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THE MINIMUM WAGE AND THE GREAT RECESSION: EVIDENCE FROM THE CURRENT POPULATION SURVEY

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Significant portions of text draw heavily on the text of Clemens and Wither (2014), which this paper supplements. This applies, in particular, to sections describing the minimum wage literature, the background associated with the minimum wage changes analyzed, the backdrop of this period's aggregate employment shifts, and the basic estimation framework. Special thanks to Michael Best, Marika Cabral, Joanna Lahey, Neale Mahoney, Day Manoli, Jonathan Meer, John Shoven, Michael Strain, Rob Valletta, and Stan Veuger, as well as to seminar participants at LSE and Marquette University, for valuable comments and discussions. Thanks also to the Stanford Institute for Economic Policy Research and the Federal Reserve Bank of San Francisco for their hospitality while working on this paper. Finally, special thanks to Jean Roth for greatly easing the navigation of the CPS-MORG files as made available through NBER. I gratefully acknowledge support from the UCSD Academic Senate through its small grant program. The views expressed herein are those of the author and do not necessarily reflect the views of the National Bureau of Economic Research.

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ABSTRACT

I analyze recent federal minimum wage increases using the Current Population Survey. The relevant minimum wage increases were differentially binding across states, generating natural comparison groups. I first estimate a standard difference-in-differences model on samples restricted to relatively low-skilled individuals, as described by their ages and education levels. I also employ a triple-difference framework that utilizes continuous variation in the minimum wage's bite across skill groups. In both frameworks, estimates are robust to adopting a range of alternative strategies, including matching on the size of states' housing declines, to account for variation in the Great Recession's severity across states. My baseline estimate is that this period's full set of minimum wage increases reduced employment among individuals ages 16 to 30 with less than a high school education by 5.6 percentage points. This estimate accounts for 43 percent of the sustained, 13 percentage point decline in this skill group's employment rate and a 0.49 percentage point decline in employment across the full population ages 16 to 64.

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The U.S. labor market recovered slowly in the Great Recession's wake. Between December 2006 and December 2009, the employment rate among individuals ages 16 to 64 declined from 72.2 to 66.4 percent. In December 2012 it stood at 67.2 percent and it has more recently settled between 68 and 69 percent. Declines were particularly large and prolonged among individuals with relatively low levels of observable skills. Between December 2006 and December 2009, employment among individuals ages 16 to 30 with less than a high school degree declined from 40 to 28 percent. Through 2014, this group's employment remained 13 percentage points (33 percent) below pre-recession levels.

Analyses of the recovery's sluggishness have considered a variety of potentially complementary explanations. Some work highlights structural factors like slow-moving demographic trends (Aaronson, Cajner, Fallick, Galbis-Reig, Smith, and Wascher, 2014). Other work points to long-unfolding declines in manufacturing and provides evidence that such trends were temporarily masked by the housing boom (Charles, Hurst, and Notowidigdo, 2013). Additional research emphasizes macroeconomic factors ranging from heightened economic uncertainty (Bloom, 2009; Baker, Bloom, and Davis, 2015; Shoag and Veuger, 2014) to "scarring" resulting from the recession's severity (DeLong and Summers, 2012).

A second set of candidate factors involves the landscape of policies with social insurance and/or redistributive objectives. Several papers consider the unemployment insurance extensions enacted during this period (Hagedorn, Karahan, Manovskii, and Mitman, 2013; Rothstein, 2011; Farber and Valletta, 2013). Additional analyses consider expansions of redistributive programs involving health insurance, food stamps, and disability benefits (Mulligan, 2012, 2014; Hall, 2014). Finally, Clemens and Wither (2014b) consider the role of contemporaneous minimum wage changes, which brought the federal minimum wage from \$5.15 in 2007 to \$7.25 in 2009. This paper supplements Clemens and Wither's (2014b) investigation of these minimum wage increases' impact on the employment of low-education, low-experience workers during this time period.

Clemens and Wither (2014b) analyze recent minimum wage increases' effects using the 2008 panel of the Survey of Income and Program Participation (SIPP). The analysis makes use of the fact that this period's federal minimum wage increases were differentially binding across states, generating natural comparison groups. As discussed below, the SIPP has both advantages and disadvantages for analyzing these differentially binding minimum wage changes.

The current paper analyzes this historical episode using the Current Population Survey (CPS). While the CPS lacks some of the SIPP's novel features, it has advantages with regards to data quality and the horizon over which it permits analysis. Because the CPS underlies the Bureau of Labor Statistics' (BLS) unemployment series, in-sample employment changes map directly into standard regional and national employment statistics. As shown below, this paper's findings reinforce Clemens and Wither's (2014b) conclusion that this period's minimum wage increases had significant, negative effects on low-skilled workers' employment.

My analysis employs two estimation frameworks. I first conduct a standard difference-in-differences analysis of the employment of a particularly low-skilled group, namely individuals ages 16 to 30 with less than a high school education. I then introduce a triple-difference extension of the difference-in-differences framework. The latter framework exploits variation in the minimum wage's bite across skill groups that fully partition the working age population.

The primary threat to these research designs is the possibility that low-skilled workers were differentially affected by the Great Recession in states that were fully versus partially bound by this period's federal minimum wage increases. House price data make it apparent that the housing crisis was relatively severe in states that were partially bound. Macroeconomic conditions thus had a more negative effect on employment among low-skilled workers in partially bound states than in fully bound states. This will tend to bias estimated effects of this period's minimum wage increases on employment towards zero. In the basic difference-in-differences framework, I take two approaches to addressing this concern. My simplest approach is to control directly for a proxy for the housing decline's severity, namely the all-transactions median house price index produced by the Federal Housing Finance Agency (FHFA). Within this approach, I then explore the baseline's sensitivity to controlling further for a variety of macroeconomic aggregates, including stimulus spending through the American Reinvestment and Recovery Act.

My baseline estimate is that the differentially binding portion of this period's minimum wage increases reduced the employment of individuals ages 16 to 30 with less than a high school education by 3.7 percentage points. Controlling for additional macroeconomic covariates has little impact on this estimate. By contrast, taking no measure to account for variation in the housing decline's severity results in a downwardly biased estimate. Employment among young, low-education adults declined by 1 percentage point more in fully bound states than in partially bound states in spite of the fact that the latter states' housing declines were significantly more severe.[1]

Within the difference-in-differences framework, my second approach involves matching states on the basis of the severity of their housing declines. In the baseline version of this approach, I restrict the sample to matches for which the difference in the median house price decline was less than $10,000. Using this criterion, I estimate that the differentially binding portion of this period's minimum wage increases reduced the employment of low-skilled individuals by 3.8 percentage points. This estimate involves no additional effort to control for heterogeneity in the Great Recession's severity. Including the controls used in the more basic difference-in-differences design results in an estimate

[1] As discussed in section 3, the implied magnitude of the bias associated with taking no steps to account for variation in the housing decline's severity is roughly what one can infer using external estimates from Charles, Hurst, and Notowidigdo (2013).

just over 4 percentage points. Allowing the threshold for what constitutes a sufficiently good match to range from $5,000 to $20,000 has essentially no effect on the resulting estimate.[2]

My second estimation framework utilizes additional variation in the extent to which minimum wage increases bite the wage distributions of different skill groups. Using baseline wage information from the CPS's Merged Outgoing Rotation Groups (CPS-MORG), I estimate variation in the minimum wage's bite across age-by-education cells.[3] I then implement a triple-difference estimation framework in which the degree of the minimum wage's bite serves as a continuous treatment variable.[4] This approach thus makes more extensive use of the information on treatment intensity that can be harnessed in the CPS's repeated cross sectional samples.

Like the difference-in-differences estimates, the estimates exploiting differential bite across skill groups provide evidence of negative effects on employment. That is, they provide evidence that the differentially binding portion of this period's minimum wage increases reduced the employment of low-skilled workers. As in the difference-in-differences results, the estimated employment declines are robust to augmenting the baseline specification with additional controls for the evolution of macroeconomic aggregates. They are similarly robust to restricting the sample to "good" matches based on the size of states' housing declines. Also as in the difference-in-differences framework, estimates on the matched sample are robust to dropping controls for macroeconomic conditions altogether.

I next assess my estimates' implications for the effects of this period's minimum wage

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[2] I also implement matching based on the percent decline rather than the level of the change.

[3] The analysis includes a 3 age group by 3 education group approach and a 5 age group by 4 education group approach.

[4] For a recent example of a similar estimation framework, see Hoynes and Schanzenbach's (2012) analysis of the food stamp program's employment effects.

increases on aggregate employment. From 2006 to 2012, the average effective minimum wage rose by $1.72 across the United States. The differential change between fully and partially bound states was $0.62. Extrapolating from in-sample estimates to the full effect of the $1.72 increase comes with standard caveats, which are discussed in section 4. My baseline estimate is that this period's minimum wage increases reduced employment among individuals ages 16 to 30 with less than a high school degree by 5.6 percentage points. This amounts to 43 percent of the decline in this group's employment between 2006 and 2012. Further, it accounts for a 0.49 percentage point decline in the employment to population ratio across all individuals ages 16 to 64. This aggregate impact, which is derived exclusively from impacts on individuals ages 16 to 30 with less than a high school education, is modestly smaller than the full-population employment estimate from Clemens and Wither (2014b).[5]

The paper proceeds as follows. Section 1 presents background information regarding the minimum wage increases I analyze. Section 2 presents the difference-in-differences model, matching strategy, and triple-difference model employed in the analysis. Section 3 presents the results. Section 4 extrapolates the baseline results into aggregate employment impacts. Section 5 briefly recapitulates the evidence. For interested readers, Appendix A contrasts the analyses in this paper and in Clemens and Wither (2014b) with other analyses of this period's minimum wage increases.

[5]In the prior paper, the SIPP's panel structure enabled a more concentrated harnessing of the minimum wage's bite on the workers it targets. Clemens and Wither's (2014b) preferred estimate implied an aggregate employment rate reduction of 0.7 percentage point.

1 Background on Recent Federal Minimum Wage Increases

I estimate the minimum wage's effects on employment using variation driven by federally mandated increases in the minimum wage rates applicable across the U.S. states.[6] On May 25, 2007, Congress legislated a series of minimum wage increases through the "U.S. Troop Readiness, Veterans' Care, Katrina Recovery, and Iraq Accountability Appropriations Act." Increases went into effect on July 24th of 2007, 2008, and 2009. In July 2007, the federal minimum rose from $5.15 to $5.85, in July 2008 it rose to $6.55, and in July 2009 it rose to $7.25.

Figure 1 shows my division of states into those that were fully and partially bound by changes in the federal minimum wage, which follows the division in Clemens and Wither (2014b).[7] The designation is based on whether a state's January 2008 minimum was below $6.55, rendering it at least partially bound by the July 2008 increase and fully bound by the July 2009 increase. Using Bureau of Labor Statistics (BLS) data on states' prevailing minimum wage rates, 27 states fit this description.

Figure 2 shows the time paths of the average effective minimum wage rates in fully bound states relative to partially bound states.[8] The average effective minimum wage in partially bound states exceeded the minimum applicable in fully bound states prior to the passage of the 2007 to 2009 federal increases. On average, the effective minimum across partially bound states rose by $1.42 between 2006 and 2012. By contrast, fully bound states saw their effective minimums rise by $2.04 as a result of the federal minimum wage legislation.

