MINIMUM WAGE EFFECTS ACROSS STATE BORDERS: ESTIMATES USING CONTIGUOUS COUNTIES[†]

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Abstract

Local case studies of minimum wages typically find no significant employment effects, while studies using national data find some negative effects for teenagers. We argue that heterogeneity in spatial employment trends generates biased estimates in national analyses and causes overstatement of precision in local and national studies. We propose two new local estimators that compare all contiguous counties or metro areas in the U.S. that straddle a state-based minimum wage gradient. We find that the negative elasticities in national fixed-effects models are generated by unobserved heterogeneities in employment trends. Our local estimators are more robust and show no employment effects.

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1. Introduction

Since the mid1980s, 32 states and four cities have implemented laws mandating minimum wages at higher than the federal level. Although the federal minimum wage increases in 1990-91 and in 1996-97 eliminated most of the extant federal-state differentials, since 1997 a new pattern has emerged (Figures 1A and 1B). State policies already in place in 2007 indicate that 27 states will continue to exceed the federal level of $5.85 per hour that was implemented in July 2007 and that at least 12 states will exceed the federal level of $7.25 that is scheduled for July 2009. In another new development, four U.S. cities (Albuquerque, San Francisco, Santa Fe and Washington, D.C.) have implemented citywide minimum wage floors and a number of others are considering such a policy. Clearly, a pattern of differential minimum wage rates has developed in the U.S. and it has become an enduring feature of U.S. labor markets.

These evolving patterns of minimum wage policy provide 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. To advance this literature we present in this paper a new method for estimating minimum wage effects. Our estimates are more accurate and precise than those of previous studies and they can explain the sources of the different findings in previous research.

Since the 1990s, minimum wage studies have been based primarily on either national or local comparisons. National studies draw upon household-based panel data and essentially 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—fast food restaurants—to compare local areas with different minimum or average wages (NJ vs. PA, within Texas).

Since the incidence of minimum wage laws and the proportional share of the workforce are similar for teenagers and restaurant workers, the estimated elasticities in the national and the local studies should also be similar.[1] But that is not the case. Studies using national-level panel data obtained point estimates of -0.1 to -0.3, with results sensitive to specifications and control groups. The local case studies failed to find significant negative employment effects, even though the minimum wage elasticity of employment should be more negative when employers need only move across a county line in response to the minimum wage. However, the local case study results may have been affected by spatial autocorrelation that compromised the precision of the estimated treatment effects, making them less detectable.

More recent studies, although utilizing improved controls, indicate that the two approaches continue to generate contrasting findings. For example, Neumark and Wascher (2007) use national panel data and report statistically significant negative employment effects, although mainly among minority teenagers, and with considerable sensitivity to specification. In contrast, in their study of the 2004 $8.50 citywide minimum wage in San Francisco, Dube, Naidu and Reich (2007) find the minimum wage employment elasticity was indistinguishable from zero for restaurant workers.

We seek to reconcile these conflicting findings within this literature and to adjudicate among them. Using county-level data on restaurant earnings and employment from the Quarterly Census of Employment and Wages for 1990 to 2006, we address

[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).

whether the inconsistent findings result from differences in controls for unobserved heterogeneity, differences in the affected group under consideration (teenagers versus restaurant workers), or differences in the length of the time window used in the analyses. We present and apply two variants of a new identification method that generates improved estimates of minimum wage effects and that accounts for the differences between the existing national and local studies. Our primary innovation involves comparisons among 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 areas that exhibited a difference in minimum wage across their component counties.

This method generalizes the case study approach by utilizing all local differences in minimum wages over sixteen and a half years, and it also permits comparisons with national-level panel studies within a common framework. Our comparisons reduce biases in the estimated treatment effect that result from spatial trends in employment that are correlated with minimum wages, an issue that is not adequately addressed by existing national studies. We also show that when such spatial trends exist, they cause an overstatement of precision in both the national fixed-effects estimates and the local case study estimates, and by amounts that would eliminate the statistical significance of coefficients in many previous minimum wage studies. By pooling local comparisons, and allowing for spatially correlated innovations in employment, we address the dual problems of omitted variables bias and the bias in the estimated standard errors.

The chronology of minimum wages in the U.S. that we summarize below implies that many state border wage gradients both increased and decreased during our time period. Consequently, many of our counties become part of the treatment group for some minimum wage events and part of the control group for others. This feature further improves our confidence that our treatment and control groups make up very close comparison sets. We show empirically that employment trends are indeed more similar in nearby counties.

Using county-level administrative payroll data, which are relatively free of measurement error, we first estimate minimum wage effects on earnings and employment using balanced national samples of all counties and all counties that are located in metropolitan areas. This specification allows us to benchmark with previous national studies. We refer to these two specifications as “fixed-effects” estimators.[2] We then estimate the effects using only variation in minimum wages within the nine census divisions, which controls for broad spatial heterogeneity. Finally, our local estimators compare counties that are either contiguous or located in the same metropolitan area but have different minimum wages.

Our key findings are as follows. All our specifications show strong and similar effects of minimum wages on restaurant workers’ earnings. Fixed-effects estimators produce negative employment elasticities of -0.15 or greater. We show, however, that spatial heterogeneity in employment growth is strongly correlated with minimum wages; this heterogeneity confounds fixed-effects estimates that use all national-level variation. In contrast, local estimators that compare contiguous counties or counties located within a cross-state metropolitan area produce employment effects that are indistinguishable from zero. We can rule out elasticities greater than -0.15 at the 90 percent confidence

[2] These estimators use the full variation in minimum wages across the country, and have no time-varying controls. In contrast, other estimators we present use only more localized variation in minimum wages and have some form of time-varying controls for each geographic region.

level. Even within-census division estimates (an intermediate specification) produces elasticities close to zero.

We check our results through a falsification test that uses only counties that *never* had any state or local minimum wage policies (and hence always had *identical* minimum wages). We show that between-census division variation in minimum wages produces large negative elasticities for this sample of counties. This result demonstrates the presence of spatial trends at the regional level that are correlated with minimum wage policies. Consistent with this result, we find that the fixed-effects estimators are very sensitive to the inclusion of state-level linear employment trends, which cause the employment elasticities to change signs and become positive. In contrast, our local estimators, using contiguous counties and cross-state metro areas, are robust to the inclusion of such state-level linear trends, thereby providing internal validation for the local estimators.

We show that the differences between the local estimates and the national fixed-effects estimates are not due to anticipation effects or lagged effects. We confirm this result with evidence by plotting the dynamic response to minimum wages using distributed lags. We also show that there is spatial and serial correlation in the residual in both the fixed-effects and local time-varying models, indicating that individual observations are not stochastically independent. We show that ignoring such autocorrelation understates standard errors by a factor of between three and twelve. Consequently, the precision of both local and fixed-effects estimators in previous studies has been substantially overstated. Finally, we conduct a series of robustness tests and find that they do no affect our results.

The rest of this paper is organized as follows. Section 2 describes the chronology of minimum wages across the United States in the past three decades. Section 3 reviews the extant literature briefly, with a focus on identification assumptions. Section 4 presents our regression specifications, followed by a discussion of our data and sample construction in Section 5. Results are presented in Section 6, and Section 7 concludes the paper.

2. Minimum Wage Developments, 1981 to 2009[3]

The federal minimum wage remained unchanged at $3.35 from January 1981 to April 1990, when it increased to $3.80. In 1985, Alaska, Connecticut and Massachusetts were the first states to institute a higher minimum wage and by 1989, 15 states (and the District of Columbia) had raised their minimum wage floors above $3.35 (see Figure 1A). In this period the highest state wage exceeded the federal level by 27 percent. After the federal minimum wage increased to $4.25 in April 1991, only three states (and DC) had higher floors. By September 1996, 10 states (and DC) had floors above the federal level and the highest state wage exceeded the federal level by 24 percent. The federal level then increased to $4.75 in October 1996, leaving only four states (and DC) with higher floors. By August of 1997 an additional 6 states raised their floor, in most cases to the $5.15 federal level that was scheduled to go into effect in September 1997. At that time, only three states and DC had floors above $5.15.

The pattern of state-federal differences from 1981 to 1997 was thus one of alternating state and federal increases. A substantial number of states increased their floors above the federal level and then federal increases initially eliminated most of the

[3] See Appendix A for the complete history of state and federal minimum wages.

state differentials. After a pause, the states again began to institute higher floors and then another federal increase again reduced the number of states with higher floors.

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 cases. 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 less concentrated in a few regions of the country. Looking ahead, the federal increase to $5.85 in July 2007 will leave 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 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.[4]

3. Related Literature

To evaluate the impacts of the minimum wage policies, recent research typically uses one of two methods. One method involves national-level panel data from the CPS and uses cross-state variation in minimum wages to identify employment effects. These studies tend to focus on employment effects among teenagers, who disproportionately are

[4] We do not use Santa Fe, as the minimum wage applies only to the City of Santa Fe. Santa Fe County contains other towns as well. We do not include Albuquerque because its minimum wage went into affect after the second quarter of 2006, the last period of our sample.

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 find a minimum wage elasticity for teens of -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 and include state-level linear trends, which their specification tests find cannot be excluded. They obtain mixed results, with negative effects only for minority teenagers.

One problem with these national panel studies concerns whether they control adequately for heterogeneity in employment growth. A state fixed effect will control for level differences between states, but as Table 2 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. This regional heterogeneity could impart an omitted variable bias toward finding negative effects of minimum wages in estimates using national panel data with state effects. Moreover, including state-level linear trends (as in Neumark and Wascher 2007) is not a panacea, since the estimated trends may themselves be affected by minimum wages.

Card (1992) uses regional wage variation to identify federal minimum wage employment effects, on the hypothesis that the federal increases will have a bigger impact in the lower wage states than in higher wage states. As he reports, the higher wage states are concentrated in New England and the Pacific, while the lower wage states are concentrated in the South and the Mountain region. Card finds that changes in teenage employment occur at the same rate in the low-wage states as in the high-wage states, controlling for regional business cycle differences and changes in educational enrollment.[5]

Card's regional method does not take into account the fact that the lower-wage states may have underlying population and employment *growth* rates that are greater than in the higher-wage states. Comparisons across regions thus will underestimate adverse effects of federal minimum wage increases.

Another group of minimum wage studies conduct local case studies, typically using restaurant data obtained from employers. The restaurant industry is of special interest because it is the most intensive user of minimum wage workers. Studies focusing on the restaurant industry are also easily compared to studies of teenage employment, as the incidence of minimum wage workers is similar among both groups.

Card and Krueger (1994, 2000) use local case studies of fast-food restaurant chains in contiguous states (NJ and PA) to obtain fine-grained comparisons. Using administrative payroll panel data, Card and Krueger do not detect any significant effects of the 1992 NJ statewide minimum wage increase on restaurant employment. Moreover,

[5] Card and Krueger (1995, ch. 5) provides an updated and expanded account, with similar results.

they obtain similar findings when the 1996-97 federal increases eliminated the NJ-PA differential.[6]

A more recent study in the same vein (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 increase in 2003. In this case, the minimum wage increased from the statewide level of $6.75 to $8.50, or 25.9 percent, with further increases indexed annually to local inflation. In comparison to Card and Krueger (1994, 2000), Dube, Naidu and Reich include full service and limited service restaurants as well as chains and independents in their sampling frame. They also collect data on hours, benefits and restaurant size, as well as on such outcomes as worker turnover and price increases, and they use zip code location data to examine the impacts of tourism and concentrations of immigrant workers.

Dube, Naidu and Reich do not find any significant effects of the minimum wage increase upon employment. 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. Relative to Card and Krueger (1994, 2000), their data exhibit considerably less measurement error and their confidence intervals are tighter. Consequently, they are able to reject some of the negative employment effects in the national studies. However, and as with the other case study literature, their data contain a limited before-after window.

