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Additional Evidence and Replication Code for Analyzing the Effects of Minimum Wage Increases Enacted During the Great Recession

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Abstract:

In previous work (Clemens and Wither, 2014), we reported evidence that minimum wage increases contributed to declines in low-skilled individuals' employment during the Great Recession. Because this work has generated both interest and disagreement, we use the current paper to present the code underlying our baseline estimates and to present supplemental results. Our supplemental analysis focuses on choices that arise when processing wage and earnings data from the Survey of Income and Program Participation to isolate samples of "low-skilled" individuals. We further assess the relevance of several alternative approaches to sample selection. We show that these data processing and sample selection margins have little effect on the qualitative implications of our estimates. We present additional evidence that minimum wage increases had a negative effect on employment entry among individuals who were unemployed throughout our baseline period.

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In a recent working paper (Clemens and Wither, 2014), we reported evidence that federal minimum wage increases contributed to employment declines among relatively low-skilled individuals during the Great Recession. Our estimates made use of the fact that this period's minimum wage changes were differentially binding across states. Using the 2008 Survey of Income and Program Participation (SIPP), we analyzed samples of individuals that we identified as being low skilled on the basis of their wage histories over the year preceding the federal minimum wage's rise from $6.55 to $7.25.

Our identification of low-skilled individuals on the basis of their wage histories complements the minimum wage literature's standard focus on employment among teenagers or in the fast food industry. This approach has three primary benefits relative to standard approaches. First, it introduces the possibility of isolating an intensively affected sample, which can aid with precision.[1] Second, it makes it possible to analyze low-skilled individuals across all ages and industries, which is relevant to external validity. Third, it enables us to longitudinally track a minimum wage changes' effects on individuals' medium-run earnings trajectories.

While our use of wage histories has benefits, it is by no means without challenges. This paper elaborates upon and investigates the relevance of several issues that arise when implementing our approach. A first issue involves the construction of the average baseline wage variable along which we select our analysis sample. We discuss coding choices that shape the extent to which our "low-skilled" designation incorporates wage rates imputed on the basis of self reported earnings and hours in addition to the wage

[1]Note that this benefit will materialize to a much greater degree in the current setting, where inflation and productivity growth were low, than in settings where inflation and productivity growth are rapid. Linneman (1982) provides an early example of analysis that accounts for the fact that counterfactual wage growth due to inflation and productivity growth will affect the degree to which a minimum wage change is binding. As the current project has developed, we have used baseline wage distributions, inflation data, and productivity data to elaborate on this point. See, for example, the wage distributions presented and discussed in Clemens (2015) and in a September 2016 revision of (Clemens and Wither, 2014), which can be found at the following link: `http://econweb.ucsd.edu/~j1clemens/pdfs/ClemensWitherMinimumWageGreatRecession.pdf`.

rates reported by individuals who are paid on an hourly basis. We observe that the SIPP's wage and earnings variables make the coding of this information somewhat less obvious than one might expect. We thus present the code underlying three alternative approaches to constructing individuals' baseline wage histories, along with the results we obtain when using each of these approaches to select our analysis sample.

The second issue we address involves the manner in which we use baseline wage data to divide the sample into the "low-skilled" and "higher-skilled" groups on which we estimate our regression specifications. In Clemens and Wither (2014), our approach was to analyze samples of workers with average baseline wage rates lower than $7.50. A potential concern with this approach relates to the fact that some states had baseline minimum wage rates in excess of $7.50. In California and Massachusetts, for example, the minimum wage was $8.00 over the entirety of our SIPP sample. Consequently, our baseline "control" group consists disproportionately of sub-minimum wage (e.g., tipped) workers. This raises the possibility that individuals in the treatment and control groups may differ from one another in ways that are relevant to their employment trajectories.

We provide evidence on this concern's relevance by considering two alternatives to selecting samples on the basis of a common nominal cutoff. One alternative is to select samples based on the relationship between individuals' average baseline wage rates and the minimum wage rates applicable in their respective states. A second alternative is to select samples on the basis of individuals' percentiles in their respective wage distributions. We show that the samples selected on these bases are relatively well balanced with regards to the fraction of individuals earning less than the minimum wage. We then show that selecting samples on these bases has little effect on our estimates.

A separate issue involves the margins that we are able to analyze using our approach. Our baseline analysis sample consists of individuals who were employed at baseline. Estimates involving such samples will detect changes in employment exit but may fail

to capture changes in employment entry. Addressing the entry margin requires analysis of individuals who were unemployed at baseline. While our original working paper included an analysis of such individuals, it was presented in an appendix. We use the current paper to further develop this analysis and present it more prominently. The data provide evidence that entry was moderately reduced by the minimum wage increases under analysis.

Our analysis of individuals who lacked employment at baseline helps us to resolve an additional potential concern involving sample composition. Differences in states' baseline minimum wage rates may alter selection into employment. This raises a potential concern because our low-skilled group consists of individuals who were employed for at least one month at baseline. Consequently, our sample selection procedure involves an employment margin that might be affected by differences in states' baseline minimum wage rates. This would bias our estimates if it translates into differences in the average treatment and control group individuals' counterfactual employment trajectories. We address this concern by analyzing samples that contain *both* the individuals with low baseline wage rates *and* the individuals who lacked employment at baseline.

A final issue involves a combination of panel balance and sample attrition. Substantial attrition occurred over the course of the 2008 SIPP panel. As a baseline panel balance criterion, we required that an individual appear in the sample for at least 36 of the 48 months that our analysis spans. In the space below, we show that our results are not sensitive to alternative criteria including 12 months, 24 months, or a fully balanced panel of 48 months. We show further that attrition patterns are very weakly correlated with the employment declines we estimate.

The remainder of this paper proceeds as follows. Section 1 provides an overview of the empirical setting and basic estimation framework. Section 2 discusses alternative sample construction procedures. Section 3 discusses alternative approaches to defining

the "target" sample. Section 4 presents descriptive statistics on the alternative analysis samples. Section 5 presents regression analysis on the alternative analysis samples. Finally, section 6 offers concluding remarks.

1 An Overview of the Empirical Setting

This section provides an overview of our empirical setting. Section 1.1 briefly describes the minimum wage changes under analysis. Section 1.2 presents our basic strategy for estimating the effects of the federal minimum wage's rise from $6.55 to $7.25. Section 1.3 presents the unadjusted employment tabulations that underlie our estimates. Section 1.4 discusses two distinct sets of threats to using the unadjusted data presented in section 1.3 to directly infer the effects of this period's minimum wage increases. The first set of threats involves omitted variables concerns linked to the forces underlying the Great Recession. The second involves issues related to the composition of the sample. A more complete description of the setting can be found in Clemens and Wither (2014).

1.1 Minimum Wage Changes under Analysis

On May 25th, 2007, Congress legislated a series of increases in the federal minimum wage. On July 24th of 2007, the minimum wage rose from $5.15 to $5.85. On July 24th of 2008, it rose from $5.85 to $6.55. On July 24th of 2009, it rose from $6.55 to $7.25.

The original Clemens and Wither (2014) analysis uses the 2008 panel of the Survey of Income and Program Participation (SIPP) to follow the employment of low-skilled workers as the federal minimum wage rose from $6.55 to $7.25. Because the SIPP panel begins in the summer of 2008, it does not track individuals across the full time period spanning the minimum wage changes' legislation and implementation.[2] Instead, the

[2]Additional analysis in Clemens (2015), some of which was incorporated into a September 2016 re-

analysis uses wage data from August 2008 through July 2009 to identify low-skilled individuals and track their employment and incomes as the July 2009 increase went into effect.

We make use of the fact that roughly half of the U.S. states were fully bound by the federal minimum wage's rise from \$6.55 to \$7.25, while the other half were essentially unbound. Figure 1 shows Clemens and Wither's (2014) division of states into those that were fully and partially bound by changes in the federal minimum wage. Figure 2 shows the time paths of the average effective minimum wage rates across these groups of states.[3] On average, the effective minimum wage rose \$0.60 to \$0.70 cents more in fully bound states than in partially bound states.

1.2 Basic Estimation Framework

The basic estimation framework of Clemens and Wither (2014) is the difference-in-differences model reproduced below:

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

Equation (1) includes controls for state effects, State_s and time effects, Time_t, which are the most basic features of difference-in-differences estimation. Because the setting

vision of Clemens and Wither (2014), uses the Current Population Survey to track employment among individuals in demographic groups with low observable skill levels across a period that captures a truly pre-legislation baseline.

[3]Both figures were initially presented in Clemens and Wither (2014).

is longitudinal, the specification further controls for individual fixed effects, Individual_i. The vector $\mathbf{X_{s,t}}$ contains time varying controls for each state's macroeconomic conditions.

Equation (1) allows for dynamics motivated by graphical evidence that we reported and discussed in Clemens and Wither (2014). Specifically, we find that the prevalence of wages between the old and new federal minimum declined rapidly beginning in April 2009. We thus code May to July 2009 as a "Transition" period. Prior months correspond to the baseline, or period $p = 0$. We code August 2009 through July 2010 as period Post 1 and all subsequent months as period Post 2. The primary coefficients of interest are $\beta_{\text{Post 1}(t)}$ and $\beta_{\text{Post 2}(t)}$, which are estimates of differential changes in the dependent variable in states that were bound by the new federal minimum relative to states that were not bound.

1.3 Evidence from Unadjusted Employment Changes

Panel A of table 1 presents tabulations of employment among individuals in the low-skilled samples on which we focus. The 5 rows of panel A are associated with 5 alternative approaches, discussed in detail below, to identifying our target group of low-skilled individuals. The 7 columns present the levels and changes underlying our difference-in-differences estimation. The first 4 columns present baseline and post-implementation employment rates in the treatment and control group. Columns 5 and 6 present changes specific to the control group and treatment group respectively. Finally, column 7 presents the differential employment changes that correspond to difference-in-differences estimates in the absence of regression adjustment for covariates. Across the 5 samples, the unadjusted difference-in-differences estimates range from -2.3 percentage points to -6.0 percentage points. The estimate associated with our baseline analysis sample is -4.2 percentage points.

