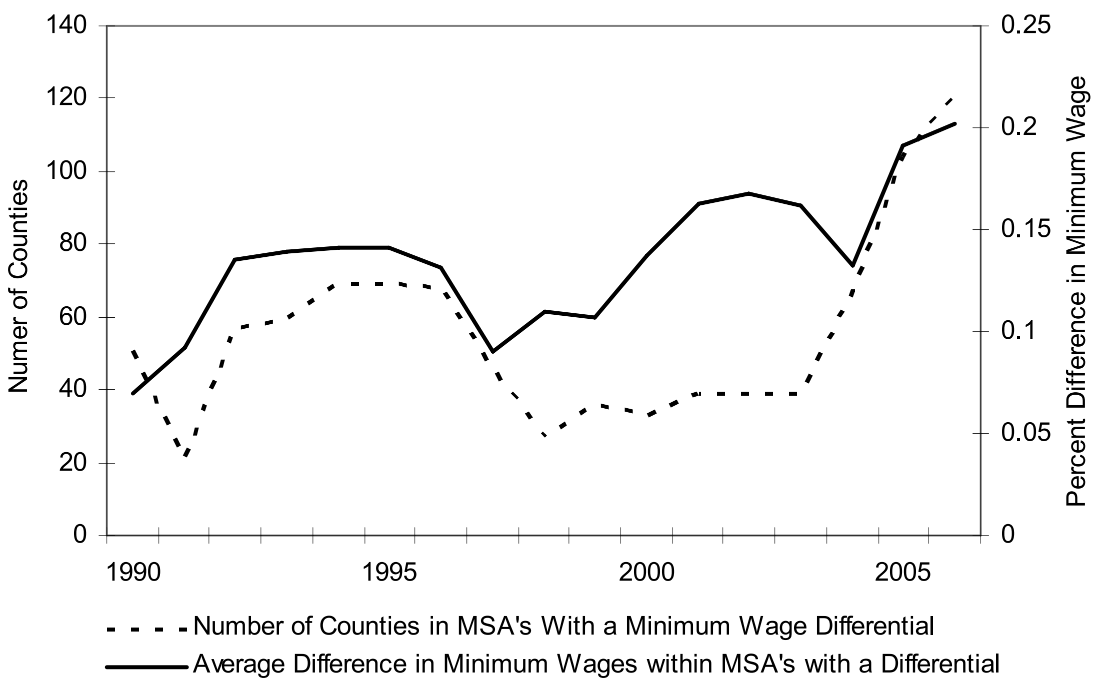
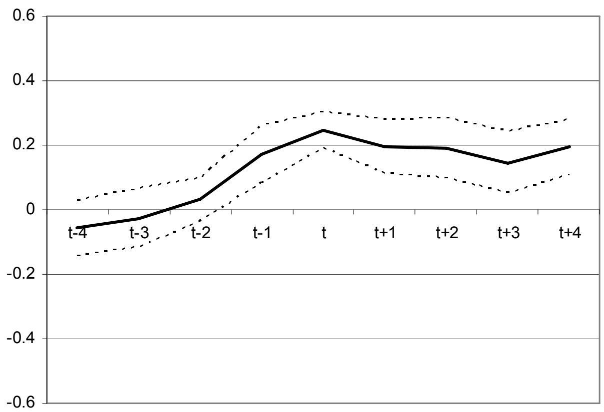
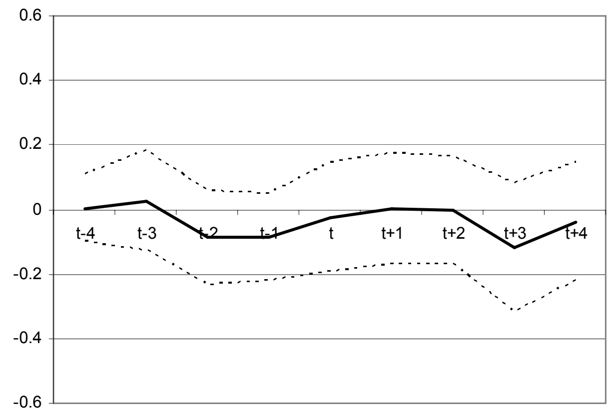