[6] 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; and Neumark and Wascher 2000 for NJ-PA); Except for Neumark and Wascher, these studies also fail to find significant disemployment effects. A study by Powers et al (2007) of fast-food chains near the Indiana-Illinois border is inconclusive on the question of employment effects; the authors emphasize concerns of measurement error in their data which limits the conclusions that one can draw.

Consequently, they cannot address whether minimum wage effects occur with a longer lag.

More important, case study methods that compare a small number of neighboring locations are susceptible to overstating the precision of the estimates of the minimum wage effect. Indeed, whenever spatial heterogeneity contaminates the fixed-effects models due to omitted variables bias, it is also likely to affect local case studies. In this situation, the error terms of establishments in individual locales are unlikely to be statistically independent and the standard errors will be biased downwards.

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 fixed-effects estimates, the time period in question, 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— the insufficient attention to spatial trends that contaminate the existing estimates that use national variation.

4. Identification Strategy

The identification strategy we use exploits the fact that a state-level treatment is applied to only one part of a common labor market area. Since unobserved heterogeneity in economic activity is likely to be reduced when we consider a contiguous county pair or a metropolitan area, we come closer to "quasi-experimental" conditions than extant studies using national variation. Additionally, by pooling across such pairs or metropolitan areas, we can also account for spatial and temporal autocorrelation that affect local case-study estimates.

First, we estimate "baseline" wage and employment effects using the national sample and including state and time fixed effects, in the mode of Neumark and Wascher (1992). However, we also account for serial correlation, which was largely unaddressed in national studies. We report the results for the full set of counties as well as for metropolitan counties (our first and second specifications). In the third specification we modify the fixed-effects estimators to examine the effect of spatial heterogeneity in employment growth. Here we allow time-varying effects within each of the nine U.S. census divisions[7], using only within-division variation in minimum wages to identify the treatment effect. This specification is "intermediate" in the sense that it stands between approaches using the full national-level variation and those using local differences in minimum wages. In our fourth specification, we include in the sample only the metro areas that cross at least one state line *and* for which at least one state has increased its minimum wage over the federal level; here we also allow time-varying effects for each of the metro areas, and hence use only within-metro policy variation. The fifth specification is the most localized; it examines variation within each of the contiguous cross-state county pairs (metropolitan or otherwise) with a minimum wage gradient. We estimate our benchmark model with all counties and only metropolitan counties separately, as these provide natural comparison points for our cross-state county pair and metro area estimates, respectively. In the following discussion we refer to specifications 1 and 2 as

[7] The U.S. Census Bureau groups states into the following nine divisions: New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific.

our “fixed-effects” models, specification 3 as the “within-census division” model, and specifications 4 and 5 as “local” models.

Specifications for Estimating Minimum Wage Effects[8]

Labor demand for restaurant workers in a county *i* and a time *t* can be represented as:

$$(1.1)\quad \ln l_{it}(w_{it}^{M}, \mathbf{w}_{it}) = a_{it} + b\ln(w_{it}^{M}) + c\ln(\mathbf{w}_{it}) + e_{it}$$

$$= a_{it} + b\ln(w_{it}^{M}) + c\ln(\mathbf{w}_{it}) + \left(\phi_{it} + \varepsilon_{it}\right)$$

Labor demand is a function of both the minimum wage, w_{it}^{M}, as well as a vector of wages of other relevant workers $\mathbf{w}_{it}$ in the labor market at a given time, and an error term e_{it}. The error term can be further decomposed into a component ϕ_{it} that is correlated with the minimum wage variable, and a true error term ε_{it} that is independent of the regressors. The problem in estimating equation (1.1) is that generally $\text{cov}(w_{it}^{M}, e_{it}) \neq 0$. Moreover, it is difficult to observe the set of wages for relevant workers facing firms at the time, and to find other instruments to estimate them consistently.

The standard approach to dealing with this problem has been to use a fixed-effects strategy and to estimate the following model:

$$(1.2)\quad \ln l_{it} = a_{it} + b\ln(w_{it}^{M}) + \phi_{i} + \eta_{t} + \hat{e}_{it}$$

Equation 1.2 provides our benchmark fixed-effects estimates. We estimate it both for all counties and for metropolitan counties only. The county fixed effect and the time dummies are thought to control for local differences and overall labor market conditions. But this assumption is problematic. The variation in minimum wages used for

[8] Although our discussion in this section refers to log of employment as the dependent variable, we also estimate parallel regressions for log of average weekly earnings for each specification.

identification comes either explicitly or implicitly from the differential movement of federal and local (state or municipal) minimum wages. But heterogeneous employment growth may be correlated with changes in minimum wages, creating an omitted variables bias in the fixed-effects model.[9]

Overcoming Omitted Variables Bias

Our approach addresses omitted variables bias by using local variation in minimum wages to estimate the treatment effect. The motivation behind this strategy is that the unobserved trends tend to be spatially correlated because economic activity is itself spatially correlated. Hence, local comparisons can address the omitted variables bias. Moreover, employment trends may also be time varying— as is indeed suggested by an inspection of regional growth in Table 2. In the presence of such time-varying trends, there is no real substitute for good control groups.

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. We estimate:

$$\ln l_{ipt} = a_{it} + b\ln(w_{it}^{M}) + \phi_i + \eta_{pt} + \hat{e}_{it} \quad (1.3)$$

Here *p* subscripts each county pair, and η_{pt} represents an arbitrary pair-period effect. Effectively, we are only exploiting the variation in minimum wages within each county pair. Our identifying assumption is $\mathrm{cov}(w_{it}^{M}, \hat{e}_{it}) = 0$, i.e., the minimum wage

[9] The regional heterogeneity of employment growth mentioned above provides one example of possible sources of omitted variable bias in the fixed-effects model. Others that are mentioned in the literature include 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. Many of these are likely to have a spatial component, and are better controlled for using local comparisons.

within the pair is uncorrelated with the residual employment in either county. This assumption is much less stringent than the one imposed by our fixed-effects model (equation 1.2), which only includes county and time fixed effects. Instead, equation 1.3 allows for arbitrary time-varying trends within each county pair in residual employment in the restaurant industry. Our observations are at the county level, but a county can have multiple observations for each quarter, since it can be part of several cross-state pairs. Consequently, we weight each county-level observation by *1/P* where *P* is the number of pairs to which a county belongs.

As a second approach, we use Metropolitan Statistical Areas that cross state boundaries to identify the minimum wage effect. The equation we estimate is:

(1.4) $$\ln l_{imt} = a_{it} + b\ln(w_{it}^{M}) + \phi_i + \eta_{mt} + \hat{e}_{it}$$

Here η_{mt} is a metro-specific time effect that captures local labor market conditions that may vary over time and place. Controlling for arbitrary trends within a metro area also means that the estimation sample consists only of counties that are part of cross-state MSAs with a minimum wage gradient.

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

(1.5) $$\ln l_{ict} = a_{it} + b\ln(w_{it}^{M}) + \phi_i + \eta_{ct} + \hat{e}_{it}$$

The inclusion of η_{ct} sweeps out the between-census division variation and estimates are based only on the variation within each census division. This specification is less likely to control for all relevant unobserved trends, and hence is less preferable to specifications (1.3) or (1.4) on grounds of identification. However, it does provide a sense of the level of geographical aggregation at which the unobserved trends contaminate the estimated elasticity.

Neumark and Wascher (1992) argue that the minimum wage effects on employment occur with a year lag. To test for such delayed effects, we also estimate each of these specifications with a four quarter (one year) lagged minimum wage in place of the contemporaneous minimum wage.

Inclusion of State-level Linear Trends

We compare our local estimators to the national estimators by testing whether the resulting elasticities are robust when we include state-specific linear trends. The inclusion of linear trends may attenuate the true effect of the minimum wage, as the policy may causally affect 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[10], we cannot identify the effect of minimum wages without assumptions about the functional form of the trend. For this reason, our preferred estimators control for unobserved trends by only using local variation. This method thus decouples the estimation of such trends from the estimation of minimum wage effects.

Although including linear time trends may be problematic as a way to address omitted variables bias, comparing how the different estimators respond to their inclusion provides a one-sided test of their internal validity. 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, and (2) there are no such

[10] Technically, this statement is too strong, as the few cities that have minimum wages provide variation within states; but this is a minute part of the national sample.

unobserved trends, but including the linear trend attenuates the minimum wage coefficient.

If, however, the inclusion of state-level linear trends does not change the minimum wage coefficient, we have strong evidence that the model controls for unobserved trends. This result will obtain insofar as the true effect is small in magnitude, and we have sufficient controls for heterogeneity in latent employment growth.

Falsification test using placebo minimum wages

To highlight the possible omitted variables bias in the fixed-effects estimates, we also perform a falsification test. We estimate the effect of placebo minimum wages on restaurant employment for counties in states that *never had a minimum wage other than the federal one (*i.e., the "placebo sample"). This sample has no cross-sectional variation in minimum wage. We define the placebo minimum wage as the mean minimum wage in each period in the nine census divisions. Specifically, for county *i* in census division c' at time t', the placebo is computed as the simple mean of minimum wages in c' at time t' across all counties *j* in c':

$$w_{ic't'}^{M,CF} = E(w_{jct}^{M} \mid c = c', t = t').$$

We then estimate (1.2) using this hypothetical minimum wage for all counties in the "placebo sample." This exercise provides a falsification test, as the coefficient from this regression should certainly be zero. To the extent it is not zero, we have evidence of the existence of spatial heterogeneity in employment that is correlated with minimum wage changes.

Stability of the Coefficients

Another criterion for comparing estimators concerns parameter stability over time. We estimate all our models for an early period (1990-1998) and a late period (1999-2006). The early period is comprised of two federal minimum wage increases and some state-level increases. The late period is comprised of a large number of state-level increases. We are interested in how the estimates from each model vary over the two periods, and how they compare to the pooled estimates.

Time Paths of Minimum Wage Effects

For each of our five primary specifications, we also estimate models with distributed lags, allowing us to plot the time path of any minimum wage effects. These time paths depict visually the evolution of employment around the times of changes in the minimum wage. We use four leads, four lags and the contemporaneous minimum wage, covering a total window of nine quarters. In other words, each lead or lag is the average minimum wage during a six month period.[11]

$$
\begin{aligned}
\ln l_{it} = a_{it} + \sum_{\tau=-4}^{-1} b_\tau \left(\frac{\ln(w^M_{i,(2\tau-1)}) + \ln(w^M_{i,(2\tau)})}{2} \right) + b_0 \ln(w^M_{it}) + \\
\sum_{\tau=1}^{4} b_\tau \left(\frac{\ln(w^M_{i,(2\tau-1)}) + \ln(w^M_{i,(2\tau)})}{2} \right) + \phi_i + \eta_{kt} + \hat{e}_{it}
\end{aligned}
\tag{1.6}
$$

Here, η_{kt} represents the relevant spatially-varying time effects depending on the specification. The time-path is calculated as the cumulative sum of leads and lags; i.e., the time-path at time θ is $\sum_{\tau=-4}^{\theta} b_\tau$, where θ is between -4 and 4, and where θ=4 represents

[11] We average across two quarters (i.e., six-month intervals) to reduce the number of coefficients estimated for the time paths. Quarterly time paths look essentially the same, although the standard errors are somewhat larger.

two years after a minimum wage increase. We also compute the 90% confidence bounds around the time path.

