# The Minimum Wage and the Great Recession: Evidence of Effects on the Employment and Income Trajectories of Low-Skilled Workers

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December 30, 2018

**Abstract:**

We estimate the minimum wage's effects on low-skilled individuals' employment and income trajectories following the Great Recession. Our approach exploits two dimensions of the data we analyze. First, we compare individuals in states that were fully bound by the 2007 to 2009 increases in the federal minimum wage to individuals in states that were not. Second, we use variation in the minimum wage's bite across skill groups to separate our samples into "target" and "within-state control" groups. Using the 2008 panel of the Survey of Income and Program Participation, we find that binding minimum wage increases had significant, negative effects on the employment and income growth of targeted workers. Although there are important limitations to our research designs, our estimates are robust to adopting a range of alternative strategies to construct our analysis samples and to account for variation in the Great Recession's underlying severity across states. In aggregate, our estimates suggest that this period's minimum wage increases reduced aggregate employment rates by at least half of a percentage point in states that were bound by the federal minimum wage increases. Because our estimates are large relative to what one would infer from past research, we emphasize the relevance of the historical episode we analyze.

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Between July 23, 2007, and July 24, 2009, the U.S. federal minimum wage rose from $5.15 to $7.25 per hour. During the concurrent recession, the employment-to-population ratio declined by 4 percentage points among prime aged adults and by 10 percentage points among those aged 16 to 21. This paper analyzes the extent to which the employment declines experienced by low skilled individuals were exacerbated by this period's minimum wage increases.

Our empirical analysis uses the fact that the 2007 through 2009 increases in the federal minimum wage were differentially binding across states. We base our "bound" designation on whether a state's January 2008 minimum wage was below $6.55, rendering it bound by entirety of the July 2009 increase. In the states we describe as "unbound," the effective minimum wage rose, on average, by $1.42 between 2006 and 2012. In the states we describe as "bound," the effective minimum wage rose, on average, by $2.04. Of the long-run differential, $0.58 took effect on July 24, 2009.

We use monthly, individual-level panel data from the 2008 panel of the Survey of Income and Program Participation (SIPP) to implement a combination of difference-in-differences and triple-difference research designs. Because we use longitudinal employment records with data on wage rates, our implementation of these research designs has two key advantages. First, we are able to pinpoint "target" groups more intensely affected by minimum wage increases than the analysis groups in many studies. Second, we are able to pinpoint workers who were not directly affected yet, as evidenced by their wage rates, were only moderately more skilled than the "target" workers. We incorporate this second group of workers into our analysis as a "within-state control" group. That is, we use this group to construct a set of counterfactuals that proxy for otherwise unobserved shocks to the low-skilled labor market.

We begin by assessing the extent to which minimum wage increases affected the wage distributions of low-skilled workers. Among workers with average baseline wages less

than $7.50, the probability of reporting a wage between $5.15 and $7.25 declined substantially in bound states relative to unbound states. We find that the wage distributions of low-skilled workers in bound and unbound states fully converge along this dimension. Further, we estimate that the minimum wage's bite on our target group's wage distribution is nearly twice its bite on comparison samples selected using approaches with long histories in the literature (Neumark and Wascher, 1992; Card and Krueger, 1994). That is, our "target" group is roughly twice as intensely treated as groups like teenagers and food service workers.

We next estimate the minimum wage's effects on employment. We find that increases in the minimum wage significantly reduced the employment of low-skilled workers. By the second year following the $7.25 minimum wage's implementation, we estimate that targeted individuals' employment rates had fallen by 6.6 percentage points (9 percent) more in bound states than in unbound states. The implied elasticity of our target group's employment with respect to the minimum wage is -1, which is large within the context of the existing literature.

We next estimate the effects of binding minimum wage increases on low-skilled workers' incomes. The 2008 SIPP panel provides a unique opportunity to investigate such effects, as its individual-level panel extends for 3 years following the July 2009 increase in the federal minimum wage. We find that this period's binding minimum wage increases reduced low-skilled individuals' average monthly incomes. Relative to low-skilled workers in unbound states, targeted individuals' average monthly incomes fell by $90 over the first year and by an additional $50 over the following 2 years. While surprising at first glance, we show that these estimates can be straightforwardly explained through our estimated effects on employment, the likelihood of working without pay, and subsequent lost wage growth associated with lost experience. We estimate, for example, that targeted workers experienced a 5 percentage point decline in their medium-run probability

of reaching earnings greater than $1500 per month.

We next assess potential threats to the validity of our estimates. The primary threat to our estimation frameworks is the possibility that low-skilled workers in the bound and unbound states were differentially affected by the Great Recession. Notably, because our data lack an extended "pre-intervention" period for assessing the relevance of pre-existing trends, our assessment of potential threats must push farther than other studies along other dimensions. Our triple-difference design is central in this regard. Further, we present evidence on the potential relevance of biases from a comprehensive set of labor market, macroeconomic, and housing market indicators. These data indicate quite strongly that the recession and housing crisis were much more severe in unbound states than in bound states. Unadjusted data on low-skilled groups' employment rates are thus likely to yield a lower bound estimate of the magnitude of binding minimum wage increases' effects during this time period.

Additional methodological concerns relate to differences in the demographic composition of the low-wage samples we identify in bound states relative to unbound states. While we do not see evidence that issues related to sample composition are biasing our estimates, their potential relevance is inherently difficult to rule out. These concerns thus point to caveats we must bare in mind.

Our estimates are at the high end of those found in the existing, highly contentious literature on the minimum wage's effects. We thus conclude by emphasizing two points. First, though the minimum wage's effects are generally estimated to be modest, large effects are not uncommon in recent research that uses modern program evaluation methods and high quality data.[1] Second, both perfectly and imperfectly competitive models

[1]For examples of recent research estimating small effects, see Dube, Lester, and Reich (2010), Harasztosi and Lindner (2018), and Cengiz, Dube, Lindner, and Zipperer (2018). For examples of recent research estimating large effects, see Sabia, Burkhauser, and Hansen (2012), Meer and West (2016), Jardim, Long, Plotnick, Van Inwegen, Vigdor, and Wething (2017), Kreiner, Reck, and Skov (2017), Baskaya and Rubinstein (2012), and Powell (2016).

of the labor market indicate that minimum wage effects may vary dramatically across settings. We analyze a setting in which labor demand was depressed and productivity growth quite slow. This is a textbook case in which a minimum wage increase's effects may be quite large because the new minimum wage will have both deep and sustained bite.

The paper proceeds as follows. Section 1 presents historical background on the minimum wage increases we analyze. Section 2 describes the data we analyze, section 3 presents our empirical methodology, and section 4 presents our results. Section 5 discusses potential threats to the validity of our research designs, along with evidence on their relevance. Section 6 relates our estimates to other research in the minimum wage literature. Section 7 discusses our estimates' implications for the minimum wage's effects on aggregate employment following the Great Recession. Section 8 concludes.

# 1 The Late 2000s Increases in the Federal Minimum Wage

We estimate the minimum wage's effects on employment and income trajectories using variation driven by federally mandated increases in the minimum wage rates applicable across the U.S. states. On May 25, 2007, the 110th 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 24 of 2007, 2008, and 2009. In July 2007, the federal minimum wage rose from $5.15 to $5.85, in July 2008 it rose to $6.55, and in July 2009 it rose to $7.25.

Panel A of Figure 1 shows our division of states into those that were and were not bound by changes in the federal minimum wage. We base this designation on whether a state's January 2008 minimum wage was below $6.55, rendering it bound by entirety

of the July 2009 increase.[2] Using Department of Labor (DOL) data on states' prevailing minimum wage rates, we designate 27 states as fitting this description.

Panel B of Figure 1 shows the time paths of the average effective minimum wages in the states to which we do and do not apply our "bound" designation. Two characteristics of the paths of the minimum wage rates in unbound states are worth noting. First, their average minimum wage exceeded the average minimum wage applicable in the bound states by roughly $1 prior to the passage of the 2007 to 2009 federal increases. Second, these states voluntarily increased their minimum wages well ahead of the required schedule. On average, the effective minimum wage across these states had surpassed $7.25 by January of 2008. This group's effective minimum wages rose, on average, by roughly 20 cents between August 2008 and August 2012. By contrast, bound states saw their effective minimum wages rise by nearly the full, legislated $0.70 on July 24, 2009. From 2006 to 2012, the average effective minimum wage rose by $1.42 in the "unbound" states and by $2.04 in the "bound" states.

## 2 Data Sources and Primary Analysis Sample

We estimate the effects of minimum wage increases using data from the 2008 panel of the Survey of Income and Program Participation (SIPP). In the 2008 SIPP panel, we analyze a sample restricted to individuals aged 16 to 64 for whom the relevant employment and earnings data are available for at least 36 months between August 2008 and

[2]Our "bound" designation has several relevant features. First, because we analyze data from the 2008 SIPP panel, which began during the summer of 2008, our designation focuses on the lone federal minimum wage increase enacted during our analysis sample. Second, our January 2008 designation ensures that our estimates are not affected by potentially endogenous state decisions made during the months immediately preceding the federal policy variation on which we focus. Third, our use of a "bound" indicator isolates variation driven by the federal increase from state increases driven by inflation indexation provisions. Because inflation indexed increases were long forecastable by firms, their contemporaneous economic effects may differ from the effects of the increases in which we are primarily interested (Brummond and Strain, Forthcoming). In robustness analyses, we show that our findings are not sensitive to making moderate changes to the basis for our "bound" designation.

July 2012. For each individual, this yields up to 12 months of data preceding the July 2009 increase in the minimum wage.

In the low-wage samples on which we focus, hourly wage rates are reported directly for 77 percent of the observations with positive earnings. For the remaining 23 percent, we impute hourly wages as earnings divided by the individual's usual hours per week times their reported number of weeks worked. We use these 12 months of baseline wage, hours, and earnings data to divide the working age population into several groups.

The "target" group we analyze includes those most directly impacted by the federal minimum wage. For our primary analysis sample, the target group includes those whose average wage, when employed during the baseline period, was less than $7.50.[3] A second group includes individuals whose average baseline wages were between $7.50 and $8.50. We view this as a group that might be moderately impacted by both direct and/or spillover effects of minimum wage increases. A third group includes individuals whose average baseline wages were between $8.50 and $10.00. Guided by the baseline wage data, we characterize these individuals as a comparison group of low-skilled workers for whom increases in the effective minimum wage had neither a mechanical effect nor any apparent spillover effect. The remainder of the population consists of those who were not employed throughout the baseline period and those employed at average baseline wage rates greater than $10.00.

Table 1 presents summary statistics on the primary SIPP samples we analyze. The data highlight that, as intended, "target" group individuals in bound states were far more likely than "target" individuals in unbound states to have baseline wage rates

[3]The average is calculated over months in which the individual was employed, excluding months when not employed. The measure's intent is to capture the individual's average marginal product as remunerated by the firms for which he or she works. One consequence of this approach is that our "target" individuals do not include individuals who were out of employment throughout the baseline period. We analyze such individuals separately to ensure that we do not fail to capture potential effects on employment entry.

between $5.15 and $7.25. The summary statistics also point to demographic differences between the target samples in bound and unbound states that are of potential concern. For example, target group individuals in bound states are moderately younger and less likely to have completed at least some college education than target group individuals in unbound states. They are also more less likely to be sub-minimum wage workers. These differences are a potential concern for our analysis to the extent to which they would lead individuals to have different counterfactual employment trajectories. We discuss this concern in considerable detail in sections 5 and B.2.2.