As observed in Clemens and Wither (2014b), the principal challenge to the estima-

[6] This section draws liberally on the text of Clemens and Wither (2014b).

[7] Clemens and Wither (2014b) is the original source for Figure 1.

[8] Figure 2 is modified from the equivalent figure in Clemens and Wither (2014b) to incorporate the full sample period analyzed in the present paper.

tion frameworks developed in the following section is the possibility that low-skilled workers in fully and partially bound states were differentially affected by the Great Recession. Figure 3 presents data from the BLS, the Bureau of Economic Analysis (BEA), and the Federal Housing Finance Agency (FHFA) on the macroeconomic experiences of fully and partially bound states over this time period.[9] The economic indicators reveal that fully bound states were less severely impacted by the Great Recession than were partially bound states. Most notably, Panel C reveals that partially bound states had relatively severe housing declines. This would, if controlled for insufficiently, tend to bias the magnitudes of estimated employment impacts towards 0. The following section describes my empirical strategy for addressing this concern.

2 Framework for Analyzing the Effects of Minimum Wage Increases Using the Current Population Survey

My analysis uses monthly employment data from the Current Population Survey (CPS), which underlies official BLS unemployment statistics.[10] The primary analysis sample extends from January 2006 through December 2012. My sole sample inclusion criterion is that surveyed individuals be between 16 and 64 years of age. The samples are thus fully representative of the working age population during this time period.

The estimation frameworks developed below make use of the fact that minimum wage increases are differentially binding on the wage distributions of different skill groups. Wage data in the CPS's Merged Outgoing Rotation Groups (CPS-MORG) in-

[9]Figure 3 is modified from the equivalent figure in Clemens and Wither (2014b) to incorporate the full sample period analyzed in the present paper. All series are weighted by state population to reflect the weighting applied in the regression analysis.

[10]Specifically, I use extracts generated using the Minnesota Population Center's Integrated Public Use Microsamples (Flood, King, Ruggles, and Warren, 2015).

form my assessment of the extent to which the relevant minimum wage increases were binding on the wage distribution of a given age-by-education skill group.

For an age-by-education group g, I use data from January 2006 through July 2009 to estimate the fraction of months during which the relevant individuals had wage rates between \$5.15 and \$7.25, which were the prevailing federal minimum wage rates on January 1, 2006, and July 31, 2009, respectively.[11] For each observation in the CPS-MORG, I first construct an indicator equal to 1 if the individual's self-reported wage rate falls between \$5.15 and \$7.25:

$$A_{i,s,t} = 1\{5.15 \geq \text{Hourly Wage}_{i,s,t} < 7.25\}. \tag{1}$$

For each demographic group $g(i)$, I then then average these $A_{i,s,t}$ separately across individuals in fully and partially bound states over the period preceding July 2009. That is, I estimate $E_g[A_{i,s,t}|(\text{Bound}_s = 1, \text{Post}_t = 0)]$ and $E_g[A_{i,s,t}|(\text{Bound}_s = 0, \text{Post}_t = 0)]$. Finally, I construct:

$$\text{Bite}_g = E_g[A_{i,s,t}|(\text{Bound}_s = 1, \text{Post}_t = 0)] - E_g[A_{i,s,t}|(\text{Bound}_s = 0, \text{Post}_t = 0)]. \tag{2}$$

$Bite_g$ thus describes the difference in the fraction of months individuals in each skill group spent at affected wage rates in fully bound states relative to partially bound states.

Table 1 presents the resulting estimates of the "bite" of this period's minimum wage increases across a set of 9 age-by-education skill groups. The minimum wage's bite across this set of skill groups is, unsurprisingly, strongest among individuals ages 16 to

[11] Some workers report wage rates below the \$5.15 federal minimum. Such cases reflect some combination of reporting error, employment in jobs not covered by the federal minimum (e.g., workers who receive tips), and possibly some illegal evasion.

30 with less than a high school education.[12] There is non-trivial, but significantly less, bite among individuals ages 16 to 30 with a completed high school education, as well as among dropouts between ages 31 and 45. This period's minimum wage increases had relatively trivial bite on the wage distributions of higher skilled individuals.

2.1 Difference-in-Differences Framework

I begin my analysis by estimating the following difference-in-differences specification on a sample restricted to the most affected skill group, namely individuals ages 16 to 30 with less than a high school education:

$$\begin{aligned} Y_{i,s,t} = \sum_{p(t) \neq 0} & \beta_{p(t)} \text{Bound}_s \times \text{Period}_{p(t)} \\ & + \alpha_{1_s} \text{State}_s + \alpha_{2_t} \text{Time}_t + \mathbf{X_{s,t}} \gamma + \varepsilon_{i,s,t}. \end{aligned} \tag{3}$$

The primary outcome variable of interest, $Y_{i,s,t}$, is employment. The specification controls for the standard features of difference-in-differences estimation, namely sets of state, State_s, and time, Time_t, fixed effects. The vector $\mathbf{X_{s,t}}$ contains time varying controls for each state's macroeconomic conditions. In my baseline specification, $\mathbf{X_{s,t}}$ includes the all-transactions FHFA median house price index, which proxies for the state-level severity of the housing crisis.[13]

[12] In the construction of $A_{i,s,t}$, I code individuals as 0 if they do not report being paid on any hourly basis. Consequently, the fractions are lower than what one would obtain if one made additional use of wage rates imputed on the basis of weekly earnings and hours.

[13] It is not uncommon for minimum wage studies to control directly for a region's overall employment or unemployment rate. Econometrically, it is likely preferable to exclude such variables. The policy change of interest may directly affect statewide employment, for example, which is the aggregate outcome of primary interest. Median house prices are less subject to this concern for two reasons. First, many minimum wage workers are dependents. Many of their incomes will thus have minimal impact on their households' housing decisions. Second, even households headed by minimum wage workers will tend

Equation (3) allows for two potentially important sources of dynamics in the federal minimum wage legislation's effects. First, and most importantly, the legislation was enacted in May 2007, with wage increases going into effect in July of 2007, 2008, and 2009. The specification thus treats May 2007 through July 2009 as a period of transition (period p = Transition) from the old policy regime to the new policy regime. Prior months correspond to the baseline, or period $p = 0$.

Second, as in Clemens and Wither (2014b), the specification allows the minimum wage's effects to unfold differentially over the short- and medium-run. I code August 2009 through July 2010 as period Post 1 and all subsequent months as period Post 2. This straightforwardly allows for the possibility that firms' and workers' short- and medium-run responses may differ. Within the recent minimum wage literature, evidence from Aaronson, French, and Sorkin (2013), Meer and West (Forthcoming), and Sorkin (2015) indicates that such effects may be important.[14] The primary coefficients of interest are $\beta_{\text{Post 1}(t)}$ and $\beta_{\text{Post 2}(t)}$, which characterize the differential evolution of employment in states that were fully bound by the new federal minimum relative to states that were partially bound. I calculate the standard errors on these coefficients allowing for the errors, $\varepsilon_{i,s,t}$, to be correlated at the state level.

2.2 Matching on the Severity of States' Housing Declines

Within equation (3)'s difference-in-differences framework, I examine the results' robustness to applying sample restrictions motivated by a matching procedure. I match states on the size of their median house price declines between 2006 and 2012 (with

to have tangential if any influence on the housing market segments in which median home values are determined. I present specifications with additional macroeconomic controls primarily to check whether my results are sensitive to the inclusion of controls used regularly in the literature. The results are affected little by the inclusion of these variables in $\mathbf{X}_{s,t}$.

[14] In the current setting, the lag between legislation and full implementation is sufficiently long that the relevant dynamics might largely be expected to unfold during the transition period.

values averaged across all months in these years). The matches are thus based on the extent of the housing decline from the first to the last year of the baseline analysis sample. To be more precise regarding the procedure, I apply nearest neighbor matching without replacement. I then restrict the sample on the basis of the quality of the resulting matches. For example, the baseline matching sample requires that the difference in matched states' housing declines be no greater than $10,000.[15] I explore the results' robustness to applying alternative thresholds of $5,000, $20,000, and 5 log points.

Table 2 presents summary statistics for the housing declines in fully and partially bound states for the analysis samples I utilize. Row 1 shows the disparity in the severity of the housing decline between the full sets of fully and partially bound states. Between 2006 and 2012, the FHFA's all-transactions median house price index declined, on average, by $72,000 in partially bound states and by $24,700 in fully bound states. Row 2 shows the comparable means for the sample restricted to pairs with declines no more than $5,000 apart from one another. For this sample, which retains 20 states, the mean decline in partially bound states was $29,700 and the mean decline in fully bound states was $27,400. The $10,000 matching criterion retains 24 states and yields means of $32,000 and $29,700 respectively. The $20,000 matching criterion retains 32 states and yields means of $38,500 and $27,200 respectively. Finally, the requirement that the declines be within 5 log points of one another retains 30 states and yields means of $36,900 and $24,100. The difference in the means in the latter sample reflects the fact that the baseline *level* of median house prices is, on average, higher in partially bound states than in fully bound states.

[15] This approach to sample selection is sometimes called the "caliper" method (Cochran and Rubin, 1973; Crump, Hotz, Imbens, and Mitnik, 2006).

2.3 A Framework for Harnessing Heterogeneity in the Minimum Wage's "Bite" across Skill Groups

I make further use of variation in the minimum wage's bite across skill groups by estimating the specification below, which is a triple-difference framework with a continuous treatment variable:

$$
\begin{aligned}
Y_{i,s,t} = & \sum_{p(t)\neq 0} \beta_{p(t)} \text{Period}_{p(t)} \times \text{Bound}_s \times \text{Bite}_{g(i)} \\
& + \alpha_{1_{s,p(t)}} \text{State}_s \times \text{Period}_{p(t)} + \alpha_{2_{s,g(i)}} \text{State}_s \times \text{Group}_{g(i)} + \alpha_{3_{p(t),g(i)}} \text{Period}_{p(t)} \times \text{Group}_{g(i)} \\
& + \alpha_{4_s} \text{State}_s + \alpha_{5_t} \text{Time}_t + \alpha_{6_g} \text{Group}_{g(i)} + \mathbf{X_{s,t,g(i)}}\gamma + \varepsilon_{i,s,t}. \qquad (4)
\end{aligned}
$$

Equation (4) augments equation (3) with the standard components of triple-difference estimation. These include group-by-time-period effects, group-by-state effects, and state-by-time-period effects. These controls account for differential changes in employment across skill groups over time, cross-state differences in the relative employment of these groups at baseline, and time varying spatial heterogeneity in economic conditions. For computational ease, I collapse the data set into state-by-group-by-month cells before estimating equation (4).[16]

A shortcoming of the triple-difference approach involves the possibility of employer substitution of workers in low "bite" skill groups for workers in high "bite" skill groups. Substitution of this form could lead the triple-difference approach to overstate a minimum wage increases' total employment impacts. In this paper's empirical context, the aggregate employment impacts implied by estimates of equations (3) and (4) are ulti-

[16] Weights are utilized throughout to maintain the national and state-level representativeness of the sample.

mately quite similar.