Figure 4 Time Paths of Minimum Wage Effects, by Sample, Semi-Annual Periods

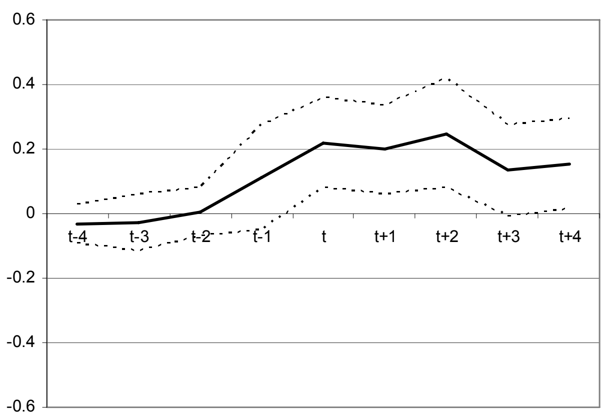
Earnings **Employment**

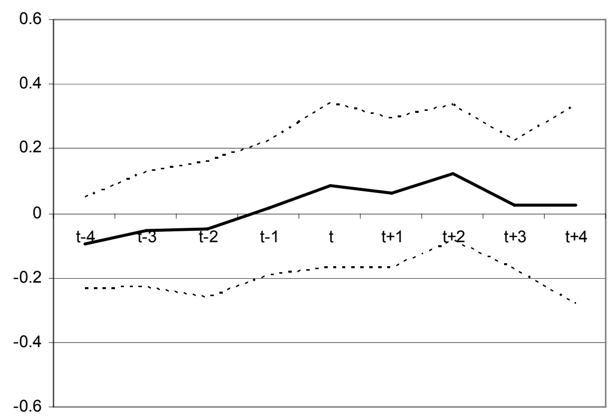
A. All Contiguous Counties



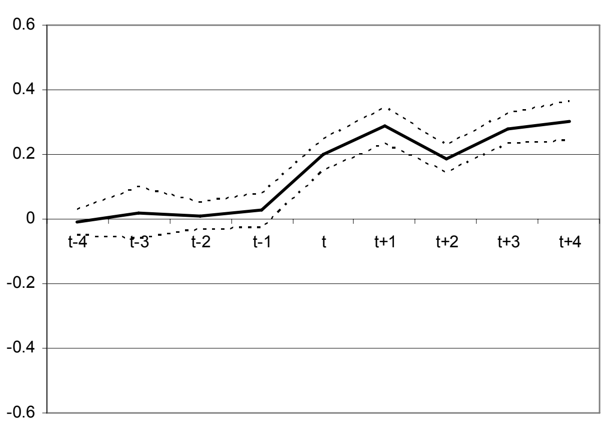


B. All Cross-State Metro Counties





C. All Counties



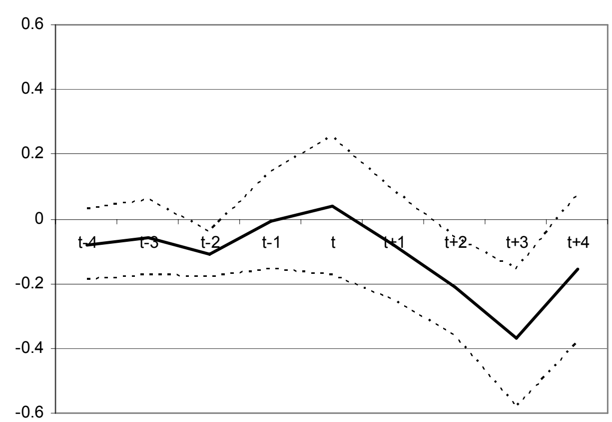
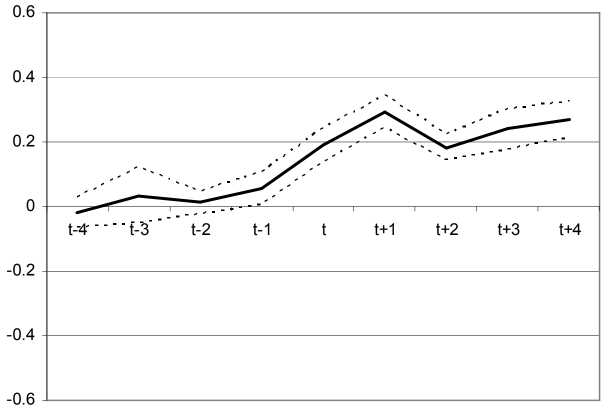
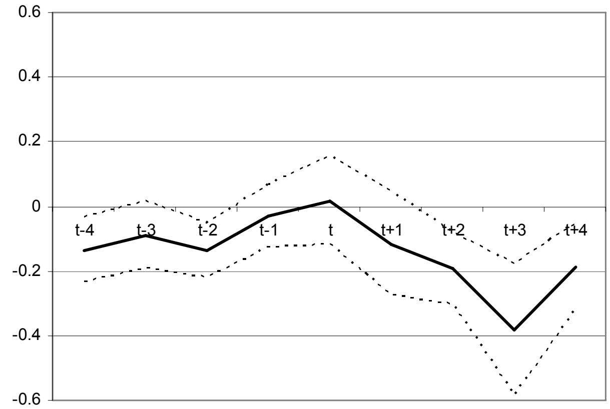