Other Robustness Checks

We perform a number of robustness checks for our preferred local estimators to ensure that our findings are not generated by other factors. To examine whether employment effects only appear with longer lags, we extend the window. Thus, we report cumulative effects over 12 quarters for both of the local specifications using three years of lags in ln minimum wages.

$$(1.7) \quad \ln l_{it} = a_{it} + \sum_{\tau=0}^{3} b_{\tau} \ln(w^{M}_{i,(4\tau)}) + \phi_i + \eta_{kt} + \hat{e}_{it}$$

The cumulative effect after three years here is calculated as $\sum_{\tau=0}^{3} b_{\tau}$.

We also conduct two other robustness tests. One examines the effects of including counties that do not have a full set of employment data over the whole period (due to non-disclosure issues). The other examines the effect of excluding geographically large counties, in the case of contiguous county pairs. We discuss the exact changes in the samples for these two specifications in the results section.

Spatial and Temporal Autocorrelation and Standard Errors

Thus far, we have focused on how the elasticity coefficient can be biased if unobserved spatial heterogeneity in employment patterns is correlated with the differential evolution of minimum wages across regions. This potential bias constitutes our key criticism of most of the fixed effects models in the literature. Yet the issue of spatial heterogeneity in employment patterns also has implications for the consistency of the standard errors. At the most general level, the problem arises from the non-independence of the error term across observations—both temporally and spatially. This concern is the now well-known “Moulton problem” (see Moulton 1990). It can be represented as:

$$\exists\ i \neq i' \text{ and/or } t \neq t' \text{ such that } \operatorname{cov}(e_{it}, e_{i't'}) \neq 0 \ .$$

This represents two potential types of autocorrelation of the error term – spatial and temporal. Such autocorrelation is particularly problematic when the correlation in the residuals is accompanied by a parallel correlation in the treatment variable across the same observations (Petersen 2007). Minimum wages are fully correlated within areas or jurisdictions deciding wage policies. Moreover, within any such jurisdiction, there is serial correlation in both employment and in minimum wages.

To date, few papers in the minimum wage literature have fully addressed this problem. The case studies in the literature (Card and Krueger 2000; Neumark and Wascher 2000; Dube, Naidu and Reich 2007) do not suffer from the serial correlation problem as they consider only two time periods (see Bertrand et al 2004). They do suffer, however, from treating individual firm level observations as being independent (i.e., spatial autocorrelation). In contrast, the state-panel type analyses are less susceptible to spatial autocorrelation, as they aggregate the data at state-period level. However, much of this literature (Neumark and Wascher 1992; Burkhauser et al 2000) contains inadequate corrections for serial (temporal) correlation of the error terms. Either way, the standard errors are downwardly biased, making inferences less precise than suggested.[12]

[12] In their most recent work, Neumark and Wascher (2007) adjust their standard errors for clustering at the state level. Relative to their previous studies, their coefficients now have much larger standard errors. These results suggest that many of their previous findings would not pass tests of significance using clustered standard errors.

We address this issue by clustering our standard errors at the level where the treatment variable is determined—i.e., the state. Cluster-corrected standard errors have been shown to perform relatively well when the number of clusters is not too small (Bertrand et. al. 2004; Kezdi 2004). The appropriate unit to cluster the standard error requires an analysis of potential autocorrelation of both the dependent and treatment variable (Petersen 2007). Since our variation comes from comparing cross-state metro units (or just states in the case of the fixed effect specifications), we cluster our standard errors at the state level.

5. Data, Samples and Descriptive Statistics

Data and Sample Construction

Our data come from the *Quarterly Census of Employment and Wages* (QCEW), which provides quarterly county-level payroll data by detailed industry. The data are based upon ES-202 filings that every establishment is required to submit quarterly for the purpose of calculating payroll taxes related to the unemployment insurance (UI) program. The Bureau of Labor Statistics indicates that the reporting rate is 98 percent, making the data a near census of employment and earnings in the county.

BLS aggregates records from each state to publish quarterly employment and payroll totals by the 2002 North American Industry Classification System (NAICS) for each county in the United States. Although 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.

We construct a panel of quarterly observations of county-level employment and earnings. Our dataset includes records 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).

With its larger number of observations and high response rates, the QCEW has the benefit of being highly accurate at small geographic scales, compared to household sample surveys, such as the CPS. This feature is crucial, since our identification strategy rests on comparing contiguous counties, or counties within the same metropolitan area that have different mandated minimum wages.

Our two primary outcome measures are average earnings and 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 to a weekly basis. This earnings measure is thus not a directly observed hourly wage attributable to an individual worker, as in the CPS.[13] Unfortunately, the QCEW does not distinguish between part-time and full time-employment (i.e. there is no measure of 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.[14]

[13] On the other hand, the proportion of CPS respondents with missing hourly or weekly wage data is high, at about 16 percent in 1990. After the 1994 CPS revisions, the proportion of missing reported wages jumped to 26 percent in 1996, and they grew to 36 percent in 2006. Source: Dr. Sylvia Allegretto, Unpublished CPS tabulations, IRLE June 2007.

[14] The QCEW also cannot distinguish between those who were employed during the entire quarter and those who entered or exited employment during the quarter.

We focus our analysis of minimum wage changes on workers in the restaurant industry for five reasons. First, wages in this industry are consistently among the lowest, both nationally and in any every region. Given the prevalence of low-wage workers in this sector, we expect changes in minimum wage laws to have more "bite" for restaurants than for firms in other industries. Much of the case study literature focuses on this industry for this very reason (Dube, Naidu and Reich, 2007; Card and Krueger, 2000; Card and Krueger, 1994; Katz and Krueger, 1992). Second, while restaurants are somewhat tied to a local market for their customer base, they are small on average and the fixed costs of relocation across a local boundary are not high. Therefore, they have a margin of adjustment in response to local differences in minimum wages. Third, in populated counties, restaurant establishments are present in numbers that preclude disclosure issues, so data are available for this industry for a large number of observations. Fourth, the output of restaurants is relatively homogeneous. Fifth, a key goal of this paper is to generalize the local case-study approach, and to reconcile the differences between local estimates and national-level fixed-effects estimates. Therefore, focusing on restaurants allows us to better compare our results with previous research.[15]

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

[15] Neumark (2006) suggests that any detailed industry study may provide misleading results, as it will not account for the expansion of that industry whenever a lower-wage close substitute industry is negatively-effected by the minimum wage. But this argument seems implausible, especially for restaurants as a whole. In particular, NAICS includes establishments devoted primarily to preparing and selling food for take out in "food service and drinking places." The closest substitute to restaurants consists of food (prepared or unprepared) purchased in supermarkets, which have a low incidence of minimum-wage workers.

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. The number of counties with a full balanced panel of reported data, the number of quarters in our time period, and the number of federal or state minimum wage events together yield a national sample of 91,146 observations.

We estimate minimum wage effects using this national sample and three subsamples, which we refer to as: all metro counties, all cross-state metro counties, and all contiguous counties. The metro county subsample denotes all the counties that are located in Metropolitan Statistical Areas (MSAs). Among the 866 counties that are located in the 359 Metropolitan Statistical Areas defined by the Census Bureau[16], the QCEW reports a full panel of restaurant industry data on 733.

The cross-state metro subsample consists of counties in MSAs that cross a state border and which had a minimum wage gradient at some point in time over the sample period. We construct this subsample to make use of the decoupling between the scale of policy intervention (the state) and the scale of the real local economy (a metropolitan area that crosses state lines). The state policy changes can be viewed as exogenous with respect to the common labor market of the entire metropolitan area. Our cross-state metro subsample contains 24 MSAs and 166 counties in 24 states (including DC).[17] We include the San Francisco-Oakland MSA in the "cross-state" sample since San Francisco passed a citywide minimum wage ordinance in 2003 that gave rise to policy variation within the

[16] We use the 2003 definition of Metropolitan Areas. This definition is also referred to as the combined statistical area classification system.
[17] Note that only 137 out of the total 166 counties contain a full balanced panel of observations for the entire sample period.

same metropolitan area.[18] Appendix B displays maps of counties that comprise each MSA in our study, and Appendix C lists the full set of counties.

Note that the same county can experience positive and negative treatment effects 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. To illustrate, consider the Philadelphia-Camden-Wilmington MSA, which includes counties from New Jersey, Pennsylvania and Delaware. In the first quarter of 1992 New Jersey raised the minimum wage to $5.05 per hour from the federal level of $4.25, while the minimum wage remained $4.25 in Pennsylvania. At this point Camden, Burlington, and Gloucester Counties in New Jersey are considered "treated," while Philadelphia and the remaining Pennsylvania and Delaware portions of the MSA make up the control group. The treatment and control status effectively reverse in 1996, when the federal minimum wage increased to $4.75 per hour and the state minimum wage of $5.05 in New Jersey remained in effect.

In addition to the cross-state metro subsample, we also construct a subsample that consists of all the pairs of contiguous counties that straddle a state boundary and have a minimum wage gradient. Similar to the cross-state metro sample, the contiguous cross-state county pairs provide a very close set of comparisons, since contiguous counties generally share spatially-correlated labor market trends. As a further advantage, there are more of these counties than counties that are located in multi-state MSAs. (As in the cross-state metro case, we include San Francisco county and its contiguous pairs in this subsample.) Overall, 231 counties and 194 county pairs in 33 states (plus DC) meet our

[18] The San Francisco-Oakland, MSA includes Alameda, Contra Costa, Marin, San Mateo and San Francisco Counties.

criteria and have continuous data over this period. Appendix B shows the map of all contiguous counties with a minimum wage gradient used in our estimation, and Appendix C lists these counties.

Nature of Minimum Wage Variation in Local Subsamples

Figures 2A and 2B display the number of counties that are part of a cross-state metro or a contiguous county pair that exhibits a minimum wage gradient in each year. We 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 gap within these counties exhibit substantial swings over time consistent with the pattern of increases in state and federal minimum wages, therefore providing the basis for identifying the treatment effect. Moreover, as Figures 2A and 2B show, there are *durable* differences in minimum wages in nearby areas in our local subsamples. Between 1997 and 2002, the minimum wage gap between contiguous pairs with a wage gradient increased from around 7% to around 20%, and stayed around that level until 2006. It is important to note that the variation in minimum wages in nearby areas has not been transitory, as business response to transitory minimum wages may be more muted.

Descriptive Statistics

Table 1 provides descriptive statistics for the national sample and for each of the three subsamples. Comparing all counties nationally (column 1) to counties that are part of cross-state pairs (column 2), we find that they are quite similar in terms of population, density, employment levels and average earnings. Cross-state metro counties (column 4) tend to be somewhat larger, more dense and have higher earnings than metro counties overall (column 3), reflecting the greater incidence of state minimum wages in more urbanized states. Table 1 also shows that our two local subsamples—cross-state metros and cross-state county pairs—are fairly different, with the former being skewed towards dense urban counties. Consequently, finding similar effects of minimum wages in these two local subsamples allays concerns about the representativeness of the results nationally.

As Table 2 shows, restaurant employment growth rates are more heterogeneous across the nine census divisions. Indeed, a simple unweighted average of restaurant employment growth in the areas that have not had many minimum wage events—all three Southern divisions plus the Mountain division—is 3.78, versus 2.32 in the other divisions. To further highlight this heterogeneous growth pattern we map the annual average growth rate of restaurant employment by state during the sample period (1990-2005) in Appendix D.[19] For comparison, we also display the overall growth in the nominal minimum wage in each state. While only illustrative rather than confirmatory, one observes that the states with the largest employment growth tend not to be those that also consistently raised the minimum wage during this period.

6. Results

We begin by reporting our estimates of the minimum wage elasticity of restaurant earnings and employment for the different identification strategies. Table 3 displays our estimates of the minimum wage effect on ln average weekly earnings and ln employment. We report five different specifications representing different samples and controls as denoted by row numbers: (1) the first specification controls only for county and time

[19] 2005 is the last full year of data in our sample.

effects across all the counties in the national sample; (2) row two again controls only for county and time effects, but across metro counties only; (3) the third specification controls for census division-cross-time effects, utilizing only within division variation; (4) the fourth row controls for arbitrary time effects within each metro area; and (5) the fifth row controls for arbitrary time effect within each contiguous county pair. The fourth and fifth rows are based on our cross-state metro and cross-state contiguous county samples, respectively.

