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Geographically-Disparate Trends and
Job Growth Equations**

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ABSTRACT

On the Robustness of Minimum Wage Effects: Geographically-Disparate Trends and Job Growth Equations

Just as the standard two-way fixed effects model for estimating the impact of minimum wages on employment has been sharply criticized for its neglect of spatial heterogeneity so, too, have the latest models been attacked for their uncritical use of state- or county-specific linear trends (and other spatial counterfactuals). Further attenuation of the effects of policy is also alleged to obtain in such circumstances where the true effect of minimum wages is upon employment growth rather than levels. This paper investigates whether such considerations call into question our earlier findings of statistically insignificant employment effects for an archetypal low-wage sector. We report that a continued focus on employment levels is indicated and that while experimentation with nonlinear trends may be productive their use is unlikely to dislodge the finding of considerably reduced negative employment effects.

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I. Introduction

In the present paper we seek to establish the extent to which our findings in Addison, Blackburn, and Cotti (2012) survive certain criticisms that have been made of attempts to control for spatial heterogeneity in minimum wage research in an important new review of the literature by Neumark, Salas, and Wascher (2013). In the process, and as a secondary exercise, we also address a potentially more radical critique having a basis in the notion that minimum wage effects are more easily detected in employment growth than in employment levels, such that conventional controls for spatial heterogeneity may attenuate estimates of how the minimum wage affects the level of employment (Meer and West, 2013). The wider backdrop to the present analysis is a recent meta-analysis of 27 modern minimum wage studies by Wolfson and Belman (2014), controlling for many aspects of the studies, that concludes that minimum wages have no economically nor statistically meaningful disemployment effects.¹

Using a large sample of county-level employment data, Addison, Blackburn, and Cotti (2012) estimated the effect of minimum wages on employment in the restaurant-and-bar sector. In addition to time and county fixed effects, our model included a county-specific effect allowed to follow a linear trend over time (along with county-level controls) in a framework that allowed us to assess the consistency of the estimates with a competitive-model explanation of employment and earnings determination. In general, we concluded that minimum wages did not reduce employment in a sector that contains the highest percentage of workers at or below the relevant minimum wage in the United States and in which a little over 40 percent of workers worked for the minimum wage plus two dollars or less. That said, our estimates could be considered largely consistent with a competitive model in which the elasticity of demand for labor is very small. Of course in a debate on minimum wages in which the respective sides do not take prisoners, “largely consistent” is unlikely to win one supporters from either side of the divide.

¹ See also an earlier meta-analysis by Doucouliagos and Stanley (2009) that, having taken publication bias into account, suggests a not dissimilar conclusion in pointing to an elasticity of -0.01.

However, our purpose here is to determine what we can learn from recent criticisms, much of which we regard as constructive and productive of research progress.

II. Two Basic Approaches, Then and Now

As is well known, research on minimum wages has gone through several stages. But we will begin with the *new minimum wage research* of the early 1990s (For a thorough review of the earlier literature, see Neumark and Wascher, 2007, 2008.) This research focused on state data because of the advantages of using simultaneous panels rather than an aggregate time series. One approach exploited geographical variation in the setting of minimum wages in an industry case study approach, whereas the second used a standard state- panel analysis in which state effects were held constant. Both approaches sought valid counterfactual control groups for what would have transpired absent increases in the minimum wage, and each reported generally divergent findings. The case studies pointed to a lack of job loss – even gains – and the two-way state panel approach suggested the opposite for long panels of data (with minimum wage elasticities in the range -0.1 to -0.3). Case studies of a particular change in the minimum wage in a particular industry typically used only a short time horizon (raising obvious concerns about missing lags in disemployment effects), and in covering individual cases raised problems of inference and external validity. For their part, the state panel studies did not allow for heterogeneous trends in states that increased minimum wages; for example, states experiencing greater increases in minimum wages might have systematically different labor market characteristics unrelated to their minimum wage policies. They also largely did not recognize the importance of within-state error correlation in constructing standard errors, thereby tending to overstate the precision of their minimum-wage elasticities (and making it more likely to find significant effects with limited data).