Differences in estimates across samples are less quantitatively substantial than they

might initially appear. This is because alternative sample selection criteria generate samples that account for varying fractions of the population of workers in states that were bound by the minimum wage changes we analyze. As revealed by the observation counts at the bottom of table 3, the sample associated with the estimate of -2.3 percentage points contains the largest number of bound state individuals. Similarly, the sample associated with the estimate of -6.0 percentage points contains the smallest number bound state individuals. As a fraction of their respective states' working age populations, the estimated employment declines thus differ fairly modestly. This relates to a broader point, made elsewhere by Sabia, Burkhauser, and Hansen (2012), that minimum wage elasticities will tend to vary with the intensity with which an analysis sample's wage rates are affected by the minimum wage increase under analysis.[4]

1.4 Threats to the Basic Methodology

The primary goal of the empirical analysis is to generate evidence on the causal effect of this period's minimum wage changes on employment. This section discusses two distinct forms of threats to directly using the unadjusted data from panel A of table 3 to infer the causal effects of this period's minimum wage changes. The first involves the possibility that variations in macroeconomic conditions are a source of omitted variables bias. These issues received considerable attention in our original analysis as well subsequent analyses (Clemens, 2017a,b), which are responsive to a comment from Zipperer (2016). The second set of issues involves the possibility that estimates are affected by the composition of the sample. These issue received relatively little attention in our original analysis. The current paper brings these sample composition issues to the fore.

[4]Neumark (2016) nicely illustrates this point in a discussion of why it is essential to know the intensity of a minimum wage change's bite on a group's wage distribution in order to determine the elasticity above and below which a minimum wage increase's overall effect on the group's income will tend to be positive or negative.

1.4.1 Variations in Macroeconomic Conditions

Panel B of table 1 presents descriptive statistics on macroeconomic conditions across the "treatment" and "control" groups in our analysis. The variables include a combination of macroeconomic aggregates (personal income per capita), employment aggregates (the overall employment rate, the unemployment rate, and the prime age employment rate) and housing market indicators (median house prices and aggregate construction output). The data thus provide an initial look at the manner in which the unadjusted changes presented in column 7 of panel B could plausibly be biased. Column 7 provides evidence that the forces underlying the recession were more severe in the control states than in the treatment states. The overall employment rate declined nearly half a percentage point less in the control states than in the treatment states, while prime aged employment declined just over half a percentage point less. House prices declined $17,000 less, while construction output declined roughly 3 percent less.

The SIPP sample begins mid-way through the Great Recession. Comparing the recession's severity in the treatment states relative to the control states thus requires considering an earlier baseline. The data presented in table 2, and in figures 3, 4, and 5 enable the relevant comparisons. Table 2 presents data on the same set of macroeconomic covariates as panel B of table 1. The difference is that the baseline period for table 2 extends from January 2006 through May 2007, which was the month during which the federal minimum wage changes were legislated.

The data reveal that the Great Recession was significantly more severe in the states that comprise the control group than in the states that comprise the treatment group. Income per capita declined by just over $1,000 less in the treatment states than in the control states. Turning to employment indicators, the table reveals that the overall employment rate declined half a percentage point less in the treatment group than in the control group. The prime aged employment rate declined nearly a full percentage point

less, while the unemployment rate rose just over 1 percentage point less. Turning finally to housing market indicators, median house prices declined an average of $84,000 less in the treatment group than in the control group. Figure 4 shows that construction's share of employment in bound and unbound states moved in parallel from 2002 through 2015, while figure 5 shows that the BEA's construction output index declined substantially more in unbound states over the course of the recession. These macroeconomic indicators, which are also presented and discussed in Clemens and Wither (2014) and in Clemens (2017a,b), thus provide substantial direct evidence of forces that would bias estimates towards positive values and no direct evidence of forces that would bias estimates towards negative values.

1.4.2 Variations in Sample Composition

Estimates of $\beta_{p(t)}$ can be biased by differences in the trajectories of the treatment and control groups' employment probabilities that are driven by or associated with differences in the samples' characteristics. Concerns of this sort are of interest due in part to the nature of the natural experiment under analysis. In the setting under analysis, a conceptual difficulty arises because the "treatment" and "control" group had different minimum wage policies at baseline. The "treatment" of the federal minimum wage increase forced the treatment states to increase their minimum wage rates towards the minimum wage rates that prevailed in the control states.

This feature of the setting raises at least two distinct issues of interest. First, differences in states' baseline minimum wage rates may alter selection into employment. Because the selection of our target group requires that an individual be employed for at least one month at baseline, this margin may be affected. This can bias estimates of $\beta_{p(t)}$ if the associated selection translates into differential employment trajectories. A straightforward way to provide evidence on this issue is to estimate our specification on samples

that contain *both* the individuals with low baseline wage rates *and* the individuals who lacked employment at baseline. We present estimates on such samples below.

Second, the analysis in Clemens and Wither (2014) is exposed to the concern that the low-skilled individuals in the control group are "peculiar." Our baseline approach was to analyze samples of workers with baseline wage rates below $7.50. A benefit of this approach is that it isolates a well targeted sample of very low-skilled individuals. A potential concern, however, arises from the fact that some states in the control group had baseline minimum wage rates that were higher than $7.50. The "control" group thus consists disproportionately of sub-minimum wage workers (e.g., tipped workers). If sub-minimum wage workers have different counterfactual employment trajectories than near minimum wage workers, this difference between our treatment and control groups could be a source of bias.

We address this potential bias by considering two alternative approaches to selecting our analysis samples. One alternative is to select samples based on the relationship between an individuals' average baseline wage and the minimum wage rate applicable in their respective states. A second alternative is to select samples on the basis of individuals' percentiles in their wage distributions. We show that the samples selected on these bases are balanced with regards to the fraction of individuals earning less than the minimum wage. We then show that selecting samples on these alternative bases has essentially no effect on our estimates.[5]

[5]Our original working paper devoted significant attention to the separate possibility that the treatment and control group may have differed in the extent to which their industries were exposed to shocks. We explored this concern by controlling for time trends interacted with indicators for individuals' modal industry of employment at baseline. We similarly investigated the relevance of differences between the treatment and control groups' demographics by controlling for time trends interacted with indicator variables specific to each year of age, educational attainment, and gender.

2 Sample Construction Choices

This section walks through the details of our construction of the average baseline wage measure we use to select analysis samples. Multiple features of the data make the ideal construction of an "average baseline wage" variable less obvious than one might expect. A first issue is standard across wage measurement settings. Some wage rates are reported as the wage rates of individuals who are paid on an hourly basis, while some wage rates must be imputed as earnings divided by hours of work. This creates a trade-off in that there is additional information in the measure of earnings divided by hours, but that information is relatively prone to measurement error.

Two additional issues elevate the difficulty of navigating the first. The SIPP's primary hourly wage variable is a "reference period" variable. This contrasts with the SIPP's employment variables, which are monthly. The "reference period" refers to the four month period about which a respondent is being questioned at the time of the interview. If the respondent reports being employed at any time during the 4 month window in question, the variable *ejobcntr* indicates employment during the reference period. If the respondent further reports working for an hourly wage during the reference period, then the variable $tpyrate1$ takes the value of that individual's wage at his or her primary job. A complication in using this information arises because a significant number of month-specific observations code individuals as having a job (the employment status recode variable, *rmesr*, is between 1 and 5), but as being outside the universe of the variable *ejobcntr* (that is, $ejobcntr = -1$).

These issues generate two data processing questions. The first question is what use should be made of earnings information for individuals who are employed but who are coded as being out of the universe for *ejobcntr* and hence also for $tpyrate1$. The second question is what use should be made of earnings information for individuals who are coded as employed by both *ejobcntr* and *rmesr*, but for whom $tpyrate1 = 0$ because they

are not paid on an hourly basis.[6]

The analysis below considers three degrees of utilization of wage rates imputed as earnings divided by hours. Our baseline approach uses the earnings divided by hours imputation to estimate wage rates in cases where the individual is employed (*ejobcntr* and *rmesr* both indicate employment) and earnings are positive ($tpearn > 0$), but $tpyrate1 = 0$. A second approach uses the earnings divided by hours imputation in all cases in which the variable *rmesr* indicates that the individual is employed, but $tpyrate1 = 0$. A third approach uses only the observations for which $tpyrate1$ takes non-zero values. The code associated with the construction of each of these "baseline wage" variables can be found in this paper's appendix.

A separate question is how the information in a given measure of the "average baseline wage" should be used to divide the sample into a "target" group and groups of individuals with higher skill levels. The analysis presented below adopts three approaches. The baseline approach from Clemens and Wither (2014) defines the target group based on an absolute nominal level of the average baseline wage. As discussed above, a criticism that this approach encounters relates to the fact that the nominal ceiling we imposed was beneath the minimum wage rates effective in California and Massachusetts during our baseline period. Consequently, our "control" group consists disproportionately of sub-minimum wage workers. One purpose of our alternative sample definitions is to provide evidence that this imbalance has not biased our results.

The second approach we consider is to define the threshold relative to the minimum wage rate effective in each state at baseline. Specifically, we define an alternative target group to include all individuals whose average baseline wage rates were within \$0.50 of the minimum wage rate effective in their state in January 2009. A third approach is

[6]Unfortunately, $tpyrate1$ takes a value of 0 for both those who are not paid on an hourly basis and those who are truly working for no pay (e.g., as interns).

based on percentiles. That is, we define the treatment and control group to include individuals in the bottom 13 percentiles of their respective states' wage distributions.[7] The summary statistics presented in section 4 reveal that these alternative approaches eliminate the imbalance in the treatment and control group members' likelihood of working at wage rates below their states' effective minimums at baseline. An additional way to achieve this is to simply raise the nominal ceiling so that it exceeds the minimum wage rates effective in all states at baseline. None of these approaches significantly alter the implications of our estimates of the effects of this period's minimum wage changes on employment.

3 Alternative "Target Group" Samples

The analysis presented below considers 5 distinct definitions of the "target" group on which we estimate equation (1). The first is the baseline analyzed by Clemens and Wither (2014). The wage measure for this sample uses information on observations that are in the universe for the variable *ejobcntr*. The baseline target group is defined to include individuals with average baseline wage rates less than $7.50.

The second definition utilizes an average baseline wage measure that incorporates additional wage imputations. Specifically, it incorporates wage imputations for cases that are outside of the universe for *ejobcntr* despite the individual being employed. Using this wage measure, the baseline target group is again defined to include individuals with average baseline wage rates less than $7.50.

The third definition uses fewer wage imputations than the baseline. The average

[7]The baseline definition of the target group contains roughly 13 percent of the employed population in the treatment states. The percentile-based definition is thus intended to equalize the control group and treatment group along this margin. We use percentiles within the full sets of treatment and control states, rather than within each state individually, due to sample size. Percentiles *within* small states are less reliable proxies for an individual's absolute skill level due to sample size.

baseline wage measure is constructed using only the cases for which the wage variable *tpyrate*1 is positive. Using this wage measure, the baseline target group is again defined to include individuals with average baseline wage rates less than $7.50.

