Did the minimum wage or the Great Recession reduce low-wage employment? Comments on Clemens and Wither (2016)

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Abstract

Clemens and Wither (2016) argue that the federal minimum wage increases over 2007-2009 significantly depressed employment by comparing the employment trends of low-wage workers in states that were "bound" and not bound by changes in the federal standard. I show that this research design negatively biases the estimates of employment effects of the minimum wage because unbound states were affected differently by the Great Recession and therefore do not provide a valid counterfactual. The differences are reflected in the distinct industrial composition between these two groups of states prior to the Great Recession, including the share of workers in construction. Consistent with this explanation, I find that the authors' baseline results are not robust to sectoral or geographic controls, which reduce the magnitude of the baseline point estimates by 35.6 to 62.7 percent. Moreover, their research design fails a placebo-based falsification test: using unbound states that did not face a significant minimum wage increase but were in regions with a prevalence of bound states, I reproduce the timing and scope of their estimated employment effects. I also show that industrial and spatial controls reduce the magnitude of the authors' supplementary estimates for younger workers by 43.2 to 97.3 percent.

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1 Introduction

Between 2007 and 2009, the federal minimum wage rose from $5.15 to $7.25 per hour, a 32.3 percent real increase.[1] At roughly the same time, the US labor market collapsed as the country entered the Great Recession. Housing prices peaked in 2007 and with their decline came the most severe employment contraction since the Great Depression.

Clemens and Wither (2016) argue that the federal minimum wage increases over this period caused large-scale declines in the employment of low-wage workers and, as a result, substantially contributed to overall employment losses during the recession and subsequent recovery. The authors arrive at this conclusion by comparing the employment changes of low-wage workers in what they call "bound" states – states with relatively low minimum wages that were bound by the federal increase to raise their minimum wages – and "unbound" states – those states which already had higher minimum wages and therefore generally did not face large increases due to the changes in the federal standard. In this paper, I show that this research design negatively biases the estimates of employment effects of the minimum wage because bound and unbound states had different industrial composition prior to the minimum wage increase and faced different employment shocks during the Great Recession.

For their primary set of results, Clemens and Wither (or CW, for brevity) use data from the 2008 panel of the Survey of Income and Program Participation (SIPP), largely restricting their focus to estimating the effects of the 2009 minimum wage increase from $6.55 to $7.25. With the longitudinal SIPP data, CW follow a group of workers who earned low wages during the August 2008-July 2009 period, prior to the July 27, 2009 federal minimum wage increase, and compare their employment changes in bound versus unbound states over the remaining months through July 2012.

The top panel of Figure 1 illustrates the change in the logarithm of the minimum wage between bound and unbound states, relative to a reference period of March 2009. Clearly, the low-wage sample in bound states faced a relative minimum wage increase over the study

[1] For the price index, see the CPI-U-RS at `http://www.bls.gov/cpi/cpiursai1977-2015.xlsx`.

period. Of course, at the same time, overall employment in the US was rapidly falling. The bottom panel of the Figure, which graphs the log difference of overall nonfarm employment from March 2009, shows that the timing of the disemployment effects estimated by CW, who find a large drop beginning in March 2009, aligns closely with the timing of recession. Using the headline estimates of the effect on the low-wage employment, CW estimate that federal minimum wage increase caused a decline of approximately 0.6 percentage points in the overall employment-to-population ratio. The authors use this estimate to calculate that the federal minimum wage increases were responsible for 12 percent of the fall in overall employment from 2006 to 2012.

Based on a reanalysis of CW, I find little support for these conclusions. The estimates reported in CW do not reflect the causal effect of the minimum wage on employment but, instead, are likely biased by incorrectly assuming that bound and unbound states would have faced similar economic shocks during the Great Recession in the absence of minimum wage increases. In particular, I find that CW's baseline results are not robust to industrial or geographic controls and that their research design fails an important falsification test.

The difficulties of estimating the causal effects of minimum wages on employment during the largest downfall since the Great Depression can in principle be overcome through successfully randomizing the policy treatment. Even though employment was falling rapidly during the 2009 minimum wage increase, as the bottom panel of Figure 1 shows, comparing bound and unbound employment can disentangle the causal effect of the minimum wage if bound and unbound states had the same counterfactual employment trend.

I present direct evidence that this assumption is false. Bound and unbound states were not comparable prior to the Great Recession and indeed faced different employment shocks during the downturn. Relative to unbound states, bound states had higher shares of construction employment prior to treatment, conditional on CW's baseline set of controls. The headline treatment effect estimate falls in magnitude by 35.6 percent after adding as an control the 2005-2006 construction share of state employment as a factor with time-varying loadings.

Including 2005-2006 shares for all industrial NAICS sectors (with time-varying loadings) as controls, the estimated treatment effect falls by 62.7 percent and is no longer statistically significant.

Figure 2 shows clearly that bound and unbound states are geographically clustered. Bound states occupy much of the South and Mountain regions of the United States, and unbound states are largely situated in the Northeast and on the West Coast. Failing to account for the non-random spatial pattern of minimum wage increases will tend to conflate regional shocks of the Great Recession with minimum wage increases. Consistent with this concern, I also show that CW's main results are sensitive to controlling for unobserved geographic heterogeneity. Including Census Region- or Division-specific time fixed effects greatly reduces the magnitude of the headline results by amounts very similar to including industrial controls. This is especially noteworthy given the widespread recognition in the minimum wage literature of the importance of controlling for regional shocks, as in Meer and West (2016) and Allegretto et al. (2016).

Relatedly, the research design used in CW fails a placebo-based falsification test. When I restrict the population to states that are not bound by the minimum wage increases, states that are in Census Regions or Divisions with a greater presence of bound states nevertheless experience differential employment falls in the target population. Unbound states do not significantly increase their minimum wages, by design, and so by comparing unbound states in "bound" and "unbound" Regions and Division of the country, one should estimate no differential employment change. In contrast, the placebo exercise obtains large, negative employment effects, recovering between 68.2 percent and 84.8 percent of the baseline estimate. The falsification test shows that, to a large extent, the disemployment estimates in CW reflect the effects of the Great Recession, which varied by region, as did the effective minimum wage increases.

Although CW provide clear evidence of a change in the wage distribution for the low-wage target population, I find, surprisingly, there is no discernible positive effect on the expected wages of their target population. I also show that the distribution of pre-treatment wages highlights another dimension along which the bound and unbound sample differs. The low-wage sample that CW considers has an average wage less than \$7.50 per hour in the pre-treatment period, which implies that the sample consists of disproportionately more sub-minimum wage workers in unbound states than in bound states, because almost half of the low-wage sample in unbound states already faced minimum wages of \$7.25 or more in the pre-treatment period. It is less clear, a priori, that minimum wage workers in bound states will respond comparably to sub-minimum wage workers in unbound states after a minimum wage increase.

CW additionally focus on the younger population and population without high school degrees using Current Population Survey (CPS) data. Using a similar framework, CW document economically large employment declines for these groups in response to the federal minimum wage increases during 2007-2009. I nevertheless demonstrate that omitted variables also negatively bias these results, as the negative employment effects reported for these groups in the CPS largely disappear after including industrial or geographic controls. The same 2005-2006 industry share controls that I incorporate above reduce the CPS employment effects from -0.023 to 0.002 for workers ages 16-21 and from -0.037 to -0.013 for workers ages 16-30 without high school degrees. None of the baseline estimates for either these two CPS samples or the SIPP low-wage sample is statistically significant after accounting for all pre-treatment industry shares or division-specific shocks.

Using the policy variation between bound and unbound states during the Great Recession is a sensible place to start that, ideally, would inform of us of the consequences of the minimum wage increases during that period. Unfortunately, the findings below imply that this research design gives misleading estimates of the effects of the policy. Much and perhaps all of the estimated disemployment effect estimated by CW for the 2007-2009 federal minimum wage increases reflect their inability to control accurately for the depth of the Great Recession.