Enter the *new new minimum wage research*. This has taken two forms. The first, and that focused upon here, uses geographic-specific linear trend variables as a means of controlling for heterogeneity in the underlying long-term growth prospects of low-wage employment (as well as other trends in teen

employment). Such geographic-specific linear trends are often supplemented with time-varying effects for more aggregated census regions or divisions, again allowing for spatial heterogeneity in differential employment patterns including region- or division-specific economic shocks. The second innovation has been to execute the case study approach using larger panels. This approach uses a research design based on cross-border pairs in a specification that (initially) included county-pair/period interactions so as to control for shocks common to both counties, thereby identifying the effect of minimum wages from differences in employment changes in paired counties on either side of a state border.

These two approaches were (mostly) to yield results at odds with the standard state panel exercises, providing little or no evidence of job loss in sectors or for groups most likely to be impacted by minimum wage increases. Thus, Allegretto, Dube, and Reich (2011), using Current Population Survey (CPS) data on teens between 1990 and 2009 obtained minimum wage effects consistent with the standard state panel model *before* sweeping out the variation across census divisions and allowing for state-specific trends, only to report essentially zero employment (and indeed hours) elasticities after their inclusion.² Other interesting results from their study were (a) an absence of anticipation effects with the inclusion of the two spatial controls, and (b) a seeming lack of employment effects over the business cycle.

Our own analysis used Quarterly Census of Employment and Wages (QCEW) administrative data for 1990-2005 for the restaurant-and-bar sector, and evinced a very similar pattern of results: negative and statistically significant coefficient estimates for the log minimum wage in employment regressions containing fixed county and time fixed effects that declined sharply in absolute magnitude and became statistically insignificant with the incorporation of county-specific trends. As we noted (Addison, Blackburn, and Cotti, 2012: 424), "...employment in the restaurant-and-bar sector tends to exhibit a downward trend in states that have increased their minimum wages relative to states that have not, biasing

² Similar results for employment are reported by Dube, Lester, and Reich (2010) using the Quarterly Census of Employment and Wages.

the fixed effect ... estimates ... towards finding a negative employment effect of minimum wages.”³ Recognizing the potential case study bias of the restaurant sector, we should note that we had earlier obtained very similar minimum wage impacts in other low-wage sectors in the retail sector at county level (Addison, Blackburn, and Cotti, 2009).

A second approach to relaxing the parallel trend assumption of the standard panel regression model is presented in the study by Dube, Lester, and Reich (2010). Using the QCEW, the authors consider all adjacent counties straddling state borders for which data are available between 1990 and 2006. Of these 504 counties, some 337 in 288 pairs recorded some difference in minimum wages. The impact of minimum wages is obtained from differences in employment changes in these paired counties, using unique dummy variables for each pair interacted with time period. No evidence of employment losses – up to four years after a minimum wage increase – is reported for the two sectors (restaurants and retail) examined in the study.⁴

III. The Critique of Using State- and County-Specific Linear Trends

The most extensive critique of the extension/application of the state panel approach is by Neumark, Salas, and Wascher [NSW] (2013). A major part of their criticism has to do with the choice of sample period, raised by other findings from this new phase of research in which significantly negative minimum wage effects do not always vanish with the incorporation of state-specific trends (see Neumark and Wascher, 2011). In particular, NSW criticize the analysis of Allegretto, Dube, and Reich (2011) noting that there were recessions at the start (1990-91) and end of their sample period. If recessions do not have an aggregate influence that is common across periods, the longer-term estimated trend could be biased. Specifically NSW (2013: 10) observe: “This in turn could lead to misclassification of periods in which

³ We also reported a similar pattern when state-level trends were substituted for county-level trends.

⁴ A similar finding for teenagers using the Quarterly Workforce Indicators dataset is reported in Dube, Lester, and Reich (2012).

teen employment was high or low relative to the predicted values *net of* the minimum wage, and hence influence the estimated minimum wage effect for reasons having nothing to do with the longer-run trends for which the specification is trying to control.” By way of illustration, NSW present results for California for a model with state-specific trends. The model is estimated initially for the period 1994-2007 thereby excluding the 1990-1991 recession and the Great Recession. They plot the actual residuals for this period and then the prediction errors for the two recessionary intervals. It is found that (teenager) employment was much higher than would have been predicted by the model for the first recession but considerably smaller for the second. When the recessionary intervals are included both separately and jointly the estimates of state-specific trends over the non-recessionary period are strongly influenced by their inclusion.