3 Estimates of Minimum Wage Increases' Effects

This section presents my estimates of the effects of binding minimum wage increases on the employment and other outcomes of low-skilled workers. Its first sub-section presents estimates of employment impacts using equation (3)'s difference-in-differences specification. Its second sub-section explores this specification's robustness. Its third sub-section explores the minimum wage increases' effects on additional employment and earnings margins. Its fourth sub-section presents additional evidence of employment impacts using equation (4)'s triple-difference specification. Its fifth subsection presents estimates that allow the minimum wage increases' effects to unfold with finer dynamics. Finally, its sixth subsection presents estimates of equation (4) on samples that exclude the very lowest skilled workers. This final piece of analysis explores whether there is evidence of employment impacts on low-wage workers within skill groups outside of individuals ages 16 to 30 with less than a high school education.

3.1 Baseline Estimates of Employment Impacts

Column 1 of Table 3 reports the baseline estimates of differentially binding minimum wage increases' effects on employment among individuals ages 16 to 30 with less than a high school degree. Conditional on the decline in states' median house prices, employment within this skill group declined by 3.7 percentage points more in fully bound states than in partially bound states. This is an economically substantial 28 percent of the total decline in this group's employment rate over this time period. Because this skill group accounts for 8.4 percent of the working age population, the associated in-sample effect on fully bound states' aggregate employment to population ratios is 0.31 percentage point. In Section 4 I consider the assumptions needed to extrapolate this estimate to infer the employment effects of this period's full set of minimum wage increases.

For columns 2 and 3, I estimate equation (3) on samples of higher-skilled workers. The sample in column 2 includes skill groups for which the minimum wage's "bite," as reported in column 3 of Table 1, was between 0.015 and 0.03. The estimated change in this group's employment is a negative, statistically insignificant -1.1 percentage points in the short run and -0.5 percentage point in the medium run. The sample in column 3 includes skill groups for which the minimum wage's "bite," as reported in column 3 of Table 1, was less than 0.015. The estimated change in this group's employment is an even smaller, and again statistically insignificant, -0.4 percentage point in the short run and -0.0 percentage point in the medium run.

Across broad skill groups, the magnitudes of the estimates in Table 3 are monotonically increasing in the measure of the minimum wage's bite. Reassuringly, the estimate for the highest of these broad skill groups is economically quite close to 0. The employment decline estimated in column 1 thus does not appear to be associated with employment declines among skill groups that would implausibly be affected by increases in the minimum wage.

The estimates associated with the housing price index are also informative. The housing price index is strongly, positively correlated with employment among individuals in each skill group. Consistent with findings from Charles, Hurst, and Notowidigdo (2013), this is particularly true among the lowest skilled. For the lowest skilled, the estimate implies that a $100,000 decline in median house prices predicts a 2.8 percentage point decline in employment. The associated estimate for relatively high skilled workers is that a $100,000 decline in median house prices predicts a 0.5 percentage point decline in employment. An implication of this finding for estimates that pool high and low skilled workers, as in estimates of equation (4), is that the coefficients on proxies for the housing decline's severity must be allowed to vary across skill groups.

3.2 Robustness of the Baseline Difference-in-Differences Estimates

Table 4 explores the relevance of the inclusion of macroeconomic controls for the baseline estimate from Table 3's column 1. Column 2 repeats the baseline estimate. Column 3 shows that controlling additionally for macroeconomic aggregates including the statewide employment rate and personal income per capita yields an estimate of -3.3 percentage points.[17] Column 4 shows that controlling additionally for stimulus spending associated with the American Reinvestment and Recovery Act has no effect on this estimate.[18] Consistent with Clemens and Wither's (2014b) analysis using the SIPP, columns 3 and 4 thus show that the baseline estimate is little affected by incorporating additional macroeconomic controls.[19]

Column 5 shows that, as implied by the data from Figure 3, taking no measures to control for variation in the severity of the housing crisis yields a downwardly biased estimate. Specifically, it shows that employment among individuals ages 16 to 30 with less than a high school degree declined by 1 percentage point more in fully bound states than in partially bound states in spite of the fact that the housing crisis was much more severe among partially bound states. The differential effect of the housing decline thus obscures

[17]As discussed in Section 2, the inclusion of these controls is less econometrically attractive than the inclusion of the median house price index. I present specifications with these additional controls primarily to show that my results are relatively insensitive to the inclusion of controls used regularly in the literature.

[18]The stimulus variable is constructed using data from Chodorow-Reich, Feiveson, Liscow, and Woolston (2012). Consistent with Chodorow-Reich, Feiveson, Liscow, and Woolston's (2012) findings, I obtain a positive coefficient on stimulus spending in regressions involving the samples of middle- and higher-skill groups (results not shown). The implied "dollars per job" numbers have magnitudes similar to those estimated by Chodorow-Reich, Feiveson, Liscow, and Woolston (2012). The associated t-statistics range between 1.5 and 2.3 depending on the combination of macroeconomic covariates included as controls. The absence of a positive correlation in my specification involving individuals ages 16 to 30 with less than a high school education suggests that stimulus spending created jobs more likely to be held by individuals in groups with higher observable skill levels.

[19]The relevant analysis in Clemens and Wither (2014b) appears in Appendix Table A6.

a substantial 2 to 3 percentage points of the effect of the differentially binding portion of this period's minimum wage increases. The magnitude of this bias is roughly what one can infer using external estimates from Charles, Hurst, and Notowidigdo (2013).[20] Because the housing decline is so relevant, the persuasiveness of the results rests in part on the consistency of the estimates obtained using equations (3) and (4), the robustness of each baseline to the matching strategy, the robustness of each baseline to the incorporation of additional controls, the plausibility of the underlying dynamics, and the consistency of the results with the analysis in Clemens and Wither (2014b).

Table 5 shows that the estimates in Table 4 are moderately reduced by expanding the baseline period to include 2005. A comparison of Table 4 and Table 5's first columns provides an explanation for this result. The dependent variable in these columns is the level of states' effective minimum wage rates. The reported coefficients thus describe the differential change, from the full pre-legislation period to the post-implementation period, in fully and partially bound states' average effective minimum wage rates. Expanding the base period reduces this differential change, reflecting the fact that some of the partially bound states increased their minimum wage rates between 2005 and 2006. The estimated employment decline per $1 increase in the minimum wage is essentially unchanged as one moves from Table 4 to Table 5.

[20] On the upside of the housing boom, Charles, Hurst, and Notowidigdo (2013) estimate that a one standard deviation change in housing demand predicts an 0.8 percentage point change in employment among the full population ages 21 to 55 (in Table 2, see the entry in column 5 of the row labeled "Housing Demand Change"). While their estimate is from the housing swing's upside, Charles, Hurst, and Notowidigdo (2013) emphasize that this period's local housing booms and busts were roughly symmetric. The housing decline was, on average, moderately less than 1 standard deviation (of the distribution across all states) larger in partially bound states than in fully bound states. If my estimation sample consisted of a skill group that exhibited the same sensitivity to housing demand, Charles, Hurst, and Notowidigdo's (2013) estimates would suggest a bias of just under 0.8 percentage points. The actual bias exceeds this because, as shown in Table 3, the employment of high school dropouts ages 16 to 30 was more sensitive to the housing crisis than the employment of other groups. For this group, the coefficient on the housing price index was 0.28. When I estimate this coefficient on a sample containing all individuals ages 21 to 55, I obtain a coefficient of 0.10. The bias suggested by Charles, Hurst, and Notowidigdo's (2013) estimate is thus on the order of $0.8 \times \frac{0.28}{0.10} \approx 2.4$. This is roughly the difference between the estimates reported in Table 4's column 5 relative to columns 2 through 4.

Tables 6 and 7 report estimates of equation (3) on samples that have been restricted using the matching criteria described in Section 2.2. The results in Table 6 show the estimates obtained when imposing the 4 match-quality thresholds described in Table 2. In column 1 the criterion is that each match involve a house price decline that differs no more than $10,000 between the fully and partially bound state. In column 2 the criterion is $5,000. In column 3 the criterion is $20,000. Finally, in column 4 the criterion is 5 log points. In all cases, the medium run estimate is between -3.7 and -4.0 percentage points. Note that these specifications include no direct controls for heterogeneity in macroeconomic conditions.[21] Matching on the size of states' housing declines thus produces estimates in line with that in Table 3's column 1 without taking additional steps to control for macroeconomic conditions.

Figure 4 presents time series tabulations of the employment rates in fully and partially bound states that underlie the matching estimate from column 1. Panel A presents unadjusted tabulations while Panel B presents tabulations adjusted to smooth out seasonal fluctuations. Averages, taken over the relevant time periods, of the presented data translate directly into the point estimates from column 1 of Table 7. Over the baseline, low-skilled workers' employment in fully bound states exceeded their counterparts' employment in partially bound states by nearly 5 percentage points. This difference narrowed to just under 2 percentage points over the year following July 2009 increase in the federal minimum wage. It averaged roughly 1 percentage point over subsequent years.

The results in Table 7 show that the estimates from column 1 of Table 6 are largely insensitive to incorporating the macroeconomic controls considered in Table 4. The estimates range from -3.8 percentage points (the matching sample specification with no controls) to -4.5 percentage points. Taken together, the results in Tables 4, 5, 6, and 7

[21]That is, the housing price index is used in the matching procedure, but then excluded from the regression.

provide evidence that the baseline result from column 1 of Table 3 is robust to taking a range of approaches to controlling for heterogeneity in Great Recession's severity across states. Appendix Tables B.2, B.3, B.4, and B.5 present further estimates in which I augment the specifications already presented with flexible sets of time-varying controls for individual-level demographics (specifically in age, education, race/ethnicity, and gender). Estimates of the primary coefficient of interest range from -2.8 to -4.3 percentage points and are statistically distinguishable from 0 at the 0.01 level in all cases.

3.3 Difference-in-Differences Estimates of Effects on Additional Labor Market Markets

This section reports estimates of equation (3) for additional labor market outcomes of potential interest. While most of these outcomes are available in the full CPS, weekly earnings are only available in the smaller samples associated with the CPS-MORG. Table 8 reports estimates for margins including full time employment, part time employment, hours, and hours conditional on employment using the full CPS. Column 1 repeats the baseline estimate. Columns 2 and 3 show that there was, on net, no change in the number of part time jobs and a significant decline in the number of full time jobs held by individuals ages 16 to 30 with less than a high school degree.

Columns 4 and 5 shed further light on the results in columns 2 and 3. The average decline in hours, including both extensive and intensive margin adjustments, was between 1.7 and 1.8 hours per week. This 1.8 hour decline accounts for an economically substantial 17 percent of this skill group's labor input at baseline. Column 5 shows that the decline in weekly hours conditional employment was roughly 1.3 over the long run. The net stability of the number of part time jobs thus appears to reflect two offsetting effects. The first effect is a conversion of some full time work into part time work. The second effect is the elimination of some initially part time jobs.

Table 9 reports estimates that use the CPS-MORG to supplement the estimates of effects on employment, hours, and hours conditional on employment with estimates of effects on weekly earnings. Columns 1 through 3 show that the overall estimated effects on employment, hours, and hours conditional on employment are quite similar when comparing the full CPS and the CPS-MORG. Column 4 reports a $15 decline in weekly earnings over the short run and a $13 decline over the medium run.[22] These declines follow directly from the overall reductions in hours. At the new minimum wage of $7.25, a decline of 1.8 hours per week translates directly into a $13 decline in weekly earnings.