2 Replication of the SIPP results

CW use individual-level SIPP microdata to compare the employment outcomes across what they call bound versus unbound states. Bound states had minimum wages less than \$6.55 at the beginning of 2008 and were therefore generally compelled to raise their minimum wages to \$7.25 during the federal increase on July 24, 2009. Given the timing of the SIPP 2008 panel, CW's SIPP sample consists of five years individual observations during August 2008 and July 2012. CW primarily examine a "target" group of workers whose average hourly earnings were less than \$7.50 per hour during August 2008 through July 2009. For the individual-level regressions for this low-wage sample, CW estimate a difference-in-difference style equation over the August 2008-July 2012 period:

$$\text{Employed}_{it} = \sum_{j} \beta_j \text{Period}_j \times \text{Bound}_s + X_{st} + \rho_s + \tau_t + \alpha_i + \epsilon_{it} \quad (1)$$

Period j refers to three periods just before and after the July 24, 2009 federal minimum wage increase: what CW calls "transition", "post 1" and "post 2" periods. The transition period runs from May 2009 through July 2009. Period Post 1 is August 2009 through July 2010. Period Post 2 is August 2010 through July 2012. The model includes state ρ_s, time τ_t, and person α_i fixed effects. The vector X_{st} consists only of a housing price index in CW's baseline specification (I explore alternative control sets below). The coefficients β_j therefore effectively compare employment differences in the bound and unbound states in transition, post 1, and post 2 periods relative to the entire pre-transition period, August 2008-April 2009.

The first column of Table 1 reports CW's key finding: employment fell significantly for the target group in the post-1 and post-2 time periods after the federal minimum wage increase to \$7.25. My replication, in the second column, closely matches CW's findings. For the estimate of β_{post2}, which I focus on in later tables and figures, I estimate a coefficient of -0.062 whereas CW estimate a similar coefficient of -0.066. Both are statistically significant at the level of one percent, where standard errors are clustered at the state level. The SIPP results reported in CW appear not to use sample weights provided by the SIPP. Because the SIPP oversamples households in low-income areas, I prefer using these weights in general and in the third column I show results from a weighted regression monthly survey weights.[2] The estimate for β_{post2} falls by less than five percent in magnitude after using sample weights, from -0.062 to -0.059, with no change in standard errors.

In addition to $\beta_{post1}$and β_{post2}, I report an estimate for $\beta_{transition}$ of -0.033. CW do not report estimates for $\beta_{transition}$ but the size of the coefficient I estimate is about half of the post-2 period employment fall that CW estimate with β_{post2}. CW call May 2009 - July 2009 the transition period because they observe that during this period the probability of being employed at a wage between $5.15-$7.25 begins to fall sharply, even though the federal minimum did not officially rise from $6.55 to $7.25 until July 24, 2009. It is not clear whether the fall in employment that I estimate prior to treatment ($\beta_{trans} < 0$) is either spurious or due to anticipatory effects of the minimum. The change in pre-treatment trends may also be due to the fact that minimum wages are changing throughout the entire period in some states, both during and prior to the transition period. To simplify the reporting of results, in the tables and figures that follow I generally report coefficients for β_{post2}.

3 Robustness of SIPP results to industrial and geographic controls

The results above demonstrate that employment for CW's target group in the SIPP sharply falls in response to minimum wage increases. This is clearly the case for the CW sample using the baseline set of controls – a state-level housing price index, and state, time, and person fixed effects – and in supplementary results CW show that their baseline estimates

[2] See the 2008 SIPP documentation at http://www2.census.gov/programs-surveys/sipp/tech-documentation/source-accuracy-statements/2008/final.-stamped.-survey-of-income-and-program-participation-(sipp)-2008-panel-source-and-accuracy-statement-for-longitudina.pdf

are robust to including state-time-varying controls such the overall state employment rate and income per capita. In this section I show that the main results estimated by CW are not robust to basic industrial or geographic controls. In short, the baseline weighted regression estimate for β_{post2} of -0.059 shrinks in magnitude by 35.6 percent, 62.7 percent, or 59.3 percent, respectively, when adding controls for pre-treatment construction shares of employment, all pre-treatment industry shares, or division-specific time fixed effects.

CW appropriately include in the baseline set of controls a time-varying state-level housing price index (HPI) to control for the severity of the Great Recession. Unbound states saw a more severe housing price fall, and failing to control for the resulting negative effect of the housing collapse on employment imparts a positive bias on the employment effect estimate. Without the state HPI index CW use, the estimate for β_{post2} is -0.040 rather than -0.059. However, conditional on housing prices, construction employment plays a larger role in bound states. Failing to control for bound states' larger construction share of employment negatively biases the estimated employment effects.

To demonstrate this, I use the Quarterly Census of Employment and Wages (QCEW) to construct the mean state-level industrial shares of private-sector employment during 2005-2006, which is prior to treatment in both the SIPP low-wage sample and also the CPS samples which I analyze in section 6. For industrial categories I use the ten high-level aggregations of NAICS categories called "supersectors."[3] I first show the correlation between bound states and pre-treatment construction shares, conditional on housing prices. Figure 3 divides the states into population-weighted thirds, those with low, medium, and high mean values of the HPI during the SIPP sample period prior to period post-1 and post-2. Bound states in low-HPI and high-HPI states had significantly larger shares of construction employment during the 2005-2006 period. Among low housing price states, bound states have 1.38 more percentage points of construction employment than do unbound states; among high states,

[3] I use aggregation level code 53 for the QCEW data available at http://www.bls.gov/cew/datatoc.htm. The ten NAICS supersectors are natural resources and mining; construction; manufacturing; trade, transportation, and utilities; information; financial activities; professional and business services; education and health services; leisure and hospitality; and other services.

the difference is 2.07 percentage points.[4]

Table 2 shows the robustness of the estimates for β_{post2} after adding alternative control sets. The first column contains the published results from CW, and the second column contains my replication using unweighted and weighted regressions. In the third column, I add as a control the pre-treatment construction share of employment interacted with time fixed effects. The treatment effect estimate falls from -0.062 to -0.046 without using sample weights and from -0.059 to -0.038 when using sample weights. Higher pre-treatment construction shares of employment in bound states led to lower employment and failing to account for this overstates the effect of minimum wages by 34.8 percent to 55.3 percent in the unweighted and weighted cases, respectively. Controlling for all pre-treatment industry shares further attenuates the estimates. In column 4, the estimates of β_{post2} fall to -0.040 (unweighted) and -0.022 (weighted) after including all pre-treatment industry shares interacted with time fixed effects. Because of oversampling in the SIPP, I prefer the weighted results, which implies that simply accounting for sectoral shocks reduces the magnitude of the estimate of β_{post2} by 62.7 percent.

One may be concerned that industrial controls do not capture all of the heterogeneity in counterfactual employment trends between bound and unbound states. In particular, Figure 2, which reproduces the map published in CW, clearly displays regional clustering of bound and unbound states instead of a uniformly distributed pattern of minimum wage increases. As a result, a standard two-way fixed effects model (1) used by CW with common time fixed effects may conflate regional shocks due to the Great Recession with the effects of minimum wage increases. To account for this possibility, I estimate a more saturated model

[4] These differences are statistically significant at the five percent level. There is not a statistically significant difference for mid-housing price states. Regressing the construction share on a bound indicator for low-, mid-, and high-housing price states yields coefficients (and standard errors) of 0.0138 (0.0049), 0.0055 (0.0097), and 0.0207 (0.0074). Alternatively, regressing the pre-treatment construction share interacted with a post-treatment dummy variable on the baseline set of controls in equation (1) obtains an estimate for β_{post2} of 0.014 with standard error 0.004.

$$\text{Employed}_{it} = \sum_j \beta_j \text{Period}_j \times \text{Bound}_s + X_{st} + \rho_s + \tau_{rt} + \alpha_i + \epsilon_{it} \tag{2}$$

where I allow the time fixed effects τ_{rt} to vary by four Census regions or by nine Census divisions. Incorporating region- and division-specific time fixed effects is an approach common in empirical labor economics (Autor 2003) and in the minimum wage literature in particular (Meer and West 2016, Allegretto et al. 2016).

The fifth column of Table 2 shows that including region-specific time fixed effects reduces the employment effect to -0.039 and -0.035 in the weighted and unweighted cases, respectively, similar to the effect size produced by including a control for pre-treatment construction shares. Division-specific time fixed effects further reduce the coefficients to -0.026 and -0.024, almost the same as including all pre-treatment industry shares as controls in the weighted regression. Notably, division-specific time-fixed effects do not worsen the precision of the estimates; if anything, the standard errors fall in the most geographically saturated model. Finally, the final column of Table 2 shows that the estimate from the model with division-specific time fixed effects is robust to adding construction shares as controls. The robustness of the division-specific time fixed effects model to construction industry controls further validates the ability of geographic controls to account for economic confounders, such as the depth of the recession, that may be correlated with minimum wage increases.