Given this potential for bias, NSW recommend the use of higher-order trends in panel data models. Alternatively, they also suggest the exclusion of sub-periods of steep recessions in estimating state-level trends while retaining the whole sample to estimate minimum wage effects, or the use of a Hodrick-Prescott filter to detrend the data. They then follow their own advice in estimating a model of teen employment, 1990-2011(Q2), using CPS data, first with a simple state-specific linear trend and then with a variety of higher-order trends and alternative detrending methods. Apart from the linear trend specifications, they report near universally negative and significant effects of minimum wages on teen employment.

As a practical matter, NSW spend more time critiquing the border-county approach. Since we, too, have expressed reservations over this estimation strategy (see in particular Addison, Blackburn, and Cotti, 2009) this is not the place to dwell on this methodology other than in the related context of NSW’s criticism of the use of census division-time period interactions in Allegretto, Dube, and Reich (2011). The justification for this control is again one of spatial heterogeneity: employment rates for low-wage groups vary by census division and may do so differentially over time. Accordingly, the inclusion of division-specific time effects eliminates between-division variation, including division-specific economic shocks,

and along with state (linear) trends offers a more complete control for spatial heterogeneity in differential employment patterns. Saturation concerns, inter al., led NSW to recommend the use of a synthetic control approach to the estimation of treatment effects. Interestingly, the synthetic control estimator methodology suggested by Abadie, Diamond, and Hainmueller (2010) has come to be regarded by all analysts as an important complement to approaches seeking to avoid confounding effects of heterogeneous patterns in low-wage employment that are coupled with the selectivity of states that have introduced wage minima. At issue are the results of incorporating synthetic controls for minimum wage effects and the overlap between synthetic and local controls (see, in addition to NSW, Allegretto, Dube, Reich, and Zipperer, 2013; Dube and Zipperer, 2013; Sabia, Burkhauser, and Hansen, 2012).

This brings us to the second major criticism of the use of state-specific trends, linear or otherwise. In a recent paper, Meer and West (2013) have argued that it is inherently more likely for the effects of minimum wage hikes to be reflected in employment dynamics than in employment levels. They also argue that the inclusion of state-specific time trends in these circumstances as a control will attenuate estimates of the effect of minimum wages on employment levels. The theoretical reasoning is obtained from a Diamond-type worker search and matching framework in which transitions to a new employment steady state may be slow.⁵ The practical reasons are two-fold. First, staggered minimum wage increases may mean that an increase in the counterfactual's minimum wage may quickly erode the gap opened up by a particular wage hike. This might suggest that there is no consistent control group in the long run. In any event, in such staggered circumstances, there is a limited time interval in which to identify the impact of minimum wages on employment levels, which problem will be compounded if minimum wages initially operate on flows and hence do not affect employment in a discrete manner. Second of all, and more important, if the true effect of policy is to change the slope for an outcome variable rather than its

⁵ Interestingly, the Meer-West model rests on a similar search-theoretic reasoning to that employed by protagonists of the argument that minimum wages will not adversely impact employment because of improved matching in the labor market, although they themselves accept that negative effects will win out because of a differentially reduced rate of job growth.

level then the mechanics of the state-specific time trend approach can introduce biases. Specifically, any post-treatment deviation in employment growth caused by the treatment will attenuate an estimated static treatment effect if the specification includes a single trend for the pre- and post-treatment periods. Meer and West use both a stylized model and a Monte Carlo simulation – in both of which scenarios the minimum wage is related to the job growth rate but where there is no discrete change in the level of employment – to illustrate the attenuation problem.

Meer and West implement a state panel difference-in-differences specification in which variables reflecting employment dynamics – the job growth rate, and (its components) the logs of job creation and destruction – as well as employment levels themselves are regressed on the log of state employment, the share of the state population aged 15 to 59 years, and the log of annual real gross state product per capita in specifications controlling for state fixed effects, region-specific time effects, and state-specific linear trends. Three data sets are used in the inquiry – Business Dynamics Statistics, the QCEW, and the Quarterly Workforce Indicators – together covering the period 1975-2012. Across all three datasets, it is reported that job growth is strongly reduced by increases in the minimum wage – the main stimulus being reduced job creation rather than destruction. On the other hand, employment levels appear unrelated to minimum wages in the quarterly data across all specifications, and for annual data any statistically significant negative policy coefficient does not survive the incorporation of state-specific time trends – even if differential employment growth rates ultimately (after five years) translate into a large decrease in overall employment. This pattern of results is consistent with Meer and West’s expectations that geographic-specific trends in employment-level regressions can mask the effects of minimum-wage changes.