Figure 4. Time Paths of Minimum Wage Effects, by Sample, Semi-Annual Periods (continued)

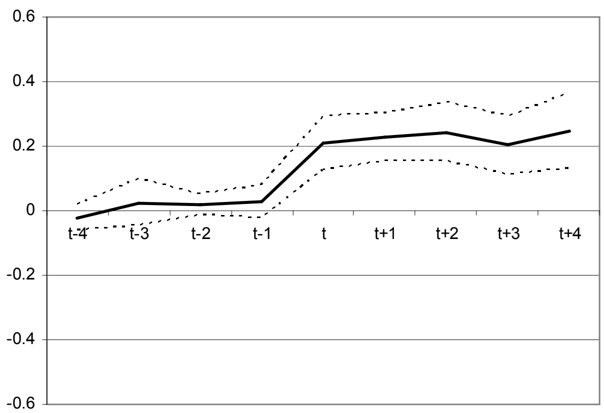
Earnings **Employment**

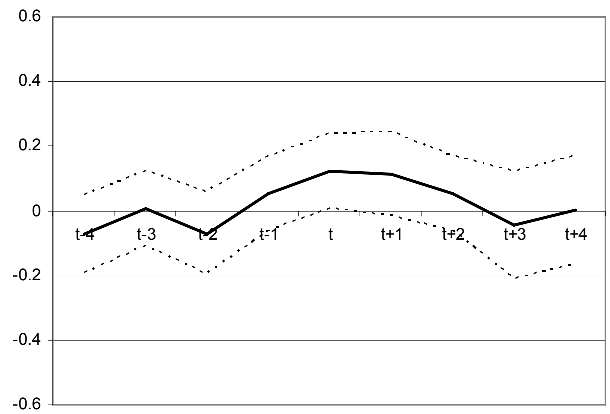
D. All Metro Counties





E. All Metro Counties, with Census Division Controls

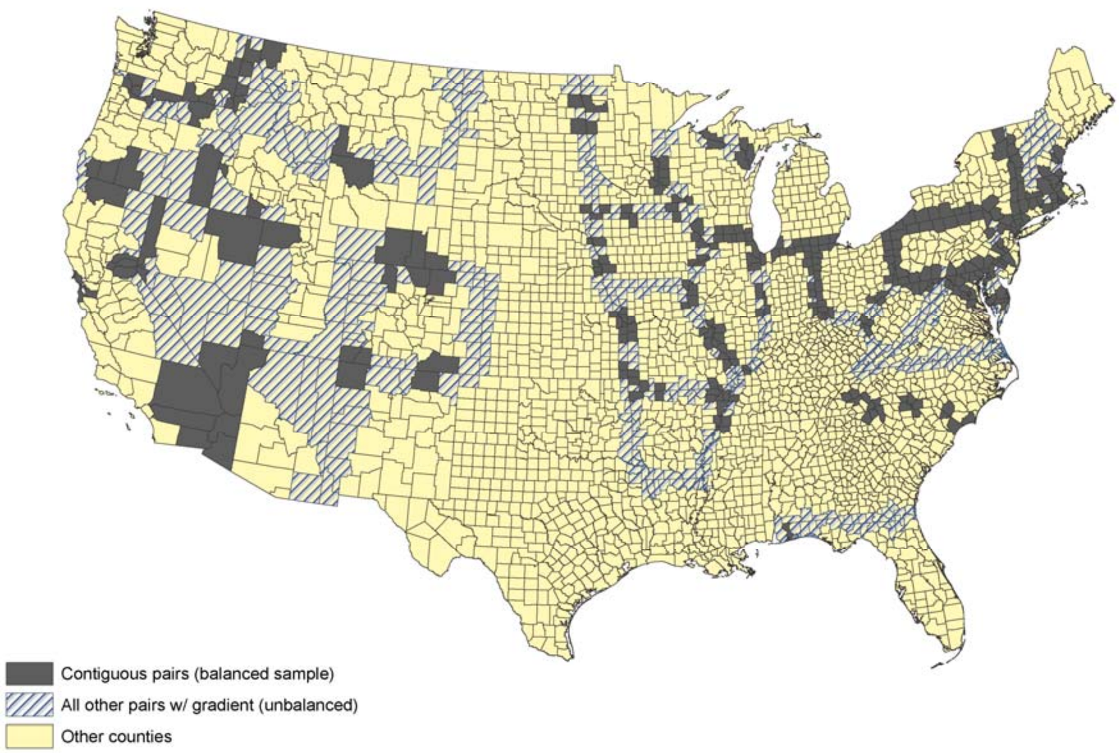




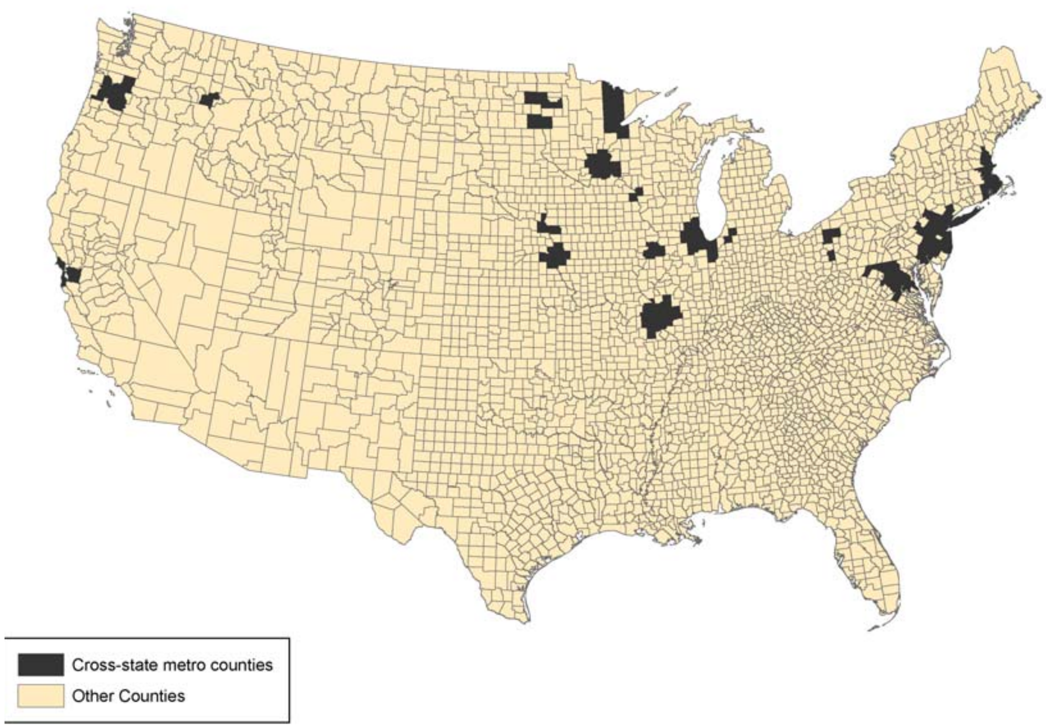
Notes: The figures above plot the cumulative response of minimum wage increases using a distributed lag specification of four leads and four lags. Here time is incremented in 6 month intervals, so *t-4* is two years before the minimum wage increase and t+4 is two years after (the coefficient for t+4 is the sum of all previous leads and lags). Panel A plots the cumulative response for our contiguous counties sample and panel B plots the response for the cross-state MSA sample. Our local specifications include county fixed effects and county-pair specific or MSA-specific period effects, respectively. Panels C through D plot the cumulative response for all counties and all metro counties and include county fixed effects and time period effects. Panel E also plots the cumulative response for all metro counties but includes a census division-specific fixed effect. For all specifications we plot the 90% confidence interval around the estimates in dotted lines. The confidence intervals were calculated using robust standard errors clustered at the border-segment level, for our local estimators (panels A and B) and the state-level for panels C through E.