We supplement the SIPP data with data on several key variables that proxy for the macroeconomic environment associated with the Great Recession. These data include a house price index from the Federal Housing Finance Administration (FHFA), aggregate state income per capita from the Bureau of Economic Analysis (BEA), and state level employment and unemployment rate data from the Bureau of Labor Statistics (BLS).

# 3 Strategy for Estimating the Minimum Wage's Effects

This section presents our regression frameworks and discusses the key assumptions required for our estimates to yield causal effects of the minimum wage changes we analyze. Our initial estimates, conducted on a sample consisting of the "target" group described above, take the following, dynamic difference-in-differences form:

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

We control for the standard features of difference-in-differences estimation, namely sets of state, $\text{State}_s$, and time, $\text{Time}_t$, fixed effects. Our ability to control for individual fixed

effects, $\text{Individual}_i$, renders controls for individual-level, time-invariant characteristics redundant. The vector $\mathbf{X_{i,s,t}}$ contains sets of time varying controls that differ across the robustness checks we implement. The controls of greatest interest include proxies for macroeconomic conditions and flexible, time-varying controls for demographic characteristics. While we explore our estimates' robustness to incorporating a broad range of proxies for macroeconomic conditions, our baseline is a parsimonious specification in which $\mathbf{X_{i,s,t}}$ only includes the FHFA housing price index, which proxies for the state-level severity of the housing crisis.[4] The variable $\text{Bound}_s$ is an indicator set equal to 1 if a state's January 2008 minimum wage was below \$6.55, rendering it bound by the entirety of the July 2009 minimum wage increase.

Equation (1) allows for dynamics motivated by data we present below. Specifically, we code May to July 2009 as a "Transition" period. Prior months correspond to the baseline, or period $p = 0$. August 2009 through July 2010 is period "Post 1" and all subsequent months are period "Post 2."

The primary coefficients of interest are $\beta_{\text{Post 1}(t)}$ and $\beta_{\text{Post 2}(t)}$. These are the coefficients on interactions between our time period indicators and our "Bound" indicator. The coefficients $\beta_{\text{Post 1}(t)}$ and $\beta_{\text{Post 2}(t)}$ are thus estimates of the differential evolution of the dependent variable in states that were bound by the new federal minimum wage relative to states that were not bound. We analyze several dependent variables ($Y_{i,s,t}$), with the primary variable of interest being an indicator for whether individual $i$ is employed. We calculate the standard errors on these coefficients allowing for correlation clusters in the errors, $\varepsilon_{i,s,t}$, at the state level.

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[4]It is not uncommon for minimum wage studies to control directly for a region's overall employment or unemployment rate. Conceptually, we find it preferable to exclude such variables because they may be affected by the policy change of interest. The house price index is a conceptually cleaner, though still imperfect, proxy for time varying economic conditions that were not directly affected by minimum wage changes. Our results are essentially unaffected by the inclusion of additional state macroeconomic aggregates in $\mathbf{X_{i,s,t}}$. An analysis of our baseline result's robustness along this margin can be found in Appendix Table B.1.

In addition to the difference-in-differences estimates associated with equation (1), we present estimates from a triple-difference estimator. When implementing the triple-difference estimator, we use individuals with average baseline wage rates between $8.50 and $10.00 as a "within-state control" group. The triple-difference estimator, which augments equation (1) with group-by-time-period effects, group-by-state effects, and state-by-time-period effects, appears below:

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

In this equation, the coefficients $\beta_{\text{Post 1}(t)}$ and $\beta_{\text{Post 2}(t)}$ are estimates of the differential change in employment among low-skilled individuals, comparing those in "bound" states to those in "unbound" states, net of any differential employment changes experienced by individuals with moderately higher baseline wage rates.

The key question for evaluating our estimation frameworks is whether factors other than the minimum wage have contributed to any differential employment changes we estimate. The key assumption is that the regression models' error terms are uncorrelated with the policy variables of primary interest. In our setting, there are two principle threats to this assumption. The first and most obvious concern is that forces underlying the Great Recession may have been differentially severe in either the treatment group or the control group. A second concern is that differences in the characteristics of individuals in our samples from "bound" and "unbound" states lead them to have different counterfactual employment trajectories for reasons unrelated to the minimum wage. We take both of these concerns quite seriously and present substantial evidence on their

potential relevance. To maintain the linearity of the paper's exposition, we reserve this discussion for section 5, after we have presented our primary results.

# 4 Analysis of the Minimum Wage's Effects

This section presents estimates of equations (1) and (2), along with simple time series tabulations of the data that underlie the estimates. We begin by verifying that the enacted minimum wage increases shifted the wage distributions of workers with average baseline wages below $7.50 as intended. We then estimate the minimum wage's effect on employment, after which we explore several additional outcomes relevant to the welfare of affected individuals and their families.

## 4.1 Effects on Low-Skilled Workers' Wage Distributions

Figure 2 presents time series tabulations of the unadjusted data underlying our estimates of equations (1) and (2). The sample underlying Panels A, C, and E corresponds with the "target" sample. The sample underlying Panels B, D, and F consists of individuals with average baseline wage rates between $7.50 and $10.00.

The panels in row 1 of Figure 2 plot the fraction of individuals that, in any given month, had an hourly wage between $5.15 and $7.25. Prior to the implementation of the $7.25 federal minimum, individuals in states that were bound by the federal minimum wage were much more likely to have wages in this range than individuals in unbound states.[5] These fractions began converging after April 2009, motivating our treatment of May through June 2009 as a "transition" period in our regression analysis. Panel B shows that the wages of individuals with average baseline wages between $7.50 and $10.00 per

[5]Those in bound states spent roughly 37 percent of their months in jobs with wages between $5.15 and $7.25, or half of the months during which they were employed. By contrast, individuals in unbound states spent 22 percent of their baseline months in jobs with hourly wages between $5.15 and $7.25.

hour were largely unaffected by the increase in the federal minimum.

The histograms in the panels of Figure 3 present the distribution of baseline wages for the groups on which our analysis focuses.[6] As shown in Panel A, the histogram for workers with average baseline wages below $7.50 has substantial mass associated with monthly wage rates between $6.50 and $7.50. Panel B shows that workers with average baseline wages between $7.50 and $8.50 have far less, though non-trivial, mass in the affected region. Panel C reveals workers with average baseline wages between $8.50 and $10.00 to be low-skilled workers who spent essentially none of their baseline months at affected wage rates.

Consistent with what one would predict using the baseline wage histograms, the estimates in Figure 3 and in rows 1 through 3 of Table 2 reveal that minimum wage increases had significant effects on the wage distributions of workers with average baseline wage rates less than $7.50, modest effects on those with average baseline wages between $7.50 and $8.50, and no detectable effect on those with average baseline wages between $8.50 and $10.00. Panel A of Figure 3 shows that, for individuals with average baseline wages below $7.50, the wage distribution shifted significantly out of precisely the targeted region. As summarized in Table 2's column 1, this group's probability of having a wage between $5.15 and $7.25 declined by just over 16 percentage points relative to the control group. This mass does not shift exclusively to the new federal minimum, as a portion collects between $7.50 and $8.00.

## 4.2 Baseline Results on Employment

Panels C and D of Figure 2 plot employment time series for individuals in our "target" and "within-state control" groups, separately in bound and unbound states. Panel C shows that low-skilled workers in states with low minimum wages initially had mod-

[6]Note that the histograms exclude the large mass of observations with no earnings.

erately higher employment rates, by nearly 4 percentage points, than those in states with higher minimum wages. As wages adjusted to the new federal minimum, this baseline difference narrows. Over subsequent years, the employment of those in bound states is, on average, roughly 1 percentage point less than that of low-skilled individuals in unbound states. Relative to the baseline period, the differential employment change observable in the unadjusted data is 4 percentage points in the first year and 5 percentage points in subsequent years.

If these employment changes were driven primarily by cross-state differences in the severity of the Great Recession, similar (perhaps slightly smaller) changes would be expected among workers with modestly greater skills. Panel D shows that the employment of workers with average baseline wages between $7.50 and $10.00 changed similarly in bound and unbound states between the initial and later years of the sample period. The forces underlying the differential employment changes presented in Panel C were thus unique to the segment of the labor market that was directly affected by this period's minimum wage increases. Appendix Figures A.1 and A.2 further translate the unadjusted data into time series that highlight the dynamics with which differential changes in employment arose. Further details on the construction of the series in these figures can be found in their respective notes.

Table 2's columns 4 through 6 present estimates of equation (1) in which the outcome is an indicator for being employed. Column 4 reports the result for individuals with average baseline wages less than $7.50. The coefficient in row 1 implies that binding increases in the federal minimum wage resulted in a 4.4 percentage employment point decline between the baseline period and the following year. The decline relative to baseline averaged 6.6 percentage points over the two subsequent years.

Column 5 shows the result for the group with average baseline wages between $7.50 and $8.50. The estimated effect of the minimum wage on this group's employment is

statistically indistinguishable from 0, with a medium-run point estimate of negative 2.6 percentage points. Finally, column 6 shows the result for the group with average baseline wages between $8.50 and $10.00. The estimated effect on this group's employment is a statistically and economically insignificant negative 0.2 percentage point. Like the unadjusted data from Figure 2, these results reveal that estimates of equation (2) will yield results similar to estimates of equation (1). This is confirmed in the first column of Table 3, in which panel A presents estimates of equation (1) while panel B presents estimates of equation (2).

Appendix Table A.1 further fleshes out our estimates of the effect of binding minimum wage increases on employment across the adult population. To the results reported in Table 2, it adds estimates associated with adults who were either not employed throughout the baseline period (column 1) or whose average baseline wages were equal to or greater than $10.00 (column 5). The estimates for individuals with relatively high baseline wages is economically and statistically indistinguishable from 0. The estimate for those who were not employed at baseline is modestly negative, suggesting an increase in the difficulty of labor force entry.

We conclude this subsection by converting our estimates into an elasticity of our target group's employment with respect to the minimum wage. Our medium-run estimate is that a binding July 2009 minimum wage increase reduced our target group's employment rate by 9 percent (6.6 percentage points on a baseline employment rate of 72 percent). The differential minimum wage increase, comparing bound and unbound states, also averaged 9 percent (60 cents on a base of $6.55). The implied elasticity of the target group's employment with respect to the minimum wage is thus roughly -1. We relate our estimate to prior research in section 6.

## 4.3 Further Employment Outcomes, Average Income, and Poverty

Table 3 reports the results of a more in depth analysis of the minimum wage's effects on employment and income related outcomes. In Table 3's second column we present evidence of a novel channel through which job markets may respond to minimum wage increases. Specifically, we estimate that binding minimum wage increases modestly increased the probability that targeted individuals work without pay, perhaps in internships, by 1.7 percentage points. Between disemployment and work without pay, column 3 reports a combined 8 percentage point reduction in paid employment. Panels E and F of Figure 2 present unadjusted time series for this "no earnings" outcome.

Table 3's columns 4 and 5 report the effect of binding minimum wage increases on average monthly incomes. Column 4 reports the effect on individual-level income while column 5 reports the effect on family-level income.[7] In our difference-in-differences specification, we estimate that binding minimum wage increases reduced the average monthly income of low-skilled individuals by $92 in the short-run and $144 in the medium-run. Results are slightly larger, though estimated with less precision, in our triple-difference specification.[8]

What accounts for the earnings declines we observe? Targeted individuals in bound states had positive earnings in 61 percent of baseline months. In 28 percent of months they lacked employment and in 11 percent they worked with no earnings. Average income for the target sample was $750 across all baseline months, and thus roughly $1,230 in months with positive earnings. For the short run (i.e., year 1), we estimated a 5.9 percentage point decline in the probability of having positive earnings. This effect

[7]We censor these outcomes at $7,500 and $22,500 per month respectively. This affects fewer than 1 percent of the observations in our "target" group, which are associated with incomes far beyond those attainable through minimum wage employment.