The estimates in the first column of Table 3 display OLS standard errors, while the second clusters them at the state level. We provide both standard errors to highlight the issue of precision when one does not allow for non-independence of error terms, as this issue is largely missing in the previous literature.

Effects on Earnings

The minimum wage effect on earnings is positive and relatively similar across each of our specifications, with elasticities ranging from 0.224 in the full sample to 0.153 in the within-MSA subsample. When the standard errors are clustered at the state level (column 2), the wage effects remain significant at conventional levels in all the models, although their precision is reduced. Since all wage estimates are positive and significant at the 1 percent level, we are confident that we are capturing a true wage effect generated by changes in the minimum wage (i.e., we find evidence of a treatment having occurred). This result is reassuring, especially for our local (i.e., contiguous county and within-metro) estimators. Even when comparing neighboring areas, we find strong and significant earnings elasticities that are quantitatively similar to those found using national panel fixed-effects approaches.

Effects on Employment

We turn next to the employment estimates reported in Table 3. In both the full sample and the metro subsample, the baseline fixed-effects models generate negative elasticities that are similar in magnitude to previous panel studies (between -0.15 and -0.22).[20] When we cluster our standard errors at the state level (column 2), these effects remain significant in the metro sample but not for the full set of counties. These results suggest that the elasticity estimates using national sample of restaurants are quite similar to some of the more negative estimates from the teenage literature. Also, our results indicate that the implied precision of the estimates in that literature has been overstated because of inattention to correcting for correlated error terms. Comparing columns 1 (unclustered) and 2 (clustered) show that the standard errors are understated by a factor between five and twelve.[21]

Column 3 shows a small increase in the effects of a minimum wage on earnings after a one-year lag, relative to the increase we find at the implementation date. Adding a one-year lag does not, however, change the effect on employment. This finding contradicts assertions in some of the literature that employment effects may be detectable only with a sizeable lag time.[22] This point is reinforced later in the paper when we examine results with even longer lags and when we present the time paths with a larger set of leads and lags.

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[20] Given the double-log specification, throughout the paper we refer to the treatment coefficient *b* as the elasticity.

[21] The use of the fixed-effects model in the context of panel data raises the issue of whether county-level restaurant employment series are stationary. We conduct a Levin-Lin-Chu test, which is a pooled adjusted Dickey-Fuller test for panel data. For our full sample, this test strongly rejects the null hypothesis that all the panels are 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 caused by non-stationarity.

[22] We find the same absence of a lagged effect on employment in all our other specifications and consequently do not report those results separately in the subsequent tables.

We turn next to the cross-state metro and contiguous county subsamples (specifications 4 and 5). The results in Table 3 indicate that minimum wage employment elasticities are close to zero. Even after correcting for autocorrelation using clustered standard errors, we can rule out minimum wage elasticities of more than -0.15. These bounds reject many of the fixed-effects estimates in the literature.[23] This result is similar to the findings of previous local case studies. Comparing clustered and unclustered standard errors, we find that the latter are understated by a factor of three to four. Here we find an overstatement using aggregated county-level employment. This suggests that the overstatement of precision is even more severe for individual case studies using disaggregated restaurant-level data.[24]

As an intermediate specification, we consider estimates that allow for arbitrary time effects within each census division (specification 3). Reassuringly, and strikingly, these produce minimum wage elasticities of employment close to zero, just like our two local estimators. Geographic controls even as coarse as census divisions erase most of the negative findings from the baseline model. This result provides further support to the hypothesis that the fixed-effects estimators suffer from serious omitted variables bias due to spatial heterogeneity. Although we do not report estimates that control for county-level overall employment, those estimates and standard errors were quantitatively very similar to the estimates presented here.

[23] To provide a contextual comparison of these results with the previous literature, we include in Table 3 the 90 percent confidence intervals on employment, when the standard errors are not clustered. For the cross-state metro and contiguous county subsamples, the lower bounds of the confidence intervals without clustering are -0.05 and -0.04 respectively.

[24] This is not simply a problem of the researchers not having the right approach to calculating standard errors. Rather, it is a problem of not having sufficient data. Although one could cluster standard errors at the state level in a two-state comparison, the reliability of clustered standard errors is quite poor with very small number of clusters.

As previously noted, our data do 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 effect. Indeed, if the labor demand elasticity (as opposed to minimum wage elasticity) 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 0.20.[25] 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.[26]

Inclusion of State-Level Linear Trends

Table 4 reports results for specifications that include state-level linear trends. As before, we find strong earnings effects that are similar across specifications. However, including linear trends reverses the signs and significance in employment estimates from the fixed-effects models (first two specifications). The Hausman tests indicate that we can reject the null hypothesis of coefficient stability from the inclusion of trends in these two specifications. In the specification with census division controls, the coefficient changes by a substantial amount as well, although the difference is not significant at conventional levels. In contrast, inclusion of state trends does not affect the coefficients of the local estimators (columns 4 and 5). The changes in the magnitude of the coefficients are much smaller, and are not statistically significant.

[25] To obtain the wage bill elasticity implied in Table 3, we add the employment and earnings elasticities.
[26] Dube, Naidu and Reich (2007) obtained hours data for part-time and full-time workers. They find no effects on scheduled hours and a shift from part-time to full-time employment in fast food restaurants.

The sensitivity of the fixed-effects estimates to the inclusion of state linear trends does not *necessarily* imply that the fixed-effects estimator is biased, since 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 provides: 1) internal validity of the local estimators and 2) further support for the contention that the fixed-effects estimator is biased downwards due to insufficient controls.

Falsification Test

To provide another demonstration of the effect of omitted variables bias in the fixed-effects estimators, we estimate the effect of placebo minimum wages (average minimum wage in the division for each period) on restaurant employment for counties in states that *never had a minimum wage other than the federal one.* This sample consists of 793 counties, including 412 metropolitan counties, and has no actual cross-sectional variation in minimum wage. Since our placebo minimum wage is constructed at the division level, we cluster the standard errors at this level as well. We also report the unclustered estimates for the standard errors for comparison.

Table 5 shows that the placebo minimum wage is negatively associated with average restaurant earnings, with an elasticity of around -0.3. Since the sample only includes counties with *identical* minimum wages throughout the period, the measured “effect” must be picking up differential earnings growth in different regions of the country. Areas such as the Northeast and the Pacific Coast with higher preponderance of state-level minimum wages apparently are experiencing lower earnings growth among restaurant workers.

The placebo story is even more striking when we consider employment. Although minimum wages were never different among these states, the placebo minimum wages are associated with large employment losses, with elasticities of -0.610 (all counties) and -0.905 (metro counties). These results are even larger than the elasticities found for the equivalent specifications in the actual sample (Table 3). For the metro counties, the results remain significant at the 10 percent level, even after clustering the standard errors at the census division level. Overall, these placebo results provide additional evidence that spatially correlated trends seriously confound minimum wage effects in fixed-effects models using national-level variation.

Minimum Wage Effects by Time Period

In Table 6 we display the effects of minimum wage increases on the restaurant industry for two time periods: an early period (1990q1 through 1998q4), and a later period (1999q1 to 2006q2). The early period contains two federal minimum wage increases as well as some state-level increases. In contrast, the second period contains a large number of state (and some local) increases. For the basic fixed-effects model using all MSA counties (column 2) we find positive and significant earnings effects for both periods, although somewhat larger for the later period (0.21 versus 0.13). Yet, the large negative employment effect reported in the full panel (-0.21 from Table 3) cannot be found in either period. This pattern is similar when we consider all counties (column 1). While the employment elasticities generated by our local estimators also change sign when comparing the early period to the later, the estimates are closer to zero and insignificant when we cluster standard-errors.

This analysis indicates that minimum wage estimates from the fixed-effects model are highly sensitive to the time period under study. In particular, the large negative effects are not found in either of the two subperiods. One explanation may be that it takes a long time for these effects to get picked up, hence requiring the full estimation sample to produce this result. As we see in the next section, however, this is not the case. In fact, the large "employment drop" in the fixed-effects model occurs with a lag of a year and half. Therefore, the evidence points to the presence of confounding factors, such as interaction between the timing of minimum wage passage and employment shocks.

Time Paths of Minimum Wage Effects using Distributed Lags

We estimate time paths of the effect from minimum wage increases using leads and lags of minimum wages. Figure 3 shows both the earnings and employment effects for our five specifications— ten graphs in total. The earnings time paths consistently show sharp increases in the minimum wages centered on *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. The consistency of the wage increases is reassuring as it suggests a similar extent of treatment across our subsamples.

In contrast, the time paths for the employment effects display differences that parallel the patterns in the regression estimates. The fixed-effects estimates (specification 1 and 2) without any time varying controls show negative employment effects, especially about a year after the minimum wage increase. Comparing *t*+4 to *t*-1, we find a cumulative decline in employment ranging from -0.1 to -0.2. The relative stability of the leads and the negative lags provides a strong appearance of a real disemployment effect.

As we see in specification 3, however, the negative lags disappear once we include regional controls. Although adding census division controls substantially reduces the overall negative employment effect, this model exhibits the same pattern of employment effects as specifications one and two (i.e. a decline at t+2). Our cross-metro and contiguous county estimates, however, show no disemployment effect at all and are relatively smooth after t.

We draw two conclusions from these results. First, the bias introduced in the fixed-effects estimates does not arise simply from long-run growth differences between places with higher minimum wages and places with lower ones. The employment reductions in these time paths are concentrated about a year and half after the minimum wage increases. The lack of any employment effects with even divisional controls, and especially with local controls, suggests a more complicated story, in which the timing of minimum wage increases and employment shocks play important roles. In other words, without adequate controls, event study methodologies can produce very misleading results.

Second, as we see from the time paths for our contiguous county and cross-state metro estimates, the short window around the time of the event in many of the case studies is not an explanation for their findings. The time paths using local variation (Figure 3, panels 4 and 5) show a stable employment trajectory for the two years following the wage increase. As we show below, allowing for even longer lagged effects does not change the magnitude of our local estimates.

Minimum Wage Effects by Type of Restaurant

In Table 7 we disaggregate the minimum wage effect for the restaurant sector by its component sub-industries—full service (i.e., table service) and limited service (i.e., fast food) restaurants. As in Table 3, we report the estimates for our five specifications. The estimated wage effect is slightly larger for full-service restaurants than for limited service restaurants in the full sample specifications, but it is nearly identical in the within-metro regressions and somewhat smaller in the contiguous county regression. All wage estimates are positive, significant and within the same general range (0.15 - 0.27), indicating a consistent treatment effect for wages.

The estimated employment effect, however, varies greatly across specifications, along the same lines as before. For the metro subsample without regional controls, the estimated disemployment effect for full service restaurants is quite high (-0.35), implying roughly a four percent reduction in employment for every 10 percent increase in the minimum wage. The wage effect is much smaller for limited service restaurants (-0.12). Both estimates are significant even after clustering the standard errors (column 2).

When we add regional controls, the employment elasticity is significantly reduced for full service restaurants (from -0.35 to -0.11) and becomes positive for limited service restaurants (0.06). However, this positive effect is no longer significant after standard errors are clustered. This pattern suggests that estimates for both sub-sectors of the restaurant industry are affected by unobserved trends.

The employment effect estimated for our within-metro sample is much smaller than for either of the full sample models. The coefficient for full service restaurants, although negative at -0.04, is not significant. The effect for limited-service restaurants is even closer to zero and it is insignificant. The employment effect for the contiguous counties subsample is positive for full service restaurants and negative for limited service restaurants, but both are small in magnitude and neither is significant with clustered standard errors.