In short, the sensitivity of the baseline results to industrial and geographic controls suggests that low-wage groups in unbound states do not offer a valid counterfactual for bound states. Dynamic pictures of the estimated treatment effects further confirm the role of industrial heterogeneity and geographic shocks in explaining differences in employment between bound and unbound states. Figure 4 follows the dynamic analysis in CW and estimates the equation

$$\text{Employed}_{it} = \sum_t \beta_t \times \text{Bound}_s + X_{st} + \rho_s + \tau_t + \alpha_i + \epsilon_{it} \tag{3}$$

allowing the treatment effect β_t to vary across all time periods t in the sample. I follow CW and use March 2009 as a reference period, so that estimates of β_t capture the conditional employment difference between bound and unbound states relative to March 2009.

The top panel of Figure 4 shows the baseline dynamic employment effects, clearly illustrating the employment begins to fall in the transition period and remains negative throughout the entire post-1 and post-2 time periods (demarcated by the dark dashed lines. Controlling for pre-treatment industrial shares, in the middle panel, dramatically changes the picture: the employment effects remain around zero until the beginning of the post-2 period, at which point employment significantly falls, but then rises back up to positive territory by the end of the sample. Division-specific time fixed effects in the final panel of of Figure 4 confirm that there is no sustained, negative employment effect of the 2009 minimum wage increase. Overall, the dynamic evidence illustrates that when we consider the "longer run" evidence by focusing on the final year of the sample, both sectoral and geographic controls suggest essentially no impact on employment.

I also assess how well additional controls capture the differences in employment differentials for the low-wage sample around the time of the Great Recession by predicting employment for the low-wage target group using industry share coefficients from the model that includes pre-treatment industry shares. Figure 5 compares the dynamic estimates from CW's baseline model (3), in dark blue, with the bound-unbound difference in predicted low-wage employment using the predicted values from pre-treatment industrial shares, in light blue. To calculate the latter, I first estimate the dynamic model with pre-treatment industrial shares interacted with time fixed effects $I_s \times \tau_t$, and then I predict individual-level employment $\hat{Y}_{it}$ based solely on the estimates for $I_s \times \tau_t$. Figure 5 shows the sample-weighted mean difference $\hat{Y}_{bound,t} - \hat{Y}_{unbound,t}$ relative to the same difference in the March 2009 reference period. If industry shares did not explain the differential pattern of employment between bound and unbound states, we would expect the industry-predicted employment differential to hover around zero for the post-treatment period. Instead, the industrial share-predicted employment aligns closely with the baseline model. Strikingly, the entirety of the fall in employment just before and during the transition period (May through July 2009) suggested by CW's baseline model is actually due to sectoral shocks and is thus not a causal consequence of the minimum wage. The only deviation from the baseline model occurs for about one year during 2010, after which the two predicted employment series again converge. The different pre-treatment industrial composition of bound and unbound states explains a substantial portion of the timing of the differential employment fall around the time of the minimum wage increase.

CW do include some specifications that control for industrial composition, described in their Appendix 2.1 and presented in their Appendix Table A.7. First, they show that their baseline employment effects are somewhat larger in magnitude when they add controls for time-trends based on the modal pre-treatment industry of their low-wage sample observations (as well as adding trends in low-wage population demographics). In results not shown I also confirm that industrial differences in bound and unbound *low-wage* samples do not play a key role.[5] Second, CW also use pre-treatment industry shares of overall state employment along with national employment changes to predict time-varying overall employment for each state, in the manner of Bartik (1991). Adding this control along with other macroeconomic controls does not change their main results.[6] The difference between this strategy and what I report above is that, above, I am directly using the industry shares of the overall private-sector employment to predict employment in the low-wage sample. By contrast, CW are using pre-treatment industry shares to predict overall employment and then using their measure of predicted overall employment covariate to predict low-wage employment. My use of interacting pre-treatment industrial shares with time dummies immediately captures,

[5] Regressing the *low-wage* pre-treatment construction share interacted with a post-treatment dummy variable on the baseline set of controls in equation (1) obtains an estimate for β_{post2} of 0.004 with standard error 0.007. I also find that controlling for differences in the pre-treatment industrial composition of the SIPP low-wage sample has little effect on the point estimate of β_{post2}, when I use interactions with time fixed effects of the pre-treatment means of nine high-level industrial dummies of the low-wage sample.

[6] It is not possible to separate out the exact contribution of this covariate because the specifications reported by CW that incorporate this Bartik-share predicted employment also include time-varying controls of actual state employment per capita, state income per capita, ARRA stimulus per capita, and a predicted state income variable.

for example, the extent to which exposure to certain industrial shocks, such as construction, transmits negative effects to the low-wage workforce. Furthermore, my construction of industry-share-predicted low-wage employment has clear explanatory power, as it successfully reproduces a nontrivial portion of the differential low-wage employment fall depicted in Figure 5.

4 Geographic placebo tests for the SIPP

Simple geographic placebo tests also confirm that CW's baseline regression conflates minimum wage increases with regional employment shocks, negatively biasing their estimates of the employment effects of the minimum wage. In this section, I focus on the unbound states, which by construction do not receive treatment, and assign them a placebo increase in the minimum wage if they reside in a Census division or region where a majority of the population lives in actually bound states. Although the placebo-based employment effect estimates for unbound states should be zero, they are in fact significantly negative, recovering 68.2 percent to 84.8 percent of the baseline estimates for β_{post2} depending on the specification.

For the placebo exercise, I label each of the four Census regions r as placebo-bound based on whether a majority of the CW low-wage population in that region is in a bound state in the pre-treatment period. The South is the only region-bound region, since the share of the population in the South that is bound is 85.6%, with the remaining three regions having bound shares of the population of 35.0% or less. Then, for the 23 actually unbound states, I estimate the modified equation

$$\mathrm{Y}_{it} = \sum_j \beta_j \mathrm{Period}_j \times \mathrm{PlaceboBound}_r + X_{st} + \rho_s + \tau_t + \alpha_i + \epsilon_{it} \tag{4}$$

Because I estimate this equation for only the unbound states, where, as I confirm, minimum wages are little changed, a negative estimate for β_j from equation (4) suggests that CW's main specification conflates minimum wage effects with omitted regional shocks. In addition to the region-based placebo exercise, I also perform the analogous Census-division-based experiment, estimating $\sum_j \beta_j \text{Period}_j \times \text{PlaceboBound}_d$, labeling each of the nine Census divisions d as placebo-bound if a majority of the CW low-wage sample in that division is in a bound state in the pre-treatment period. In this case, the South Atlantic and Mountain regions are the division-bound areas containing unbound states. Below I estimate this equation when the outcome Y_{it} is either the employment indicator or the logarithm of the individual's state minimum wage.[7]

One difficulty with designing a placebo test using unbound states is that minimum wages are changing throughout the sample period in these states as well. The top panel of Figure 1 shows that in January 2012, many unbound states increase their state-level minimum wages relative to the bound states, when the latter are mostly following the lower, federal standard. To minimize the occurrence of unbound states increasing their minimum, my main placebo specification limits the sample to data prior to 2012. The new sample therefore comprises August 2008 - December 2011, but results for the full sample are similar, which also I discuss below.

Table 3 shows estimates of β_{post2} where the outcome is the logarithm of the state minimum wage in column 1, and where the outcome is employment in column 2. For purposes of comparison, in the first row I estimate the original, baseline specification (1) on the full sample of bound and unbound. In the pre-2012 sample, the minimum wage is 8.0 log points higher in bound states in the post-2 period and employment fell by 6.6 percentage points, very similar to the baseline estimates for the full time period.

The second row of Table 3 limits the sample to unbound states and performs the placebo test estimating equation (4) using the region-based PlaceboBound_r treatment. Among

[7] In the region-based placebo exercise, the South is region-bound, and the other three regions, the Northeast, Midwest, and West, are not region-bound. In the division-based placebo exercise, the South Atlantic and Mountain divisions are division-bound; New England, Middle Atlantic, East North Central, West North Central, and Pacific divisions are not division-bound; and East South Central and West South Central are excluded because they do not contain any actually unbound states. Of the actually unbound states, the region-bound states are the District of Columbia, Florida, and West Virginia, and the division-bound states are Arizona, Colorado, District of Columbia, Florida, and West Virginia.

unbound states, the region-bound states do not see a significant increase in the minimum wage (1.4 log points), but do see a large fall in employment for the low-wage sample (4.5 percentage points). The placebo employment effect is larger in magnitude using a division-based PlaceboBound_r treatment, reported in the third row. Among unbound states, there is little change in the minimum wage for division-bound states, but employment falls by 5.6 percentage points. The region-based and division-based placebo effect estimates are, respectively, equal to 68.2 percent and 84.8 percent of the baseline employment effect estimate of -0.066.