[8]Robustness across equations (1) and (2) is particularly relevant for outcomes involving income. It reassures us that the results are not spuriously driven by convergence in the control-group workers' incomes towards the relatively high per capita incomes associated with the states in which they live.

is thus directly associated with an average decline of roughly \$73, or $\$1,230 \times 0.059$. The decline in months with positive earnings rises to 8.2 percentage points over the following two years, implying a direct earnings decline of \$101. Gains for workers successfully shifted from the old minimum to the new minimum offset relatively little of this decline.[9]

The effects of lost employment rise over time due to lost experience. Minimum wage workers tend to be on the steep portion of the wage-experience profile (Murphy and Welch, 1990). Using mid-1980s SIPP data, Smith and Vavrichek (1992) found that 40 percent of minimum wage workers experienced wage gains within 4 months and that nearly two-thirds did so within 12 months. The median gain among the one-year gainers was a substantial 20 percent. Among those not employed or working without pay, foregone wage growth of these magnitudes brings the implied medium-run earnings decline to \$130.[10] Targeted workers who maintain employment may also experience slow earnings growth if employers reduce opportunities for on the job training.

Our estimates of the minimum wage increase's effect on income are initially somewhat surprising. As illustrated above, however, they follow from the magnitude of our estimated employment effects coupled with two more conceptually novel factors. These factors include a modest "internship" effect and effects on income growth through re-

[9]Recall that we estimated a 16 percentage point decline in the probability of having a wage between \$5.15 and \$7.25. Nearly half of this turns out to involve shifts into unemployment or unpaid work. The wage increase for the remaining 8 percentage points was roughly 10 percent (from the \$6.55 minimum for 2008 to the \$7.25 minimum for 2009). A 10 percent increase on the \$1,213 base, realized by 8 percent of workers, averages to a gain of \$10. Measurement error in self-reported wage rates likely leads this approach to understate the true gain; it likely attenuates our estimates of the minimum wage's bite on the wage distributions of low-skilled workers. An alternative approach, likely generating an upper bound, is to infer the minimum wage's bite from the data displayed in Figure 2. Figure 2's panel A showed that low-skilled workers in bound states saw their probability of reporting a wage between \$5.15 and \$7.25 decline by roughly 35 percentage points from a base of just over 40 percentage points. Even the 35 percentage points of bite one could maximally infer from Figure 2 implies quite modest offsets of the income losses associated with disemployment, work without pay, and lost experience accumulation.

[10]Two years of early-career earnings growth at 15 percent per year would bring earnings from a baseline of \$1,230 to \$1,627. An 8.2 percentage point decline in months at such earnings implies an average reduction of \$133.

duced experience accumulation.

We next estimate the minimum wage's effects on family-level outcomes. On average in our sample, each targeted worker is in a family with 1.2 targeted workers. This is roughly the average of the ratio of our estimates of the minimum wage increase's effect on family-level income to its effect on individual-level income. In the difference-in-differences specifications, for example, the short-run effect on individual-level income is -$92 per month while the estimated effect on family-level income is -$118 (the medium-run estimates are -$144 and -$273). Finally, column 6 shows that the effect of binding minimum wage increases on the incidence of poverty was statistically indistinguishable from 0. Unsurprisingly, given our finding on family-level earnings, the point estimate for the medium-run effect on the likelihood of being in poverty is positive.

Figure 4 shows the unadjusted time trends that underlie our estimates for the effects of this period's minimum wage increases on individual income, family income, and the probability of being in poverty. Figures A.1 and A.2, which were discussed earlier in the context of our employment results, further translate the unadjusted data into time series that trace out the dynamics with which differential changes in individual earnings and family incomes arise. In all cases, it is quite readily apparent how the data in Figures 4, A.1, and A.2 translate into the regression-adjusted estimates in Table 3.

## 4.4 Transitions out of Low-Wage Work

We next analyze income growth through the lens of economic mobility. Concern regarding the minimum wage's effects on upward mobility has a long history (Feldstein, 1973). There exists little direct evidence, however, of the minimum wage's effects on individuals' transitions into employment at higher wages and earnings levels.

Because we observe individuals for four years, we are able to track transitions of low-wage workers into middle and lower middle class earnings. The data reveal that initially

low-wage workers spend non-trivial numbers of months with earnings exceeding those of a full time, minimum wage worker. Consider earnings of $1500, which could be generated by full time work at $8.66 per hour. During the first year of our sample, workers with average baseline wages less than $7.50 earn more than $1500 in 8 percent of months. By the sample's last two years this rises, adjusting for inflation, to 18 percent. We investigate the minimum wage's effects on the likelihood of reaching such earnings.

Table 4 reports the results. We find significant reductions in economic mobility, in particular for transitions into lower middle class earnings. For the full sample with average baseline wages less than $7.50, the difference-in-differences estimate implies that binding minimum wage increases reduced the probability of reaching earnings above $1500 by 4.7 percentage points. As with previous results, this finding cannot readily be explained by cross-state differences in economic conditions. Netting out the experience of individuals with baseline wages between $8.50 and $10.00 moderately increases the point estimate to 5.4 percentage points.

The estimated reductions in the probability of reaching lower middle class earnings levels are particularly meaningful for low-skilled workers with no college education. In our estimates of equations (1) and (2), the estimated reduction in this group's probability of earning more than $1500 per month is, respectively, one third and one half of the control group's end-line probability of having such earnings. For those with at least some college education, the estimated reductions average less than one fifth of the control group's end-line probability of having such earnings. Figure 5 presents the unadjusted data underlying these results.

We next examine the probability of reaching the middle-income threshold of $3000 per month. For the full sample, we estimate that binding minimum wage increases reduced this probability by 1.7 percentage points. In the difference-in-differences specification, this estimate is statistically distinguishable from 0 at the 10 percent level; in

the triple-difference specification this is not the case, although the point estimate is essentially unchanged. Though our sub-sample analysis has little precision, the average medium-run effect appears to be driven primarily by those with at least some college education.

We interpret the evidence as implying that binding minimum wage increases reduced the medium-run class mobility of low-skilled workers. The dynamics of our estimated employment and class mobility results are suggestive of the underlying mechanisms. Our employment results emerge largely during the first year following the increase in the federal minimum wage. By construction, our mobility outcomes are not outcomes that can be affected by the loss of a full time minimum wage job. Effects on mobility into lower middle class earnings only emerge over subsequent years. It appears that binding minimum wage increases blunted these workers' prospects for medium-run economic mobility by reducing their short-run access to opportunities for accumulating experience and developing skills. This period's minimum wage increases may thus have reduced upward mobility by making the low rungs on the earnings ladder more difficult for low-skilled workers to reach.

# 5 Our Estimation Framework's Strengths and Weaknesses

As with any empirical analysis, our framework and data come with distinct disadvantages as well as advantages. We begin this section by summarizing their distinct advantages. We then discuss two primary sources of potential bias in our estimates, namely variations in the severity of the Great Recession and issues connected to the composition of our analysis sample. Finally, we discuss statistical inference.

## 5.1 Our Estimation Strategy's Key Strengths

The key strengths of our estimation strategy relate to its capacity to describe the minimum wage's effects on a broad population of targeted workers. Past work focuses primarily on the minimum wage's effects on particular demographic groups, such as teenagers (e.g., Card (1992) and Neumark and Wascher (1995)), and/or specific industries, like food service and retail (e.g., Dube, Lester, and Reich (2010)). While minimum and sub-minimum wage workers are disproportionately represented among these groups, both are selected snapshots of the relevant population. In contrast, we estimate the minimum wage's effects on a broader population of low-skilled workers.[11] Our sample consists primarily of low-skilled adults, who are more relevant than teenagers when analyzing the minimum wage from an anti-poverty policy perspective.

A second advantage of our estimation strategy relates to our ability to provide evidence on the relevance of sources of bias over which the literature has long been in conflict. The SIPP gives us 12 months of baseline wage data with which we can identify low skilled individuals. This has two key benefits. First, we are able to pinpoint "target" groups more intensely affected by minimum wage increases than the analysis groups in many studies. Second, we are able to pinpoint workers who were not directly affected yet, as evidenced by their wage rates, were only moderately more skilled than the "target" workers. We use this second group to construct a set of within-state counterfactuals that proxy for otherwise unobserved shocks to the low-skilled labor market. This enables us to check and account for labor market shocks experienced by workers who are quite similar to the minimum wage workers of primary interest.

Our data and framework enable us to generate two additional novel contributions. A first point of interest is that we are able to provide evidence from a very particular but

[11] Linneman (1982) similarly discusses this benefit of analyzing individual-level panel data in the context of minimum wage increases enacted during the 1970s.

important historical episode, namely the Great Recession and its aftermath. Efforts to understand this period's labor market developments have filled many pages in leading economics journals, and we provide a novel contribution to that literature. Finally, we are able to investigate outcomes that can only be investigated using longitudinal data. Specifically, we are able to provide novel evidence on the minimum wage's effects on earnings trajectories.

## 5.2 Threats Associated with Macroeconomic Conditions

In addition to the advantages discussed above, our data and setting come with weaknesses. As in any observational study, there are standard threats to interpreting $\beta_{\text{Post 1}(t)}$ and $\beta_{\text{Post 2}(t)}$ from equation (1) as unbiased, causal estimates of the effect of binding minimum wage increases. The primary threat to our estimation framework is the possibility that bound and unbound states experienced housing crises of different average severity. In the remainder of this subsection, we discuss the available evidence on the relevance of this threat.

An important limitation to our analysis is that we lack a long "pre-intervention" period during which we are able to examine the relevance of pre-existing trends.[12] This heightens the importance of other features of our analysis. In particular, our case for causal identification rests to a significant degree on our triple-difference design and on the comprehensiveness of the variables we have examined as potential proxies for the shocks that occurred during the time period we analyze.

Figure 6 presents data from the BLS, the BEA, and the FHFA on the macroeconomic experiences of bound and unbound states during the Great Recession. The data reveal

[12]We note, however, this may be a classic setting in which "pre trends" estimated on a longer pre-intervention period would be strongly misleading. Research by Charles, Hurst, and Notowidigdo (2016) highlights that the local magnitudes of the housing bubble's upswing and downswing were fairly symmetric. Consequently, state-specific "pre trends" estimated during the boom would tend to project future shocks with the opposite sign of the mean-reverting shocks that occurred in practice.

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. From 2006 to 2012, income per capita declined by just over $1,000 less in the bound states than in the unbound states. The overall employment rate declined half a percentage point less, the prime aged employment rate declined nearly a full percentage point less, and the unemployment rate rose just over 1 percentage point less. Finally, the median house price index declined dramatically less, while overall construction output declined roughly 12 percent less. Consequently, analyses that do not adjust for variations in the severity of the underlying recession will tend to be biased towards positive values. The robustness analyses summarized below are consistent with this assessment.

We present a first set of robustness analyses in table A.2. The results in column 1 replicate the findings from Table 3's column 1. Column 2 presents results in which we exclude all controls for states' macroeconomic conditions. Column 3 introduces state-specific linear time trends. Column 4 incorporates extensive sets of age, education, and family-size indicators interacted with linear time trends. Column 5 alters our panel balance criterion. Finally, for columns 6 and 7 we modify our criteria for categorizing the bound and unbound states.[13] The medium-run point estimates in Table A.2 range from -4.8 to -8.5 percentage points. Appendix Tables A.3, A.4, A.5, and A.6 show that the effects we estimate on all of our primary outcomes of interest are fairly robust to this same set of specification checks.