Robustness Checks

Finally, we conduct a series of robustness checks on our preferred local estimators. First we relax the restriction made in our sample construction that required all included counties to have valid employment data for the entire sample period (66 quarters). Since the QCEW disclosure rules limit the number of observations from smaller, less urbanized counties, the results may be biased if the disemployment effects are concentrated in smaller counties. Allowing unbalanced panels increases the number of usable counties from 137 to 163 in the cross-state metro sample, and from 231 to 460 in the county pair sample.[27] As reported in Table 8, the results are virtually identical in the unbalanced and balanced panels for both cross-state metro and contiguous county pairs.

Second, to allow potential longer run effects on employment, we report the cumulative effect of a change in minimum wage after three years (row 2). Here the cumulative employment effects are still very close to zero for both the cross-state metro specification and the county pair specification (0.025 and 0.003, respectively) and insignificant.

Lastly, we restrict the contiguous county-pair model to only those counties with a total land area less than 5,000 square miles. This restriction removes 14 counties from our sample. Counties in the western United States tend to be much larger than in the rest of

[27] Unsurprisingly, the fraction of counties with non-disclosure at some point in time is smaller in the metro sample, where the number of restaurants is more likely to meet disclosure criteria, his pattern explains why the unbalanced sample is relatively larger for the county pair case than for the cross-state metro case.

the country and it is possible that the populated portions of contiguous counties may be too far apart to provide effective controls. However, as is shown in Table 8, limiting the sample according to size does not affect the magnitude or sign of the local estimates.[28]

7. Conclusions

In this paper, we propose 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 they generate overstatements of the precision of estimates in both national estimates and individual case studies.

Our generalization of the local case study method shows 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 fixed-effects models. The differences do not arise from focusing only on a specific industry, nor from using short before-after windows in local case studies.

For cross-state metro counties and cross-state contiguous counties, we find strong earnings effects and no employment effects of minimum wage increases. The large negative elasticities in fixed-effects 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 division-level placebo minimum wages are negatively associated with employment in counties with identical minimum wage profiles.

[28] Considering a range of alternative cutoffs did not change these findings.

In all our specifications, we find that adjusting standard errors for clustering at the state level increases the estimated errors by three to twelve times. This large effect suggests a cautionary note. If the reported significance of coefficients in earlier minimum wage studies turn out to be overstated by a similar magnitude, it is likely that many of the negative (or positive) employment effects reported in the literature will need to be substantially revised.

Our local estimators perform better in a number of tests of internal validity. Unlike fixed-effects panel regressions, they are robust to the inclusion of state-level trends. And unlike specifications that rely on national comparisons, they generate similar estimates of both earnings and employment effects when we split our sample into two periods. Overall, our results support the findings of small or no employment effects in the individual local case studies.

Several factors warrant caution in applying these results to different economic and institutional settings. 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 that there are 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. A third caveat concerns labor-labor substitution. Since we do not have any information on individual worker characteristics, we are unable to address this issue directly. At the same time, studies that have found labor-labor substitution rely upon fixed-effects estimators, which we have found to be biased. We conclude that the importance of labor-labor substitution is still an open issue.

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 estimators provide more reliable estimates, which here are typically are close to zero. Our analysis highlights the importance of utilizing localized controls in future work on this topic.

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Table 1. Descriptive Statistics by Sample

	All Counties	*Contiguous County Pairs*	*All Metro Counties*	*Cross-state Metro Counties*
	Mean	***Mean***	***Mean***	***Mean***
Population, 2000	180,945	166,828	302,131	425,063
	(423,274)	*(339,525)*	*(552,758)*	*(610,049)*
Population density, 2000	464	591	807	2,575
	(2,552)	*(3,551)*	*(3,463)*	*(7,656)*
Land area (square miles)	1,107	1,236	972	543
	(1,760)	*(2,161)*	*(1,705)*	*(624)*
Total private employment	66,668	62,817	113,985	165,941
	(172,259)	*(156,618)*	*(225,984)*	*(276,880)*
Restaurant employment	4,509	4,095	7,593	9,212
	(10,517)	*(8,340)*	*(13,693)*	*(13,884)*
Restaurant average weekly earnings ($)	170.79	171.50	183.18	197.16
	(43.76)	*(34.19)*	*(45.38)*	*(58.72)*
Minimum wage	4.62	4.73	4.62	4.92
	(0.80)	*(0.91)*	*(0.61)*	*(0.74)*
Number of counties	1,381	504	733	137
Number of MSAs	n/a	n/a	359	24
Number of states	50	33	50	24
Number of federal minimum wage events	11,540	1,159	3,884	532
Number of state minimum wage events	2,844	1,771	1,132	350

Note: Standard deviations in parentheses below mean.
Sources: Bureau of Labor Statistics, *Quarterly Census of Employment and Wages (QCEW)*; U.S Department of Labor, Employment Standards Administration, Wage and Hour Division. U.S. Bureau of the Census, *2000 Census.*

Table 2. Average Annual Employment Growth Rates, by Census Division

	New England	Middle Atlantic	East North Central	West North Central	South Atlantic	East South Central	West South Central	Moun-tain	Pacific
Restaurants									
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
All 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: QCEW.

Table 3. Minimum Wage Effects on Earnings and Employment

	Not Clustered SE	Clustered SE	Clustered SE + 1 year lag	Controls† Census Div x Period	Controls† MSA x Period	Controls† County Pair x Period
Ln Earnings						
1. All counties	0.224***	0.224***	0.263***			
	(0.004)	(0.033)	(0.036)			
2. All metro counties	0.209***	0.209***	0.242***			
	(0.005)	(0.030)	(0.034)			
3. All metro counties	0.196***	0.196***	0.222***	Y		
	(0.008)	(0.037)	(0.044)			
4. Cross-state metro counties	0.153***	0.153***	0.167***		Y	
	(0.013)	(0.030)	(0.032)			
5. Contiguous county pairs	0.190***	0.190***	0.208***			Y
	(0.010)	(0.032)	(0.036)			
Ln Employment						
1. All counties	-0.147***	-0.147	-0.142			
	(0.010)	(0.120)	(0.128)			
90% Confidence interval††	(-0.163 to -0.131)		(-0.344 to 0.050)			
2. All metro counties	-0.215***	-0.215***	-0.216***			
	(0.012)	(0.055)	(0.080)			
90% Confidence interval††	(-0.234 to -0.196)		(-0.351 to -0.079)			
3. All metro counties	-0.017	-0.017	-0.075	Y		
	(0.019)	(0.075)	(0.079)			
90% Confidence interval††	(-0.049 to 0.015)		(-0.140 to 0.105)			
4. Cross-state metro counties	-0.00002	-0.00002	0.007		Y	
	(0.029)	(0.093)	(0.096)			
90% Confidence interval††	(-0.048 to 0.048)		(-0.149 to 0.149)			
5. Contiguous county pairs	0.012	0.012	-0.003			Y
	(0.020)	(0.073)	(0.086)			
90% Confidence interval††	(-0.045 to 0.022)		(-0.134 to 0.110)			

Notes: Robust standard errors in parentheses; we report both unclustered, and clustered at the state level. N (all counties) = 91,146; N (metro counties) = 43,378; N (cross-state metro counties)= 9,042; N (county pairs) = 23,628. Number of counties (all counties)=1,381; (metro counties)=733; (cross-state metro counties) = 137; (county pairs) = 231.
* Significant at 10%; ** significant at 5%; *** significant at 1%.
†All samples and specifications include county and time period fixed effects (66 total periods). Additional time varying effects are specified in the table.
†† Confidence intervals are based on the unclustered and clustered standard error specifications respectively.

Table 4. Effects of Including State-Level Linear Trends

	1. All Counties		*2. All Metro Counties*		*3. All Metro Counties*		*4. Cross-State Metro Counties*		*5. Contiguous County Pairs*	
Ln Earnings	0.224***	0.213***	0.209***	0.198***	0.196***	0.187***	0.156***	0.113***	0.190***	0.152***
	(0.033)	(0.043)	(0.030)	(0.051)	(0.037)	(0.039)	(0.030)	(0.025)	(0.023)	(0.025)
Ln Employment	-0.147	0.071	-0.215***	0.095*	-0.017	0.093*	0.001	0.082**	-0.012	-0.017
	(0.120)	(0.047)	(0.083)	(0.052)	(0.075)	(0.048)	(0.090)	(0.0350)	(0.052)	(0.035)
Controls:†										
Census Div x Period					Y	Y				
Metro Area x Period							Y	Y		
County Pair x Period									Y	Y
Linear State Trends		Y		Y		Y		Y		Y
p Value††*: Hausman Test*		0.090*		0.004***		0.188		0.382		0.925
N	91,146	91,146	48,378	48,378	48,378	48,378	8,976	8,976	37,752	37,752
Number of Counties	1,381	1,381	733	733	733	733	136	136	337	337

Notes: Robust standard errors in parentheses and clustered at the state level.
* significant at 10%; ** significant at 5%; *** significant at 1%.
† All samples and specifications include county and period fixed effects. Additional time-varying effects are specified below.
†† Probability value reported for the null hypothesis that the employment elasticities are same in the specification with added state-level linear trends as the corresponding specification without.

Table 5. Effect of Census Division-Based Placebo Minimum Wages on Earnings and Employment

	Ln Earnings		Ln Employment		Controls †		
Sample	Not Clustered SE	Clustered SE	Not Clustered SE	Clustered SE	Census Division x Time	MSA x Time	County Pair x Time
1. All counties							
	-0.306***	-0.306*	-0.610***	-0.610			
	(0.028)	(0.122)	(0.062)	(0.687)			
2. All metro counties							
	-0.282***	-0.282*	-0.905***	-0.905*			
	(0.036)	(0.134)	(0.089)	(0.510)			

Notes: Robust standard errors in parentheses; we report both unclustered, and clustered at the census-division level. N (All counties) = 52,338; N (Metro counties)=27,192. Number of counties (All counties)=793; Number of counties (Metro counties)=412. Placebo minimum wage is the simple average of minimum wages in the census division in a given time period. The estimation sample consists of all counties where the minimum wage for the entire period was just the federal minimum – i.e., there was no actual variation in the minimum wage between any of these counties.
* significant at 10%; ** significant at 5%; *** significant at 1%.
†All samples and specifications include county and time period fixed effects. Additional time-varying effects are in the table.

Table 6. Minimum Wage Effects on Earnings and Employment, by Time Period

	Ln Earnings		Ln Employment		Controls †		
Sample	Not Clustered SE	Clustered SE	Not Clustered SE	Clustered SE	Census Division x Time	MSA x Time	County Pair x Time
1. All Counties							
1990-1998	0.172***	0.172**	0.009	0.009			
	(0.014)	(0.073)	(0.024)	(0.081)			
1999-2006	0.216***	0.216***	-0.053***	-0.053			
	(0.007)	(0.038)	(0.013)	(0.075)			
2. All metro counties							
1990-1998	0.125***	0.125	0.005	0.005			
	(0.017)	(0.087)	(0.030)	(0.108)			
1999-2006	0.205***	0.205***	-0.075***	-0.075			
	(0.008)	(0.033)	(0.014)	(0.072)			
3. All metro counties							
1990-1998	0.150***	0.150**	-0.047	-0.047*	Y		
	(0.017)	(0.069)	(0.031)	(0.054)			
1999-2006	0.208***	0.208***	0.084***	0.084	Y		
	(0.009)	(0.040)	(0.018)	(0.065)			
4. Cross-state metro counties							
1990-1998	0.101***	0.101***	0.013	0.013		Y	
	(0.023)	(0.034)	(0.040)	(0.088)			
1999-2006	0.137***	0.137***	-0.051	-0.051		Y	
	(0.017)	(0.024)	(0.032)	(0.062)			
5.Contiguous county pairs							
1990-1998	0.174***	0.174***	-0.072**	-0.072			Y
	(0.023)	(0.046)	(0.034)	(0.087)			
1999-2006	0.128***	0.128***	0.006	0.006			Y
	(0.015)	(0.032)	(0.025)	(0.065)			

Notes: Robust standard errors in parentheses; we report both unclustered, and clustered at the state level.
N (all counties) = 49,716, 41,430; N (metro counties)= 26,388, 21,990; N (cross-state metro counties)= 4,932, 4,110.
N (county pairs) = 12,888, 10,740.
* significant at 10%; ** significant at 5%; *** significant at 1%.
†All samples and specifications include county and time period fixed effects. Additional time-varying effects are specified below.