The minimum wage increases' mechanical effect on earnings offsets much less of the decline due to reductions in hours than one might initially expect. Recall from Table 1 that the differential bite of this period's minimum wage increases on the lowest-skilled workers in fully bound states relative to partially bound states was only 5 percentage points. This largely reflects three factors. First, individuals in this skill group worked in just under 40 percent of months at baseline. Second, during months when these individuals were employed, their self-reported wage rates were between the old and new federal minimum roughly one third of the time. Finally, some of the instances when individuals report wage rates in the affected range involve employment in uncovered jobs (e.g., workers who receive tips). The minimum wage increases' mechanical contribution to this group's average weekly earnings turns out to be less than $3.[23]

[22]These estimates relate quite closely to estimates from Clemens and Wither (2014b). The prior paper analyzes a more concentrated sample of low wage workers. That paper's estimates of employment effects and declines in average earnings are correspondingly moderately larger. For example, Clemens and Wither (2014b) estimate a $90 short run decline in average monthly earnings, which is moderately larger the monthly decline implied by a $15 decline in weekly earnings. Clemens (Forthcoming) further analyzes the implications of Clemens and Wither's (2014b) estimates for payroll tax revenues and spending through a variety of public assistance programs.

[23]The steps in this calculation are as follows. First, the minimum wage increases' differential bite on this skill group's wage distribution, in fully bound states relative to partially bound states, was 5 percent of survey months as reported in Table 1. Second, this skill group's average hours of work when working is 27. The product of these values would thus yield a $2.84 increase in weekly earnings in fully bound states relative to partially bound states if the minimum wage's bite in each instance was the full $2.10 increase from $5.15 to $7.25. This is an upper bound because affected workers' initial wage rates were

3.4 Estimates Utilizing Continuous Measures of the Minimum Wage's "Bite" across Skill Groups

Table 10 presents estimates of equation (4), in which identification comes from variation in the minimum wage's bite across skill groups. Informally speaking, the procedure involves stacking the skill groups on which separate difference-in-differences regressions were estimated for Table 3. Columns 1 and 2 allow for variation across the 9 age-by-education skill-groups described in Table 1. The 38,556 observations are the product of 9 skill groups by 84 months by 51 states. Columns 3 and 4 allow for 20 distinct skill groups defined by four education groups (separating those with some college from those with a BA or more education) and five 10-year age bands. The 85,680 observations are the product of 20 skill groups by 84 months by 51 states. In columns 1 and 3 the housing price index is the only macroeconomic control, while columns 2 and 4 control further for the state employment rate, state personal income per capita, and stimulus spending per capita. Because skill groups were differentially affected by the housing crisis, as found by Charles, Hurst, and Notowidigdo (2013) and shown directly in Table 3, I allow the relationship between these controls and employment to vary across groups.[24]

The estimates in Table 10 provide evidence that, comparing changes in fully bound states to partially bound states, skill groups more intensively affected by minimum wage increases experienced greater declines in employment. The estimates, which fall between -0.7 and -0.8, relate to the estimates from Table 3 as follows. Recall Table 3's finding that, comparing fully bound states to partially bound states, employment in the lowest skill group declined by 3.7 percentage points. Recall further that employment among the highest skill groups was essentially unchanged. Next, recall Table 1's presentation of

often between \$5.15 and \$7.25 rather than pinned directly to \$5.15.

[24]Note that if these controls were not allowed to vary across groups they would be perfectly collinear with a complete set of state by time effects.

the measure of "bite." Table 1 showed that "bite" is around 4 percentage points higher among workers in the lowest skill group than in the highest skill groups. Multiplying this 4 percentage point difference by -0.75, which is the average of the point estimates from Table 10, yields 3 percentage points. This returns us to the estimated decline in employment among individuals ages 16 to 30 with less than a high school degree net of employment changes among higher skilled groups (that is, just over 3 percentage points).

Tables 11 and 12 explore the robustness of estimates of equation (4) to restricting the sample using the matching criteria discussed in Section 2.2. Table 11 presents estimates that allow the minimum wage's bite to vary across 9 skill groups, while Table 12 allows for 20 skill groups. As with the estimates reported in Table 7, none of these specifications contain direct controls for heterogeneity in macroeconomic conditions. The results thus reveal that the estimated employment effects are robust to applying the matching strategy in lieu of controlling directly for heterogeneity in the severity of the housing crisis across states.

3.5 Graphical Presentation of Finer Dynamics

The panels of Figure 5 display graphical presentations of regression estimates that allow the effects of differentially binding minimum wage increases to unfold with finer dynamics over time. Panels A and B present estimates of equation (3), while Panels C and D present estimates of equation (4). In the underlying regressions, each time period is a 6 month interval. Period $p = 0$, relative to which subsequent changes are estimated, extends from January to June of 2006.

In Panel A, the marker associated with July through December of 2008 reveals that there was little net differential change in the employment of low-skilled workers in fully and partially bound states between the base period and the second half of 2008. There is weak evidence of initial employment declines between July 2008 and June 2009, which follows the second increment of the increase in the federal minimum wage. A 3 to 4 percentage point decline in low-skilled workers' employment in fully bound states relative to partially bound states emerges in all periods following July 2009. Panel B shows that these declines were not experienced among higher-skilled worker groups, as shown previously in Table 3.

In Panels C and D, the estimates again reveal that there was little net differential change in the employment of low-skilled workers (relative to higher-skilled workers) in fully and partially bound states from the base period through the end of 2007. In these specifications, initial evidence of employment declines emerges during 2008, as the federal minimum wage rose from $5.85 to $6.55. Estimated employment declines hold steady through 2009, 2010, and 2011, possibly moderating near the sample's end.

The dynamics displayed in Figures 4 and 5 provide evidence on two points deserving emphasis. First, the figures provide evidence against the concern that differential declines in employment among low-skilled workers predated the legislation of this period's differentially binding minimum wage increases. That is, they provide evidence against potentially confounding pre-existing trends. Second, they reveal economically relevant dynamics in the legislation's effects. Specifically, they provide evidence that these minimum wage increases' effects began to unfold around the time of the July 2008 implementation of the increase from $5.85 to $6.55.

The observed dynamics are of precisely the sort that can be partially misattributed in difference-in-differences specifications with state-specific trends. In the minimum wage context, simulations from Meer and West (2013) demonstrate the downward bias that can arise in such specifications. Studies in settings ranging from divorce law (Wolfers, 2006) to school desegregation (Baum-Snow and Lutz, 2011) have similarly demonstrated that state-trends specifications can bias estimates when a policy change's effects unfold with these kinds of dynamics.[25]

3.6 Estimates Excluding the Very Lowest Skilled Workers

An important question for assessing the aggregate employment effects of this period's minimum wage increases is whether employment declined among low-wage workers in groups other than the very lowest skilled. Recall from Table 3 that, across broad skill groups, estimated employment declines increased monotonically in the extent of the minimum wage's bite. Estimates for skill groups outside of individuals ages 16 to 30 with less than a high school degree were negative, but small and statistically indistinguishable from 0. While the point estimates were small, it is important to note that the pools of individuals in these higher-skilled groups are much larger than the pool of individuals ages 16 to 30 with less than a high school education. Small effects on large skill groups can thus significantly alter one's assessment of aggregate employment impacts.

I explore the presence of employment declines among low-wage workers in higher skilled demographic groups by estimating equation (4) on samples that exclude the very lowest skilled workers. Specifically, in both the 9-group and 20-group specifications I drop groups for which the minimum wage's estimated bite is greater than 0.05.[26] I present the estimates in Table 13. The estimates are negative, but vary nontrivially across specifications and are generally not statistically distinguishable from 0. The presence and magnitudes of effects within these groups is thus uncertain. The negative point estimates suggest that estimates of aggregate employment impacts that assume effects

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[25]Controlling directly for state-specific trends can, of course, be appropriate in many circumstances. It is also worth noting that fully saturated triple difference models are not sensitive to this form of specification error because state-specific trends are subsumed by state by time period fixed effects.

[26]In the 9-group specification this drops individuals ages 16 to 30 with less than a high school education. In the 20-group specification this drops individuals aged 16 to 25 with less than a high school education.

on these populations are 0 will be conservative.

4 Implications for Changes in Aggregate Employment Over the Great Recession

Between 2006 and 2012, the average effective minimum wage rose from \$5.82 to \$7.55 across the United States. Over this same time period, the employment to population ratio among adults ages 16 to 64 declined by nearly 5 percentage points. Clemens and Wither (2014a) more fully characterize the demographic and cross-country dimensions of this period's employment declines. The demographic and cross-country data highlight that U.S.-specific developments associated with low-skilled workers are an important dimension of the employment slump.

Sustained U.S. employment declines were particularly dramatic for low-education, low-experience individuals. Figure 6's Panels A and B provide detailed looks at the evolution of the employment and wage distributions of individuals ages 16 to 30 with less than a high school education between 2002 and 2014. Between 2006 and 2010, this skill group's employment rate declined by 13 percentage points, from 40 percent to 27 percent. It remained down by 13 percentage points through 2014.[27] Panel A reveals that the profile of this group's real wage distribution either shifted down or was stagnant from 2002 to 2006, from 2006 to 2010, and again from 2010 to 2014. These developments are broadly consistent with findings from recent work on the labor market implications of technological advance (Beaudry, Green, and Sand, 2013; Autor, Dorn, and Hanson, Forthcoming) and the composition of recent trade growth (Autor, Dorn, and Hanson, 2013). For present purposes, these developments' most relevant implication is that the

[27] This does not primarily reflect a shift between employment and schooling among teenage dropouts. The employment rate among dropouts ages 20 to 25 similarly declined by 13 percentage points between 2006 and 2010 and remained down by 13 percentage points as of 2014.

minimum wage has become more binding on the distribution of low-education, low-experience individuals' wages over time. The federal minimum wage's rise from \$5.15 to \$7.25 occurred over the same time period as a 20 percent downward shift in the profile of this group's real wage distribution.

Table 3 contains the full set of results underlying my extrapolation to aggregate employments effects. Recall that the table presents estimates of equation (3) on sub-samples that fully partition the population ages 16 to 64. Column 1 reports a 3.7 percentage point decline among individuals ages 16 to 30 with less than a high school degree. Columns 2 and 3 report near 0 estimates for groups with higher levels of experience and education.

Extrapolating the estimates from Table 3 requires sensitivity to the fact that the employment effects of minimum wage increases may be non-linear, likely with larger effects for final increments than for initial increases. Further, a given nominal value of the minimum wage has more purchasing power, and hence likely more bite, in the typical fully bound state relative to the typical partially bound state. Figure 3 showed that fully bound states have relatively low incomes and housing costs, perhaps making it unsurprising that they had not voluntarily brought their minimums to the levels seen in the partially bound states. Indeed, the July 2009 increase in the federal minimum wage brought the minimum to nearly 50 percent of the median wage in many fully bound states, and did so during the weakest national labor market of the last 75 years.