For further analysis of the minimum wage's effects on employment, we refer readers to appendix B.2. Here we emphasize, as shown in appendix Table B.1, that our

[13]Column 6 drops unbound states in which the January 2008 minimum wage was less than $7.00, as such states were moderately bound by subsequent increases in the federal minimum. Column 7 removes from the sample any bound state with a January 2009 minimum wage above $6.55. Our baseline designation uses states' January 2008 minimum wage rates to ensure that it is based on decisions made before our sample begins. We observe that 4 states (Montana, Nevada, New Hampshire, and New Mexico) with January 2008 minimum wage rates below $6.55 voluntarily increased their minimums before they were required to do so.

results are robust to augmenting our regressions with controls for a range of additional direct proxies for variations in macroeconomic conditions. These include the aggregate employment rate, aggregate income per capita, a measure of stimulus spending, a bartik-style control for exposure to industries in decline, and additional variables of potential interest. Across Tables A.2 and B.1, the medium-run estimate of the effect of this period's binding minimum wage increases ranges from -4.8 to -8.6 percentage points. Unsurprisingly, the smallest estimates result from specifications in which the covariates in the regression include no direct proxies for the fact that the housing decline was far more severe in unbound states than in bound states.

As an additional check on our results, we estimate equations (1) and (2) on samples selected through matches on the size of states' housing declines. We match states on the size of their median house price declines between 2006 and 2012 (with values averaged across all months in these years). To be more precise regarding the procedure, we apply nearest neighbor matching without replacement. We then restrict the sample on the basis of the quality of the resulting matches. Specifically, we require that the difference in matched states' housing declines be no greater than 20 index points. The results can be found in Table B.2. The medium-run point estimates in the table range from -6.8 percentage points to -8.7 percentage points.

Because we are working with observational data, it is of course impossible for us to rule out every possible source of bias. We have shown that our results are robust to controlling for a broad set of covariates that proxy for variations in the performance of the housing market, aggregate income, and other segments of the labor market. Further, they are robust implementing a triple-difference design in which individuals with modest baseline wage rates are the within-state control group. Remaining biases would thus need to involve economic forces that affect the minimum wage segment of the labor market without having a detectable impact on modestly more skilled segments of the

labor market or on the economy as a whole.

## 5.3 Threats Associated with Sample Composition

A second class of concerns involve the possibility that differences in the composition of our bound and unbound states' target samples may be a source of differences in their counterfactual employment trajectories. Several differences between the samples from bound and unbound states were apparent in Table 1's presentation of summary statistics. At baseline, individuals in bound states are moderately more likely to be employed, less likely to work without pay, and less likely to be employed at sub-minimum wage rates than are individuals in unbound states. They also tend to be slightly younger and less likely to have obtained at least some college eduction.

Demographic differences create the risk that one might expect the employment trajectories of individuals in bound and unbound states to differ. This motivated our earlier test of our specifications' robustness to the inclusion of an extensive set of demographic dummy variables interacted with linear time trends. We have similarly confirmed that our estimates are robust to controlling for linear trends interacted with dummy variables for each individual's modal industry of employment over the baseline period.

We conduct an additional line of robustness analyses, presented in full in appendix B.2.2, in which we alter the procedure underlying the construction of our analysis sample. Two of the alternatives involve making either greater or lesser use of imputations when constructing the measure of average baseline wage rates along which we select our samples. A third alternative involves selecting the samples based on percentiles within states' wage distributions, while the fourth involves selecting samples based on the average baseline wage rate's distance from state-specific minimum wage rates. Both the details of and motivations for each of these approaches are discussed more fully in the appendix. Importantly, at least one of the alternative samples exhibits substan-

tially greater balance along each of the dimensions along which our primary bound and unbound state samples are unbalanced.

Across the alternative analysis samples, our baseline estimate of the medium-run effects of binding minimum wage rates ranges from -3.7 to -7.7 percentage points. Notably, the smallest estimate comes from the most expansive, and hence least intensely treated, sample. The estimates thus have quite similar implications for effects on population-wide employment rates.

## 5.4 Inference

The standard errors reported in Tables 2, 3, and 4 are conventional cluster-robust standard errors, with allowance for state level clusters. Simulations from Cameron, Gelbach, and Miller (2008) suggest that the inferences we draw on the basis of these standard errors are likely to be appropriately conservative, as we have a setting in which "treatment" applied to 27 of 51 state units. Nonetheless, we have checked for whether we obtain similar p-values when we conduct classic randomization tests in which we iterate over procedures in which we assign treatment at random to 27 of 51 state units.

We conduct three randomization tests. The first applies the classic approach as described by Imbens and Rosenbaum (2005), in which we assign treatment at random across the 51 states as they appear in our baseline analysis sample. Next, we conduct a test in which we assign treatment to 27 of 51 states drawn at random (with replacement) from the states in our control group. Finally, we conduct a similar test using the states in our treatment group. In each case, we conduct 500 iterations of the exercise. The histograms in the panels of Figure 7 plot the resulting distributions of placebo point estimates. The true point estimate is exceeded by one of the 500 placebo estimates in panel A and by none in panels B or C. This provides supportive evidence that the p-values implied by conventional cluster-robust standard errors are appropriately conservative.

# 6 Analysis of Our Estimates' Magnitudes

Our estimated employment effects imply that our target group had an employment elasticity of -1 with respect to the minimum wage. This places our estimates towards the high end of the estimates in the existing, contentious literature on the minimum wage's effects on employment.[14] In this section we thus emphasize two points. First, while most estimates of minimum wage employment elasticities lie between 0 and -0.3, larger elasticity estimates are quite common in recent research. Second, theory indicates that minimum wage effects should be expected to vary substantially across settings.

## 6.1 Comparing Our Estimates with Previous Research

Estimates of the minimum wage's effects in recent studies vary substantially (Neumark, 2017). Several studies from the last decade find that minimum wage increases have substantial effects on employment among low-skilled individuals. Notably, this includes research using a randomized experiment (Horton, 2017), research using administrative employment and wage records (Jardim, Long, Plotnick, Van Inwegen, Vigdor, and Wething, 2017; Kreiner, Reck, and Skov, 2017), and research using a variety of methods that are common in recent program evaluation literatures (Sabia, Burkhauser, and Hansen, 2012; Baskaya and Rubinstein, 2012; Powell, 2016).[15]

Other work using common program evaluation methodologies concludes that employment effects are quite small and/or indistinguishable from 0. Two such studies

[14]See, for example, research by Card and Krueger (1995); Neumark and Wascher (2008); Allegretto, Dube, Reich, and Zipperer (2017); Neumark and Wascher (2017).

[15]Sabia, Burkhauser, and Hansen (2012) estimate an elasticity quite similar to ours in the specific context of New York's 2006 minimum wage increase. Baskaya and Rubinstein (2012) find elasticities on the order of -0.5 in an analysis that isolates variation driven by federal minimum wage increases for a sample extending from 1977 to 2007. Their identification strategy is motivated by evidence that state-initiated minimum wage increases are correlated with declines in aggregate unemployment, and will thus tend to yield estimates biased towards 0. Powell (2016) analyzes minimum wage changes from 1979 to 2014 using a generalizable synthetic control approach. He finds a teenage employment elasticity of -0.44.

include Dube, Lester, and Reich (2010), which uses a border-county research design, and Cengiz, Dube, Lindner, and Zipperer (2018), which uses a bunching estimator. Both papers analyze several decades of minimum wage changes simultaneously, rather than focusing on well-defined historical episodes. Harasztosi and Lindner (2018) also apply a bunching estimator. They estimate small employment elasticities in the context of a large national minimum wage increase enacted by Hungary.

Taken together, the recent literature thus includes many estimates that are both large and small relative to the consensus around which earlier research had converged. It is difficult to disentangle the extent to which different results are driven by sampling variations, differences in research designs, and real differences in the minimum wage's causal effects across settings. Our large elasticity estimates may, in principle, be driven by any combination of these factors.

## 6.2 Understanding Magnitudes: The Minimum Wage's Bite

Theory indicates that minimum wage effects may vary dramatically across settings. Notably, this is true in the most basic of either the competitive or monopsonistic model of the labor market, as well as in more nuanced frameworks that showcase search frictions. In the most basic models, a minimum wage increase's effects will be either zero or positive when the minimum wage lies below its "efficient" level, but negative thereafter. Considerations related to the life cycles of firms further point to reasons why the depth and duration of a minimum wage change's bite can lead to nontrivial nonlinearities in its effects (Sorkin, 2015; Aaronson, French, Sorkin, and To, 2018).[16]

Our setting, in which labor demand was depressed and productivity growth quite slow, is a textbook case in which a minimum wage increase's effects may be quite large

[16]Research on minimum wage changes enacted between 2011 and 2016 finds evidence that nonlinearities may be quite important in practice (Jardim, Long, Plotnick, Van Inwegen, Vigdor, and Wething, 2017; Clemens and Strain, 2018).

because the new minimum wage will have both deep and sustained bite. From 2006 to 2014, economy-wide productivity growth averaged 1.3 percent per year. Further, work by Bosler, Daly, Fernald, and Hobijn (2016) shows that within-skill-group productivity growth was much lower following the recession than the aggregate productivity data suggest. From 2008 to 2010, for example, they find that overall productivity growth was buoyed by a 0.9 percentage point contribution from changes in "labor quality." That is, productivity growth was artificially inflated by declines in employment among low-skilled groups. Due to slow productivity growth and modest inflation, rational firms would have expected the minimum wage's rise from $5.15 to $7.25 to have much larger implications for the costs of hiring low-skilled individuals than would more modest minimum wage increases enacted under stronger economic conditions.

# 7 Implications for Aggregate Employment

In this section we consider what our estimates imply for the minimum wage's effects on aggregate employment during the Great Recession. We do this in three steps. First, we infer aggregate within-sample employment declines by multiplying our baseline estimates by the population share implied by sample weights. Second, we discuss the difficult question of whether our estimates might extrapolate to either the initial phases of the federal minimum wage increase or to the increases enacted in the unbound states. Third, we discuss additional research on this same period's minimum wage changes that draws on data from the Current Population Survey (CPS).

We have estimated local average treatment effects for the differentially binding portion of the federal minimum wage increases enacted from 2007 to 2009. We estimate that this differential increase reduced our target sample's employment rate by 6.6 percentage points. The estimates in Table A.1 reveal little evidence of net employment changes

across other groups of working age individuals. Applying the relevant weights, the target group accounts for 7.4 percent of the U.S. population aged 16 to 64. A 6.6 percentage point decline in this group's employment thus implies a $7.4 \times 0.066 = 0.49$ percentage point decline in the employment-to-population ratio in fully bound states.

As noted in the previous section, theory provides reason to expect the minimum wage's effects to be strongly non-linear and perhaps non-monotonic. We thus hesitate to extrapolate away from the local average treatment effect we estimate. We conclude that the best reading of the SIPP data we analyze is that minimum wage increases reduced aggregate employment rates in bound states by at least half of a percentage point during the Great Recession.

An overall assessment of this period's minimum wage increases should consider existing evidence from the CPS as well as the SIPP. Several papers have analyzed this period's minimum wage changes using CPS data (Hoffman, 2014; Clemens, 2015; Zipperer, 2016; Clemens, 2017). Unsurprisingly, given the state of the literature, these papers arrive at different conclusions. Importantly, however, there is no dispute regarding the following descriptive statistical statement: employment rates among low-skilled individuals in bound states under-performed what one would predict based on developments in these states' housing markets, in their per capita incomes, and in moderately higher skilled segments of their labor markets. Differential employment changes are much less sharp in the CPS than in the SIPP, however, and the degree of low-skilled employment's under-performance is smaller in magnitude. An aggregation of SIPP and CPS evidence thus points to a moderately smaller overall effect than what we infer from the SIPP data alone. The difference between the conclusions one would tend to draw from CPS data relative to SIPP data may be driven by some combination of sampling variations, biases to which either the SIPP or CPS analyses are uniquely exposed, or to our ability to more precisely isolate low-skilled individuals in the SIPP relative to the CPS.