The fourth and fifth definitions return to the baseline definition of the average baseline wage. These definitions differ in their use of this information for dividing the sample. For the fourth definition, the target group consists of individuals whose average baseline wage is no more than $0.50 higher than their state's effective minimum wage rate in January 2009. The fifth definition is based on percentiles. The fifth target group includes all individuals who were in the bottom 13 percentiles of the treatment and control groups' baseline wage distributions.[8]

4 Summary Statistics on the Alternative "Target" Samples

Table 3 presents baseline summary statistics on the 5 "target" samples described above. Columns 1 and 2 describe the control group and treatment group, respectively, of the baseline sample. Columns 3 through 10 present the same sets of summary statistics for the 4 alternative definitions of the target group. The groups are presented in the same order in which they were initially discussed above.

We focus on two aspects of these summary statistics. First, the second and third variables summarized in the table reveal one of the imbalances that may be a source of concern in our baseline analysis. They reveal that the baseline control group has a relatively large fraction of individuals who were sub-minimum wage workers at baseline.[9] Because differences between sub-minimum wage workers and near minimum

[8]As noted above, this is equivalent to simply adding a few percentiles to the control group sample such that the treatment and control group cover the same percentiles of their respective wage distributions.

[9]It is worth noting that it is by no means obvious that this particular imbalance is a genuine "threat" to our estimates. The wage rates of all individuals in these samples reveal them to be quite low-skilled. The key question is whether differences across the treatment and control group might generate differences in

wage workers *might* generate different counterfactual employment trajectories, this difference is worth investigating further. Note that this imbalance is addressed by the sample inclusion criteria adopted for target groups 4 and 5. In target groups 4 and 5, similar fractions of the treatment and control samples reported sub-minimum wage rates at baseline. These samples are also more closely balanced on age, education, and the probability of working for no pay at baseline.

A second issue involves variations in the extent of the federal minimum wage's bite. This is summarized by the first variable in the table, which describes the fraction of baseline months in which individuals' wage rates were between the old and new federal minimum wage. The variations we emphasize are captured by the first set of regressions reported in the following section. Target groups 4 and 5 involve definitions that generate differential bite that moderately exceeds that associated with the baseline analysis sample. By contrast, target group 2 involves a definition under which the treatment and control group are less differentiated along this margin. All else equal, we would thus expect estimated employment effects to appear moderately larger than the baseline when we analyze the 4th and 5th target groups, and to be moderately smaller than the baseline when we analyze the 2nd target group.

5 Regression Analysis

This section is divided into four sets of regression estimates. Section 5.1 reports estimates of equation (1) on each of the 5 "target" group samples discussed above. The outcomes considered include a "first stage" likelihood of having a wage in the affected range, the probability of employment, and the probability of either having no job or working at a self-reported wage rate of 0. Section 5.2 presents estimates that, using our

their employment trajectories.

5 approaches to dividing the population across skill groups, explore the relationship between binding minimum wage increases and employment across the entirety of the working age population. Section 5.3 presents evidence on whether our estimates are sensitive to controlling for additional macroeconomic covariates. Section 5.4 presents evidence on the sensitivity of Clemens and Wither's (2014) baseline estimates to changes in the panel balance criterion we apply.

5.1 Estimates across Target Samples

This section presents estimates of equation (1) on the 5 "target" group samples discussed above. Table 4 presents estimates of the relationship between binding minimum wage increases and the probability that an individual reports a wage between $5.15 and $7.25. The estimates thus describe the extent to which the "treatment" state sample was bound by the increase in the federal minimum wage.

The estimate in column 1 reveals that, in the baseline sample, individuals in the treatment states saw their probability of reporting an affected wage rate decline 16 percentage points more than individuals in control states. Estimates vary moderately across the 4 supplemental samples. For the group in column 2, the differential change was 13 percentage points. This group, which has a moderately larger sample than the baseline group, is less intensively treated than the baseline group. For the remaining three groups, the differential changes is moderately higher than in the case of the baseline group. These groups are thus moderately more intensively treated by the minimum wage change than is the baseline group.

Panel A of table 5 presents estimates of the effect of binding minimum wage increases on employment. Column 1 reports the Clemens and Wither (2014) baseline. It reveals that, conditional on the magnitude of states' house price declines, employment declined 6 percentage points more among low-skilled individuals in treatment states relative to

control states. Column 2 reports a smaller estimate of just under 4 percentage points. Note that because this sample is larger, the estimates translate into similar changes in the full-population's employment rate. This reflects the fact that the sample in column 2 was, as noted above, less intensively treated than the baseline group. The estimates in columns 3 through 5 range from 5.4 percentage points to 7.7 percentage points, with the largest estimate coming from the sample that, as shown in table 4, was the most intensively treated. Adjusted for the intensity of treatment observed in table 4, the estimates are thus quite similar across approaches to assembling the target group.

Panel B of table 5 presents estimates of the effect of binding minimum wage increases on the probability of either having no job or working for no pay, as in an internship. The pattern of estimates is quite similar to that observed in panel A. Estimated medium run effects range from 6.2 percentage points to 8.3 percentage points.

Tables 6 and 7 present estimates that involve two additional alterations to the sample's composition. First, the estimates in table 6 apply the SIPP's sample weights. The SIPP's sampling design is such that weighting does not serve an obvious purpose in this setting. The SIPP is designed to oversample low income households, which is more or less the basis along which our baseline analysis sample is selected. Weights are thus relevant for generating an appropriate estimate of the fraction of the U.S. population our low-skilled sample represents. Within that sample, however, the SIPP's design is not such that weighting corrects for a dimension along which the sample is unrepresentative.[10] The application of sample weights has little effect on our results.

Second, the estimates in table 7 involve two changes to the sequence of data cleaning operations. The first change effectively alters the panel balance criterion. Specifically,

[10]This can be contrasted with the sampling design of the Current Population Survey. The Current Population Survey (CPS) over samples individuals in small states relative to individuals in large states. Equally weighting the observations in the CPS thus yields a weighting scheme that corresponds with neither an equal weighting of all states nor a nationally representative weighting of the individuals in the sample.

it requires that an individual appear for at least 36 months of the entire SIPP panel rather than for at least 36 months during the 4 years of the analysis period. Second, in our baseline data cleaning procedures we censored the monthly earnings variable at 0 before imputing wage rates on the basis of earnings divided by hours. In constructing the samples analyzed in table 7, we effectively omitted this step in the data cleaning process. The results in table 7 reveal that these choices have modest effects on the estimates.

The similarity of the estimates across a variety of approaches to selecting the "target" sample provides evidence on two points of interest. First, it highlights that our estimates are not particularly sensitive to reasonable alternative choices in the use of wage-related information available in the SIPP. Second, it provides evidence that our estimates were not driven by the fact that a disproportionate share of the individuals in our control group sample were sub-minimum wage workers rather than near-minimum wage workers. Both sets of workers were quite low-skilled and, the evidence suggests, had similar counterfactual employment trajectories. Evidence in the following subsections further supports this claim.

5.2 Estimates across the Skill Distribution

This section presents estimates that, using our 5 alternative approaches to dividing the population across skill groups, explore the relationship between binding minimum wage increases and employment across the entirety of the working age population. For clarity, consider table 8, which replicates estimates that initially appeared in Clemens and Wither (2014). Column 1 presents an estimate of equation (1) on a sample consisting of individuals who lacked employment throughout the baseline period. Column 2 presents the estimate involving the "target" group, which also appeared in the first column of table 8. In column 3 the sample combines the samples from columns 1 and 2. The

remaining columns present estimates that involve the remainder of the population ages 16 to 64. The sample in column 4includes individuals with average baseline wage rates between $7.50 and $8.50. The sample in column 5 includes individuals with average baseline wage rates between $8.50 and $10.00. The sample in column 6 includes individuals with average baseline wage rates above $10.00. Tables 9, 10, 11, and 12 present similarly structured sets of estimates associated with the four alternative approaches to dividing the working age population across skill groups.

The estimates in tables 8 through 12 provide evidence on two points of interest. First, Zipperer (2016) and an anonymous referee interpreted our baseline analysis as susceptible to the concern that it does not account for the minimum wage's potential effects on entry into employment. If a minimum wage increase leads individuals to enter the labor force, the argument goes, the estimates associated with our target group may be partially offset by employment gains among those who were unemployed at baseline. The estimates in columns 1 and 3 of tables 8 through 12 provide evidence that this is not the case. Binding minimum wage increases were associated with declines in employment entry among individuals who were unemployed at baseline. Combining this sample with each table's "target" sample generates the results one would tend to expect based on the samples analyzed separately. The implied contributions to declines in the employment rate across the full population ages 16 to 64 is substantial in all cases.

Estimates associated with higher skill groups provide evidence relevant to the validity of the estimated effects on employment among low-skilled individuals. The "effect" of binding minimum wage increases on employment among the groups labeled as "Middle" and "High" skilled are economically quite close to 0. One estimate is statistically distinguishable from 0 at the 0.10 level, and the others are indistinguishable from 0 at all conventional significance levels. The differential employment declines we estimate among individuals in the "target" group are thus not associated with differential em-

ployment declines among groups higher in the skill distribution.

Among the estimates associated with "High" skilled groups, the one marginally significant estimate involves the sample selected on the basis of individuals' percentiles in their respective groups' wage distributions. We take this as suggestive evidence that estimates involving samples selected on the basis of percentiles are more prone to bias than estimates associated with either our baseline approach or with selecting the sample based on the distance between individuals' baseline wage rates and their respective states' minimum wage rates. In retrospect, this is not entirely surprising. Individuals in upper percentiles of the unbound states' wage distributions have moderately higher average educational attainment and baseline wage rates than do their counterparts in bound states. While selection on the basis of percentiles strikes us as being a natural approach to consider, other approaches may be superior for this reason.

5.3 Robustness to the Inclusion of More Macroeconomic Controls

Table 13 presents estimates in which we augment the regressions reported in panel A of table 5 with additional controls for variations in macroeconomic conditions. Specifically, we augment these regressions with controls for aggregate income per capita (in thousands of dollars), for the state unemployment rate, and for the BEA's construction output index. The inclusion of these covariates has essentially no effect on the point estimates, which shift by less than 5 percent in all cases.