Figure 6 estimates the dynamic version of the baseline model (1) and the placebo-based model (4) using the region-bound treatment. The timing of the employment fall is remarkably similar for the baseline treatment on the original sample and the placebo-based treatment on the unbound states. Employment falls sharply just prior to the transition period (indicated by the light grey, vertical line) and during the transition period in both the actual baseline specification and in the placebo specification. Because the placebo test recovers both much of the magnitude of the original employment and also the timing of the employment decrease, the large, negative employment effects reported by CW are likely to be spurious, failing to account for the non-random assignment of bound and unbound states.

As unbound minimum wages do increase throughout the sample, particularly in January 2012, Table 3 and Figure 6 limited the estimation sample to data prior to 2012, or August 2008 - December 2011. Appendix Table A1 and Appendix Figure A1 show similar placebo results for the full sample, August 2008 - July 2012. The region-based and division-based placebos recover 76.3 to 105.1 percent of the baseline employment estimate. Minimum wages do rise a small amount in region-bound and division-bound states, so that the placebo exercise with the full sample of data through July 2012 shows a significant, but small effect of placebo treatment on the minimum wage (2.3 to 2.4 log points). For this reason, and because the actual and placebo-based employment results are similar for the full and abbreviated sample prior to 2012, I prefer the placebo results of Table 3 and Figure 6 using the abbreviated sample that excludes the seven months of 2012.

5 Wage effects and distributions in the SIPP

Depending on the specification, some of the estimated disemployment effects for the CW sample are quite large, so it is natural to ask what exactly is the implied trade-off between employment and raising wages? CW document a clear change in the wage distribution of the CW sample around the $7 range, where one would expect to see the largest changes. To assess the size of the wage gains, I regress the logarithm of the hourly wage on the log minimum wage along with the same covariates in baseline specification (1). Table 4 reports these wage elasticities by affected group, along with specifications including pre-treatment construction and industry shares as well as region-specific and division-specific time fixed effects.

Surprisingly, rising minimum wages do not actually appear to be raising average wages of the CW low-wage sample. In row 1, wage elasticities for the CW sample are statistically significant and negative, ranging from -0.930 to -0.630. Wages in the CW sample are falling, not rising, after the minimum wage increase. The fact that hourly wages do not increase in response to minimum wage increases casts further doubt on the causal role of the minimum wage in determining the negative employment effects estimated by CW. Moreover, it is difficult to rationalize why low-wage workers would not see an hourly wage increase. Even in the world where all workers are dismissed whose marginal product is less than the new minimum wage, we would expect to find a positive wage elasticity: with the employment reduction, still-employed workers would have a higher marginal product and wage.

Because these specifications include person fixed effects, one might interpret the negative wage elasticities not as falling wages but rather as declining wage mobility after a minimum wage increase. Person fixed effects limit the identifying variation to within-person differences over time, and one would normally expect wage growth over time for a given person, for example due to increasing work experience. However, this interpretation does not fully explain the negative wage impacts, for when I drop the person fixed effects, I still estimate negative wage elasticities for the CW low-wage sample. Row 2 of Table 4 shows estimates without person fixed effects ranging from -1.33 to -0.93.

I also try to uncover a positive wage effect using CW's framework by reducing the measurement error in reported hours worked when defining the target population. CW reasonably defines hourly wages as the reported hourly wage or, when workers do not report an hourly wage, as weekly earnings divided by weekly hours. In row 3 of Table 4, I try to reduce measurement error by focusing on reported hourly wage observations. Here I alternatively define the target population to be those with a mean pre-treatment wage less than \$7.50/hour, but where the wage is the reported hourly wage, excluding non-hourly worker-observations from the calculation of the mean pre-treatment wage (but not necessarily from the regression sample). The estimated wage elasticities in this sample the point estimates for these specifications in row 3 are small and close to zero—no longer clearly negative but nonetheless inconsistent with substantial wage gains for those low-wage workers who have jobs.

Perhaps related to the difficulty of finding an average wage effect in the CW low-wage sample is the problem that sample consists of a large population of sub-minimum wage workers disproportionately from the bound states. CW defines low-wage workers as those whose average hourly wage across months in the pre-treatment period is strictly less than \$7.50/hour. However, during the pre-treatment period, the mean statutory minimum wage in the bound states was \$7.35/hour, whereas in in the unbound states, the mean minimum wage was \$6.58. As a result, the CW low-wage sample contains a substantial number of observations with wages less than the statutory minimum, particularly for the unbound states.

Table 5 divides the pre-treatment observations with positive wages in the CW low-wage sample into bound and unbound states, and into groups earning strictly below 90%, between

90% and 120%, and above 120% of their respective state minimum wages. In the CW low-wage sample, 44.1% of the observations in unbound states earn below 90% of their minimum wage, compared to 26.3% earning sub-minimum wages in bound states. It is not clear that we should expect the bound and unbound samples to respond similarly to labor demand shocks when many more bound state workers are in fact sub-minimum wage workers. The particular definition of the CW sample causes sub-minimum wage workers to comprise about one-third of the entire regression sample and on this dimension, low-wage workers in CW's sample do not appear to be comparable to each other across bound and unbound states.

6 Replication and robustness of CPS results

In a supplement to the main findings using SIPP data, CW use the Current Population Survey (CPS) and a similar difference-in-difference framework to estimate the effects of the full set of July 2007, July 2008, and July 2009 federal minimum wage increases on individual employment. Two of their most striking findings using the CPS are severe differential employment declines in bound states for the young population ages 16 to 21 and for younger individuals ages 16 to 30 without a high school degree. In this section, I show that these results are not robust to industry-based and geographic controls, like the prior results using the SIPP.

CW expand the sample period to 2006-2012 for this evaluation, which is not possible with the SIPP 2008 panel used in the previous analysis. Similar to before, CW estimate the same linear probability model

$$\text{Employed}_{it} = \sum_j \beta_j \text{Period}_j \times \text{Bound}_s + X_{st} + \rho_s + \tau_t + \epsilon_{it} \tag{5}$$

where now period j refers to three slightly different periods: in this case CW call the May 2007-July 2009 the "transition" period, during which minimum wages were being raised, and the same "post 1" (August 2009-July 2010) and "post 2" (August 2010-July 2012) periods as before. In this specification, contrary to the SIPP sample, the transition period actually encompasses periods during which the federal minimum wage was changing. As before, in CW's baseline estimates the vector X_{st} consists only of a housing price index, but below I explore control sets that include the same sectoral-specific and geographic-specific time fixed effects.

Table 6 shows that I replicate the CW results exactly, confirming that employment appears to fall sharply in bound states for these groups along with the minimum wage increase. Although CW do not publish their coefficients on $\beta_{transition}$, reflecting the employment change in May 2007-July 2009 relative to the prior period, Table 6 shows that for the sample of individuals ages 16-21, the employment fall during the transition is 95.7% of the medium term effect described by β_2. For individuals ages 16 through 30, the effect during the transition is approximately 43.2% of the total estimated effect.

As with the low-wage sample in the SIPP, I show that these employment effects are not at all robust to industry-based or geographic controls in Table 7, which reports the estimates of the medium term effect β_2. Columns 3 and 4 report estimates incorporating as controls the same state-specific 2005-2006 industry shares (interacted with time fixed effects) used above in the SIPP reanalysis. Notably, a control for the construction share of overall state private-sector employment effectively kills the large, negative employment effects estimated by CW for the sample of individuals ages 16-21: the baseline point estimate falls by 65 percent from -0.023 to -0.008 and is no longer statistically significant at conventional levels. Controls for all industry shares as well as region- and division-specific fixed time effects have similar effects, as point estimates are very close to zero. None of the models with sectoral controls or more saturated geographic controls have statistically significant employment effects, but this is not because these models have less statistical precision, as standard errors across specifications are rather similar.

The baseline estimates reported in CW for the less-educated group of individuals ages 16-30 also fail robustness checks. All state-specific industrial controls (interacted with time fixed effects) reduce the baseline employment effect of -0.037 by 64.9 percent to -0.013. Geographic-specific time fixed effects again further attenuate the magnitude of the estimated employment effects. Because the baseline estimates reported by CW are particularly sensitive to industrial and regional controls, they should not be interpreted as the causal effects of the minimum wage policy.

7 Conclusion

The large number of federal, state, and local area minimum wage increases has blessed labor market research on the United States with a variety of minimum wage "experiments." The bad news, however, is that this variation is not uniformly distributed and is instead highly selective, which the spatial pattern of bound and unbound states in Figure 2 makes clear. The selectivity of minimum wage increases poses a challenge for policy evaluation in general, as illustrated by the debate between Neumark, Salas and Wascher (2014) and Allegretto et al. (2016), but the problem of constructing counterfactuals is particularly acute during an economic downturn. When employment is changing rapidly, specification errors can have large consequences.