# 8 Conclusion

We use data from the SIPP to investigate the effects of the 2007 to 2009 increases in the federal minimum wage on the employment and income trajectories of low-skilled workers. We estimate that the minimum wage increases enacted during the Great Recession had negative effects on affected individuals' employment, income, and income growth. The SIPP data suggest that this period's minimum wage increases reduced aggregate employment rates by at least half of a percentage point in states that were fully bound by the federal minimum wage's rise from $5.15 to $7.25.

We emphasize that the minimum wage's effects depend crucially on the economic factors underlying low-skilled individuals' wages. Its intended effects can be large when low wage rates reflect weaknesses in low-skilled individuals' bargaining positions. Its unintended effects can be large when low wage rates reflect low demand for low-skilled individuals' output.

Wage and productivity data reveal that the minimum wage increases we analyze had much deeper and more sustained bite on low-skilled groups' wage distributions than have prior minimum wage increases. This reflects both the magnitude of the minimum wage increases we analyze and the effects of trade, technology, and the housing market on demand for low-skilled labor. We conclude that it will be important to analyze future minimum wage increases with reference to low-skilled labor demand's evolution.

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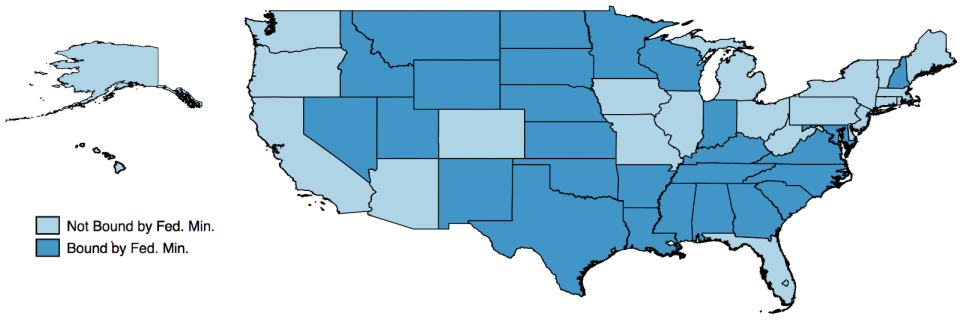
# Tables and Figures

*Panel A*




*Panel B*

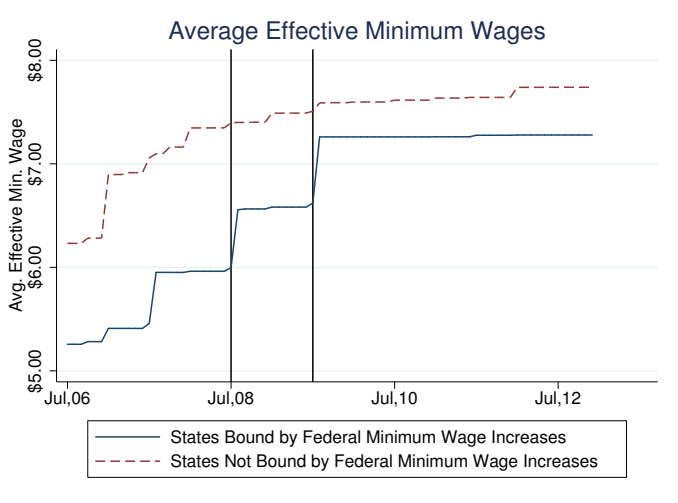



Figure 1: **Minimum Wage Policy Variation:**
The map in panel A labels states on the basis of whether we characterize them as bound by the July 2009 increases in the federal minimum wage. We define bound states as states reported by the Department of Labor (DOL) to have had a minimum wage less than $6.55 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. Panel B presents time series on the average minimum wage (weighted by population) in the bound states relative to the unbound states. The first solid vertical line indicates the timing of the July 2008 increase in the federal minimum wage as well as the first month of data in our samples from the 2008 panel of the Survey of Income and Program Participation. The second solid vertical line indicates the timing of the July 2009 increase in the federal minimum wage.

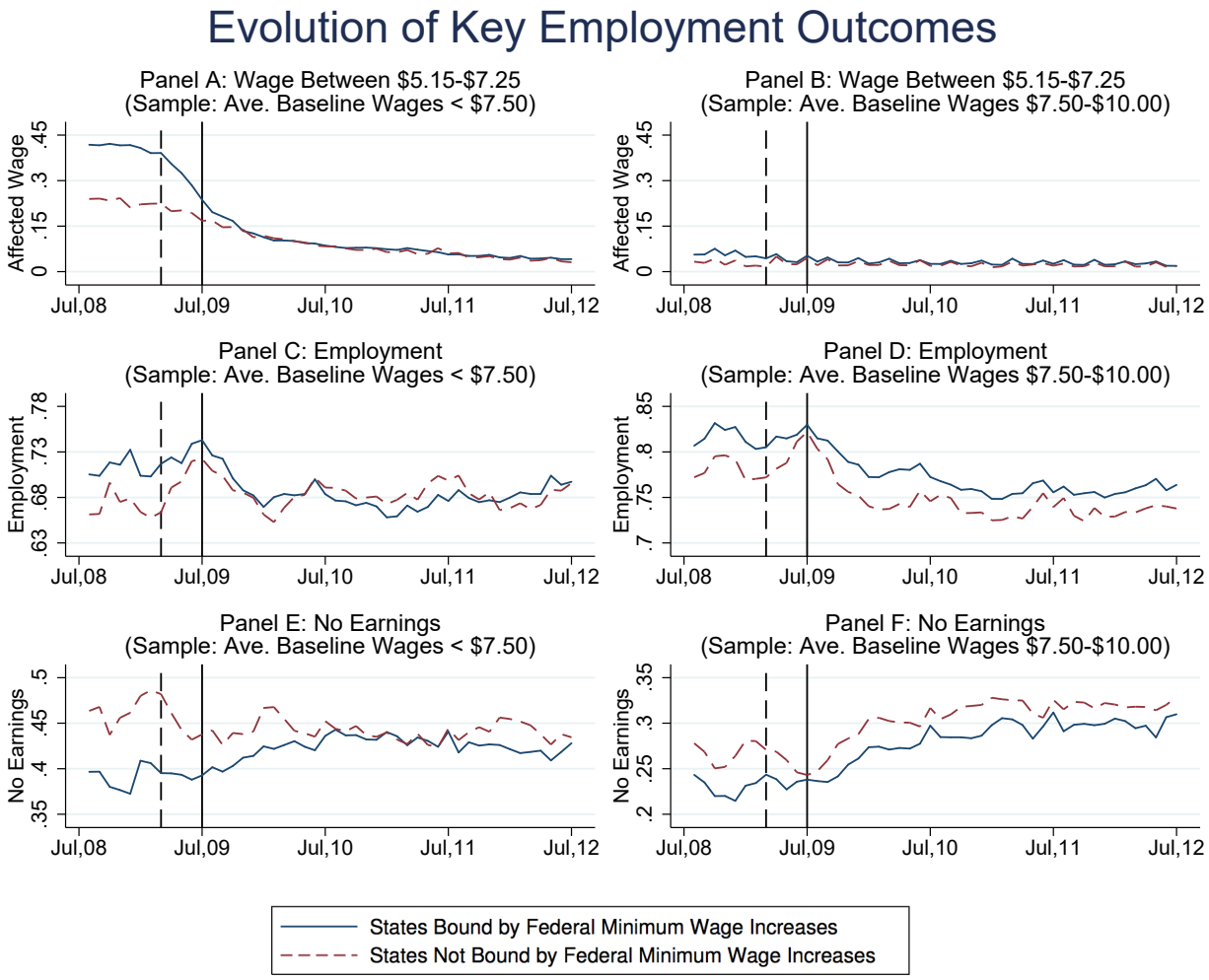



Figure 2: **Evolution of Key Employment Outcomes in Bound and Unbound States:** Bound and unbound states are defined as in previous figures. The figure plots the evolution of three wage, employment, and earnings related outcomes for groups of low-skilled workers. In all cases the series are constructed by the authors using data from the 2008 panel of the Survey of Income and Program Participation (SIPP). In column 1, the samples in each panel consist of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In column 2, the samples in each panel consist of individuals whose average baseline wages are between $7.50 and $10.00. In row 1, the reported outcome is the fraction of observations for which an individual's wage falls between $5.15 and $7.25. In row 2, the reported outcome is the fraction of observations for which an individual is employed. In row 3, the reported outcome is the fraction of observations for which an individual has zero earnings. In each panel, the solid vertical line indicates the timing of the July 2009 increase in the federal minimum wage. The dashed vertical line indicates the April 2009 beginning of the transition of wages out of the range between the old and new federal minimum; the date for the latter designation is driven by the data displayed in this figure's Panel A.