The appendix of Clemens and Wither (2014) reports a similar analysis of the robustness of our column 1 baseline to the inclusion of a variety of macroeconomic controls. This evidence underlay our decision to organize our analysis around a baseline that controlled exclusively for the house price index. Note that this decision was not based on an *ex ante* judgment that the house price index is a particularly special control variable. Instead, it reflects an *ex post* assessment that additional proxies for variations in

macroeconomic conditions were, in practice, largely redundant. The house price index has the additional benefit of being a more plausibly exogenous control variable than the other proxies for variations in macroeconomic conditions. The recent minimum wage literature has been relatively loose in using variables like the aggregate unemployment rate or aggregate employment as controls in spite of the fact that information on employment among low-skilled individuals is contained directly in these variables.[11] As table 13 shows, our estimates are robust to incorporating controls of this form as well.

5.4 Sample Attrition and Alternative Panel Balance Criteria

Table 14 presents estimates on samples for which we alter our panel balance criterion. Significant sample attrition occurred over the course of the 2008 SIPP sample. For our baseline analysis sample, we required that an individual appear for at least 36 out of the 48 months in our sample window. The criteria applied to construct the samples analyzed in table 14 include 12 months, 24 months, 36 months, and a fully balanced panel of 48 months. The estimates reveal that altering the panel balance criterion has little effect on the estimates of interest. The "post 2" employment estimates range from -5.5 to -6.6 percentage points. The estimated changes in the probability of having no earnings range from 7.1 percentage points to 8.2 percentage points.

Figure 6 presents evidence on whether attrition patterns correlate with the employment changes we estimate. The analysis involves estimates of equation (1) in which the time periods correspond with individual months. The base month relative to which all changes are estimated is March 2008, which falls a month prior to the period we coded earlier as the Transition period. The blue dots in figure 6 correspond with the differential evolution of employment (panel A) or the "no earnings" outcome (panel B). The green

[11] This mechanical relationship between the dependent variable and macroeconomic control variables could be eliminated by taking the straightforward step of constructing the control variable as an unemployment or employment rate among skill groups or industries outside of the group under analysis.

Xs correspond with the differential evolution of the probability that an observation had to be "filled in" because it is missing due to sample attrition.[12]

The estimates in figure 6 reveal that there is a very weak correlation between attrition patterns and the differential changes in employment and the "no earnings" outcome. The figure shows that the estimated effects on employment and on the "no earnings" outcome had fully emerged as of July 2010. As of this time, attrition in the bound and unbound state samples was the same. A modest amount of differential attrition appears to emerge over the sample's final year. Only one of the monthly coefficients in the "missing" regression has an associated p-value less than 0.05.

6 Conclusion

This paper presents analysis that complements results originally reported in Clemens and Wither (2014). The results speak to several threats to which, absent further information, our earlier analysis is potentially subject. We conclude by discussing several dimensions of our analysis and summarizing their implications.

The current paper focuses largely on issues related to the composition of our analysis samples. The analysis in sub-section 5.1 shows that our conclusions are robust to considering a range of alternative approaches to constructing our analysis sample. This includes approaches that account for potential differences between sub-minimum wage workers and near-minimum wage workers. More generally, the analysis provides evidence that our results are not dependent on the particulars of our data cleaning and sample construction procedures. Finally, we complement our analysis of employment exit among low-skilled individuals who were employed at baseline with analysis of in-

[12]To construct this variable we first "filled" the data set to generate a balanced panel containing all of the missing person-month observation lines. We then assigned missing observations to the modal state in which the individual resided at baseline.

dividuals who lacked employment at baseline. We find that binding minimum wage increases modestly reduced transitions into employment among the latter group over the period we analyze.

Beyond issues of sample composition, the current paper supplements Clemens and Wither (2014) with regards to the information readers can use to assess the potential biases posed by variations in macroeconomic conditions. The unadjusted data in tables 1 and 2, most entries of which can also be found in Clemens (2017a,b), serve this purpose. These tables summarize variations in comprehensive indicators of employment, aggregate income, and housing market conditions as they varied between the "bound" and "unbound" states that constitute the "treatment" and "control" groups in our analysis. These data are quite clear that negative macroeconomic shocks were more severe in our control group than in our treatment group. Macroeconomic conditions would thus tend to bias unadjusted estimates of the employment effects of this period's minimum wage increases towards positive values. Arguments to the contrary must rationalize why sources of negative bias left no trace in standard indicators of the strength of the housing market, the labor market, or the macroeconomy.

We present additional evidence regarding potential threats to our baseline analysis from omitted variables. In sub-sections 5.2 and 5.3, we present two types of evidence against the relevance of fairly broad threats to our interpretation of our estimates. The evidence in section 5.2 shows that, when comparing bound states to unbound states, the differential employment decline occurred exclusively among the very least skilled workers and among individuals who were not employed at baseline. There is no evidence of differential employment declines among individuals who are either high skilled or who have skills that are modestly greater than the skills of the individuals in our "target" analysis samples. If our estimates are biased, the bias would have to arise from forces that have remarkably concentrated effects on minimum and near-minimum wage

workers.

The analysis presented in sub-section 5.3 provides additional evidence that our estimates are not driven by the effects of observable changes in macroeconomic conditions on employment among low-skilled individuals. We are able to rule out forces that reveal themselves through the relationship between low-skilled employment and aggregate income per capita or through additional proxies (e.g., the BEA's construction output index) for exposure to the shocks that were relevant during the period under analysis.

Across the analyses we report, we are thus able to rule out a broad range of sources of potential bias. Remaining sources of bias would have to involve factors that are not captured by the labor market, macroeconomic, and housing market indicators for which we have controlled directly. They would simultaneously have to be factors that have no detectable effect on employment among individuals with skills that are only modestly greater than the skills of minimum wage workers. These aspects of our statistical analysis, which builds on the unadjusted data from tables 1 and 2, guide our reading of the evidence.

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Figures and Tables

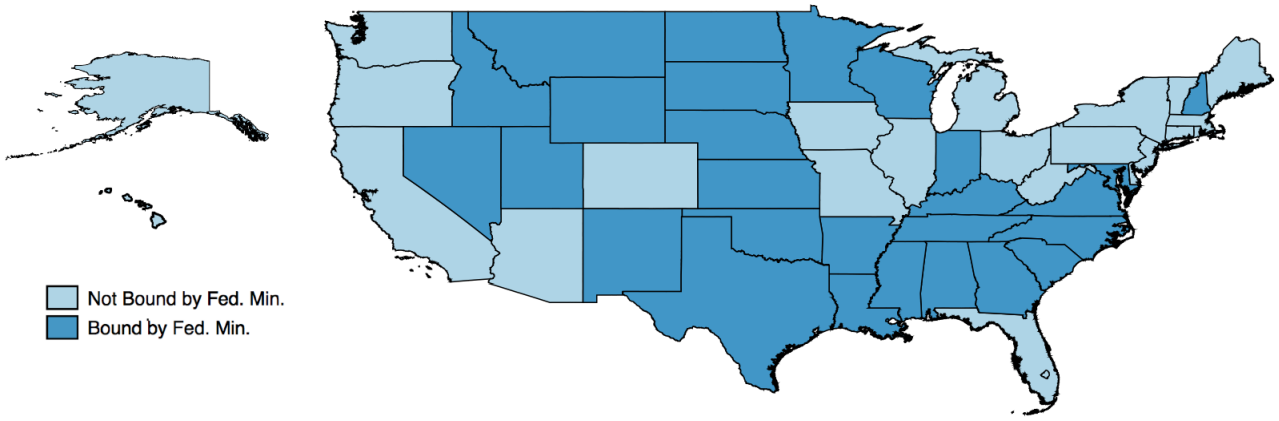


Figure 1: **States Bound by the 2008 and 2009 Federal Minimum Wage Increases:**
The map differentiates states on the basis of whether they were fully or partially bound by the July 2007, 2008 and 2009 increases in the federal minimum wage. I define states as fully bound if their January 2008 minimum 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.

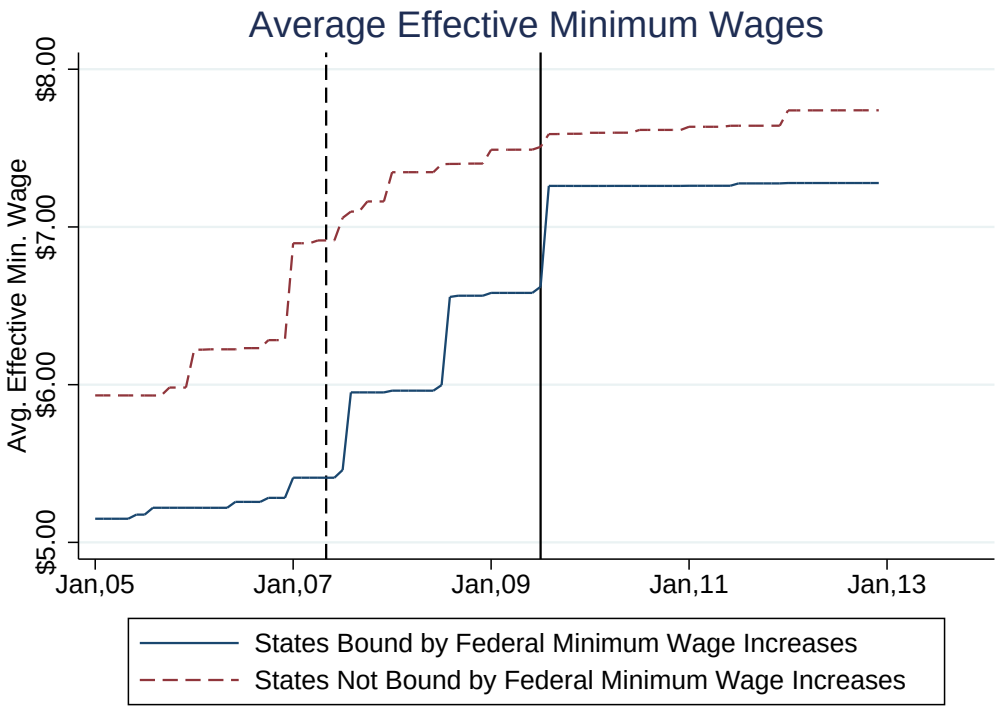


Figure 2: **Evolution of the Average Minimum Wage in Bound and Unbound States:** As in the previous figure, states are defined as fully bound if they were reported by the Bureau of Labor Statistics (BLS) to have had a minimum wage less than $6.55 in January 2008. Such states were at least partially bound 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.