The extrapolation proceeds as follows. My in-sample estimate is that the differentially binding portion of this period's minimum wage increases reduced the employment of individuals ages 16 to 30 with less than a high school education by 3.7 percentage points (9 percent). The 95 percent confidence interval on this estimate extends from 1.5 to 5.9 percentage points. Applying the relevant sample weights, this group accounted for 8.4 percent of the full population ages 16 to 64. A 3.7 percentage point decline in this group's employment thus implies an $8.4 \times 0.037 = 0.31$ percentage point decline in fully bound states' employment to population ratios. The confidence interval on this estimate extends from 0.13 to 0.49 percentage point. Because fully bound states account for 41 percent of the full U.S. population, the purely in-sample decline in employment implies a $0.31 \times 0.41 \approx 0.13$ percentage point decline in the national, working-age employment to population ratio.

The 0.13 percentage point decline in the national employment to population ratio is a purely in-sample estimate for the differential change in the minimum wage in fully bound states relative to partially bound states. That is, it is an estimate of the effect of a \$0.62 differential increase in the minimum wage in states that account for 41 percent of the working age population. On average across all states, the effective minimum wage rose by \$1.72 over this time period. A purely linear extrapolation, both within and across states, thus implies an aggregate effect of roughly $0.13 \times \frac{1}{.41} \times \frac{1.72}{.62} \approx 0.86$ percentage point.

My baseline extrapolation splits the difference between a purely linear extrapolation and failing to extrapolate at all. I estimate that this period's minimum wage increases account for a 0.49 percentage point decline in the employment to population ratio among *all* individuals ages 16 to 64. This estimate is derived solely from estimated employment declines among individuals ages 16 to 30 with less than a high school degree, as my estimates of effects on low-wage workers within older and more educated demographic groups were relatively imprecise.[28] Among individuals ages 16 to 30 with less than

[28] In Figure 6's panels C and D, the 2010 and 2014 wage distributions provide additional, suggestive evidence that this period's minimum wage increases reduced employment among the lowest-skilled individuals in slightly higher observable skill groups. The panels plot wage distributions for high school dropouts between ages 31 and 45. The relevant feature of this group's real wages, as shown in Panel C, is that the 2010 and 2014 distributions were indistinguishable from one another at all wage rates above \$7.25 in nominal terms. That is, the real wages of individuals in percentiles 1 through 55 of this skill group were essentially the same in 2014 as in 2010. Between 2010 and 2014, the real value of the minimum wage declined by 8 percent, from \$7.25 to \$6.68 in 2010 dollars. In 2014, the figure reveals that employment had emerged at wage rates between \$7.25 and \$6.68 (again in 2010 dollars). Erosion of the minimum wage's real value corresponded with a 3 percentage point, or 5 percent, increase in this skill group's employment rate. The absence of shifts elsewhere in the wage distribution is suggestive that, as the minimum wage's

a high school degree, I estimate that this period's minimum wage increases reduced employment by 5.6 percentage points. This amounts to 43 percent of the decline in this group's employment from 2006 to 2010, as illustrated in Figure 6.

5 Concluding Discussion

I conclude by briefly recapitulating the key elements and findings of this paper's analysis. I analyze the effects of recent federal minimum wage increases, which were differentially binding across states. Because these minimum wage increases took effect during the Great Recession, I document heterogeneity in the housing decline's severity and implement several approaches to accounting for its influence on low-skilled workers' employment.

My analysis of this period's minimum wage increases' effects has the following structure. First, I take a straightforward approach to controlling for heterogeneity in the housing decline's severity within a standard difference-in-differences specification. Second, I show that controlling for additional macroeconomic covariates has little effect on the initial estimates. Third, I show that the bias associated with making no attempt to control for heterogeneity in the housing decline's severity is of a magnitude in line with what one can infer from Charles, Hurst, and Notowidigdo's (2013) analysis of the housing boom and bust. Fourth, I show that the results are robust to implementing triple-difference specifications that make relatively extensive use of variation in the minimum wage's relevance across skill groups. Finally, I show that the results from both the difference-in-differences and triple-difference specifications are robust to accounting

real value eroded, we are observing movement down a labor demand curve. A similar picture emerges among individuals ages 16 to 30 with exactly a high school degree. By contrast, employment among individuals ages 16 to 30 with less than a high school degree did not recover between 2010 and 2014. As shown in Panel A, this group's real wage distribution shifted modestly down between 2010 and 2014, such that its nominal wage distribution (and with it the $7.25 minimum wage rate's bite) had not shifted (Panel B). The $7.25 minimum wage was thus similarly binding on this group's wage distribution in both years.

for heterogeneity in the housing decline's severity using a standard matching procedure. The evidence supports the view that this period's minimum wage increases had significant, negative effects on low-skilled workers' employment.

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Figures and Tables

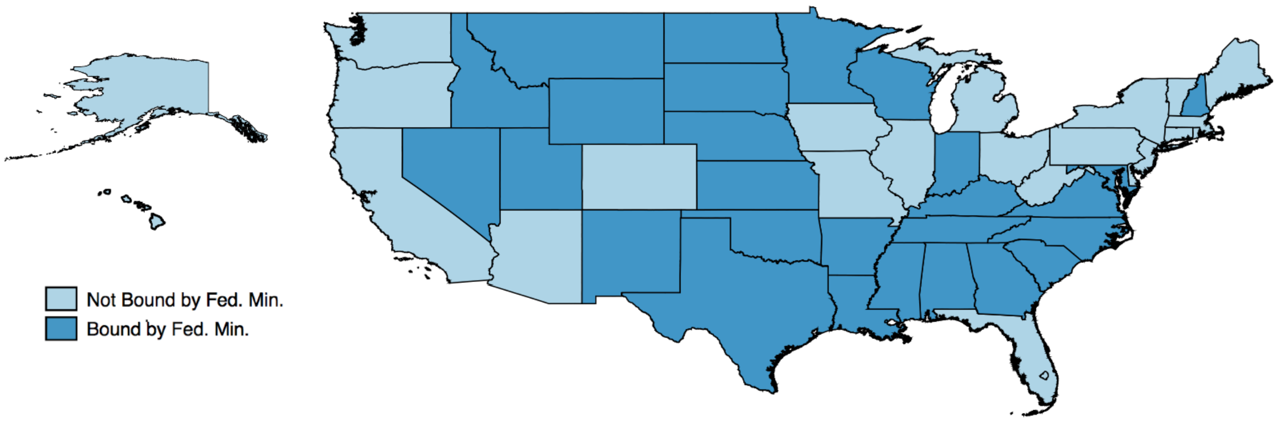


Figure 1: **States Bound by the 2008 and 2009 Federal Minimum Wage Increases:** The map differentiates states on the basis of whether they were fully or partially bound by the July 2007, 2008 and 2009 increases in the federal minimum wage. I define states as fully bound if their January 2008 minimum wage, as reported by the Bureau of Labor Statistics (BLS), was less than $6.55. Such states were at least partially bound by the July 2008 increase in the federal minimum and fully bound by the July 2009 increase from $6.55 to $7.25.

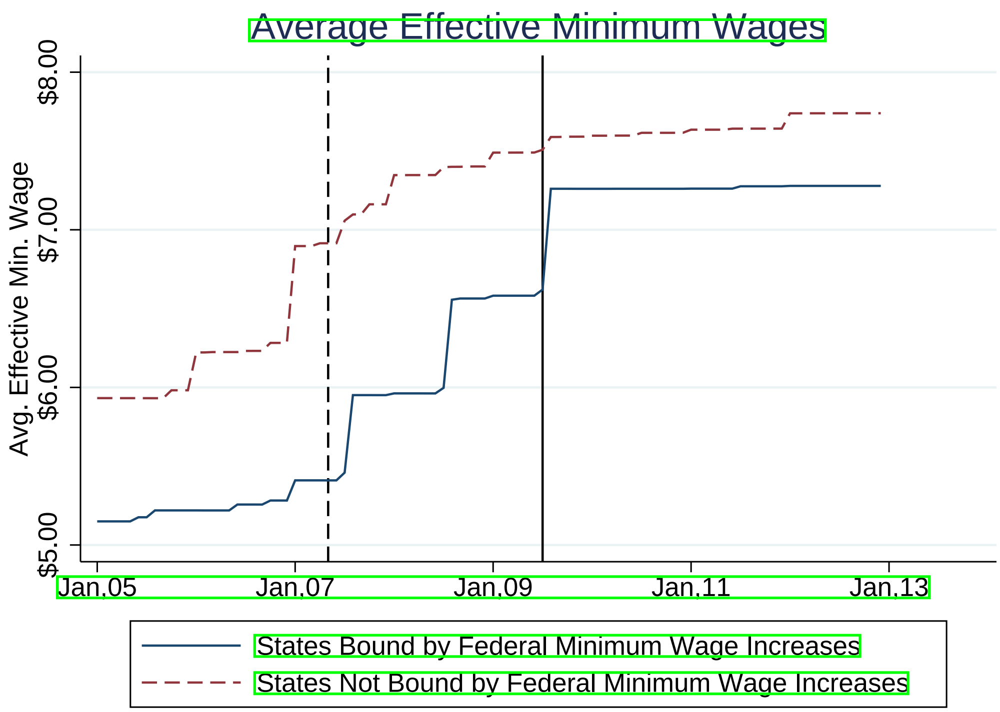


Figure 2: **Evolution of the Average Minimum Wage in Bound and Unbound States:** As in the previous figure, states are defined as fully bound if they were reported by the Bureau of Labor Statistics (BLS) to have had a minimum wage less than \$6.55 in January 2008. Such states were at least partially bound by the July 2008 increase in the federal minimum and fully bound by the July 2009 increase from \$6.55 to \$7.25. Effective monthly minimum wage data were taken from the detailed replication materials associated with Meer and West (Forthcoming). Within each group of states, the average effective minimum wage is weighted by state population. The dashed vertical line indicates the May 2007 passage of the federal minimum wage increases, while the solid vertical line indicates the timing of the July 2009 implementation of the final increase from \$6.55 to \$7.25.