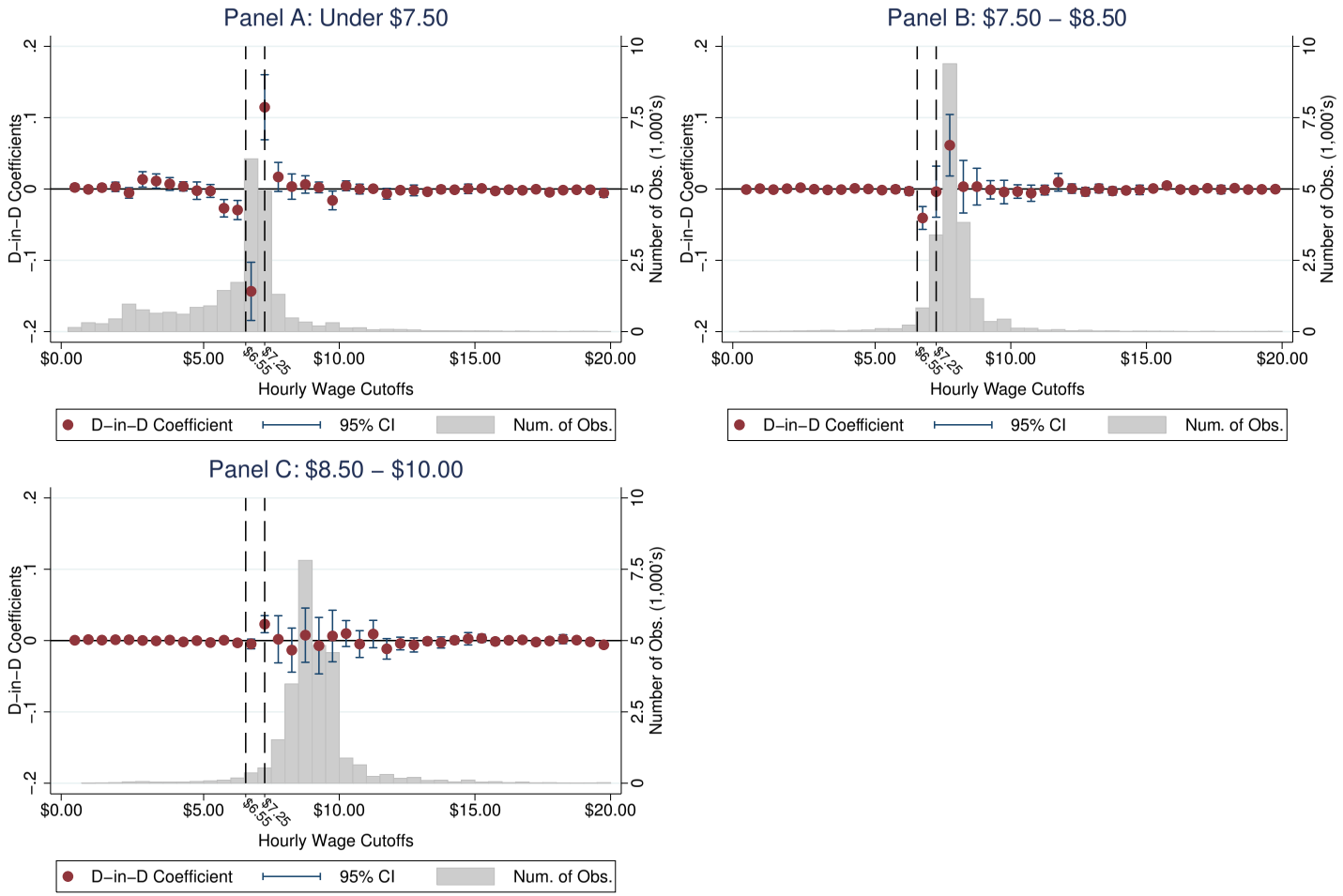



Figure 3: **Estimated Effects of the Minimum Wage on Hourly Wage Distributions:** The figure reports estimates of binding minimum wage increases' medium run effects on the wage distributions of three groups of low-skilled earners. More specifically, each dot is an estimate of the coefficient $\beta_{p(t)}$ from equation (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. The dependent variables in each specification take the form $Y^j_{i,s,t} = 1\{W^{j-1} < \text{Hourly Wage}_{i,s,t} < W^j\}$. These $Y_{i,s,t}$ are indicators equal to 1 if an individual's hourly wage is in the band between $W^{j-1}$ and $W^j$, where each band is a 50 cent interval. The results can thus be described as estimates of the minimum wage's effect on the wage distribution's probability mass function. In Panel A, the sample consists of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In Panel B, the sample consists of individuals whose average baseline wages are between $7.50 and $8.50. In Panel C, the sample consists of individuals whose average baseline wages are between $8.50 and $10.00. In the background of each panel is a histogram displaying the frequency distribution of hourly wages during the sample's baseline period.

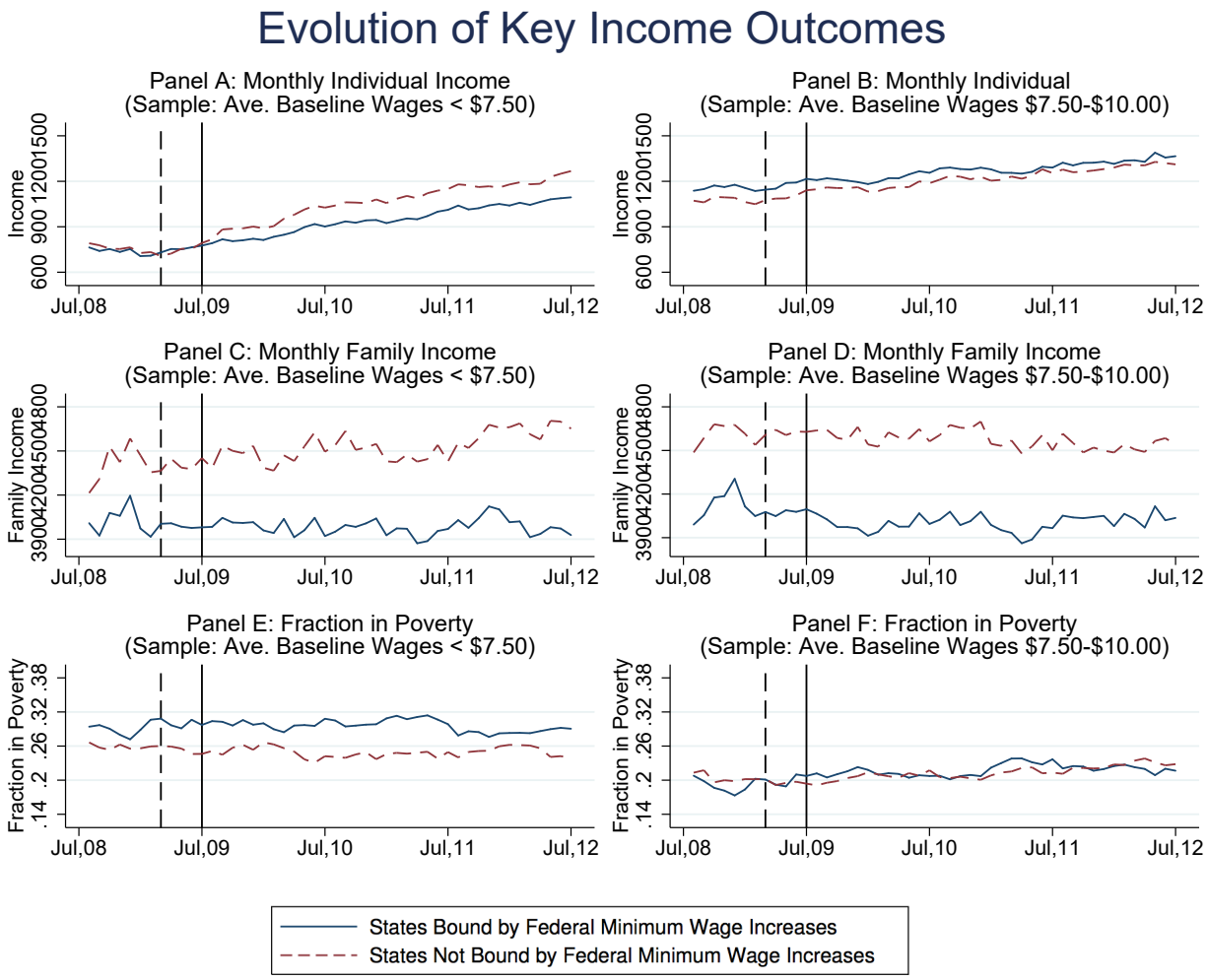



Figure 4: **Evolution of Key Income Outcomes in Bound and Unbound States:** Bound and unbound states are defined as in previous figures. The figure plots the evolution of three earnings related outcomes for groups of low-skilled workers. In all cases the series are constructed by the authors using data from the 2008 panel of the Survey of Income and Program Participation (SIPP). In column 1, the samples in each panel consist of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In column 2, the samples in each panel consist of individuals whose average baseline wages are between $7.50 and $10.00. In row 1, the reported outcome is individual-level income. In row 2, the reported outcome is family-level income. In row 3, the reported outcome is poverty status. In each panel, the solid vertical line indicates the timing of the July 2009 increase in the federal minimum wage. The dashed vertical line indicates the April 2009 beginning of the transition of wages out of the range between the old and new federal minimum.

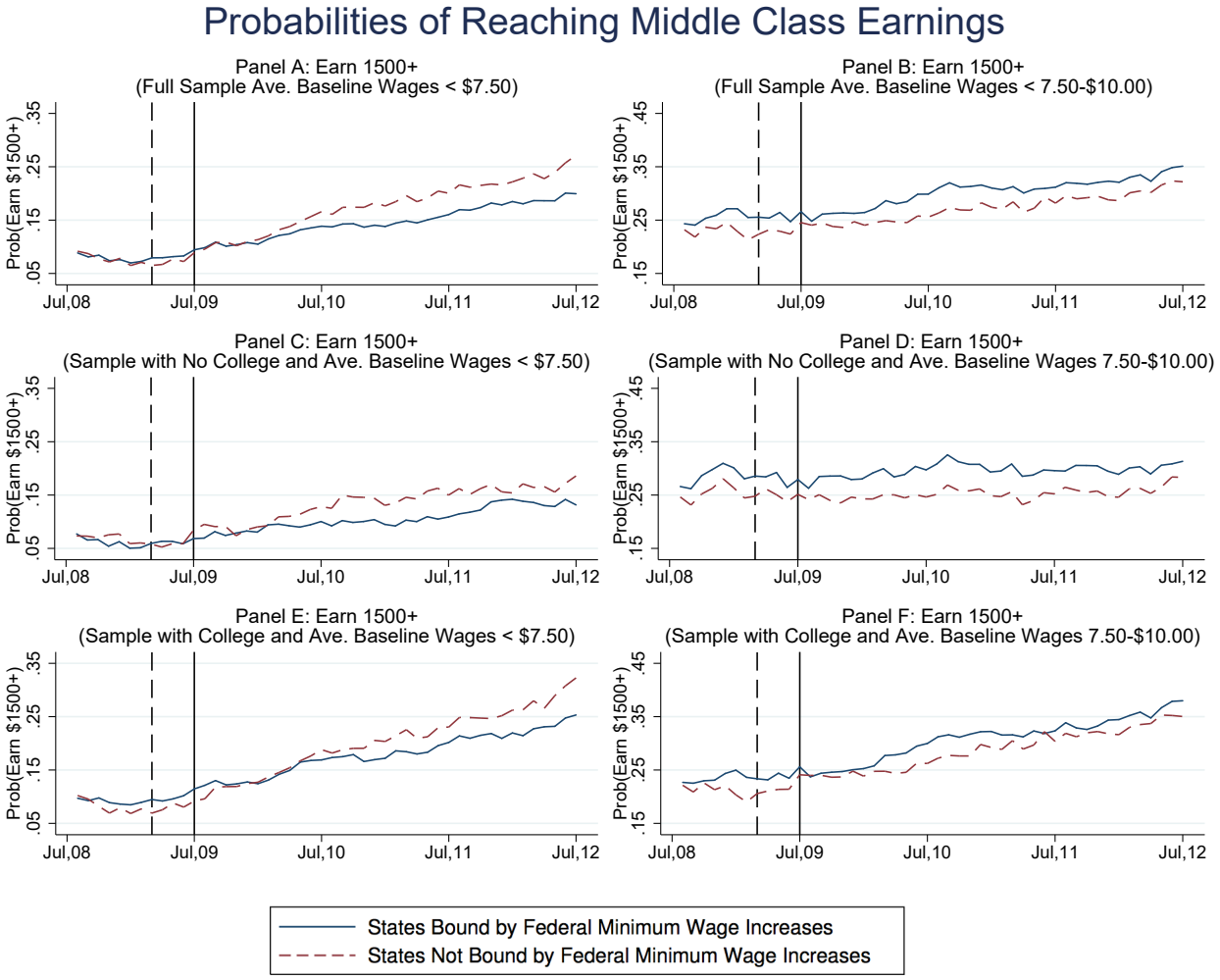



Figure 5: **Probabilities of Reaching Middle Class Earnings:**
Bound and unbound states are defined as in previous figures. In all panels, the figure plots the evolution of the fraction of all in-sample individuals with earnings greater than $1500, which is equivalent to full time work at a wage of $8.66. The series are constructed by the authors using data from the 2008 panel of the Survey of Income and Program Participation (SIPP). In column 1, the samples in each panel consist of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In column 2, the samples in each panel consist of individuals whose average baseline wages are between $7.50 and $10.00. Row 1 presents tabulations of the outcome of interest for the full sample of individuals as defined above. In row 2 the sample is limited to individuals with no college education, while in row 3 the sample is limited to individuals with at least some college education. In each panel, the solid vertical line indicates the timing of the July 2009 increase in the federal minimum wage. The dashed vertical line indicates the April 2009 beginning of the transition of wages out of the range between the old and new federal minimum.