CW attempt to deepen our understanding by focusing on the employment patterns of low-wage workers facing a clear increase in the minimum wage during the midst of the Great Recession. How labor markets respond differently to stronger wage floors during a downturn is an area of considerable importance for empirical research, as economic theory does not offer a clear answer. Are firms more labor demand-constrained than supply-constrained during recession, so that employment effects of the minimum wage become more negative when unemployment is already high? Or do stronger minimum wages help to stabilize consumer demand? In the empirical literature, the famous New Jersey-Pennsylvania comparison of Card and Krueger (1994) does not find a negative effect on fast-food employment after a minimum wage increase that occurred during the weak labor market following the 1990-1991 recession.

Allegretto, Dube and Reich (2011) also show no indication that teen employment elasticities of the minimum wage are different when unemployment is higher over the 1990-2009 period. In a study mostly limited to the 2005-2010 period, similar to what is examined in this analysis, Addison, Blackburn and Cotti (2013) estimate employment elasticities significantly more negative for teens in states harder hit by the recession. It would be interesting to explore whether the latter paper's findings are also sensitive to the issues raised in this paper's comments, controlling for low-wage employment shocks that were correlated with but not caused by the federal minimum wage increases.

Also important for future research is the consideration of alternative groups of individuals likely to be affected by minimum wage policies, as CW stress by defining a low-age target group based on pre-treatmenNovembert wages available in the longitudinal SIPP data. Many studies of US minimum wages focus on a single demographic group like teenagers or younger workers, who are disproportionately minimum wage earners but nonetheless a limited subset of affected individuals. Studying a population defined by those earning low wages prior to treatment has the clear advantage that it may encompass many demographic groups of workers who earn near the minimum wage – although, as I show above, the specific low-wage target group defined by CW consists of a large number of sub-minimum wage workers, with substantially more in unbound states. Another potential limitation not explored by this paper is that defining a target population based on actual wage histories may exclude labor market entrants from the sample. To the extent that a higher minimum wage increases the labor supply of those who were not earning wages in the first place, conditioning on prior wages will at best be a partial guide to the effects of the policy, as it will primarily capture exit and miss some entry into employment.

As for the results presented by CW, accounting for heterogeneity in employment patterns between bound and unbound states clearly attenuates the estimated employment effects. In the case of the SIPP low-wage sample, controlling for observed heterogeneity by using all industrial shares reduces the magnitude of the primary estimate by 62.7 percent. Controlling for unobserved spatial heterogeneity by focusing on within-Division variation has similar effects. In the case of the two samples from the CPS – younger workers and young workers with no high school degree – industrial and geographic controls greatly shrink the point estimates, leaving none statistically different from zero.

For all of these groups, it appears that federal minimum wage increases did not, contrary to the claims in CW, significantly depress employment. Although there may be much to learn from how labor markets adjust to a minimum wage during a recession, the main research design in CW does not disentangle the two forces. Both conventional controls for observed heterogeneity such as pre-treatment industrial shares and standard controls for unobserved geographic heterogeneity (such as those used in Meer and West 2016) point to much smaller and statistically insignificant employment effects of the 2007-2009 federal minimum wage increases.

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Table 1: Replication of SIPP employment effects in CW

	CW (2016)	Replication - unweighted	Replication - weighted
Bound × Post 1	–0.044**	–0.043**	–0.038**
	(0.019)	(0.018)	(0.018)
Bound × Post 2	–0.066***	–0.062***	–0.059***
	(0.020)	(0.018)	(0.018)
Bound × Transition		–0.033	–0.026
		(0.021)	(0.021)
Observations	147,459	147,558	147,558

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Each column reports the coefficients β_j of the $\text{Period}_j \times \text{Bound}_s$ terms from the baseline regression equation (1), where the outcome is employment. Column 1 contains the published results of Clemens and Wither (2016); they do not report $\beta_{transition}$. Columns 2 and 3 are my replications not using or using, respectively, the SIPP monthly sample weight `wpfinwgt`. Standard errors are clustered at the state level.

Table 2: Replication and robustness of SIPP employment effects in CW

	CW (2016)	Replication	Robustness to additional controls				
			Unweighted regression				
Bound × Post 2	–0.066***	–0.062***	–0.046**	–0.040**	–0.039**	–0.026*	–0.026*
	(0.020)	(0.018)	(0.018)	(0.017)	(0.017)	(0.013)	(0.013)
	147,459	147,558	147,558	147,558	147,558	147,558	147,558
			Weighted regression				
Bound × Post 2		–0.059***	–0.038**	–0.022	–0.035*	–0.024	–0.025
		(0.018)	(0.018)	(0.020)	(0.021)	(0.016)	(0.017)
		147,558	147,558	147,558	147,558	147,558	147,558
Construction share			Y				Y
Industry shares				Y			
Common time FE		Y	Y	Y			
Region × Time FE					Y		
Division × Time FE						Y	Y

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: The table reports the coefficient on the $\text{Period}_2 \times \text{Bound}_s$ term from the baseline regression equation (1), where the outcome is employment. Column 1 contains the published results of Clemens and Wither (2016), column 2 shows my replication, and the remaining columns add additional control sets. Industries are NAICS supersectors and any industry share is the share of total private-sector employment over 2005-2006; in the regression, these state-specific shares are interacted with time fixed effects. Weighted regressions use the SIPP monthly sample weight `wpfinwgt`. Standard errors are clustered at the state level.

Table 3: Actual and placebo treatment effects on the pre-2012 SIPP low-wage sample

	log(minimum)	Employment
Sample = All states, pre-2012	0.080***	–0.066***
Actual treatment =	(0.007)	(0.019)
Period_{j} × Bound State	127,293	127,293
Sample = Unbound states, pre-2012	0.014	–0.045**
Actual treatment =	(0.011)	(0.018)
Period_{j} × (Region > 50% bound)	57,698	57,698
Sample = Unbound states, pre-2012	0.014	–0.056**
Placebo treatment =	(0.011)	(0.024)
Period_{j} × (Division > 50% bound)	57,698	57,698

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: In Row 1, the sample is all states and the treatment variables are $\text{Period}_j \times \text{Bound}_s$ and in Row 2 and Row 3, the sample is unbound states and the treatment variables are $\text{Period}_j \times \text{Placebobound}_r$, where j is transition, 1, or 2. I report the coefficients on $\text{Period}_2 \times \text{Bound}_s$ or $\text{Period}_2 \times \text{Placebobound}_r$. All regressions are weighted using SIPP person-level weights `wpfinwgt` and standard errors are clustered at the state level. The sample is limited to data prior to 2012, as described in the text. In Appendix Table A1 I show the results from the full sample.

Table 4: Elasticities of SIPP wages with respect to the minimum wage, by model and sample

CW	–0.955***	–0.916***	–0.621***	–0.909***	–0.824***	–0.810***
	(0.274)	(0.303)	(0.183)	(0.293)	(0.252)	(0.251)
	94,148	94,148	94,148	94,148	94,148	94,148
CW, no person FE	–1.378***	–1.300***	–0.693***	–1.428***	–1.138***	–1.169***
	(0.405)	(0.475)	(0.237)	(0.432)	(0.280)	(0.292)
	94,199	94,199	94,199	94,199	94,199	94,199
CW, hourly wages	0.025	0.083	0.160	–0.082	–0.081	–0.042
	(0.171)	(0.176)	(0.151)	(0.186)	(0.166)	(0.161)
	77,032	77,032	77,032	77,032	77,032	77,032
Industry shares		Y				Y
Construction share			Y			
Common time FE	Y	Y	Y			
Region × Time FE				Y		
Division × Time FE					Y	Y

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: All models regress the log hourly wage on the log minimum wage and controls from the baseline regression equation (1), with additional control sets indicated at the bottom of the table. Industries are NAICS supersectors and any industry share is the share of total private-sector employment over 2005-2006; in the regression, these state-specific shares are interacted with time fixed effects. Each column denotes a separate sample or model. "CW" is the CW low-wage sample, and "CW, no person FE" is the same sample but excludes person fixed effects from the regression. "CW, hourly wages" defines the low-wage sample based on reported hourly wages only, not calculating hourly wages for workers who do not report being paid hourly. All regressions are weighted using SIPP person-level weights `wpfinwgt` and standard errors are clustered at the state level.