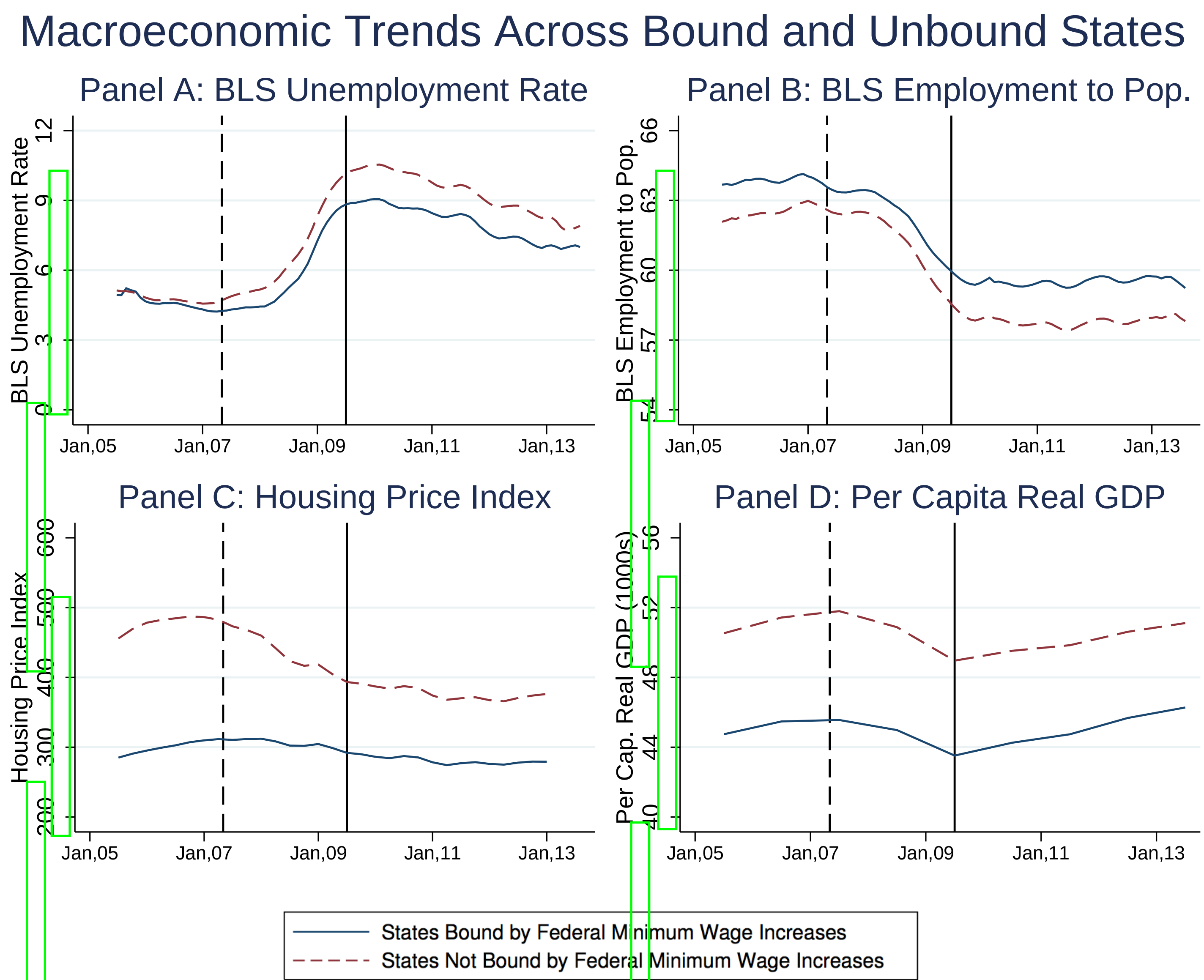
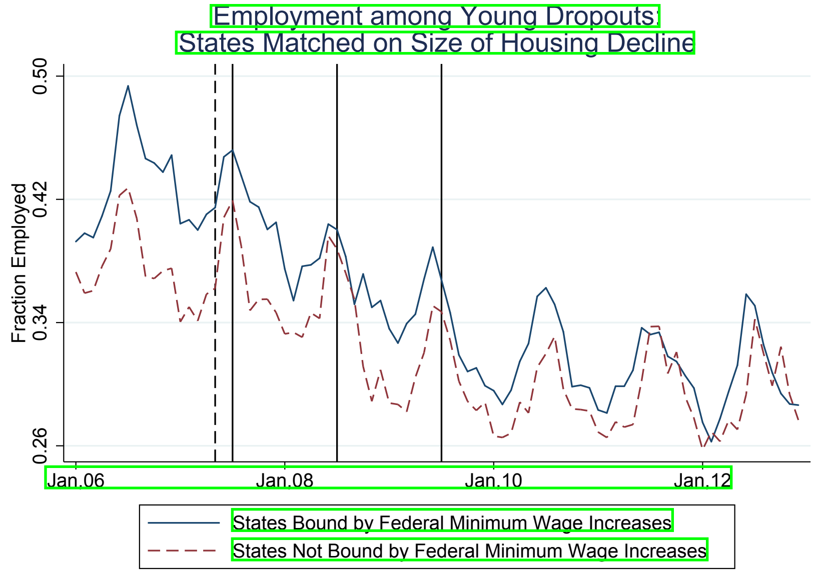


Figure 3: **Macroeconomic Trends in Bound and Unbound States:** Bound and unbound states are defined as in previous figures. This figure's panels plot the evolution of macroeconomic indicators over the course of the housing boom and bust. All series are weighted by state population so as to reflect the weighting applied in the regression analysis. Panel A plots the average monthly unemployment rate, as reported by the BLS. Panel B plots the average monthly employment to population ratio, also as reported by the BLS. Panel C plots the average of the quarterly Federal Housing Finance Agency's all-transactions median housing price index. Panel D plots the average of annual real per capita Gross State Product, as reported by the Bureau of Economic Analysis (BEA). In each panel, the dashed vertical line indicates the May 2007 passage of the federal minimum wage increases, while the solid vertical line indicates the timing of the July 2009 implementation of the final increase from $6.55 to $7.25.

Tabulations from the Baseline Matched Sample

Panel A



Panel B

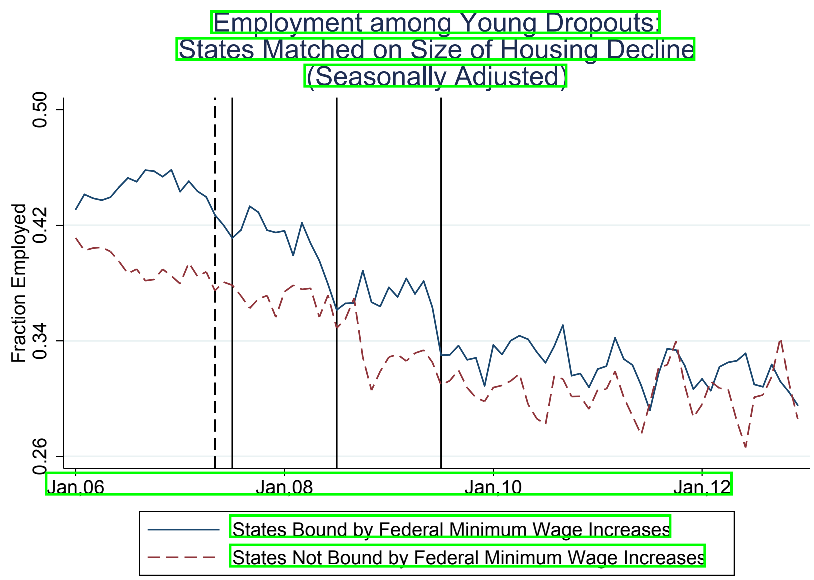


Figure 4: **Tabulations from the Baseline Matched Sample:** The figure presents the means of employment among individuals ages 16 to 30 with less than a high school education in bound and unbound states. The means in Panel A are unadjusted while the means in Panel B are adjusted for state-specific seasonal effects estimated on 2006 samples. The sample of states is restricted in accordance with the criterion that nearest neighbor matches, selected on the size of states' housing declines between 2006 and 2012, have housing declines that differ no more than $10,000 within a matched pair. The dashed vertical line indicates the May 2007 passage of the federal minimum wage increases, while the solid vertical lines indicate the implementation of the July 2007, July 2008, and July 2009 increases in the federal minimum wage.

Dynamic Regression Estimates

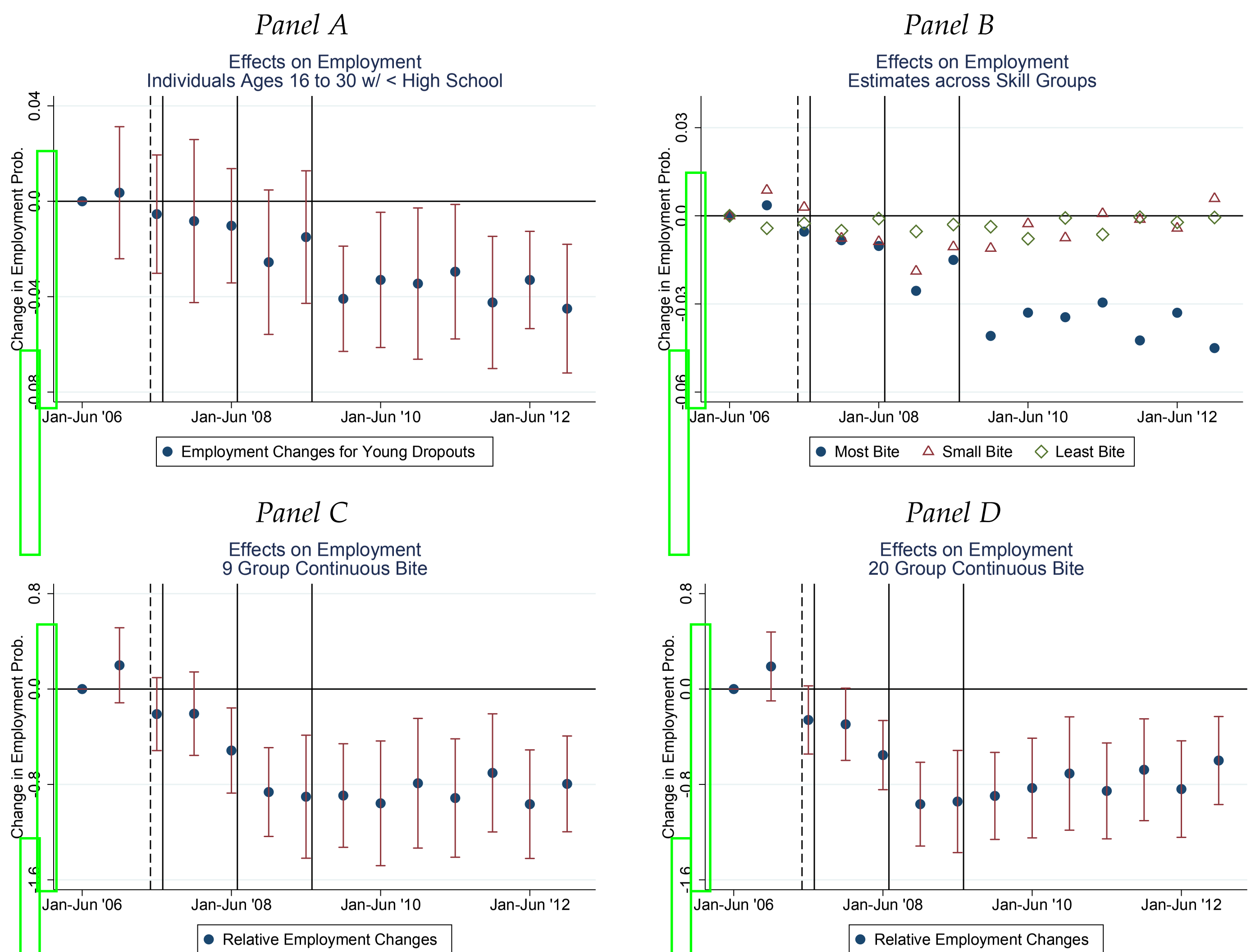


Figure 5: **Dynamic Regression Estimates:** The figure reports dynamic estimates of differentially binding minimum wage increases' effects on the employment of low-skilled workers relative to higher skilled workers. Each marker in Panels A and B is an estimate of a coefficient of the form $\beta_{p(t)}$ from equation (3), where the relevant $p(t)$ correspond with six months intervals and period $p = 0$ is January through June 2006, namely the first half year of the sample. In Panel B, the "Most Bite," "Small Bite," and "Least Bite" division of the sample fully partitions the population ages 16 to 64, just as in the estimates reported in Table 3. Each marker in Panels C and D is an estimate of a coefficient of the form $\beta_{p(t)}$ from equation (4), where the relevant $p(t)$ correspond with six months intervals and period $p = 0$ is January through June 2006, namely the first half year of the sample. The dashed vertical line indicates the May 2007 passage of the federal minimum wage increases, while the solid vertical lines indicate the implementation of the July 2007, July 2008, and July 2009 increases in the federal minimum wage.