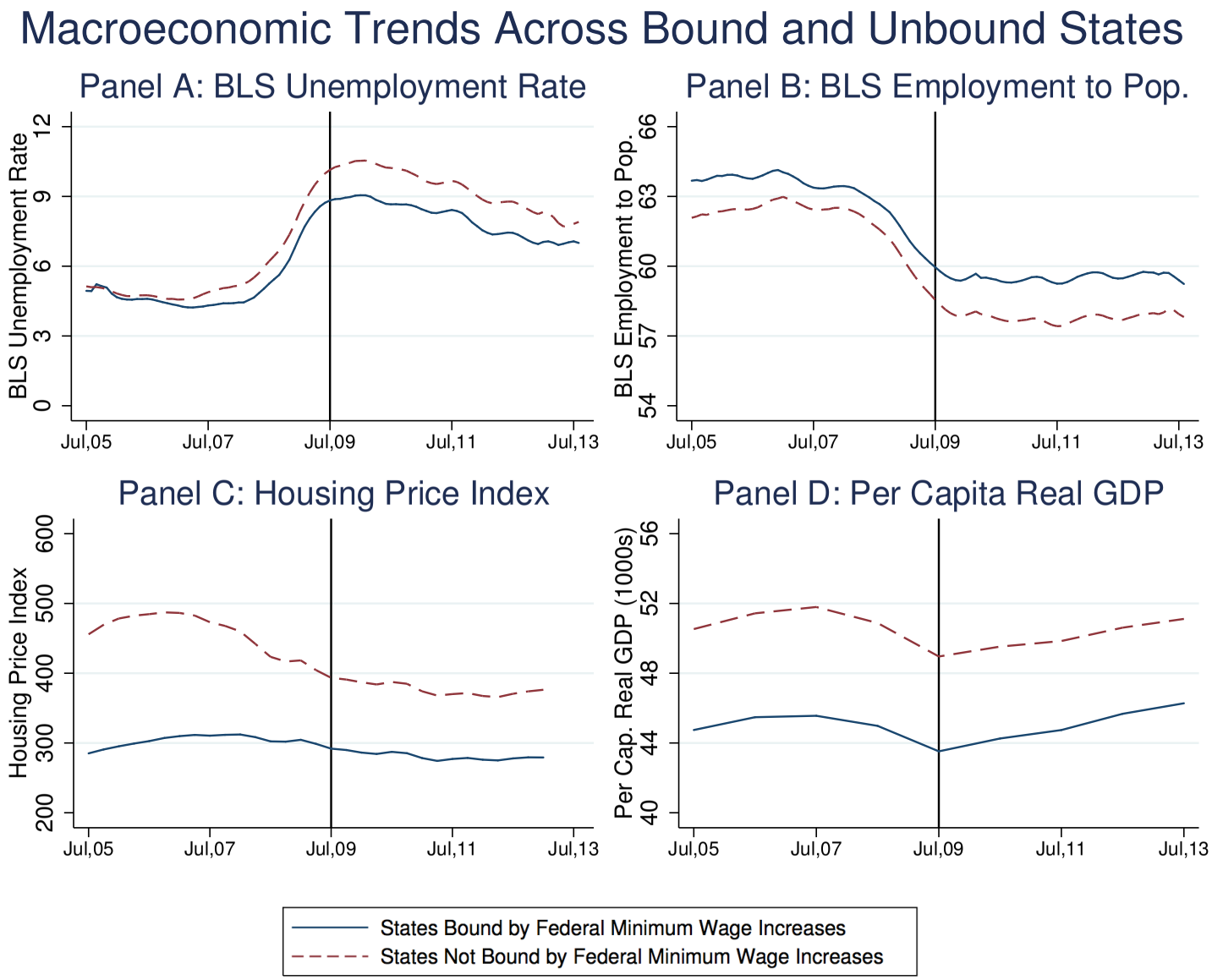



Figure 6: **Macroeconomic Trends in Bound and Unbound States:**
Bound and unbound states are defined as in previous figures. This figure's panels plot the evolution of macroeconomic indicators over the course of the housing bubble and Great Recession. All series are weighted by state population so as to reflect the weighting implicit in our individual-level regression analysis. Panel A plots the average monthly unemployment rate, as reported by the BLS. Panel B plots the average monthtly employment to population ratio, also as reported by the BLS. Panel C plots the average of the quarterly Federal Housing Finance Agency's housing price index. Panel D plots the average of annual real per capita GDP, as reported by the Bureau of Economic Analysis (BEA). In each panel, the solid vertical line indicates the timing of the July 2009 increase in the federal minimum wage.

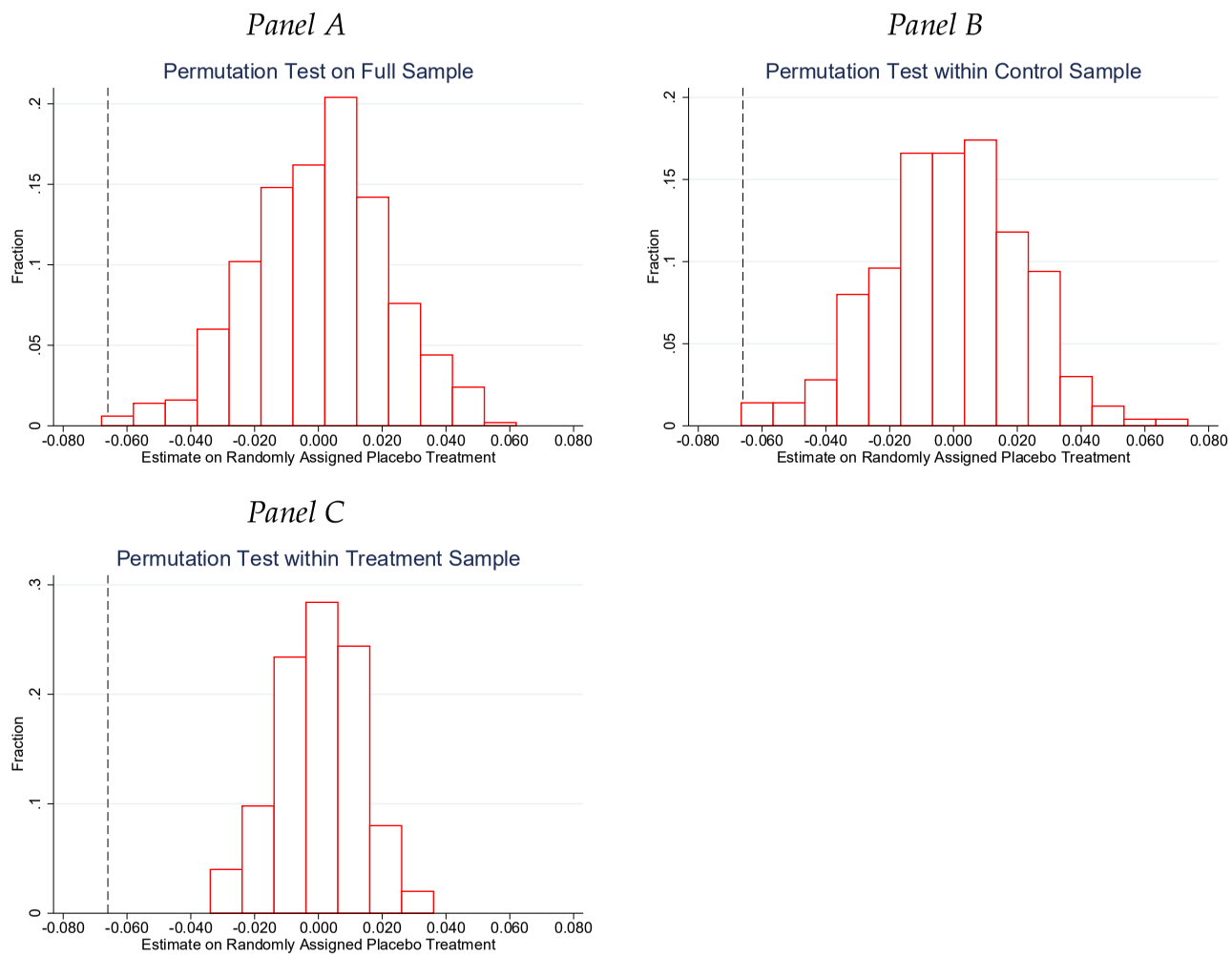



Figure 7: **Placebo Distributions for Randomization Inference:**
Note:The figure presents the distributions of placebo point estimates generated through random assignment of treatment status across states. In each case, the placebo regressions are run on samples that conform to the key properties of the actual analysis sample. Specifically, they involve assigning treatment at random to 27 states out of a sample of 51. For the distribution in panel A, treatment was assigned at random to 27 states out of the standard sample of 51 US states. For panel B, the exercise was restricted to the "unbound" states. From the sample of unbound states, a total of 51 states were drawn with replacement, and treatment was assigned at random to 27 of these 51 units. For panel C, the same exercise was conducted on the "bound" states. In each case, the histogram presents estimates from a total of 500 placebo regressions. The dashed vertical line at -0.066 corresponds with our baseline estimate using the actual data and treatment assignment.

Table 1: **Baseline Summary Statistics by Treatment Status and Average Baseline Wages**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Ave. Baseline Wage | Wage < $7.50 | | $7.50-$8.49 | | $8.50-$9.99 | |
| Treatment Status | Bound | Not Bound | Bound | Not Bound | Bound | Not Bound |
| Wage $5.15-$7.25 | 0.373 | 0.217 | 0.0775 | 0.0402 | 0.0320 | 0.0220 |
| | (0.484) | (0.412) | (0.267) | (0.196) | (0.176) | (0.147) |
| Employed | 0.718 | 0.684 | 0.775 | 0.743 | 0.851 | 0.824 |
| | (0.450) | (0.465) | (0.418) | (0.437) | (0.356) | (0.381) |
| Unpaid Work | 0.110 | 0.142 | 0.0536 | 0.0527 | 0.0448 | 0.0492 |
| | (0.313) | (0.349) | (0.225) | (0.223) | (0.207) | (0.216) |
| No Earnings | 0.392 | 0.459 | 0.279 | 0.310 | 0.193 | 0.225 |
| | (0.488) | (0.498) | (0.448) | (0.462) | (0.395) | (0.418) |
| Num hours worked/week | 24.44 | 23.76 | 27.00 | 23.89 | 31.57 | 29.66 |
| | (18.50) | (19.23) | (17.61) | (16.93) | (15.89) | (16.63) |
| Income | 743.7 | 754.2 | 980.5 | 866.5 | 1317.9 | 1267.4 |
| | (962.0) | (1008.1) | (911.1) | (911.6) | (968.9) | (1030.7) |
| Below FPL | 0.294 | 0.256 | 0.217 | 0.237 | 0.177 | 0.170 |
| | (0.456) | (0.436) | (0.412) | (0.425) | (0.381) | (0.376) |
| Age | 31.58 | 33.02 | 32.51 | 30.30 | 36.24 | 33.65 |
| | (13.96) | (14.56) | (13.54) | (13.47) | (13.09) | (13.31) |
| Num. of Children | 1.091 | 1.015 | 1.053 | 1.055 | 0.921 | 0.920 |
| | (1.302) | (1.281) | (1.279) | (1.247) | (1.275) | (1.187) |
| More than H.S. Deg. | 0.564 | 0.628 | 0.572 | 0.569 | 0.584 | 0.589 |
| | (0.496) | (0.483) | (0.495) | (0.495) | (0.493) | (0.492) |
| Same Job 6+ Months | 0.489 | 0.486 | 0.545 | 0.517 | 0.614 | 0.568 |
| | (0.500) | (0.500) | (0.498) | (0.500) | (0.487) | (0.495) |
| Emp. Entire Baseline | 0.478 | 0.425 | 0.544 | 0.495 | 0.671 | 0.620 |
| | (0.500) | (0.494) | (0.498) | (0.500) | (0.470) | (0.486) |
| Emp. Preceding Hike | 0.703 | 0.671 | 0.758 | 0.713 | 0.834 | 0.809 |
| | (0.457) | (0.470) | (0.428) | (0.452) | (0.373) | (0.393) |
| Num. of Individuals | 1783 | 1477 | 1000 | 1262 | 1185 | 1526 |
| Observations | 20241 | 16857 | 11394 | 14406 | 13649 | 17526 |

Sources: Baseline summary statistics were calculated by the authors using data from the 2008 panel of the Survey of Income and Program Participation. The baseline corresponds with the period extending from August 2008 through July 2009. Columns 1, 3, and 5 report summary statistics for individuals in states we designate as bound by increases in the federal minimum, as described in the note to Figure 1. Column 2, 4, and 6 report summary statistics for individuals in the remaining states, which we designate as unbound. In Columns 1 and 2, the sample consists of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In Columns 3 and 4, the sample consists of individuals whose average baseline wages are between $7.50 and $8.50. In Columns 5 and 6, the sample consists of individuals whose average baseline wages are between $8.50 and $10.00.