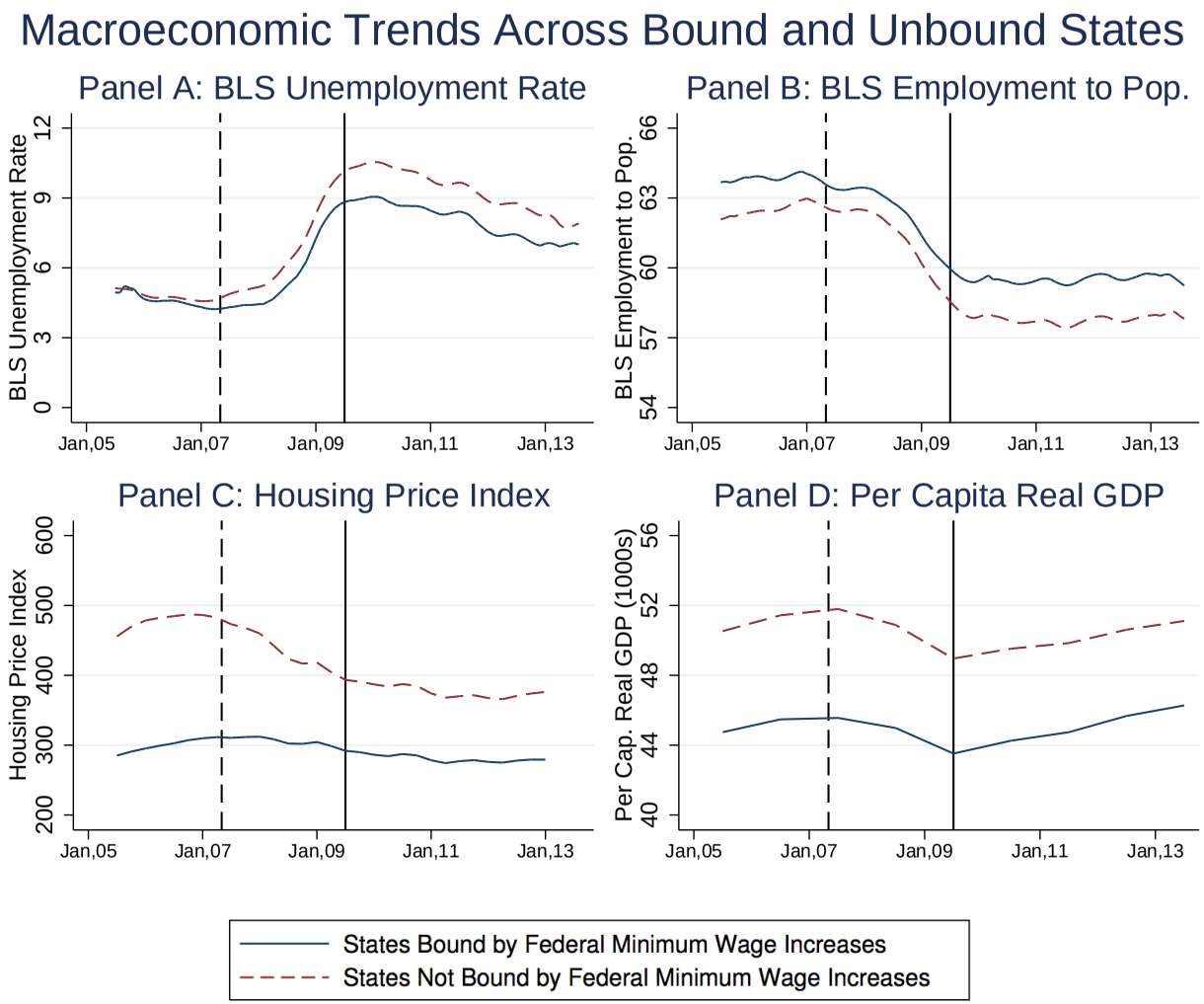
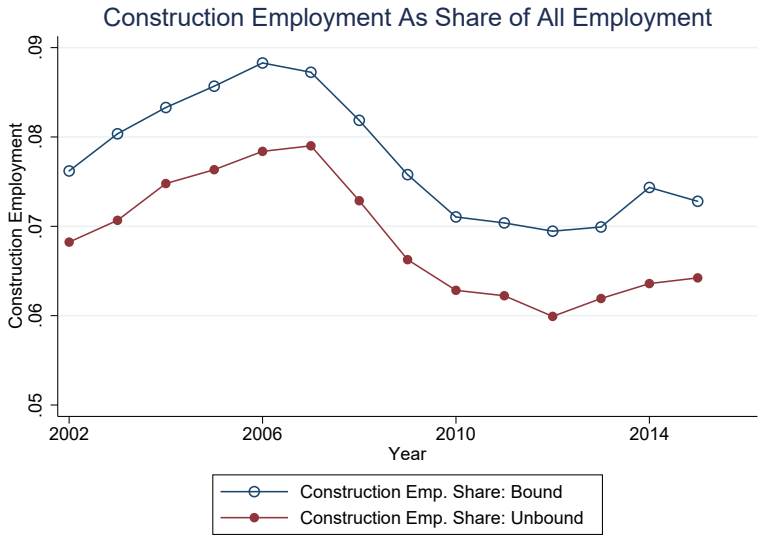


Figure 3: **Macroeconomic Trends in Bound and Unbound States:**
Note: Bound and unbound states are defined as in previous figures. This figure's panels plot the evolution of macroeconomic indicators over the course of the housing boom and bust. All series are weighted by state population so as to reflect 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.

Evolution of Construction Employment

Panel A



Panel B

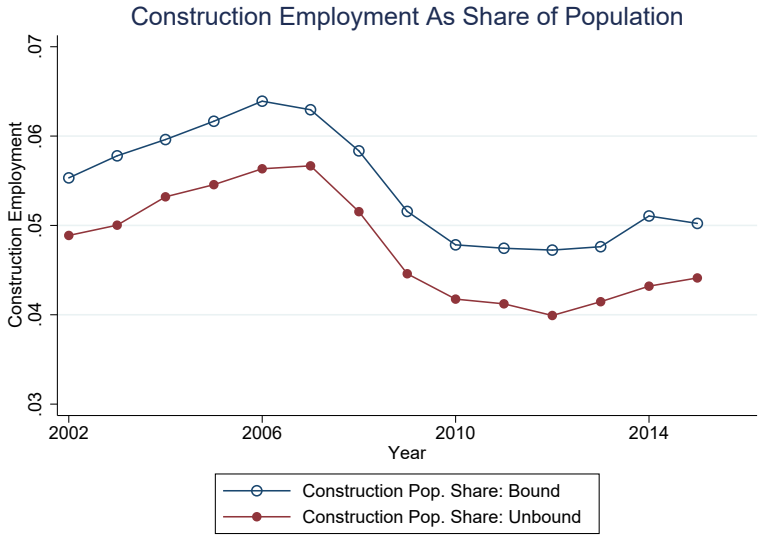
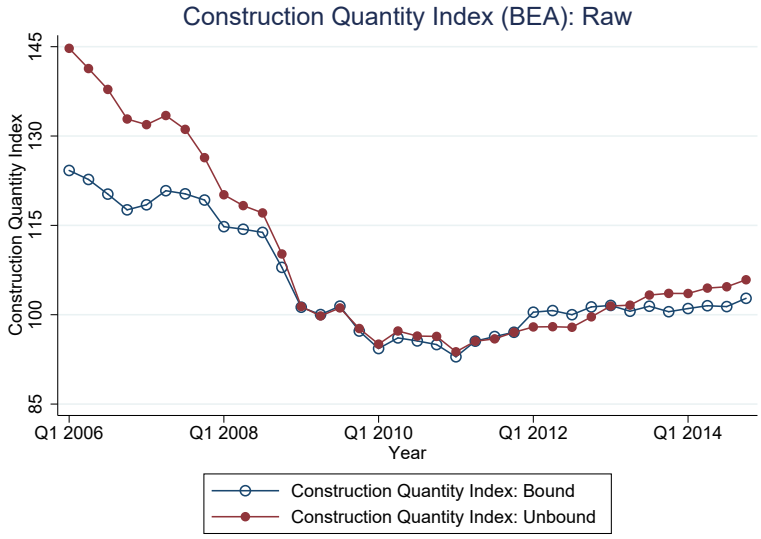


Figure 4: **Evolution of Construction Employment:**
Note: The figure presents the evolution of construction employment as estimated using data from the basic CPS. The series in panel A reports construction employment as a fraction of total employment. The series in panel B reports construction employment as a fraction of the working age population.

Evolution of Construction Output

Panel A



Panel B

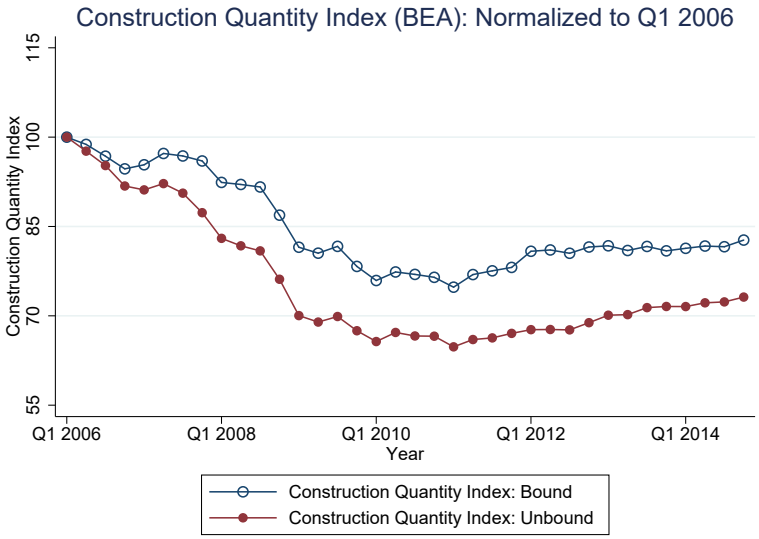
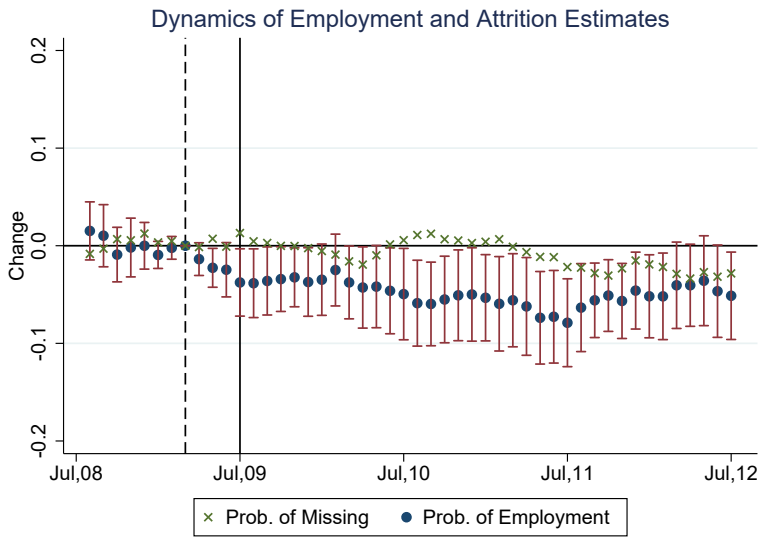


Figure 5: **Evolution of Construction Employment:**
Note: The figure presents the evolution of construction output as reported by the Bureau of Economic Analysis (BEA). The series in panel A are population weighted averages of the BEA's regional accounts construction quantity index. The series in panel B renormalizes the indices to take a value of 100 in the first quarter of 2006.