Table 7. Minimum Wage Effects, by Restaurant Industry Sector

	Ln Earnings		Ln Employment		Controls †		
Sample	**Not Clustered SE**	**Clustered SE**	**Not Clustered SE**	**Clustered SE**	**Census Division x Period**	**MSA x Period**	**County Pair x Period**
1. *All counties*							
Full service	0.269***	0.269***	-0.204***	-0.204			
	(0.006)	(0.042)	(0.014)	(0.162)			
Limited service	0.204***	0.204***	-0.075***	-0.075			
	(0.006)	(0.036)	(0.012)	(0.126)			
2. *All metro counties*							
Full service	0.239***	0.239***	-0.349***	-0.349***			
	(0.006)	(0.035)	(0.016)	(0.116)			
Limited service	0.190***	0.190***	-0.122***	-0.122			
	(0.006)	(0.042)	(0.014)	(0.084)			
3. *All metro counties*							
Full service	0.199***	0.199***	-0.111***	-0.111	Y		
	(0.011)	(0.047)	(0.025)	(0.084)			
Limited service	0.176***	0.176***	0.055***	0.055	Y		
	(0.010)	(0.030)	(0.024)	(0.087)			
4. *Cross-state metro counties*							
Full service	0.152***	0.152***	-0.049	-0.049		Y	
	(0.017)	(0.038)	(0.037)	(0.093)			
Limited service	0.153***	0.153***	-0.022	-0.022		Y	
	(0.018)	(0.054)	(0.039)	(0.154)			
5. Contiguous county pairs							
Full service	0.160***	0.160***	0.042*	0.042			Y
	(0.014)	(0.051)	(0.029)	(0.149)			
Limited service	0.224***	0.224***	-0.056*	-0.056			Y
	(0.015)	(0.047)	(0.026)	(0.106)			

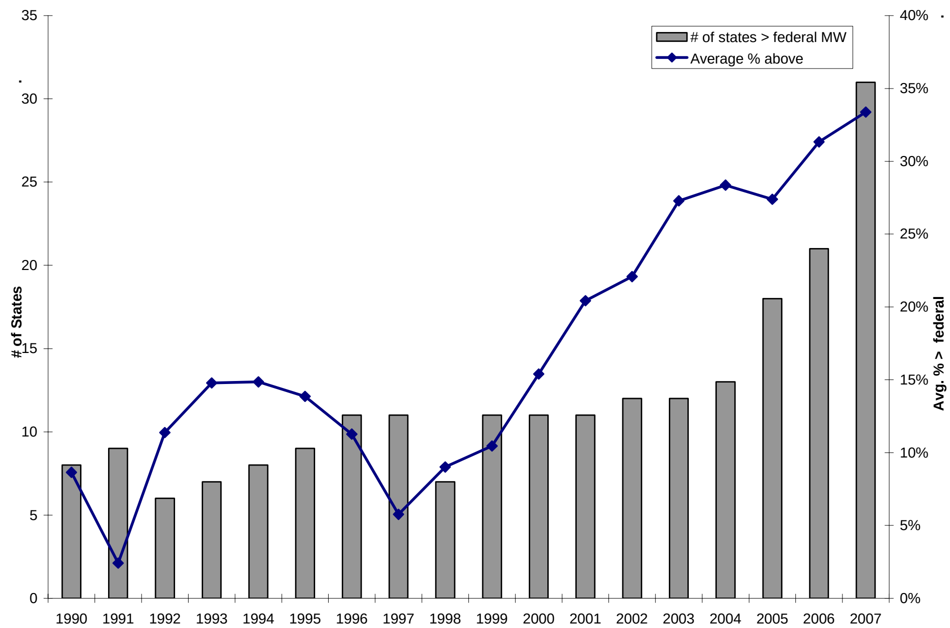
Notes: Robust standard errors in parentheses; we report both unclustered, and clustered at the state level.
N (all counties) = 108,240, 118,206; N (metro counties)= 51,414, 53,992; N (cross-state metro counties)= 9,900, 9,306.
N (county pairs)= 23,628, 23,628.
* significant at 10%; ** significant at 5%; *** significant at 1%.
†All samples and specifications include county and time period fixed effects. Additional time varying effects are specified in the table.

Table 8. Robustness Checks for Sample Balance Restriction, Long-Term effects, and County Size

	Cross-State Metro Counties		Contiguous County Pairs	
Sample	**Ln Earnings**	**Ln Employment**	**Ln Earnings**	**Ln Employment**
Balanced	0.153***	-0.00002	0.190***	-0.012
	(0.030)	(0.090)	(0.032)	(0.073)
N	9,042	9,042	37,752	37,752
Number of Counties	137	137	231	231
Unbalanced	0.121***	-0.075	0.188***	0.003
	(0.041)	(0.328)	(0.046)	(0.080)
N	9,918	9,918	66,710	66,710
Number of Counties	163	163	460	460
3 year lag	0.165***	0.025	0.209***	0.003
	(0.037)	(0.137)	(0.042)	(0.126)
N	7,398	7,398	30,888	30,888
Number of Counties	137	137	231	231
Large counties omitted††			0.190***	0.005
			(0.035)	(0.073)
N			36,168	36,168
Number of Counties			217	217

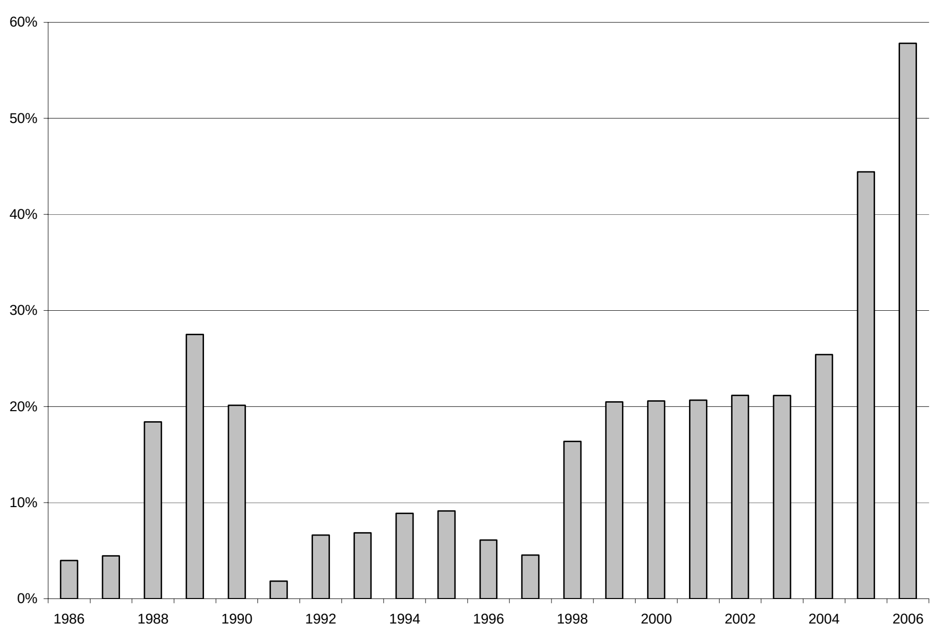
Notes: Robust standard errors in parentheses, clustered at the state level.
* significant at 10%; ** significant at 5%; *** significant at 1%.
†All samples and specifications include county and time period fixed effects. Additional time varying effects are specified in the table.
†† Sample excludes 14 counties with with land area of 5,000 square miles or greater.

Figure 1A. Number of States with Minimum Wages above Federal Level and Average Percentage above Federal, 1990-2007



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

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



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

Figure 2A. Number of County Pairs with Minimum Wage Gradient and Average Wage Differential, County Pair Sample

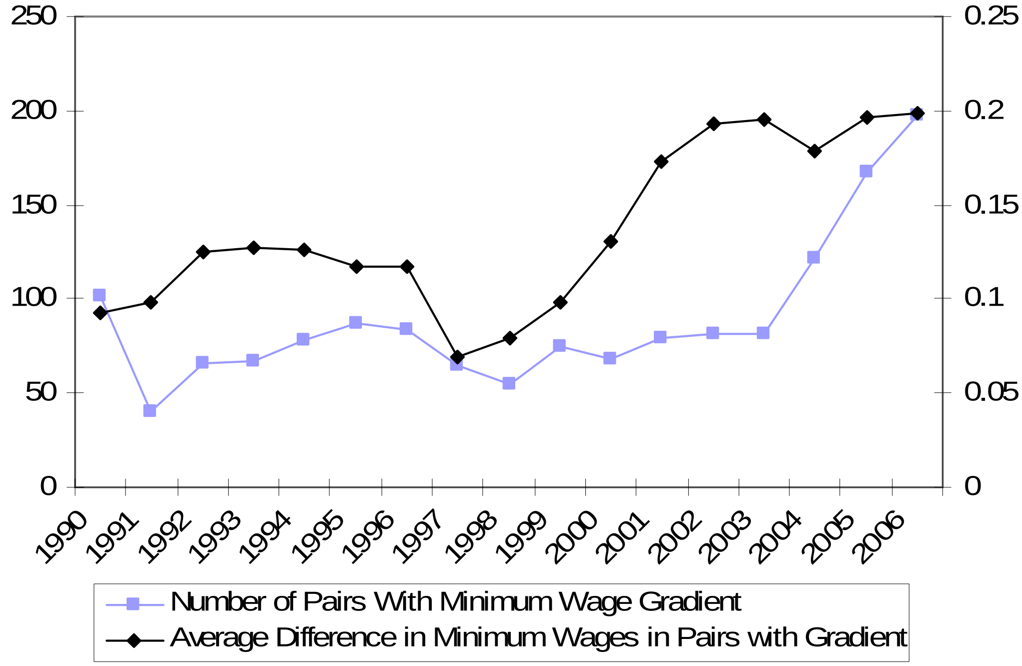


Figure 2B. Number of Counties in Metro Areas with Minimum Wage Gradient, and Average Minimum Wage Differential in Cross-State Metro Sample