IV. Response

In Addison, Blackburn, and Cotti [ABC] (2012) we estimated employment and earnings equations for the restaurant-and-bar sector using the QCEW for the period 1990-2005. Our sample comprised a balanced panel of 1,825 counties, providing some 116,800 quarterly observations. Our basic empirical model regressed the log of employment (and earnings) on the log of the minimum wages, and a vector of supply and demand factors (viz. population, total employment, total average weekly earnings, the unemployment rate and the enrolment rate), while controlling for fixed county and fixed time effects. With these data, the standard panel regressions provided statistically significant positive minimum wage coefficients in the earnings equation and statistically significant negative minimum wage coefficients in the employment equation. Familiarly, with the addition of county-specific trends the significance of the earnings result was unaffected but the coefficient for the minimum wage though still negative was now very small and statistically insignificant.

(Table 1 near here)

Although we considered potential shifts in the regression model's employment trend more directly – by incorporating a new variable that allowed the trend to shift when a county's minimum wage was above the federal minimum wage – we did not consider at that time any other modifications, including those suggested by NSW. In response, Table 1 now re-estimates the ABC employment equation implementing the first procedure suggested by NSW, namely to allow the state-specific (here county-specific) trends to be of a higher order than linear. Specifically, second-, third-, fourth-, and fifth-order polynomials are considered in Table 1, preceded by specifications that first exclude county-specific trends and then include them in a linear form.⁶ The use of higher-order trends in two instances serves to render the small estimated minimum wage effect statistically significant. Interestingly, the coefficient estimates for the other regressors are little changed by polynomial detrending with the exception of findings for the

⁶ These latter results differ very slightly from those reported in ABC, as we now exclude the enrolment rate as a control (whose inclusion has been criticized as it may itself be a function of the minimum wage).

unemployment rate variable in the last two columns of the table (the signs of which are now perverse). Overall, however, the results of this first exercise are decidedly mixed and the suggested minimum wage elasticities quite modest.

(Table 2 near here)

Table 2 takes up NSW's other suggestions. The first column of the table provides summary results for the minimum wage argument when the county-specific trend is estimated using only the data for an interval that nets out the recession years at the beginning of the sample period, and then uses these trend estimates to detrend the data for the full sample period. Use of this revised single trend estimate is inconsequential in our case: the coefficient estimate changes from negative and insignificant to positive and insignificant. The next two columns of the table show results for alternative detrending of the data. Calculating the trend in each variable as a linear spline between business cycle peaks (as in NSW, from 1990Q3 to 2001Q1) also yields a small positive and statistically insignificant minimum wage coefficient. Passing each data series by county through a Hodrick-Prescott filter does yield a marginally significant negative coefficient estimate for the minimum wage regressor, but the estimated effect remains small (an elasticity of -0.04).

(Table 3 near here)

In the above exercises we use the same interval (1990-2005) as in ABC so as to determine the sensitivity of the (minimum wage) results reported there to alternative representations of county-specific trends suggested by NSW. Next, we extend the QCEW sample period as far as we can – namely up to 2012 – recalling that the period examined by NSW is very similar (1990-2011Q2) albeit using a different sample and dataset (teens from the CPS). Table 3 replicates the procedures earlier employed in Tables 1 and 2. The sample size increases to 146,749 observations, though with a reduced balanced panel of 1,595 counties. What difference does allowing for a longer sample period make? Perhaps the first observation to be made is that running the standard two-way county panel model with just fixed effects for county and

time now provide no evidence of minimum wages impacting employment, whereas a small negative but marginally statistically significant coefficient estimate is obtained using a simple linear trend. Second, use of higher-order county-specific trends yields just one marginally significant minimum wage elasticity. All such coefficients are now less negative than for the linear trend and vis-à-vis their counterparts in Table 1. It is worth noting that this failure to support minimum-wage effects is not due to an increased imprecision of the estimates induced by the additional trend controls, as the standard errors are actually smaller with the higher-order trend polynomials. Third, turning to the lower panel of the table, we see that neither method that uses subperiods of the 1990-2012 period to estimate the county-specific linear trend yields statistically significant results. Finally, use of the Hodrick-Prescott filter does again lead to a small but marginally significant coefficient for the minimum wage, although on this occasion it is to all intents and purposes identical to that for the simple county linear-trend specification.