Table 5: Distribution of pre-treatment wages in the SIPP low-wage sample

	Unbound states	Bound states	All states
Less than 90% of minimum	44.1	26.3	34.0
90-120% of minimum	48.0	63.5	56.7
Above 120% of minimum	8.0	10.1	9.2
Total	100.0	100.0	100.0

Note: The table shows the share of pre-treatment observations within certain wage bins defined as relative to state minimum wages, for bound, unbound, and all states in the CW low-wage sample, limited to those with positive reported or calculated hourly wages. All values are weighted using SIPP person-level weights `wpfinwgt`.

Table 6: Replication of the CPS employment effects in CW

	Ages 16-21		Ages 16-30, less than high school	
	CW (2016)	Replication	CW (2016)	Replication
Bound × Post 1	–0.027***	–0.027***	–0.037***	–0.037***
	(0.009)	(0.009)	(0.011)	(0.011)
Bound × Post 2	–0.023**	–0.023**	–0.037***	–0.037***
	(0.010)	(0.010)	(0.011)	(0.011)
Bound × Transition		–0.022***		–0.016**
		(0.007)		(0.007)
Observations	894,384	894,384	580,248	580,248

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Each column reports the coefficients β_j of the $\text{Period}_j \times \text{Bound}_s$ terms from the baseline regression equation (5) for the CPS samples, where the outcome is employment. Columns 1 and 3 contain the published results of Clemens and Wither (2016); they do not report $\beta_{transition}$. Columns 2 and 4 are my replications. All regressions are weighted using the basic monthly sample weights and standard errors are clustered at the state level.

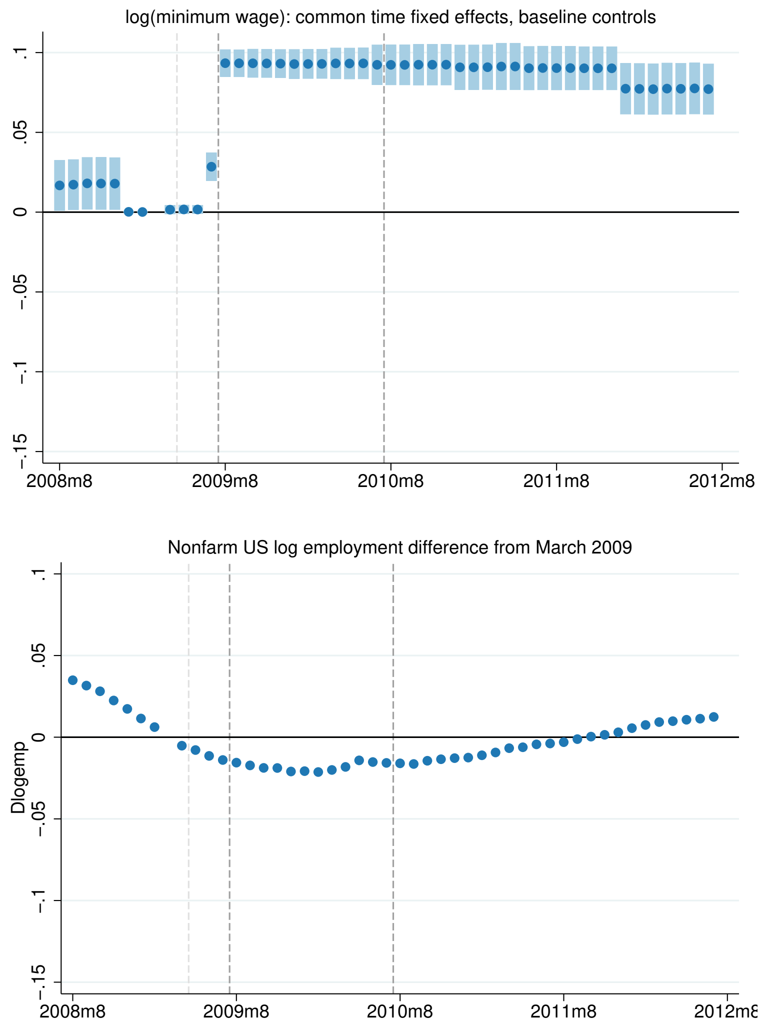
Table 7: Robustness of CPS employment effects in CW

	CW (2016)	Replication	Robustness to additional controls				
Ages 16-21	–0.023**	–0.023**	–0.008	0.002	0.003	0.005	0.005
	(0.010)	(0.010)	(0.009)	(0.008)	(0.010)	(0.010)	(0.009)
	894,384	894,384	894,384	894,384	894,384	894,384	894,384
Ages 16-30, LTHS	–0.037***	–0.037***	–0.021	–0.013	–0.003	0.001	0.002
	(0.011)	(0.011)	(0.013)	(0.013)	(0.017)	(0.015)	(0.013)
	580,248	580,248	580,248	580,248	580,248	580,248	580,248
Construction share			Y				Y
Industry shares				Y			
Common time FE		Y	Y	Y			
Region × Time FE					Y		
Division × Time FE						Y	Y

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

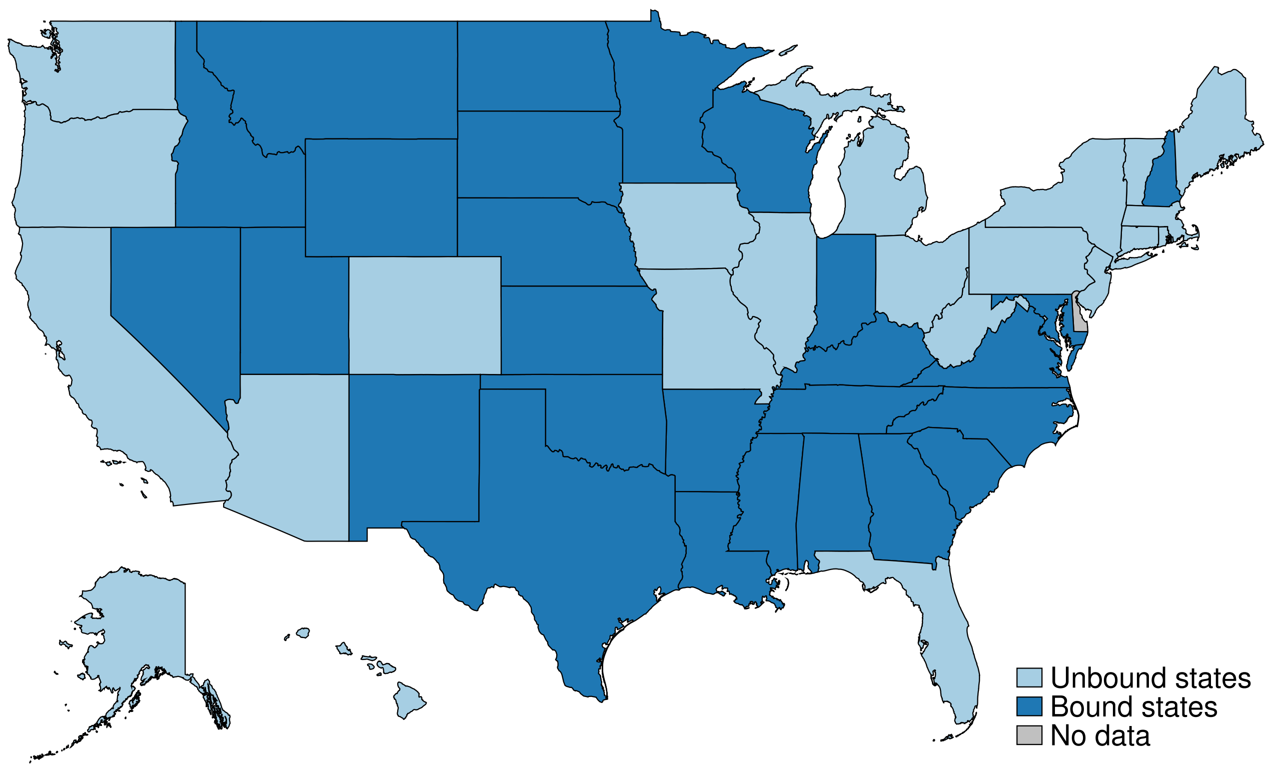
Note: The table reports the coefficient on the $\text{Period}_2 \times \text{Bound}_s$ term from the baseline regression equation (5) for the CPS samples, where the outcome is employment. Column 1 contains the published results of Clemens and Wither (2016), column 2 shows my replication, and the remaining columns add additional control sets. Industries are NAICS supersectors and any industry share is the share of total private-sector employment over 2005-2006; in the regression, these state-specific shares are interacted with time fixed effects. All regressions are weighted using the basic monthly sample weights and standard errors are clustered at the state level.