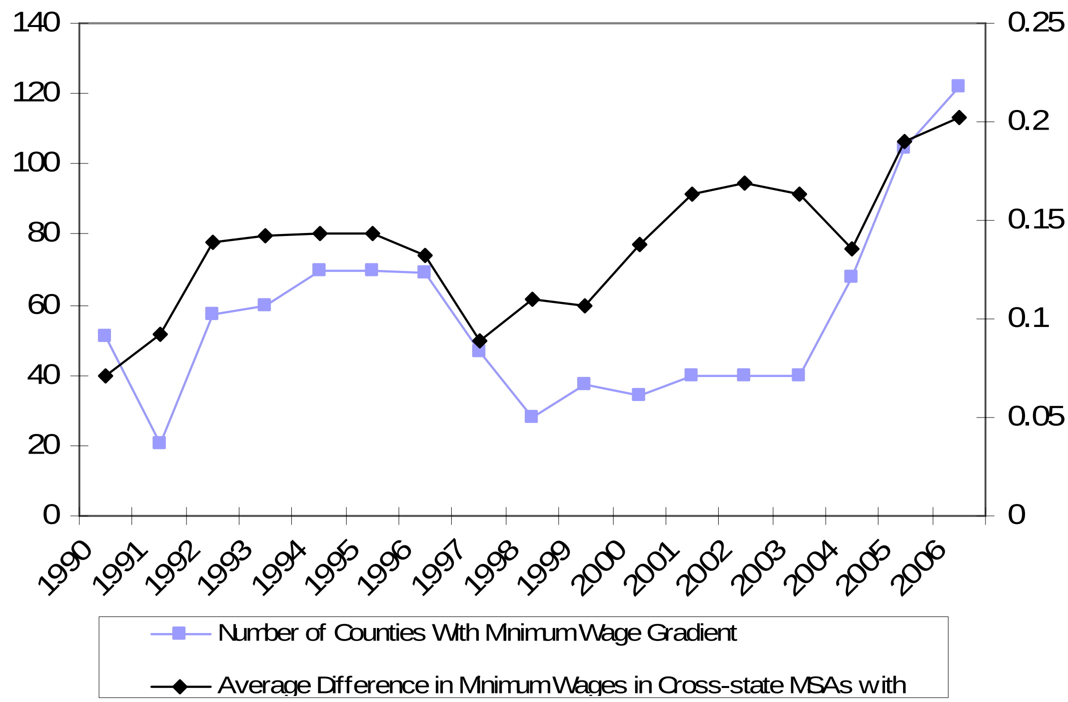
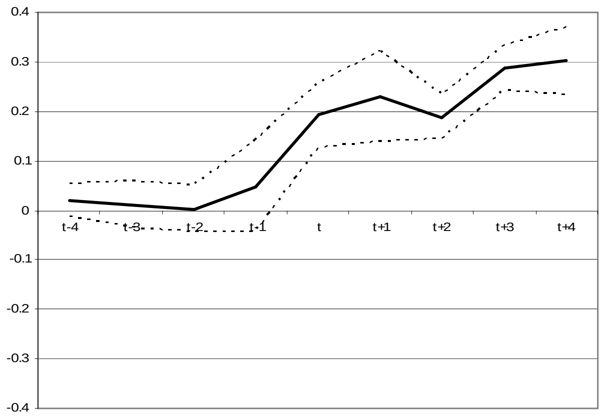
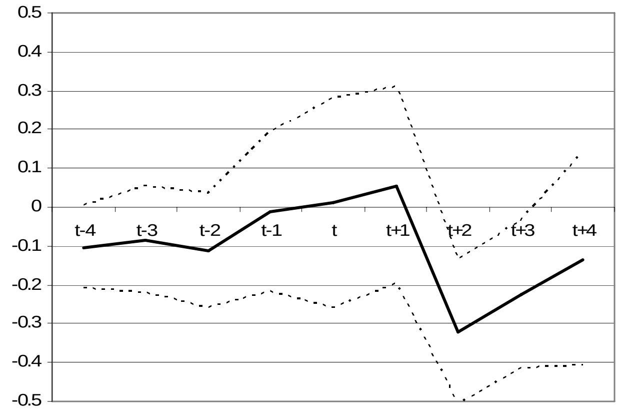


Figure 3. Time Paths of Minimum Wage Effects, by Sample, Semi-Annual Periods

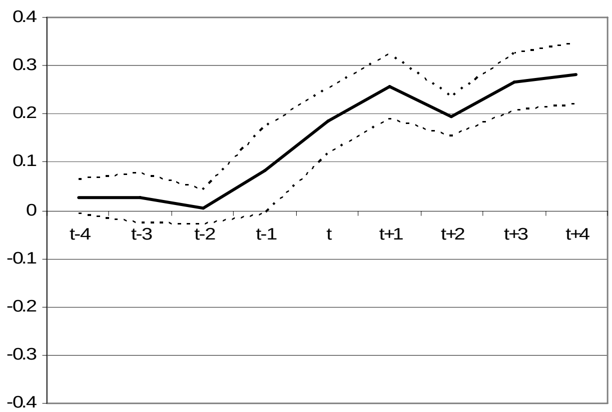
Earnings **Employment**

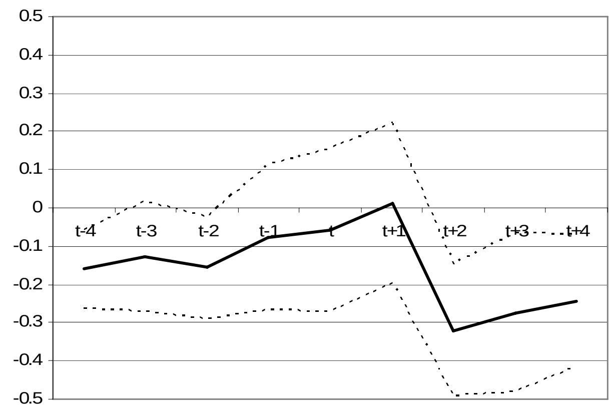
1) All Counties



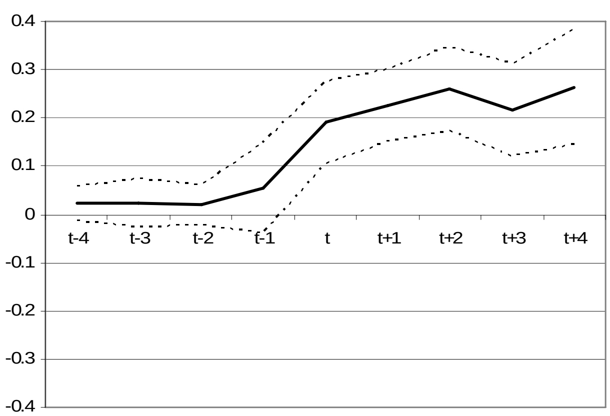


2) All Metro Counties





3) All Metro Counties, with Census Division Controls



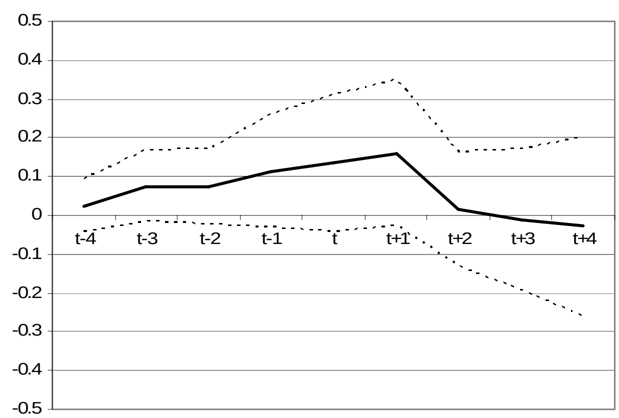
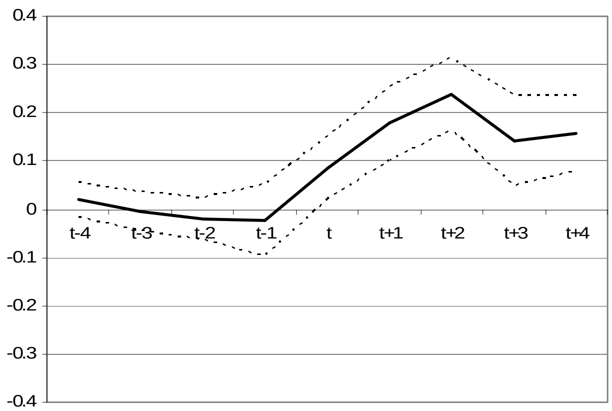
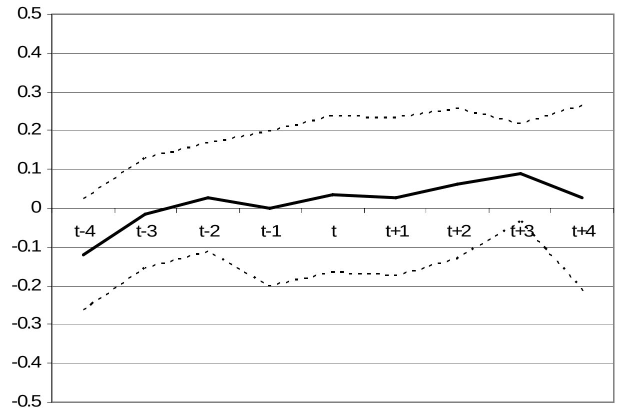


Figure 3. Time Paths of Minimum Wage Effects, by Sample, Semi-Annual Periods (continued)

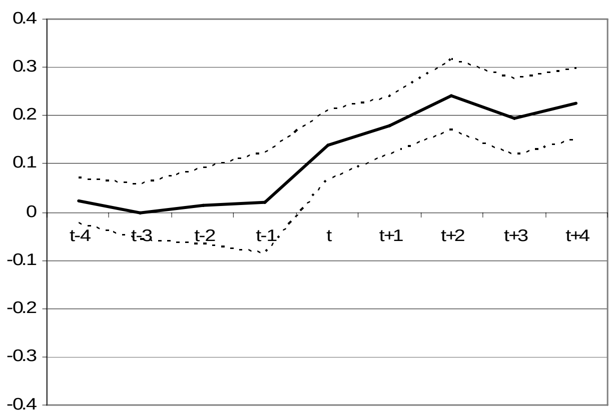
Earnings **Employment**

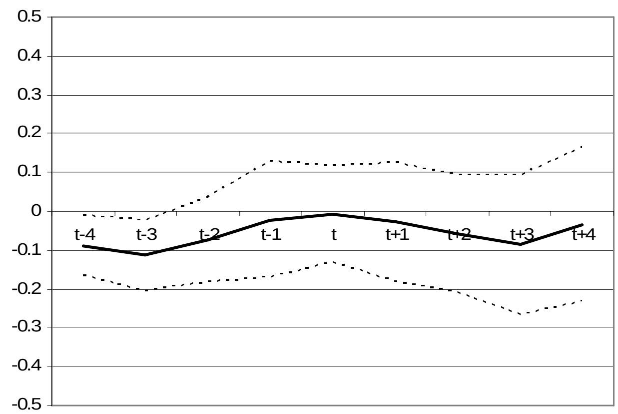
4) All Cross-State Metro Counties





5) All Contiguous Counties





Notes: 90% confidence bounds in dashed lines, with standard errors clustered at the state level. All samples and specifications include county and time period fixed effects. Additional time varying controls are (a) census division X period effects for panel 3; (b) MSA X period for panel 4; and (c) County pair X period for panel 5.