Evolution of Low-Skilled Individuals' Wage Distributions

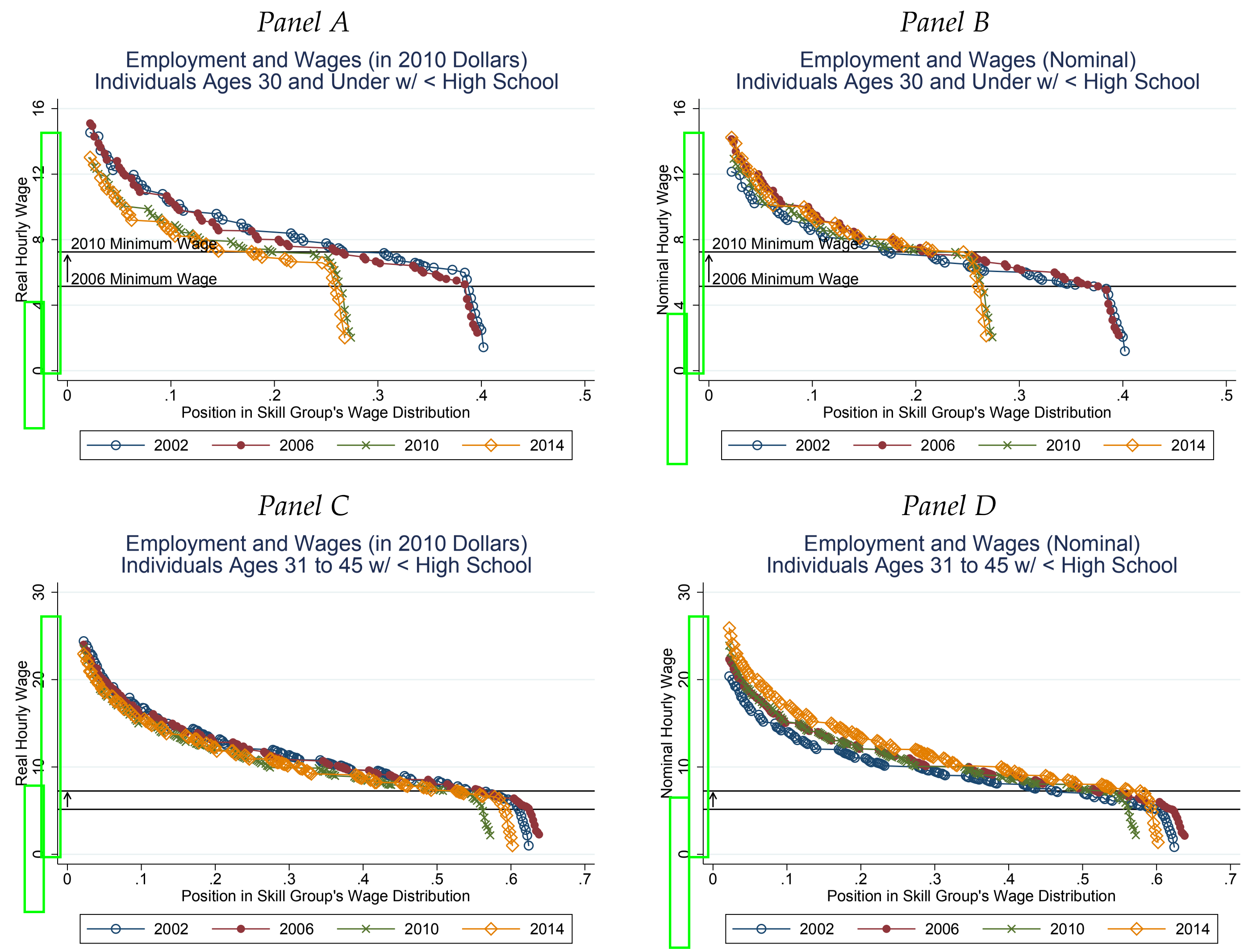


Figure 6: **Evolution of Low-Skilled Individuals' Wage Distributions: 2002, 2006, 2010, and 2014**
Note: The panels of the figure present wage distributions constructed using data from the NBER's CPS-MORG files for 2002, 2006, 2010, and 2014. The samples in Panels A and B consist of individuals ages 16 to 30 with less than a high school education who reside in states that had January 2008 minimum wage rates below $6.55. The samples in Panels C and D consist of individuals ages 31 to 45 with less than a high school education who reside in states that had January 2008 minimum wage rates below $6.55. When available, individual-level wages are taken to be the reported values of the variable "earnhre" divided by 100. When "earnhre" is missing, individual-level wages are estimated as "earnwke/hours." For Panels A and C, nominal wage rates are converted into constant dollars using the Consumer Price Index with July 2009 as the base period. Workers were sorted according to their wage rates, with unemployed individuals assigned wage rates of 0. The wage rates for each year were then divided into 500 quantiles with application of the CPS's population weights. The markers indicate all positive wage quantiles outside of the top 2 percentiles of each distribution.

Table 2: **Summary Statistics on the Magnitudes of Declines in the FHFA Median House Price Index**

	(1)	(2)
	Unbound States	Bound States
	Median House Price Decline (Millions of Dollars)	
Full Sample	0.0720	0.0247
	(0.0666)	(0.0538)
Matched within 5K	0.0297	0.0274
	(0.0403)	(0.0412)
Matched within 10K	0.0332	0.0297
	(0.0397)	(0.0429)
Matched within 20K	0.0385	0.0272
	(0.0371)	(0.0423)
Matched within 5 percent	0.0369	0.0241
	(0.0379)	(0.0394)
No. of States (Full Sample)	24	27

Note: This table reports summary statistics on the magnitudes of declines in states' all-transactions FHFA median house prices indices. Changes are calculated as the average in 2006 minus the average in 2012. Row 1 reports the means of these changes for the full samples of fully and partially bound states. Subsequent rows report means for samples of states that have been restricted based on the quality of nearest neighbor matches. For row 2 the criterion was that the nearest neighbor match deliver a match for which the difference in the states' housing declines was less than $5,000. Row 3 is similar, with a threshold of $10,000, row 4 with a threshold of $20,000, and row 5 with a threshold of 5 log points.

Table 3: **Effects of Binding Minimum Wage Increases across Broad Population Groups**

	(1) Employed	(2) Employed	(3) Employed
Bound x Post 1	-0.037**	-0.011	-0.004
	(0.011)	(0.008)	(0.005)
Bound x Post 2	-0.037**	-0.005	-0.000
	(0.011)	(0.008)	(0.005)
Housing Price Index	0.280*	0.185***	0.054**
	(0.107)	(0.035)	(0.020)
N	580,248	2,098,879	4,621,282
Mean of Dep. Var.	0.38	0.69	0.78
Estimation Framework	D-in-D	D-in-D	D-in-D
State and Time Effects	Yes	Yes	Yes
Degree of Bite	Most Bite	Small Bite	Least Bite

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. The table reports estimates of the minimum wage's short and medium run effects on the relevant dependent variables, which are named in the heading of each column. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (3), where the relevant $p(t)$ corresponds with the period beginning in August 2009 and extending through July 2010. The estimates in row 2 are of the coefficient $\beta_{p(t)}$ from equation (3), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. Across the three columns, the samples comprise all individuals ages 16 to 64 who were surveyed in the full Current Population Survey during the relevant years. The sample in column 1 consists of all individuals between ages 16 and 30 with less than a high school education. The sample in column 2 includes all groups from Table 1 for which the differential bite reported in column 3 was between 0.015 and 0.030. The sample in column 3 includes all groups from Table 1 for which the differential bite reported in column 3 was less than 0.015. The sample extends from January 2006 through December 2012. The reported standard error estimates allow for state level correlation clusters across the estimation errors.

Appendix Material

A Comparisons with Prior Research

This appendix contrasts the current paper's analysis with the analyses in other recent minimum wage studies. I focus primarily on comparisons with other analyses of the minimum wage changes implemented during the Great Recession. The empirical debate over the longer panel of state and federal minimum wage changes, in which Dube, Lester, and Reich (2010), Neumark, Salas, and Wascher (2014), and Meer and West (Forthcoming) are prominent entries, is beyond this paper's scope.

The current paper's analysis relates most closely to the analysis in Clemens and Wither (2014b). My estimation frameworks mirror those in Clemens and Wither (2014b) as closely as possible. They are the repeated cross sectional analogs of the prior paper's analysis of individual level panel data. One purpose of the current paper is thus to explore whether CPS data cross validate the prior paper's analysis of SIPP samples from the same historical episode.

Several differences between the CPS and SIPP data environments are worth discussing. The CPS enjoys advantages with regards to data quality and the time frame over which I am able to construct analysis samples. A first point worth noting is that the CPS underlies national statistical agencies' best estimates of the unemployment rate at both the state and national levels. As noted above, this paper's analysis samples cover the full universe of CPS respondents ages 16 to 64. In-sample employment changes thus map directly into national employment statistics. Second, the 2008 SIPP panel begins during the summer of 2008, limiting the analysis in Clemens and Wither (2014b) to an interval spanning the July 2009 increase in the federal minimum wage.[29] By contrast,

[29]A related point is that Clemens and Wither's (2014b) approach to identifying affected individuals relies on observed wage rates, and thus generates samples of individuals who were employed for at

the CPS allows me to initiate samples in the years preceding the May 2007 legislation behind this period's minimum wage increases. This makes it possible to investigate the full transitional dynamics associated with the law's implementation.

The SIPP, by contrast, has advantages with respect to the identification of low-skilled workers and the outcomes it is able to track. Monthly panel data makes it possible to identify affected workers with greater precision than the demographic and industrial proxies used regularly in the literature. Specifically, the analysis in Clemens and Wither (2014b) uses 12 months of wage and employment information, extending from August 2008 through July 2009, to identify workers whose wage rates would be affected by the July 2009 minimum wage increase. The analysis shows that such individuals were roughly 3 times as likely to report wage rates between the old and new federal minimums as were teenagers. With regards to outcomes, the 2008 SIPP panel makes it possible to track the evolution of an individual's employment and earnings for 3 years following the July 2009 minimum wage increases.[30]

Addison, Blackburn, and Cotti (2013) aptly describe several recent analyses of this period's minimum wage increases as follows:[31]

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least some portion of the year between August 2008 and July 2009. This limits the ability of Clemens and Wither's (2014b) primary analysis sample to speak to the effects of minimum wage increases on transitions *into* employment. The analysis does, however, include separate estimates of transitions into employment among samples of individuals who were unemployed each month from August 2008 through July 2009.