Figure 1: Estimated treatment effects the log of the minimum wage in the SIPP low-wage sample, and the change in overall US log employment relative to the March 2009 reference period



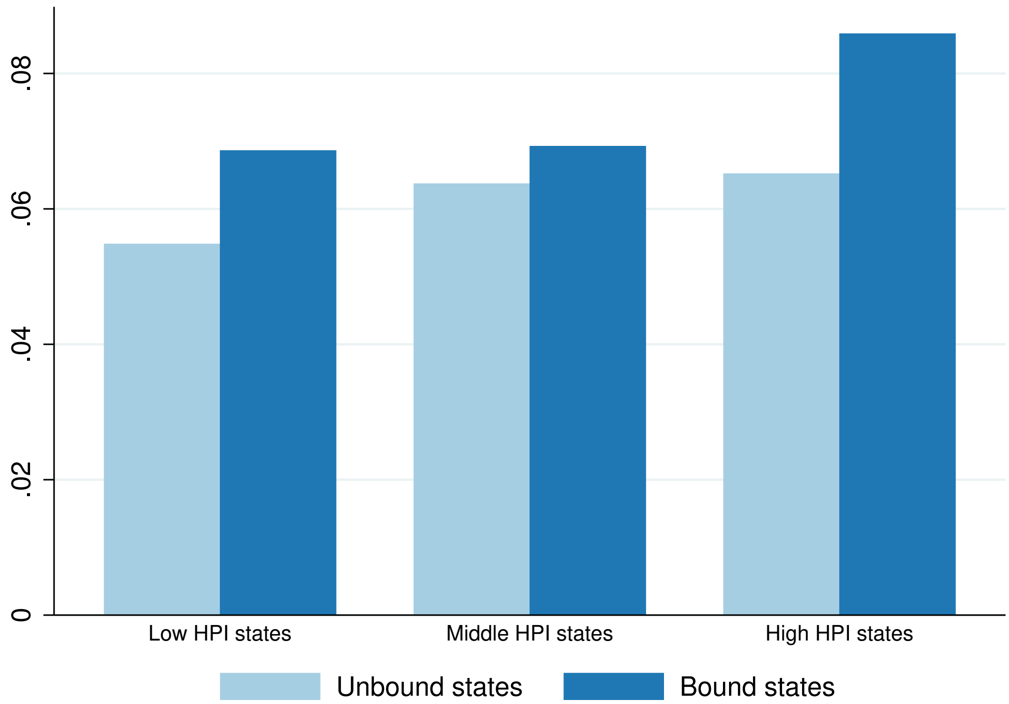
Note: The top figure reports the coefficient on the $\text{Period}_2 \times \text{Bound}_s$ term from the dynamic form of the baseline regression equation (1), where the omitted period is March 2009, the outcome is the logarithm of the minimum wage, and the sample is the CW low-wage sample from the SIPP. The SIPP regression is weighted using the SIPP monthly sample weight `wpfinwgt` and standard errors are clustered at the state level. Error bars are 95% confidence intervals. The bottom figure plots the log employment difference of total nonfarm US employment from its March 2009 value.

Figure 2: Regional clustering of bound and unbound states



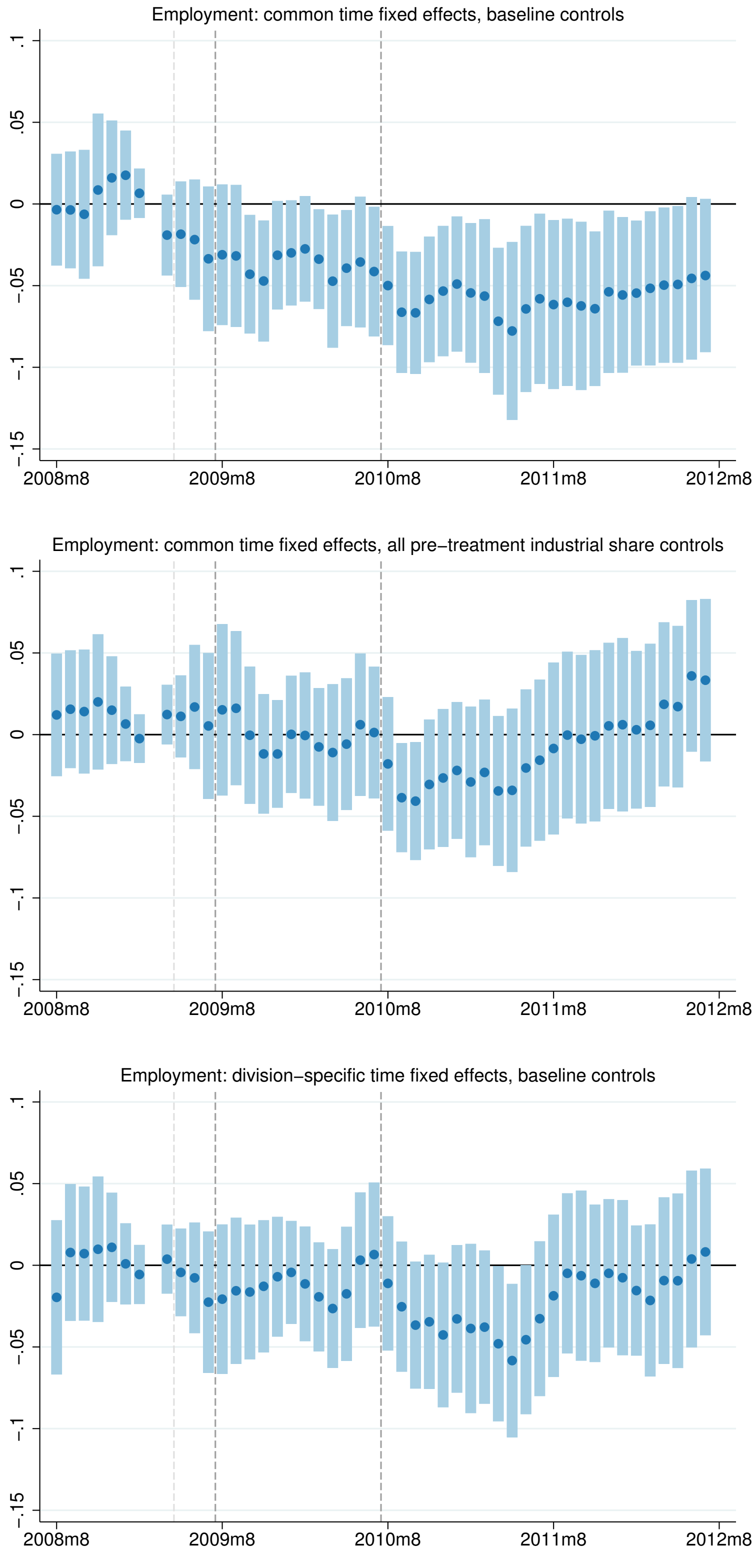
Note: CW define bound (or unbound) states as those with minimum wages less than (or greater than or equal to) \$6.55 in January 2008. There are no observations for Delaware in my replication of the SIPP low-wage sample.

Figure 3: Mean 2005-2006 construction shares of state employment for bound and unbound states, by mean pre-treatment housing price category



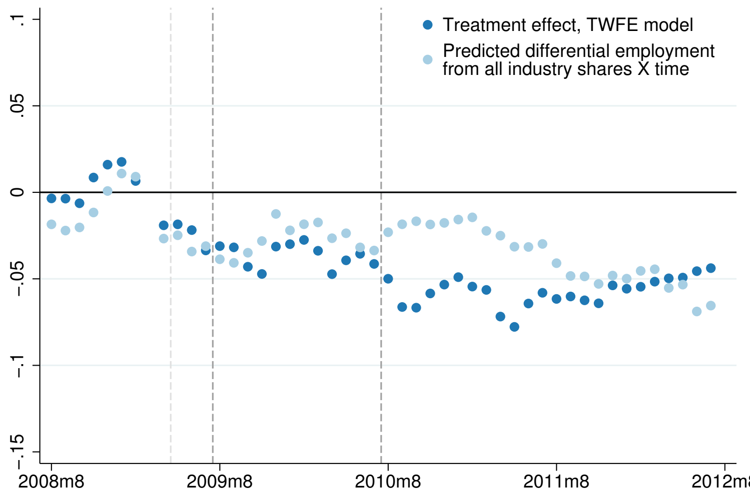
Note: State construction shares are the mean share of construction in private-sector employment during 2005-2006, prior to treatment in both the SIPP and CPS samples. The mean HPI for a state is calculated over the SIPP pre-treatment period and mean construction shares and housing price tertiles are weighted using the SIPP monthly sample weight `wpfinwgt`.