We next consider the second criticism of the now common practice of including geographic-specific trends, namely that their inclusion in the model serves to attenuate the measured effect of the minimum wage on employment by virtue of the true effect of policy being upon the rate of job growth. This seems to be essentially an argument that minimum wage effects may have lagged responses – Meer and West’s (2013) findings support their intuition that this is because minimum wages largely serve to lower the rate of job creation in the following time periods. A similar motivation would seem to lie behind Sabia’s (2009: 88) argument that state-specific trends in an employment model may “[reduce] potentially important identifying variation.” We can see two reasons why an empirical researcher might consider omitting a statistically-significant set of independent variables (in this case, geographic-specific trends) from a model estimating minimum-wage effects. One is that a significant collinearity problem is induced, but at least in our results this does not seem to be a concern – as consistent estimates of standard errors for the minimum-wage elasticities are generally not increased by the inclusion of county-specific trends. The other concern is that minimum wage changes cause the other independent variables to change, so that controlling for the effects of those variables masks the “total effect” of minimum wages.

This is Meer and West’s argument: minimum wages may be causing a fall in the trend in employment growth in areas raising the minimum wage, so that controlling for these underlying trends is inappropriate. While worth considering, we do not see this as a relevant argument in the current analysis – as we report in ABC, the downward trends in employment in states raising their minimum wages seem to be actually lessened after minimum-wage increases, rather than become more severely negative as Meer and West’s argument would imply.

(Table 4 near here)

As noted earlier, Meer and West do find a significantly negative minimum wage impact on job growth in models that allow for state-specific trends in the job-growth rate. Our own sense is that the particular specification that Meer and West estimate is somewhat hard to defend, as it implies a single minimum-wage increase will have a permanent effect on job growth. Nonetheless, these kinds of specifications where job-growth rates are a function of levels of variables are not uncommon, and likely able to pick up lagged effects in a parsimonious way relative to the less restrictive dynamic specifications one sees in the autoregression literature.⁷ So, as an attempt to explore the importance of Meer and West’s concerns in our data, we estimated similar models with our 1990-2012 data on restaurants and bars from the QCEW. We preface our findings in Table 4 by recalling that Meer and West did use the QCEW in some of their regressions, but their aggregation remained at the state (rather than the county) level, while they also chose to look at the broader-based *accommodation and food* sector rather than the more low-wage *restaurant-and-bar* sub-sector. Further, we will also use the more standard growth rate measure – the change in log employment – than the alternative job growth rate used by Meer and West, although our results are robust to using the latter measure. The first two columns of Table 4 present results in which employment growth is regressed on the levels of variables (also incorporating county-specific trends). In

⁷ Simple lag structures have been incorporated in several studies in the recent minimum-wage literature (e.g. via inclusion of a simple lagged minimum wage as an additional control), although our sense is that these embellishments are generally inconsequential in terms of conclusions of the studies.

contrast with Meer and West, however, our estimate of the job-growth regression provides tiny and statistically insignificant minimum wage coefficients.

Our own preference for addressing the concerns raised by Meer and West is to consider models that explain long-run changes in employment as a function of similar long-run changes in the independent variables. For example, consider a state that raises its minimum wage one time in the panel. An empirical model based on 4-year changes would then have that minimum-wage change showing up as potential employment change factor for each of the quarters in the corresponding 4-year period. With lagged effects we would expect at least some of those quarters in the following 4 years to have reduced employment, leading to a nonzero coefficient on the minimum-wage change variable. The more typical short-run quarterly differenced models would, on the other hand, miss these lagged impacts. As noted in ABC, one advantage of the differenced models is that they also difference out any static geographic-specific effects, and the inclusion of geographic dummies is equivalent to controlling for geographic-specific linear trends.