Appendix A. Maps of County-based subsamples.

(1) **Contiguous County Pairs**



(2) **Cross-state Metropolitan Counties**



Appendix B- List of Counties by Subsample

(1) Counties in Balanced Panel of Contiguous County Pairs

Alabama
Baldwin
Arizona
La Paz
Mohave
Yuma
California
Alameda
El Dorado
Imperial
Marin
Nevada
Placer
Riverside
San Bernardino
San Francisco
San Mateo
Siskiyou
Connecticut
Fairfield
Hartford
Litchfield
New London
Tolland
Delaware
New Castle
Sussex
District of Columbia
DC
Florida
Escambia
Idaho
Bonner
Canyon
Kootenai
Latah
Nez Perce
Illinois
Adams
Boone
Carroll
Clark
Cook
Hancock
Jackson
Jersey
Kankakee

Illinois (cont)
Lake
McHenry
Madison
Massac
Monroe
Randolph
Rock Island
St. Clair
Stephenson
Union
Vermilion
Will
Winnebago
Indiana
Lake
Vermillion
Vigo
Iowa
Dubuque
Emmet
Harrison
Jackson
Kossuth
Lee
Lyon
Muscatine
Pottawattamie
Scott
Winneshiek
Woodbury
Kentucky
McCracken
Maine
York
Maryland
Allegany
Baltimore
Carroll
Cecil
Charles
Dorchester
Frederick
Garrett
Harford
Kent
Montgomery

Maryland (cont)
Prince George's
Washington
Wicomico
Worcester
Massachusett
Berkshire
Bristol
Essex
Hampden
Middlesex
Norfolk
Worcester
Michigan
Dickinson
Gogebic
Menominee
Minnesota
Chisago
Clay
Dakota
Fillmore
Goodhue
Martin
Nobles
Polk
Washington
Winona
Missouri
Cape Girardeau
Jefferson
Marion
Perry
St. Charles
St. Louis
St. Louis City
Nebraska
Dakota
Douglas
Sarpy
Washington
Nevada
Clark
Douglas
Washoe
Carson City

New Hampshire
Hillsborough
Strafford
New Jersey
Bergen
Burlington
Camden
Gloucester
Hudson
Hunterdon
Mercer
Passaic
Sussex
New York
Allegany
Bronx
Broome
Cattaraugus
Chautauqua
Chemung
Clinton
Columbia
Delaware
Dutchess
Essex
New York
Orange
Putnam
Rensselaer
Rockland
Steuben
Sullivan
Tioga
Washington
Westchester
North Dakota
Cass
Grand Forks
Ohio
Ashtabula
Columbiana
Mahoning
Trumbull

Oregon
Columbia
Hood River
Jackson
Josephine
Klamath
Malheur
Multnomah
Umatilla
Pennsylvania
Adams
Beaver
Bedford
Bradford
Bucks
Chester
Crawford
Delaware
Erie
Fayette
Franklin
Greene
Lancaster
Lawrence
McKean
Mercer
Monroe
Philadelphia
Potter
Somerset
Susquehanna
Tioga
Warren
Washington
Wayne
York
Rhode Island
Kent
Newport
Providence
Washington
South Dakota
Minnehaha

Vermont
Addison
Bennington
Chittenden
Rutland
Virginia
Arlington
Fairfax
Loudoun
Alexandria
Washington
Asotin
Benton
Clark
Cowlitz
Klickitat
Spokane
Walla Walla
Whitman
West Virginia
Berkeley
Brooke
Hancock
Jefferson
Marshall
Mineral
Monongalia
Ohio
Preston
Wisconsin
Grant
Green
Kenosha
La Crosse
Marinette
Pierce
Polk
Rock
St. Croix
Vilas
Walworth