Evidence on Sample Attrition

Panel A



Panel B

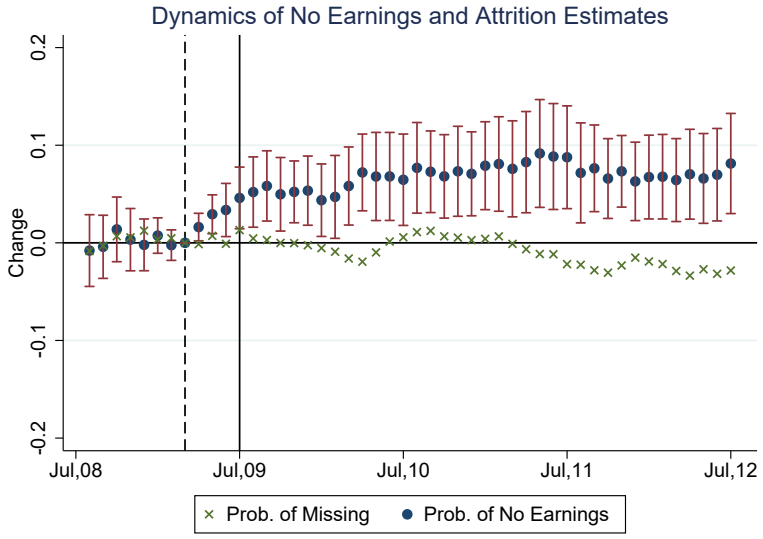


Figure 6: **Evidence on Sample Attrition:**
Note: The figure reports fully dynamic estimates of the minimum wage's effects on employment alongside estimates of differential sample attrition. The green X's correspond with the attrition estimates of the probability that a person-month observation was missing. The blue dots in the top (bottom) panel are estimates of the effect of binding minimum wage increases on the probability of employment (having no earnings).

Table 1: **Variations Underlying Regression Estimates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Control		Treatment		Control	Treatment	Difference
	Baseline	Post	Baseline	Post	Change	Change	
Panel A:	Employment among Low-Skilled Groups						
Baseline Target Sample	0.672	0.683	0.714	0.683	0.011	-0.031	-0.042
Alternate Wage Variable A	0.776	0.740	0.778	0.719	-0.036	-0.059	-0.023
Alternate Wage Variable B	0.720	0.702	0.722	0.672	-0.018	-0.05	-0.032
Near State Min. Wage	0.707	0.686	0.749	0.668	-0.021	-0.081	-0.06
Percentile-Based Sample	0.676	0.682	0.710	0.683	0.006	-0.027	-0.033
Panel B:	Macroeconomic and Housing Covariates						
Income Per Cap. (1000s)	54.84	54.17	50.34	49.76	-0.67	-0.58	0.09
Employment Rate	0.606	0.581	0.618	0.597	-0.025	-0.021	0.004
Unemployment Rate	7.923	9.570	6.897	8.316	1.647	1.419	-0.228
Med. House Price (Millions)	0.419	0.380	0.309	0.287	-0.039	-0.022	0.017
Const. Output Index (BEA)	107.9	96.21	106.4	97.41	-11.69	-8.99	2.7
Prime Aged Employment	0.774	0.748	0.779	0.760	-0.026	-0.019	0.007

Note: The table presents summary statistics that describe levels and changes in a combination of macroeconomic, housing market, and employment variables over the years surrounding the federal minimum wage's rise from $6.55 to $7.25. The first variable is aggregate income per capita, as reported by the Bureau of Economic Analysis. The second and third variables are the aggregate state employment rate and unemployment rate, as reported by the Bureau of Labor Statistics. The fourth variable is an all-transactions median house price index, as reported by the Federal Housing Finance Administration. The fifth variable is an index of value added in the construction industry, as reported by the Bureau of Economic Analysis; because BEA does not adjust this index for local house price changes, the series captures regional variations in quantities of construction output. The sixth variable is the employment rate among (prime-aged) individuals ages 25 to 54, as estimated using the Current Population Survey. The variable in panel B is the employment rate among individuals in the 2008 SIPP panel whose average wage between August 2008 and July 2009 was less than $7.50. Column 1 presents baseline levels of these variables in the "control" group, meaning states with high baseline minimum wage rates. The baseline period corresponds with January 2006 through May 2007, which was the month during which the federal minimum wage increases were legislated. Column 2 presents "post implementation" levels of the various variables, again in the states that comprise the control group. The "post implementation" period corresponds with August 2009 through December 2012. Columns 3 and 4 present the same figures for the treatment group. Column 5 presents the control group's changes in the various variables from the baseline period to the post-implementation period (column 2 minus column 1). Column 6 presents changes for the treatment group (column 4 minus column 3). Column 7 presents the difference in differences between the treatment and control group (column 6 minus column 5).

Table 2: **Further Variations in Macroeconomic Covariates: Pre-Legislation through Post-Implementation**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Control		Treatment		Control	Treatment	Difference
	Baseline	Post	Baseline	Post	Change	Change	
	Macroeconomic and Housing Covariates						
Income Per Cap. (1000s)	56.01	54.26	50.29	49.70	-1.75	-0.59	1.16
Employment Rate	0.626	0.578	0.638	0.595	-0.048	-0.043	0.005
Unemployment Rate	4.701	9.495	4.498	8.210	4.794	3.712	-1.082
Med. House Price (Millions)	0.484	0.378	0.303	0.281	-0.106	-0.022	0.084
Const. Output Index (BEA)	109.1	84.25	113.7	101.2	-24.85	-12.5	12.35
Prime Aged Employment	0.796	0.748	0.800	0.760	-0.048	-0.04	0.008

Note: The table presents summary statistics that describe levels and changes in a combination of macroeconomic, housing market, and employment variables over the years surrounding the federal minimum wage's rise from $5.15 to $7.25. The first variable is aggregate income per capita, as reported by the Bureau of Economic Analysis. The second and third variables are the aggregate state employment rate and unemployment rate, as reported by the Bureau of Labor Statistics. The fourth variable is an all-transactions median house price index, as reported by the Federal Housing Finance Administration. The fifth variable is an index of value added in the construction industry, as reported by the Bureau of Economic Analysis; because BEA does not adjust this index for local house price changes, the series captures regional variations in quantities of construction output. The sixth variable is the employment rate among (prime-aged) individuals ages 25 to 54, as estimated using the Current Population Survey. The variables in panel B are employment rates among low skilled groups, which are the outcome of interest in the empirical analysis. The first is employment among individuals ages 16 to 21 and the second is employment among individuals ages 16 to 30 with less than a completed high school education. Column 1 presents baseline levels of these variables in the "control" group, meaning states with high baseline minimum wage rates. The baseline period corresponds with January 2006 through May 2007, which was the month during which the federal minimum wage increases were legislated. Column 2 presents "post implementation" levels of the various variables, again in the states that comprise the control group. The "post implementation" period corresponds with August 2009 through December 2012. Columns 3 and 4 present the same figures for the treatment group. Column 5 presents the control group's changes in the various variables from the baseline period to the post-implementation period (column 2 minus column 1). Column 6 presents changes for the treatment group (column 4 minus column 3). Column 7 presents the difference in differences between the treatment and control group (column 6 minus column 5). The data reveal that the control group faced relatively severe recessions.

Table 3: **Summary Statistics across Alternative SIPP Samples**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Baseline		Alternative A		Alternative B		Near State Min.		Percentiles	
	Unbound	Bound	Unbound	Bound	Unbound	Bound	Unbound	Bound	Unbound	Bound
Wage $5.15-$7.25	0.217	0.373	0.163	0.299	0.341	0.491	0.164	0.434	0.155	0.354
	(0.412)	(0.484)	(0.369)	(0.458)	(0.474)	(0.500)	(0.370)	(0.496)	(0.362)	(0.478)
Wage less than 0.9 x Min.	0.500	0.316	0.327	0.231	0.328	0.197	0.305	0.343	0.337	0.299
	(0.500)	(0.465)	(0.469)	(0.421)	(0.469)	(0.398)	(0.460)	(0.475)	(0.473)	(0.458)
Wage btw 0.9 and 1.2 x Min.	0.395	0.554	0.260	0.410	0.585	0.731	0.616	0.548	0.573	0.575
	(0.489)	(0.497)	(0.439)	(0.492)	(0.493)	(0.444)	(0.486)	(0.498)	(0.495)	(0.494)
Employed	0.683	0.719	0.785	0.783	0.730	0.725	0.716	0.745	0.686	0.715
	(0.466)	(0.450)	(0.411)	(0.412)	(0.444)	(0.446)	(0.451)	(0.436)	(0.464)	(0.451)
Unpaid Work	0.142	0.110	0.375	0.295	0.0500	0.0463	0.0769	0.0836	0.110	0.106
	(0.349)	(0.313)	(0.484)	(0.456)	(0.218)	(0.210)	(0.267)	(0.277)	(0.313)	(0.308)
Hours Worked/week	23.72	24.47	28.15	27.51	22.39	22.31	23.72	24.70	23.25	24.32
	(19.23)	(18.50)	(20.47)	(19.61)	(16.70)	(16.26)	(17.99)	(18.08)	(18.53)	(18.47)
More than H.S. Deg.	0.628	0.564	0.642	0.559	0.620	0.560	0.593	0.557	0.602	0.563
	(0.483)	(0.496)	(0.480)	(0.497)	(0.485)	(0.496)	(0.491)	(0.497)	(0.489)	(0.496)
Age	33.01	31.59	36.40	34.66	27.98	27.89	30.68	30.79	31.82	31.43
	(14.56)	(13.96)	(14.73)	(14.38)	(13.03)	(12.64)	(13.77)	(13.52)	(14.26)	(13.91)
Observations	16839	20259	21939	25140	8853	13856	23395	14801	25846	21587

Note: This table presents baseline (August 2008 through July 2009) summary statistics for several definitions of "low-skilled" individuals in the 2008 SIPP panel. In columns 1 and 2, the definition of low-skilled is that an individual had an average baseline wage of less than $7.50. Columns 3 and 4 apply a definition similar to that applied in columns 1 and 2, but use an alternative approach to constructing the "average baseline wage" variable (see main text for details). The same applies to columns 5 and 6. The alternative "average baseline wage" variables differ in the extent to which they supplement data on self-reported wage rates with "earnings/hours" imputations. In columns 7 and 8, the definition is that an individual had an average baseline wage no more than 50 cents higher than their states' effective minimum wage as of January 2009. In columns 9 and 10, the definition is that an individual had an average baseline wage in the bottom 13 percentiles of the distribution across individuals in the treatment or control group (whichever applies to the individual in question). Columns 1, 3, 5, 7, and 9 describe individuals in control states while columns 2, 4, 6, 8, and 10 describe individuals in treatment states.