In ABC, we estimated such differenced models, but only considered one-quarter and four-quarter differences (in the latter case requiring any lagged effects to show up within a year). These estimations were similar to our non-differenced results in finding little supporting evidence of minimum-wage employment effects. Here, we consider the robustness of this finding to expanding the sample period to 2012, and considering even longer differences to allow for more significant lagged effects. As the longest difference we consider is 6 years, we maintain a consistent sample across these additional specifications by starting our estimation with observations beginning in 1996 rather than 1990. The second column of Table 4 reestimates the Meer-West growth-rate specification with this restricted time period, leading to a similar conclusion as with the full sample period. The next four columns report estimates from fully-differenced equations with differences measured over 1, 4, 16, and 24 quarters. In all of these cases, the estimated minimum-wage elasticities are small and statistically insignificant. In our focus of study, then, lagged minimum-wage effects do not seem to be of a concern.

Dube (2013) has also directly questioned Meer and West's employment growth equation. That is to say, he regresses employment change on levels of variables for two of the three datasets used by Meer and West (viz. the BDS and the QCEW). He broadly replicates the Meer/West result on aggregate, but claims that disaggregation – using the QCEW – only supports the employment growth result in manufacturing not in retail or accommodation and food services, although as a practical matter he annualizes the quarterly data used by Meer and West while using a more parsimonious specification that excludes state-specific time trends and business cycle controls. That said, Dube's final specification using a border matching approach including county pair specific year effects fails to reveal any significant association between net employment growth and the log of the minimum wage.

V. Conclusions

The debate on the impact of minimum wages is ongoing. Although a new consensus has not emerged, a glance at the conclusions of two main evaluations of the debate (viz. NSW and Allegretto, Dube, Reich, and Zipperer, 2013) and more particularly what they see as the components of a viable research agenda point in not necessarily dissimilar directions. We refer to the search for specifications that provide the most reliable counterfactuals and the potential benefits of a synthetic control approach in this regard.

Our focus has been to take seriously a number of criticisms that have been leveled against the use of state/county-specific trends since in the past criticism has proven constructive. A pertinent example is the common-sense suggestion that an environment of deep recession might well produce clearer evidence of disemployment that has been reported in much of the modern minimum wage literature. In Addison, Blackburn, and Cotti (2013) we focused on two high-risk groups over the years 2005-2010 and while the evidence for a general disemployment effect was not uniform our estimates did suggest that the presence of negative minimum wage effects in states hardest hit by the recession. In the present treatment, we have taken seriously two sets of other criticisms of the state-specific trends approach while continuing to focus

on a high-risk group – here employees in the restaurant-and-bar sector – but without being tied to looking at region-specific time effects in conjunction with state-specific trends. Our results, however, do not serve to dislodge the persistent finding of considerably low (and possibly zero) minimum-wage elasticities in the restaurant-and-bar sector. In one sense however that particular battle may have already been won, as David Neumark and his colleagues now admit that “similar analyses of restaurant employment in the QCEW are a bit more mixed” (NSW, 2013: 46). We think it will be difficult to overturn this finding, but this conclusion should not be used to argue that minimum wage effects are ‘always and everywhere’ of this magnitude or for that matter as offering support of the conclusion that there are “no detectable employment losses from the kind of minimum wage increases we have seen in the United States” (Dube, Lester, and Reich, 2010: 962).

Also our findings might again stimulate research into concerns having to do with the effects of minimum wages on hours (reduction), non-wage benefits, and training as well as along some other margins of adjustment as suggested by Hirsch, Kaufman, and Zelenka (2011). And although we did not on this occasion find any great support for the argument that state-specific time trends serve to attenuate the measured effects on employment levels, the notion that minimum wages might have an effect on employment dynamics (including firm births) merits further exploration, building on the work of Portugal and Cardoso (2006).