(2) List of Counties in Balanced Cross-State Metro Subsample

Allentown-Bethlehem-Easton, PA-NJ[33]
Carbon, PA
Lehigh, PA
Northampton, PA
Boston-Cambridge-Quincy, MA-NH
Essex, MA
Middlesex, MA
Norfolk, MA
Plymouth, MA
Suffolk, MA
Strafford, NH
Chicago-Naperville-Joliet, IL-IN-WI
Cook, IL
DeKalb, IL
DuPage, IL
Grundy, IL
Kane, IL
Kendall, IL
Lake, IL
McHenry, IL
Will, IL
Jasper, IN
Lake, IN
Porter, IN
Kenosha, WI
Cumberland, MD-WV
Allegany, MD
Mineral, WV
Davenport-Moline-Rock Island, IA-IL
Henry, IL
Rock Island, IL
Scott, IA
Duluth, MN-WI [35]
Carlton, MN
St. Louis, MN
Fargo, ND-MN
Clay, MN
Cass, ND
Grand Forks, ND-MN
Polk, MN
Grand Forks, ND
Hagerstown-Martinsburg, MD-WV
Washington, MD
Berkeley, WV
La Crosse, WI-MN[34]
La Crosse, WI
Lewiston, ID-WA
Nez Perce, ID
Asotin, WA
Minneapolis-St. Paul, MN-WI
Anoka, MD
Carver, MD
Chisago, MD
Dakota, MD
Hennepin, MD
Isanti, MD
Ramsey, MD
Scott, MD
Sherburne, MD
Washington, MN
Wright, MD
Pierce, WI
St. Croix, WI
New York-Northern NJ, NY-NJ-PA
Bergen, NJ
Essex, NJ
Hudson, NJ
Hunterdon, NJ
Middlesex, NJ
Monmouth, NJ
Morris, NJ
Ocean, NJ
Passaic, NJ
Somerset, NJ
Sussex, NJ
Union, NJ
Bronx, NY
Kings, NY
Nassau, NY
New York, NY
Putnam, NY
Queens, NY
Richmond, NY
Rockland, NY
Suffolk, NY
Westchester, NY

[33] Warren County, NJ did not have a full balanced panel of observations. This results in the whole metro area being dropped from the main regression analysis. We list it here because it is appears in the unbalanced panel estimates.
[34] Houston County, MN did not have a full balanced panel of observations.
[35] Douglas County, WI did not have a full balanced panel of observations.

2) List of Counties in Balanced Cross-State Metro Subsample (continued)

Omaha-Council Bluffs, NE-IA
- Harrison, IA
- Pottawattamie, IA
- Douglas, NB
- Sarpy, NB
- Saunders, NB
- Washington, NB

Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- New Castle, DE
- Cecil, MD
- Burlington, NJ
- Camden, NJ
- Gloucester, NJ
- Bucks, PA
- Chester, PA
- Delaware, PA
- Montgomery, PA
- Philadelphia, PA

Portland-Vancouver-Beaverton, OR-WA
- Clackamas, OR
- Columbia, OR
- Multnomah, OR
- Washington, OR
- Yamhill, OR
- Clark, WA

Providence-New Bedford-Fall River, RI-MA
- Bristol, MA
- Kent, RI
- Newport, RI
- Providence, RI
- Washington, RI

San Francisco-Oakland-Fremont, CA
- Alameda, CA
- Contra Costa, CA
- Marin, CA
- San Francisco, CA[36]
- San Mateo, CA

Sioux City, IA-NE-SD
- Woodbury, IA
- Dakota, NB

South Bend-Mishawaka, IN-MI[37]
- St. Joseph, IN

St. Louis, MO-IL
- Clinton, IL
- Jersey, IL
- Macoupin, IL
- Madison, IL
- Monroe, IL
- St. Clair, IL
- Franklin, MO
- Jefferson, MO
- Lincoln, MO
- St. Charles, MO
- St. Louis, MO
- St. Louis City, MO

Washington-Arlington-Alexandria, DC-VA-MD-WV
- District of Columbia
- Calvert, MD
- Charles, MD
- Frederick, MD
- Montgomery, MD
- Prince George's, MD
- Arlington, VA
- Fairfax, VA
- Loudoun, VA
- Alexandria, VA
- Jefferson, WV

Weirton-Steubenville, WV-OH
- Jefferson, OH
- Brooke, WV
- Hancock, WV

Youngstown-Warren-Boardman, OH-PA
- Mercer, PA
- Mahoning, OH
- Trumbull, OH

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[36] Minimum wage variation within the MSA stems from San Francisco County's minimum-wage in 2004.
[37] Cass County, MI did not have a full balanced panel of observations. This results in the whole metro area being dropped from the main regression analysis. We list it here because it is appears in the unbalanced panel estimates.