Table 4: **Alternative Definitions of the Primary Analysis Sample: Wage in Affected Range**

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Affected Wage				
Bound x Post 1	-0.160***	-0.125***	-0.194***	-0.219***	-0.180***
	(0.021)	(0.019)	(0.027)	(0.025)	(0.019)
Bound x Post 2	-0.163***	-0.129***	-0.197***	-0.251***	-0.198***
	(0.024)	(0.023)	(0.027)	(0.030)	(0.022)
House Price Index	-0.616	-0.941*	-0.042	-1.022	-0.573
	(0.424)	(0.398)	(0.447)	(0.653)	(0.400)
N	147,459	186,827	90,322	155,857	188,668
Wage Variable	Baseline	Alternate A	Alternate B	Baseline	Baseline
Division of Sample	Baseline	Baseline	Baseline	Dist. to Min.	Percentile
Skill Group	Target	Target	Target	Target	Target
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with an alternative approach to constructing the analysis samples. The alternatives are analyzed in the sample order as they are presented in table 3. Note that the aggregate changes in the dependent variable implied by each point estimate depend on the underlying sample size. Large samples tend to include more individuals that are unlikely to be affected by the minimum wage, and thus tend to be associated with coefficients of smaller magnitudes. The implied aggregate changes, however, are fairly similar across the samples. Standard errors allow for correlation clusters across errors at the state level.

Table 5: **Alternative Definitions of the Primary Analysis Sample: Employment and No Earnings Outcomes**

	(1)	(2)	(3)	(4)	(5)
Panel A:			Dependent Variable: Employed		
Bound x Post 1	-0.044*	-0.031*	-0.050+	-0.073***	-0.034*
	(0.019)	(0.014)	(0.025)	(0.015)	(0.015)
Bound x Post 2	-0.066**	-0.037*	-0.063**	-0.077***	-0.054**
	(0.020)	(0.014)	(0.023)	(0.018)	(0.017)
House Price Index	0.755*	0.415+	1.210**	0.758*	0.716*
	(0.323)	(0.230)	(0.372)	(0.360)	(0.331)
Panel B:			Dependent Variable: No Earnings		
Bound x Post 1	0.059**	0.037**	0.053+	0.080***	0.044**
	(0.019)	(0.013)	(0.027)	(0.014)	(0.014)
Bound x Post 2	0.082***	0.060***	0.068**	0.083***	0.062***
	(0.021)	(0.017)	(0.024)	(0.018)	(0.017)
House Price Index	-0.707*	-0.029	-0.997*	-0.798*	-0.616
	(0.348)	(0.294)	(0.387)	(0.362)	(0.368)
N	147,459	186,827	90,322	155,857	188,668
Wage Variable	Baseline	Alternate A	Alternate B	Baseline	Baseline
Division of Sample	Baseline	Baseline	Baseline	Dist. to Min.	Percentile
Skill Group	Target	Target	Target	Target	Target
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with an alternative approach to constructing the analysis samples. The alternatives are analyzed in the sample order as they are presented in table 3. Note that the aggregate changes in the dependent variable implied by each point estimate depend on the underlying sample size. Large samples tend to include more individuals that are unlikely to be affected by the minimum wage, and thus tend to be associated with coefficients of smaller magnitudes. The implied aggregate changes, however, are fairly similar across the samples. Standard errors allow for correlation clusters across errors at the state level.

Table 6: **Alternative Definitions of the Primary Analysis Sample: Employment and No Earnings Outcomes**

	(1)	(2)	(3)	(4)	(5)
Panel A:			Dependent Variable: Employed		
Bound x Post 1	-0.039*	-0.028*	-0.037	-0.068***	-0.030*
	(0.018)	(0.013)	(0.023)	(0.014)	(0.014)
Bound x Post 2	-0.059**	-0.030+	-0.058*	-0.065***	-0.049**
	(0.021)	(0.015)	(0.022)	(0.018)	(0.017)
House Price Index	0.637+	0.281	0.957*	0.581+	0.606+
	(0.323)	(0.248)	(0.433)	(0.303)	(0.308)
Panel B:			Dependent Variable: No Earnings		
Bound x Post 1	0.055**	0.037*	0.039	0.077***	0.042**
	(0.020)	(0.014)	(0.024)	(0.013)	(0.015)
Bound x Post 2	0.080***	0.059**	0.063**	0.076***	0.061***
	(0.022)	(0.020)	(0.022)	(0.017)	(0.017)
House Price Index	-0.641+	-0.040	-0.804+	-0.653*	-0.563+
	(0.350)	(0.317)	(0.425)	(0.294)	(0.326)
N	147,459	186,827	90,322	155,857	188,668
Wage Variable	Baseline	Alternate A	Alternate B	Baseline	Baseline
Division of Sample	Baseline	Baseline	Baseline	Dist. to Min.	Percentile
Skill Group	Target	Target	Target	Target	Target
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Weighted	Yes	Yes	Yes	Yes	Yes

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with an alternative approach to constructing the analysis samples. The alternatives are analyzed in the sample order as they are presented in table 3. Note that the aggregate changes in the dependent variable implied by each point estimate depend on the underlying sample size. Large samples tend to include more individuals that are unlikely to be affected by the minimum wage, and thus tend to be associated with coefficients of smaller magnitudes. The implied aggregate changes, however, are fairly similar across the samples. Observations are weighted using the SIPP's sample weights. Standard errors allow for correlation clusters across errors at the state level.

Table 7: **Employment and No Earnings Outcomes with Basic Cleaning Commands Conducted at the End**

	(1)	(2)	(3)	(4)	(5)
Panel A:			Dependent Variable: Employed		
Bound x Post 1	-0.033+	-0.029*	-0.037	-0.065***	-0.035*
	(0.019)	(0.014)	(0.023)	(0.016)	(0.014)
Bound x Post 2	-0.057**	-0.038*	-0.053*	-0.073***	-0.056***
	(0.019)	(0.015)	(0.021)	(0.018)	(0.016)
House Price Index	0.767*	0.635*	1.149**	0.770*	0.926**
	(0.311)	(0.246)	(0.359)	(0.323)	(0.309)
Panel B:			Dependent Variable: No Earnings		
Bound x Post 1	0.052**	0.037**	0.041+	0.070***	0.048***
	(0.019)	(0.012)	(0.024)	(0.015)	(0.014)
Bound x Post 2	0.072***	0.057***	0.058*	0.077***	0.060***
	(0.020)	(0.016)	(0.022)	(0.018)	(0.016)
House Price Index	-0.605+	-0.083	-0.821*	-0.828*	-0.843*
	(0.333)	(0.274)	(0.362)	(0.336)	(0.335)
N	163,271	209,929	100,328	194,924	198,620
Wage Variable	Baseline	Alternate A	Alternate B	Baseline	Baseline
Division of Sample	Baseline	Baseline	Baseline	Dist. to Min.	Percentile
Skill Group	Target	Target	Target	Target	Target
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with an alternative approach to constructing the analysis samples. The alternatives are analyzed in the sample order as they are presented in table 3. Note that the aggregate changes in the dependent variable implied by each point estimate depend on the underlying sample size. Large samples tend to include more individuals that are unlikely to be affected by the minimum wage, and thus tend to be associated with coefficients of smaller magnitudes. The implied aggregate changes, however, are fairly similar across the samples. Standard errors allow for correlation clusters across errors at the state level.

Table 8: **Regressions Using Baseline Wage Measure and Baseline Division of the Sample**

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Employed					
Bound x Post 1	-0.009+	-0.044*	-0.018**	0.004	-0.008	0.002
	(0.005)	(0.019)	(0.005)	(0.021)	(0.012)	(0.004)
Bound x Post 2	-0.022*	-0.066**	-0.038***	-0.026	-0.002	-0.003
	(0.009)	(0.020)	(0.009)	(0.021)	(0.013)	(0.005)
House Price Index	0.153	0.755*	0.140	0.610	-0.335	0.027
	(0.195)	(0.323)	(0.172)	(0.440)	(0.371)	(0.075)
N	523,086	147,459	670,545	102,193	122,786	1,076,148
Wage Variable	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Division of Sample	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Skill Group	Unemp. at Base	Target	T. + Un.	MidLow	Middle	High
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No	No

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with estimates involving different skill groups, which are described within the table itself. The information used to divide the initially employed into the "Target" group the "Midlow" group, the "Middle" group, and the "High" group corresponds with the first sample presented in table 3. Standard errors allow for correlation clusters across errors at the state level.