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Table 1
Employment Equations for the Restaurant-and-Bar Sector, 1990-2005, Polynomial Detrending

	<i>Order of Polynomial for County-Specific Trends</i>					
	<i>No Trends</i>	<i>1st</i>	<i>2nd</i>	<i>3rd</i>	<i>4th</i>	<i>5th</i>
Log(Minimum Wage)	-0.101** (0.039)	-0.006 (0.033)	-0.051*** (0.014)	-0.041 (0.027)	-0.062* (0.033)	-0.046 (0.033)
Log(Average Wage)	-0.139*** (0.048)	-0.129*** (0.036)	-0.116*** (0.032)	-0.097** (0.038)	-0.089** (0.040)	-0.079** (0.043)
Log(Total Employment)	0.596*** (0.053)	0.770*** (0.061)	0.776*** (0.081)	0.824*** (0.097)	0.849*** (0.109)	0.869*** (0.120)
Unemployment Rate	-0.001 (0.002)	0.001 (0.002)	0.001 (0.001)	0.002 (0.001)	0.003* (0.002)	0.004** (0.002)
Log(Population)	0.327*** (0.101)	0.289*** (0.066)	0.247* (0.136)	0.241* (0.133)	0.226* (0.125)	0.326** (0.150)

Notes: The dependent variable is the log of employment. The standard errors in parentheses are corrected to allow intra-cluster correlation in errors for all observations within a state. All regressions included fixed-effects for county and quarter. Regressions are weighted by the average population in the respective county. The sample size in all regressions is 116,800, for a balanced panel of 1,825 counties.

***, **, * denote statistical significance at the 0.01, 0.05 and 0.10 levels, respectively.

Table 2
Employment Equations for the Restaurant-and-Bar Sector, 1990-2005, Alternative Detrending Methods

	<i>Post-1993 Trends</i>	<i>Peak-to-Peak Trends</i>	<i>H-P Filter Trends</i>
Log(Minimum Wage)	0.001 (0.062)	0.027 (0.071)	-0.042* (0.023)

Notes: See Notes to Table 1. All equations include the same controls as in Table 1. Standard errors are block bootstrapped by state using 500 replications. *Post-1993 Trends* detrends all observations based on county-specific trends estimated over the 1994-2005 period. *Peak-to-Peak Trends* detrends all data based on county-specific trends estimated over 1990-Q3 to 2001-Q1. *H-P Filter Trends* are the filtered series from a county-specific application of a Hodrick-Prescott filter (smoothing parameter=1600) applied individually to each data series.

Table 3
Employment Equations for the Restaurant-and-Bar Sector 1990-2012, Various Detrending
Methods

		<i>Order of Polynomial</i>				
	<i>No Trends</i>	<i>1st</i>	<i>2nd</i>	<i>3rd</i>	<i>4th</i>	<i>5th</i>
Log(Minimum Wage)	-0.000 (0.035)	-0.040* (0.021)	-0.024 (0.018)	-0.035* (0.019)	-0.023 (0.014)	-0.010 (0.014)
		<i>Post-1993 Trends</i>	<i>Peak-to-Peak Trends</i>	<i>H-P Filter Trends</i>		
Log(Minimum Wage)		-0.038 (0.028)	0.058 (0.072)	-0.041* (0.021)		

Notes: All specifications include the same controls (and approaches to calculating standard errors) as in Tables 1 and 2. Sample size is 146,740 in all equations, for a balanced panel of 1,595 counties.

Table 4						
Differenced Employment Equations for the Restaurant-and-Bar Sector, 1990/1996-2012						
<i>Time Period</i>	<i>1990-2012</i>	<i>1996-2012</i>				
<i>Difference Length</i>	<i>1 quarter</i>	<i>1 quarter</i>	<i>1 quarter</i>	<i>1 year</i>	<i>4 years</i>	<i>6 years</i>
Log(Minimum Wage)	-0.007 (0.009)	-0.004 (0.010)	-0.005 (0.010)	-0.010 (0.008)	-0.014 (0.017)	0.008 (0.025)
<i>Specification of MW and other RHS vars.</i>	Levels	Levels	Differenced	Differenced	Differenced	Differenced
<p><i>Notes:</i> See Notes to Table 1. All specifications include the same controls as in Table 1, along with region/quarter fixed effects. The first two columns of results are based on specifications that also detrend the data at the county level. In the “levels” equation, the dependent variable is first-differenced but all right-hand-side variables are measured in levels. In the differenced equation, all variables are differenced over the same stated period. Sample size is 145,145 in the first column, and 108,460 in the other columns.</p>						