[30]Early analyses of employment-based human capital accumulation highlighted the theoretical possibility that minimum wage increases may reduce opportunities for on-the-job training (Rosen, 1972). In standard models of general and firm specific human capital, the incidence of firms' costs for general human capital training falls on workers through wage offsets. Wage floors may reduce the scope for these implicit exchanges of compensation for training. Direct analyses of the minimum wage's effects on training are rare and arrive at mixed results (Neumark and Wascher, 2001; Mincer and Leighton, 1980; Acemoglu and Pischke, 1999; Arulampalam, Booth, and Bryan, 2004). Clemens and Wither's (2014b) evidence on earnings trajectories suggest that dynamics of this sort, coupled with more straightforward losses in experience accumulation, may have been at work during this historical episode.

[31]I have changed the years associated with Addison, Blackburn, and Cotti's (2013) references if the reference has subsequently been published or if I was unable to find the original source cited. As of this writing, the first three references remain in working paper form, while Hirsch, Kaufman, and Zelenska's (2015) analysis of Georgia and Alabama quick-service restaurants has subsequently been published.

> Even and Macpherson (2010) use CPS data from the 2005-2009 period to estimate standard minimum-wage employment equations, and report negative minimum wage effects on employment for teenagers during this interval. Gittings and Schmutte (2014), using both CPS and Quarterly Workforce Indicators data, find a negative effect on teenage employment for the longer sample period 2000-2009. Hoffman and Ke (2010) focus on the final installment of the latest federal minimum wage increase in 2009. They estimate cross-state differenced models of the minimum-wage impact, finding no significant disemployment effects among teenagers but a significantly negative effect for workers aged 20 to 24. On the other hand, Hirsch, Kaufman, and Zelenska (2015), who investigate establishment-level data for quick-service restaurants in Georgia and Alabama, 2007-2009, fail to uncover any evidence of a negative employment effect.

The most obvious difference between the current paper and the analyses in the papers referenced above is their emphasis on employment among teenagers and/or in food service establishments.[32]

Analyzing populations beyond teenagers and food service establishments is important for two reasons. First, teenagers are less relevant than low-skilled adults from an anti-poverty policy perspective.[33] They are more likely than low-skilled adults to be

[32] Somewhat earlier work on particular demographic groups, such as teenagers, includes Card (1992a), Card (1992b), Neumark and Wascher (1995), and Currie and Fallick (1996). Additional work on teenagers and/or specific industries, like food service and retail, includes Katz and Krueger (1992), Card and Krueger (1994), Kim and Taylor (1995), Neumark and Wascher (2000), Dube, Lester, and Reich (2010), and Giuliano (2013). An exception is Meer and West (Forthcoming), who analyze aggregate employment growth.

[33] As emphasized by Burkhauser and Sabia (2007) and Sabia and Burkhauser (2010), it is primarily low-skilled adults, rather than teenage dependents, who are the intended beneficiaries of anti-poverty efforts. In the European context, Dolado, Kramarz, Machin, Manning, Margolis, Teulings, Saint-Paul, and Keen (1996) observed during the mid-1990s that teenagers, at that time, accounted for a relatively small share of all low-paid workers.

dependents and less likely to have dependents of their own. Additionally, teenage employment is now significantly less common than it was 30, or even 15, years ago. Between 2003 and the present, the teenage employment rate's peak was at 37.4 percent in February 2004. In 2012 it averaged 26.4 percent. By contrast, the teenage employment rate averaged 44 percent over the 1980s and 1990s. Early analyses of teenagers, for example those of Card (1992a,b), thus involved periods during which teenage employment was a more prominent phenomenon. In the current labor market, teenage employment is a less relevant object of analysis.

Second, labor demand elasticities may vary across both industries and demographic groups. This would be expected, for example, if the shapes of within-skill-group productivity distributions vary meaningfully across groups. A similar point applies to analyses of food service establishments. While it is interesting to observe how such firms adjust, food service establishments likely face different input substitution and output demand elasticities than firms in other sectors. Direct analysis of the minimum wage's effects across sectors is thus preferable to inferring economy-wide effects using estimates associated with food service establishments alone.

Assessing the minimum wage's effects on poverty rates, employment opportunities, and the earnings distribution requires directly analyzing the employment and earnings of low-skilled workers themselves. In contrast with the studies discussed above, Clemens and Wither (2014b) use panel data to identify a much broader population of very low-skilled workers. In the CPS's repeated cross-sectional setting, the present study's differences-in-differences analysis focuses on individuals ages 16 to 30 with less than a high school education.[34] Its second empirical approach exploits continuous variation in the minimum wage's bite across sets of skill groups that fully partition the

[34] See Sabia, Burkhauser, and Hansen (2012) for a recent paper that primarily analyzes low-skilled adults.

working age population. The current paper and Clemens and Wither (2014b) thus attempt to utilize as much variation in the minimum wage's bite as possible in the CPS and SIPP data environments respectively.

To compare results across potential analysis samples, Table A.1 reports estimates of equation (3) on a sample restricted to teenagers. The outcomes are the same as those analyzed in Table 8. I estimate that the differentially binding portion of this period's minimum wage increases reduced teenage employment by nearly 2 percentage points. The point estimates are thus non-trivially smaller than the estimated impact on individuals ages 16 to 30 with less than a high school education. Both the short- and medium-run estimate are on the margins of statistical significance at the 5 percent level, with the short-run estimate achieving it and the medium-run estimate failing to do so. In this particular historical episode, analyses of teenagers thus appear to have less power to detect a minimum wage change's employment effects than do analyses of broader populations with low observable skill levels.

The current paper and Clemens and Wither (2014b) also differ from recent analyses in their approach to documenting and accounting for the housing decline's effect on low-skilled workers' employment. The housing crisis is sufficiently well documented that the need to account for it is readily apparent. The data required to do so are also readily at hand. As shown in Table 2, partially bound states' housing declines were significantly less severe than fully bound states' housing declines. As also noted above, estimates from Charles, Hurst, and Notowidigdo (2013) make it possible to gauge the likely bias associated with this heterogeneity in the housing decline's severity across states. I thus adopt a baseline specification with several relevant characteristics. First, I take a straightforward approach to controlling directly for heterogeneity in the housing declines' severity. Second, I demonstrate that controlling for additional macroeconomic covariates leaves the baseline estimates little changed.[35] Third, I show that the bias associated with making no attempt to control for heterogeneity in the housing declines' severity is of the expected sign and magnitude. Fourth, I show that the results are robust to implementing triple-difference extensions that make more extensive use of variation in the minimum wage's relevance across skill groups. Fifth, I show that the results are robust to accounting for heterogeneity in the housing decline's severity using a standard matching procedure.

Finally, the analyses in this paper and Clemens and Wither (2014b) hew closely to standard program evaluation methods. This contrasts with what Addison, Blackburn, and Cotti (2013) describe as the traditional estimation of "standard minimum-wage employment equations." It is increasingly recognized that the traditional approach embeds quite strong assumptions regarding the dynamics with which a minimum wage change influences employment. It can also obscure dynamics linked to lags between legislation and implementation. Because lags between legislation and implementation may differ significantly across natural experiments, this can be particularly problematic for studies of the longer panel of state and federal minimum wage changes.

Implementation lags and dynamics at least partially underly observers' difficulty in adjudicating the empirical debate over historical minimum wage changes' effects. The debate within the literature suggests that analyses of the longer panel of minimum wage changes are sensitive to a combination of specification choices and sample selection procedures.[36] Given the diversity of natural experiments involved, it seems unlikely that any one approach to selecting control groups or any one specification of dynamics would be universally appropriate.

[35]The point estimates decline modestly in the full sample analysis and increase modestly in the analysis of samples in which states were matched on the size of their housing declines.

[36]The discussion of the strengths and weaknesses of various estimation strategies by Dolado, Kramarz, Machin, Manning, Margolis, Teulings, Saint-Paul, and Keen (1996) is, in many respects, quite prescient regarding sources of disagreement in the recent literature.

A key element of the program evaluation approach adopted here is the visual presentation of results (see Figures 4 and 5) that provide evidence on potentially relevant dynamics. Potentially relevant dynamics include anticipation effects and the possibility of differences between short and long run responses. Allowance for anticipation effects is particularly important in this setting due to the 26 month lag between the relevant legislation and the implementation of the third and final increase in the federal minimum wage. Regarding employment dynamics, simulations by Meer and West (2013) show why analyses of the longer panel of state minimum wage changes will tend to have difficulty estimating anything other than relatively short-run effects.

Sorkin (2015) provides a theoretical basis for expecting the distinction between short- and long-run effects to be economically meaningful. The considerations Sorkin (2015) highlights point to two conceptually relevant features of the setting I analyze. A first point is that my post-legislation analysis window is long by the literature's standards. It includes both a 26 month implementation phase and a 40 month post-implementation period.[37] Second, because the analysis involves a period of significant labor market upheaval, the shifts in production technologies Sorkin (2015) emphasizes may have been hastened relative to less turbulent historical episodes.

The empirical approaches in this paper and Clemens and Wither (2014b) are facilitated in part by the historical setting. In contrast with the longer panel of state and federal minimum wage changes, the period analyzed here has three attractive features. First, there is a relatively clean division of states into those that were fully and partially bound by this period's federal minimum wage changes. Second, this division roughly evenly divides the U.S. population. Third, because states enacted very few minimum wage changes between July 2009 and December 2012, the analysis period has a less con-

[37] After 2012, the setting encounter's the minimum wage literature's usual problem that the post-policy change period becomes contaminated with additional state (and municipal) minimum wage changes.

taminated "post" policy change interval than is available to many studies. These features of the policy space make this a well suited application for relatively straightforward program evaluation methods.

B Additional Robustness Tables

Table B.2: **Effects of Binding Minimum Wage Increases across Broad Population Groups**

	(1) Employed	(2) Employed	(3) Employed
Bound x Post 1	-0.029**	-0.008	-0.004
	(0.009)	(0.007)	(0.005)
Bound x Post 2	-0.028**	-0.002	0.000
	(0.009)	(0.007)	(0.004)
Housing Price Index	0.209*	0.173***	0.066**
	(0.083)	(0.030)	(0.024)
N	580,248	2,098,879	4,621,282
Mean of Dep. Var.	0.38	0.69	0.78
Estimation Framework	D-in-D	D-in-D	D-in-D
State and Time Effects	Yes	Yes	Yes
Time Varying Demographic Controls	Yes	Yes	Yes
Degree of Bite	Most Bite	Small Bite	Least Bite

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. The table reports estimates of the minimum wage's short and medium run effects on the relevant dependent variables, which are named in the heading of each column. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (3), where the relevant $p(t)$ corresponds with the period beginning in August 2009 and extending through July 2010. The estimates in row 2 are of the coefficient $\beta_{p(t)}$ from equation (3), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. Across the three columns, the samples comprise all individuals aged 16 to 64 who were surveyed in the full Current Population Survey during the relevant years. The sample in column 1 consists of all individuals between ages 16 and 30 with less than a high school education. The sample in column 2 includes all groups from Table 1 for which the differential bite reported in column 3 was between 0.015 and 0.030. The sample in column 3 includes all groups from Table 1 for which the differential bite reported in column 3 was less than 0.015. The sample extends from January 2006 through December 2012. The time varying demographic controls include full sets of period ($p(t)$ as defined in the text) indicators interacted with full sets of indicators for the values taken by the CPS variables "age," "educ," "sex," and a recoded race/ethnicity variable with separate values for Asian, Black, White Non-Hispanic, Hispanic, and Other. The reported standard error estimates allow for state level correlation clusters across the estimation errors.