Table 9: **Regressions Using Alternate Wage Measure A and Baseline Division of the Sample**

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Employed					
Bound x Post 1	-0.004	-0.031*	-0.018**	-0.014	-0.006	0.001
	(0.006)	(0.014)	(0.006)	(0.020)	(0.012)	(0.004)
Bound x Post 2	-0.023*	-0.037*	-0.041***	-0.046*	-0.003	-0.003
	(0.011)	(0.014)	(0.011)	(0.019)	(0.014)	(0.005)
House Price Index	0.117	0.415+	0.024	0.817+	-0.212	0.019
	(0.230)	(0.230)	(0.206)	(0.431)	(0.389)	(0.072)
N	406,729	186,827	593,556	104,899	125,288	1,147,929
Wage Variable	Alternate A	Alternate A	Alternate A	Alternate A	Alternate A	Alternate A
Division of Sample	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Skill Group	Unemp. at Base	Target	T. + Un.	MidLow	Middle	High
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No	No

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with estimates involving different skill groups, which are described within the table itself. The information used to divide the initially employed into the "Target" group the "Midlow" group, the "Middle" group, and the "High" group corresponds with the second sample presented in table 3. Standard errors allow for correlation clusters across errors at the state level.

Table 10: **Regressions Using Alternate Wage Measure B and Baseline Division of the Sample**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variable: Employed						
Bound x Post 1	-0.007	-0.050+	-0.021**	-0.008	-0.006	-0.003	0.004
	(0.005)	(0.025)	(0.007)	(0.021)	(0.013)	(0.006)	(0.003)
Bound x Post 2	-0.025*	-0.063**	-0.048***	-0.034	-0.011	-0.002	-0.004
	(0.011)	(0.023)	(0.013)	(0.022)	(0.013)	(0.007)	(0.005)
House Price Index	0.112	1.210**	0.032	0.788	-0.243	-0.015	0.021
	(0.238)	(0.372)	(0.262)	(0.479)	(0.394)	(0.113)	(0.073)
N	392,911	90,322	483,233	101,735	111,383	605,678	665,359
Wage Variable	Alternate B	Alternate B	Alternate B	Alternate B	Alternate B	Alternate B	Alternate B
Division of Sample	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Skill Group	Unemp. at Base	Target	T. + Un.	MidLow	Middle	High	Salaried
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No	No	No

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with estimates involving different skill groups, which are described within the table itself. The information used to divide the initially employed into the "Target" group the "Midlow" group, the "Middle" group, and the "High" group corresponds with the third sample presented in table 3. Standard errors allow for correlation clusters across errors at the state level.

Table 11: **Regressions Using Baseline Wage Measure and Division of the Sample Based on Distance from the Minimum Wage**

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Employed					
Bound x Post 1	-0.009+	-0.073***	-0.020***	0.024+	0.002	0.002
	(0.005)	(0.015)	(0.005)	(0.012)	(0.012)	(0.004)
Bound x Post 2	-0.022*	-0.077***	-0.028**	0.005	0.003	-0.003
	(0.009)	(0.018)	(0.008)	(0.012)	(0.013)	(0.005)
House Price Index	0.153	0.758*	0.204	0.075	0.221	-0.003
	(0.195)	(0.360)	(0.154)	(0.311)	(0.276)	(0.073)
N	523,086	155,857	674,659	228,395	149,965	902,416
Wage Variable	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Division of Sample	Dist. to Min.	Dist. to Min.	Dist. to Min.	Dist. to Min.	Dist. to Min.	Dist. to Min.
Skill Group	Unemp. at Base	Target	T. + Un.	MidLow	Middle	High
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No	No

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with estimates involving different skill groups, which are described within the table itself. The information used to divide the initially employed into the "Target" group the "Midlow" group, the "Middle" group, and the "High" group corresponds with the fourth sample presented in table 3. Standard errors allow for correlation clusters across errors at the state level.

Table 12: **Regressions Using Baseline Wage Measure and Percentile-Based Division of the Sample**

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Employed					
Bound x Post 1	-0.009+	-0.034*	-0.014**	0.002	-0.008	0.000
	(0.005)	(0.015)	(0.005)	(0.009)	(0.007)	(0.003)
Bound x Post 2	-0.022*	-0.054**	-0.029**	-0.004	-0.012	-0.009+
	(0.009)	(0.017)	(0.009)	(0.010)	(0.008)	(0.005)
House Price Index	0.153	0.716*	0.145	0.073	0.274*	0.000
	(0.195)	(0.331)	(0.172)	(0.264)	(0.105)	(0.119)
N	523,086	188,668	711,754	299,362	363,936	1,119,706
Wage Variable	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Division of Sample	Percentile	Percentile	Percentile	Percentile	Percentile	Percentile
Skill Group	Unemp. at Base	Target	T. + Un.	MidLow	Middle	High
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No	No

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with estimates involving different skill groups, which are described within the table itself. The information used to divide the initially employed into the "Target" group the "Midlow" group, the "Middle" group, and the "High" group corresponds with the fifth sample presented in table 3. Standard errors allow for correlation clusters across errors at the state level.

Table 13: **Alternative Definitions of the Primary Analysis Sample: Estimates with More Macroeconomic Controls**

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Employed				
Bound x Post 1	-0.043*	-0.031*	-0.053*	-0.073***	-0.032*
	(0.019)	(0.014)	(0.025)	(0.015)	(0.015)
Bound x Post 2	-0.067**	-0.038*	-0.065**	-0.077***	-0.054**
	(0.020)	(0.014)	(0.022)	(0.018)	(0.016)
House Price Index	0.802*	0.479+	1.473***	0.754*	0.670+
	(0.326)	(0.267)	(0.415)	(0.365)	(0.343)
Income Per Capita	0.002	0.001	-0.004	0.003	0.001
	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)
Unemployment Rate	0.486	0.356	-0.171	0.158	0.481
	(0.634)	(0.417)	(0.660)	(0.592)	(0.490)
Construction Output	-0.020	-0.024	-0.052	-0.017	0.020
	(0.057)	(0.043)	(0.049)	(0.055)	(0.054)
N	147,459	186,827	90,322	155,857	188,668
Wage Variable	Baseline	Alternate A	Alternate B	Baseline	Baseline
Division of Sample	Baseline	Baseline	Baseline	Dist. to Min.	Percentile
Skill Group	Target	Target	Target	Target	Target
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with an alternative approach to constructing the analysis samples. The alternatives are analyzed in the sample order as they are presented in table 3. Note that the aggregate changes in the dependent variable implied by each point estimate depend on the underlying sample size. Large samples tend to include more individuals that are unlikely to be affected by the minimum wage, and thus tend to be associated with coefficients of smaller magnitudes. The implied aggregate changes, however, are fairly similar across the samples. Standard errors allow for correlation clusters across errors at the state level.

Table 14: **Changes in Sample Inclusion Criteria: Employment and No Earnings Outcomes Outcome**

	(1)	(2)	(3)	(4)
Panel A:		Dependent Variable: Employed		
Bound x Post 1	-0.037*	-0.041*	-0.044*	-0.043+
	(0.017)	(0.017)	(0.019)	(0.024)
Bound x Post 2	-0.055**	-0.059**	-0.066**	-0.065*
	(0.019)	(0.018)	(0.020)	(0.025)
Panel B:		Dependent Variable: No Earnings		
Bound x Post 1	0.054**	0.058**	0.059**	0.050*
	(0.017)	(0.017)	(0.019)	(0.024)
Bound x Post 2	0.071***	0.076***	0.082***	0.080**
	(0.019)	(0.018)	(0.021)	(0.026)
N	187,932	173,065	147,459	89,952
Estimation Framework	D-in-D	D-in-D	D-in-D	D-in-D
Weighted	No	No	No	No
Individual Fixed Effects	Yes	Yes	Yes	Yes
Inclusion Criterion	12 Months Plus	24 Months Plus	36 Months Plus	48 Months

Note: The table presents estimates of the primary coefficients of interest from equation (1). The estimates in each column correspond with estimates involving different panel balance criteria. Standard errors allow for correlation clusters across errors at the state level.

A Wage Variable Construction

```
*=============================================================
*== Create wage and average baseline wage variables ====
*=============================================================
gen hours = ehrsall
replace hours = 0 if ehrsall<0
gen tothours = hours*rmwkwjb

gen hourwage = tpearn/tothours if ejobcntr >=0
gen hrwage = tpyrate1 if ejobcntr >=0
replace hrwage = hourwage if tpyrate1==0 & !missing(hourwage)
replace hrwage = round(hrwage,.01)
gen affectedwage = hrwage>=5.15 & hrwage<7.25

gen hourwagealt = tpearn/tothours if employed ==1
gen hrwagealt = tpyrate1 if employed == 1
replace hrwagealt = hourwagealt if tpyrate1==0 & !missing(hourwagealt)
replace hrwagealt = round(hrwagealt,.01)

gen hrwagealtB = tpyrate1 if ejobcntr >=0 & tpyrate1 != 0
replace hrwagealtB = round(hrwagealtB,.01)

gen wagevar = hrwage if  Post==0
egen avewage = mean(wagevar), by(personid)

gen wagevaralt = hrwagealt if  Post==0
egen avewagealt = mean(wagevaralt), by(personid)

gen wagevaraltB = hrwagealtB if  Post==0
egen avewagealtB = mean(wagevaraltB), by(personid)
```

B Identification of Bound States and Division of Sample Period

```
*================================================
*===== Identify Bound States and =====
*================================================
preserve
use "Employment Replication/Data/Macro/MinimumWageRatesByStateAndMonth.dta", clear
keep if year==2008 & month==1
keep statefip mw
replace mw = round(mw,.01)
rename mw mw2008
save "Employment Replication/Data/Macro/Jan2008MWs.dta", replace
restore
merge m:1 statefip using "Employment Replication/Data/Macro/Jan2008MWs.dta", nogenerate

gen Treatment = mw2008<6.55
gen Post = time>=200908
gen Post1 = time>=200908 & time<=201007
gen Post2 = time>=201008
gen Transition = time>=200905 & time<=200907
gen Period = 0
replace Period = 1 if Transition==1
replace Period = 2 if Post1==1
replace Period = 3 if Post2==1
gen Treatment_Post = Treatment*Post
gen Treatment_Post_1 = Treatment*Post1
gen Treatment_Post_2 = Treatment*Post2
gen Treatment_Trans = Treatment*Transition
```

C Initial Data Cleaning and Variable Construction

```
*================================================
*===== Initial Cleaning =====
*================================================
egen personid = group(ssuid epppnum)
drop if missing(personid)

drop if tage>=65
drop if tage<16

gen time = 100*year+month
drop if time<=200807
drop if time>201207

foreach inc in tpearn {
replace `inc' = `inc'/CPI
replace `inc' = 0 if `inc'<0
replace `inc' = 7500 if `inc' > 7500 & `inc' != .
}

egen personidcount = count(personid), by(personid)
```