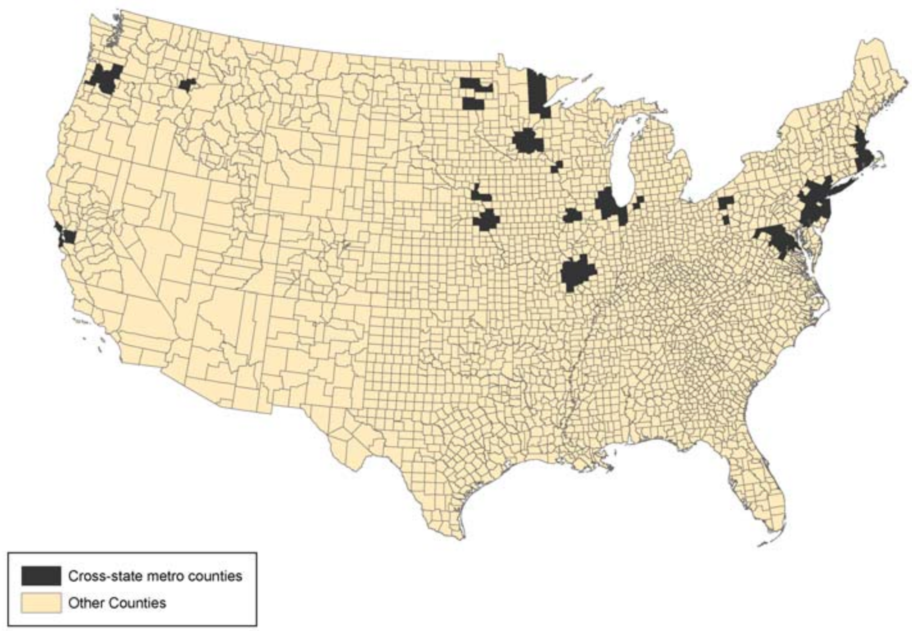
Appendix A. Minimum Wages by State and Year (in dollars), 1990-2009†

State	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009
Alaska	4.30	4.75	4.75	4.75	4.75	4.75	5.25	5.65	5.65	5.65	5.65	5.65	5.65	7.15	7.15	7.15	7.15	7.15	7.15	
Arizona																		6.75	6.90	
Arkansas																	6.25	6.25	6.55	
California	4.25	4.25						5.00	5.75	5.75	5.75	6.25	6.75	6.75	6.75	6.75	6.75	7.50	8.00	8.00
Colorado																		6.85	6.98	
Connecticut	4.25	4.25								5.65	6.15	6.40	6.70	6.90	7.40	7.40	7.40	7.65	7.65	
Delaware							4.65	5.00		5.65	6.15	6.15	6.15	6.15	6.15	6.15	6.15	6.65	7.15	
D.C.				5.25	5.25	5.25	5.75	6.15	6.15	6.15	6.15	6.15	6.15	6.15	6.15	6.60	7.00	7.00	7.00	
Florida																6.15	6.40	6.67	6.80	
Hawaii	3.85	3.85	4.75	5.25	5.25	5.25	5.25	5.25	5.25	5.25	5.25	5.25	5.75	6.25	6.25	6.25	6.75	7.25	7.25	
Illinois															5.50	6.50	6.50	7.50	7.75	8.00
Iowa	3.85	4.25	4.65	4.65	4.65	4.65	4.65											6.20	7.25	
Maine	3.85	3.85											5.75	6.25	6.35	6.5	6.75	7.00	7.00	
Maryland																	6.15	6.15	6.55	
Massachusetts	3.75						4.75	5.25	5.25	5.25	6.00	6.75	6.75	6.75	6.75	6.75	6.75	7.50	8.00	8.00
Michigan																	6.95	7.15	7.40	7.40
Minnesota	3.95	4.25														6.15	6.15	6.15		
Missouri																		6.50	6.62	
Montana																		6.15		
Nevada																		6.15	7.03	7.73
New Hampshire	3.75	3.85																		
New Jersey			5.05	5.05	5.05	5.05	5.05	5.05								6.15	7.15	7.15	7.15	
New York																6.00	6.75	7.15		
New Mexico																				7.50
North Carolina																		6.15		7.50
North Dakota	3.40																			
Ohio																		6.85	7.00	
Oregon	4.25	4.75	4.75	4.75	4.75	4.75	4.75	5.50	6.00	6.50	6.50	6.50	6.50	6.90	6.90	6.90	7.50	7.80	7.95	8.15
Pennsylvania	3.70																	7.15	7.15	
Rhode Island	4.25	4.45	4.45	4.45	4.45	4.45	4.75	5.15		5.65	6.15	6.15	6.15	6.15	6.75	6.75	7.10	7.40	7.40	7.40
Vermont	3.85	3.85				4.50	4.75	5.25	5.25	5.75	5.75	6.25	6.25	6.25	6.75	7.00	7.25	7.53	7.67	7.85
Washington	4.25	4.25			4.90	4.90	4.90	4.90		5.70	6.50	6.72	6.90	7.01	7.16	7.35	7.63	7.93	8.08	8.27
West Virginia																		6.55		
Wisconsin	3.65															5.70	6.50	6.50		
Federal	3.80	4.25	4.25	4.25	4.25	4.25	4.75	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.85	6.55	7.25

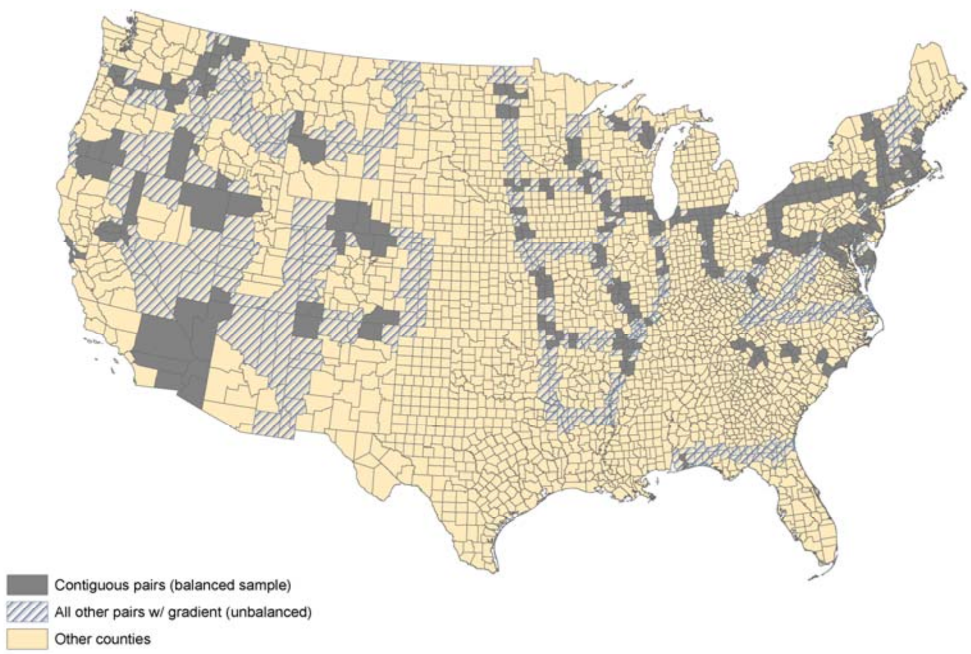
Source: U.S. Department of Labor, Wage and Hour Division. Economic Policy Institute (2007-2009 data). †Table includes only states that had a minimum wage above the federal level between 1990 and 2009. Listed minimum wage rates are based on rate in fourth quarter of each year. San Francisco, CA minimum wage rate was $8.50 in 2004, $8.62 in 2005, $8.82 in 2006 and$9.14 2007. 2008 and 2009 values will be adjusted based on the Consumer Price Index.

Appendix B. Maps of County-based subsamples.

(1) Cross-state Metropolitan Counties



(2) Contiguous County Pairs



Appendix C- 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[a]
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 [c]
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[b]
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

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[a] 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.
[b] Houston County, MN did not have a full balanced panel of observations.
[c] 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[d]
- San Mateo, CA

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

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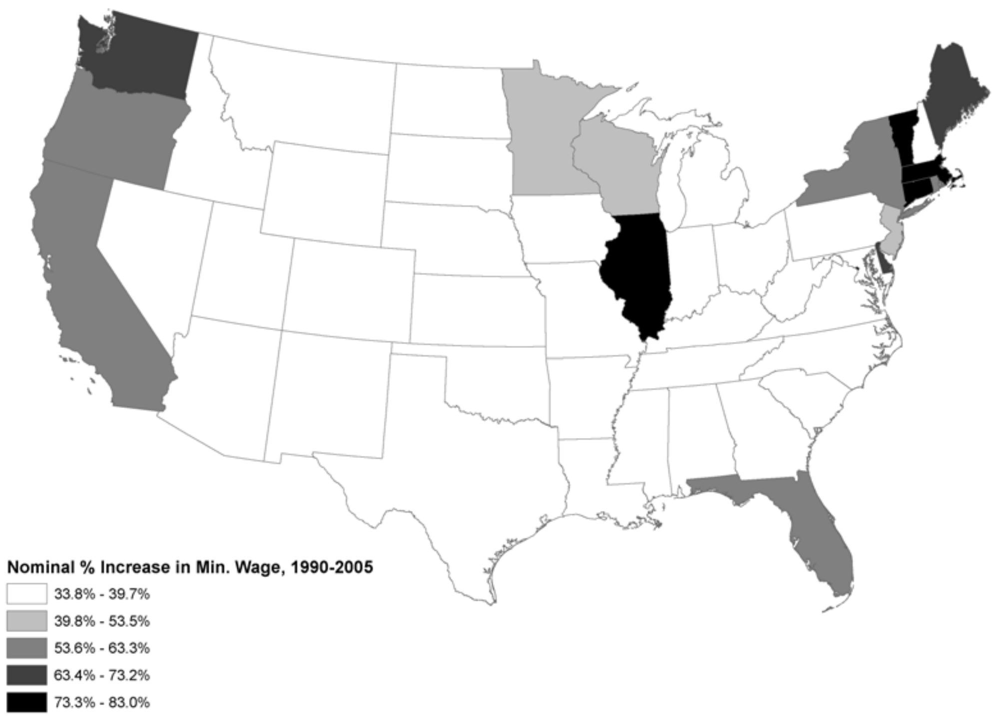
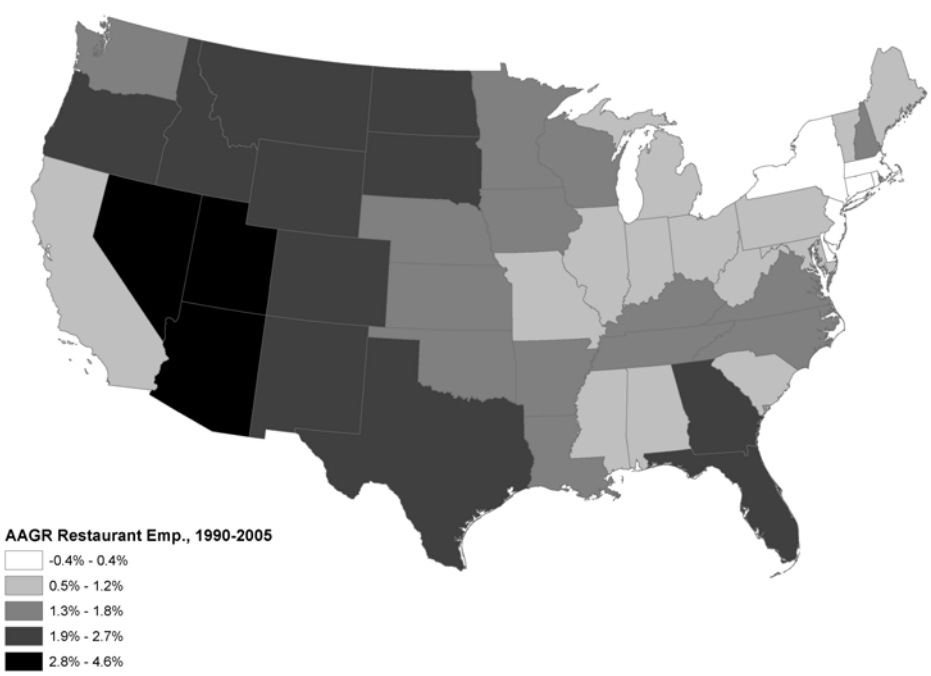
[d] Minimum wage variation within the MSA stems from San Francisco County's minimum-wage in 2004.
[e] Cass County, MI did not have a full balanced panel of observations. This results in the whole metro area being dropped from the main regression analysis. We list it here because it is appears in the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

Appendix D

(1) Percent Increase in Nominal Minimum Wage Rates by State, 1990-2005



(2) Annual Average Restaurant Employment Growth by State, 1990-2005.



Source: Bureau of Labor Statistics, Quarterly Census of Employment and Wages, 1990-2005.

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