Figure 4: Dynamic employment effects for the SIPP low-wage sample



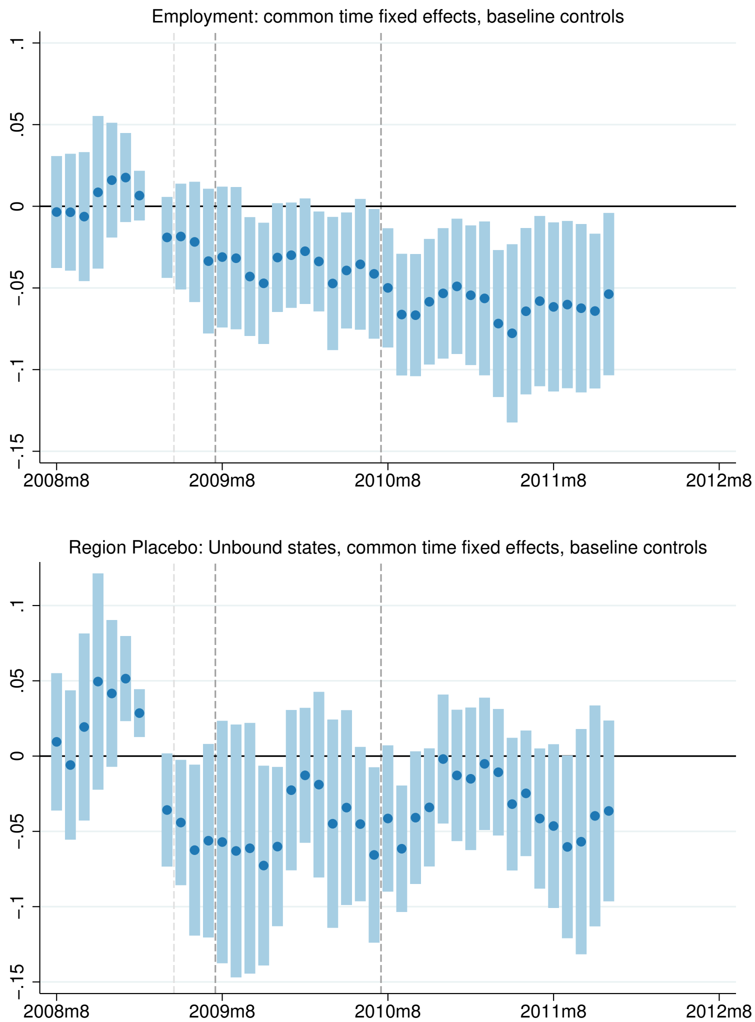
Note: The figures report the coefficient on the $\text{Period}_2 \times \text{Bound}_s$ term from the dynamic form of the regression equation (1), where the omitted period is March 2009 and the outcome is employment. All regressions are weighted using the SIPP monthly sample weight `wpfinwgt` and standard errors are clustered at the state level. Error bars are 95% confidence intervals.

Figure 5: Actual treatment effect for the SIPP low-wage sample and predicted employment differentials based on industrial controls



Note: The dark blue series is the coefficient on the $\text{Period}_2 \times \text{Bound}_s$ term from the dynamic form of the regression equation (1), where the outcome is employment. The light blue series is the bound-unbound difference in predicted low-wage employment using the predicted values of employment from pre-treatment industrial shares, as described in the text. In both cases the reference period is March 2009. All regressions are weighted using the SIPP monthly sample weight `wpfinwgt`.

Figure 6: Actual treatment effect and placebo-based treatment effect for the pre-2012 SIPP low-wage sample



*Note:*The top figure reports the coefficient on the $\text{Period}_2 \times \text{Bound}_s$ term from the dynamic form of the regression equation (1), where the omitted period is March 2009 and the outcome is employment. The bottom figure limits the sample to unbound states and reports the coefficient on the $\text{Period}_2 \times \text{PlaceboBound}_r$ term. All regressions are weighted using the SIPP monthly sample weight `wpfinwgt` and standard errors are clustered at the state level. Error bars are 95% confidence intervals. The sample is limited to data prior to 2012, as described in the text. In Appendix Figure A1 I show the results for the full sample.

Appendix Tables and Figures

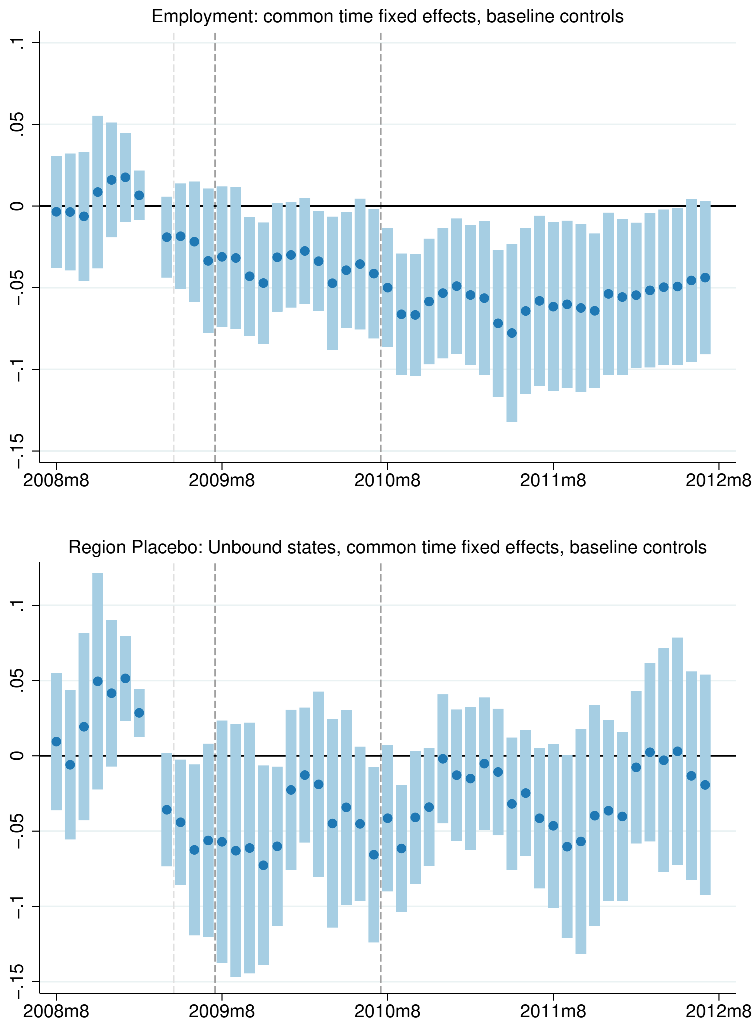
Table A1: Actual and placebo treatment effects on the full SIPP low-wage sample

	log(minimum)	Employment
Sample = All states	0.078***	–0.059***
Actual treatment =	(0.008)	(0.018)
$\text{Period}_\text{j} \times$ Bound State	147,558	147,558
Sample = Unbound states	0.023*	–0.045*
Actual treatment =	(0.012)	(0.023)
$\text{Period}_\text{j} \times$ (Region > 50% bound)	66,817	66,817
Sample = Unbound states	0.024*	–0.062**
Placebo treatment =	(0.012)	(0.027)
$\text{Period}_\text{j} \times$ (Division > 50% bound)	66,817	66,817

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: In Row 1, the sample is all states and the treatment variables are $\text{Period}_j \times \text{Bound}_s$ and in Row 2 and Row 3, the sample is unbound states and the treatment variables are $\text{Period}_j \times \text{Placebobound}_r$, where j is transition, 1, or 2. I report the coefficients on $\text{Period}_2 \times \text{Bound}_s$ or $\text{Period}_2 \times \text{Placebobound}_r$. Table 3 shows the results for the sample limited to data prior to 2012.

Figure A1: Actual treatment effect and placebo-based treatment effect for the SIPP low-wage sample



Note: The top figure reports the coefficient on the $\text{Period}_2 \times \text{Bound}_s$ term from the dynamic form of the regression equation (1), where the omitted period is March 2009 and the outcome is employment. The bottom figure limits the sample to unbound states and reports the coefficient on the $\text{Period}_2 \times \text{PlaceboBound}_r$ term. All regressions are weighted using the SIPP monthly sample weight `wpfinwgt` and standard errors are clustered at the state level. Error bars are 95% confidence intervals. Figure 6 shows the results for the sample limited to data prior to 2012.