Table 2: **Effects on Wage Distributions and Employment by Average Baseline Wages**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent Variable | Wage between $5.15 and $7.25 | | | Employed | | |
| Bound x Post 1 | -0.160*** | -0.034** | -0.008* | -0.044* | 0.004 | -0.008 |
| | (0.021) | (0.010) | (0.004) | (0.019) | (0.021) | (0.012) |
| Bound x Post 2 | -0.163*** | -0.042*** | -0.005 | -0.066** | -0.026 | -0.002 |
| | (0.024) | (0.009) | (0.005) | (0.020) | (0.021) | (0.013) |
| Housing Price Index | -0.616 | -0.072 | 0.100 | 0.755* | 0.610 | -0.335 |
| | (0.424) | (0.161) | (0.088) | (0.323) | (0.440) | (0.371) |
| $N$ | 147,459 | 102,193 | 122,786 | 147,459 | 102,193 | 122,786 |
| Mean of Dep. Var. | 0.302 | 0.057 | 0.026 | 0.702 | 0.757 | 0.836 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| Weighted | No | No | No | No | No | No |
| Individual Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | Under $7.50 | $7.50-$8.49 | $8.50-$9.99 | Under $7.50 | $7.50-$8.49 | $8.50-$9.99 |

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 (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. In columns 1-3, the dependent variable is an indicator for whether an individual's hourly wage is between $5.15 and $7.25. In columns 4-6, the dependent variable is an indicator for whether an individual is employed. In Columns 1 and 4, the sample consists of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In Columns 2 and 5, the sample consists of individuals whose average baseline wages are between $7.50 and $8.50. In Columns 3 and 6, the sample consists of individuals whose average baseline wages are between $8.50 and $10.00. Standard errors are clustered at the state level.

Table 3: **Effects on Employment Status, Income, and Poverty Status**

| Dependent Variable | (1) Employed | (2) Unpaid Work | (3) No Earnings | (4) Ind. Income | (5) Fam. Income | (6) Below FPL |
|---|---|---|---|---|---|---|
| *Panel A*: | | | Difference-in-Differences Specifications | | | |
| Bound x Post 1 | -0.044* | 0.015+ | 0.059** | -92.087* | -117.957 | 0.013 |
| | (0.019) | (0.009) | (0.019) | (36.474) | (85.300) | (0.012) |
| Bound x Post 2 | -0.066** | 0.017+ | 0.082*** | -144.042** | -273.063* | 0.017 |
| | (0.020) | (0.010) | (0.021) | (44.748) | (119.576) | (0.012) |
| *N* | 147,459 | 147,459 | 147,459 | 147,459 | 147,459 | 147,459 |
| Mean of Dep. Var. | 0.702 | 0.125 | 0.422 | 748.459 | 4,190.870 | 0.277 |
| *Panel B*: | | | Triple Difference Specifications | | | |
| Bound x Post 1 x Target | -0.039 | 0.008 | 0.047* | -105.029+ | -94.301 | 0.008 |
| | (0.026) | (0.011) | (0.023) | (55.231) | (112.504) | (0.017) |
| Bound x Post 2 x Target | -0.068** | 0.014 | 0.082*** | -174.255* | -276.503* | 0.015 |
| | (0.022) | (0.012) | (0.022) | (73.107) | (133.386) | (0.018) |
| *N* | 270,245 | 270,245 | 270,245 | 270,245 | 270,245 | 270,245 |
| Mean of Dep. Var. | 0.763 | 0.089 | 0.326 | 995.515 | 4,249.402 | 0.230 |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Panel A 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 (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. Panel B reports analogous estimates of $\beta_{p(t)}$ from equation (2), namely our triple-difference specification. In Panel A the sample consists exclusively of individuals with average baseline wages less than \$7.50. In Panel B the sample is augmented to include individuals whose average baseline wages are between \$8.50 and \$10.00 as a within-state control group. Standard errors are clustered at the state level.

Table 4: **Effects on the Probability of Reaching Lower Middle or Middle Class Earnings Levels**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent Variable | | Earn $1,500+ | | | Earn $3,000+ | |
| *Panel A*: | | | Difference-in-Differences Specifications | | | |
| Bound x Post 1 | -0.016 | -0.021 | -0.012 | -0.004 | -0.007 | -0.002 |
| | (0.011) | (0.015) | (0.012) | (0.007) | (0.008) | (0.009) |
| Bound x Post 2 | -0.047*** | -0.047** | -0.042* | -0.017+ | -0.009 | -0.020 |
| | (0.013) | (0.015) | (0.017) | (0.009) | (0.009) | (0.013) |
| *N* | 147,459 | 60,507 | 86,952 | 147,459 | 60,507 | 86,952 |
| Mean of Dep. Var. | 0.206 | 0.154 | 0.237 | 0.068 | 0.032 | 0.089 |
| *Panel B*: | | | Triple Difference Specifications | | | |
| Bound x Post 1 x Target | -0.003 | -0.034 | 0.016 | -0.013 | -0.023+ | -0.007 |
| | (0.016) | (0.026) | (0.022) | (0.011) | (0.012) | (0.014) |
| Bound x Post 2 x Target | -0.054* | -0.081** | -0.038 | -0.015 | -0.010 | -0.019 |
| | (0.024) | (0.029) | (0.030) | (0.015) | (0.016) | (0.018) |
| *N* | 270,245 | 111,341 | 158,904 | 270,245 | 111,341 | 158,904 |
| Mean of Dep. Var. | 0.206 | 0.154 | 0.237 | 0.068 | 0.032 | 0.089 |
| Sample | Full | H.S. or Less | Some Coll.+ | Full | H.S. or Less | Some Coll.+ |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Panel A 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 (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. Panel B reports analogous estimates of $\beta_{p(t)}$ from equation (2), namely our triple-difference specification. In Panel A the sample consists exclusively of individuals with average baseline wages less than $7.50. In Panel B the sample is augmented to include individuals whose average baseline wages are between $8.50 and $10.00 as a within-state control group. Standard errors are clustered at the state level.

# Appendix Materials
# (Intended For Online Publication Only)

# A.1 Supplemental Tables and Figures

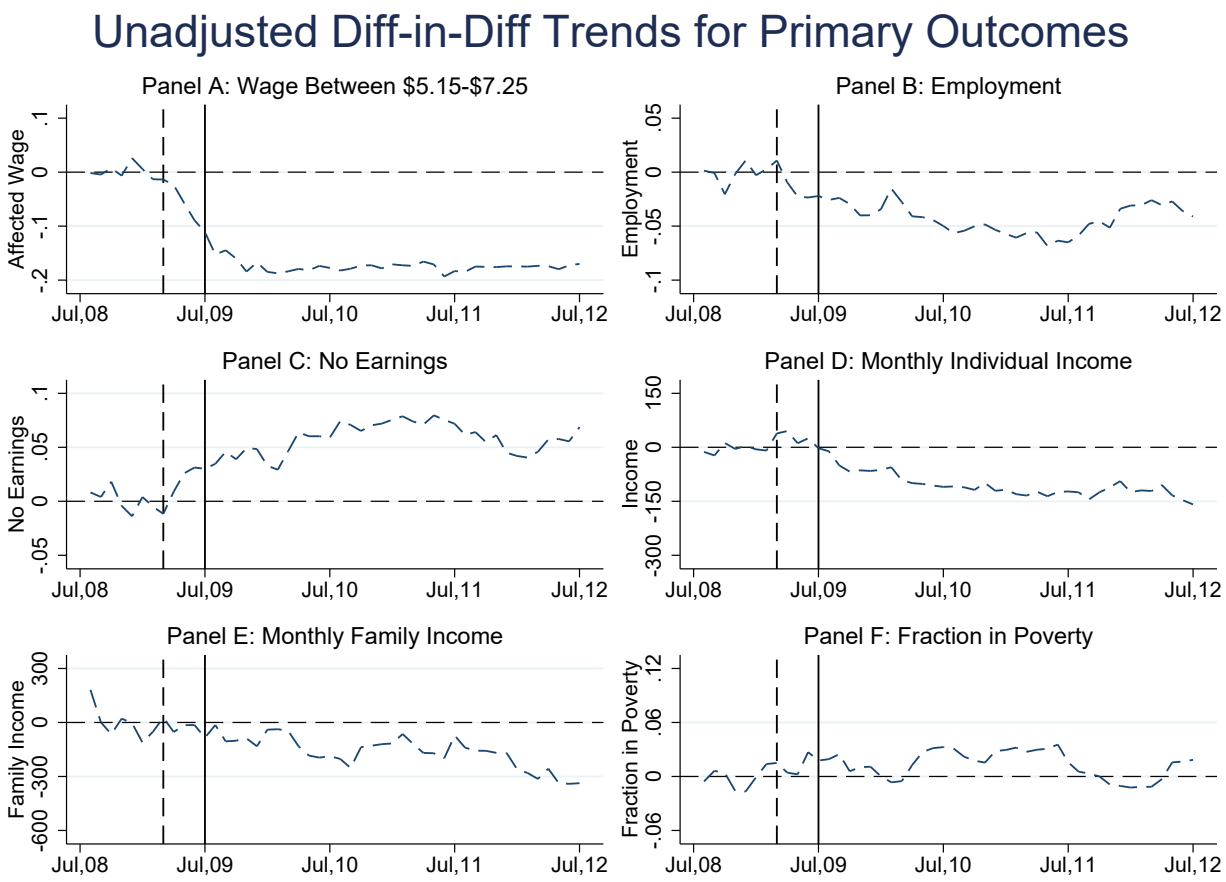



Figure A.1: **Unadjusted Diff-in-Diff Trends for All Outcomes:**
The figure plots trends in six wage, employment, and earnings related outcomes for groups of low-skilled workers. In all cases the series are constructed by the authors using data from the 2008 panel of the Survey of Income and Program Participation (SIPP). Each series can be described as plotting trends in "unadjusted differences-in-differences." This involves two steps. First, the series for bound and unbound states are centered relative to the baseline period. Second, the series for unbound states is subtracted from the series for bound states. Series names are provided in the title for each panel in the figure. In each panel, the solid vertical line indicates the timing of the July 2009 increase in the federal minimum wage. The dashed vertical line indicates the April 2009 beginning of the transition of wages out of the range between the old and new federal minimum; the date for the latter designation is driven by the data displayed in this figure's Panel A.

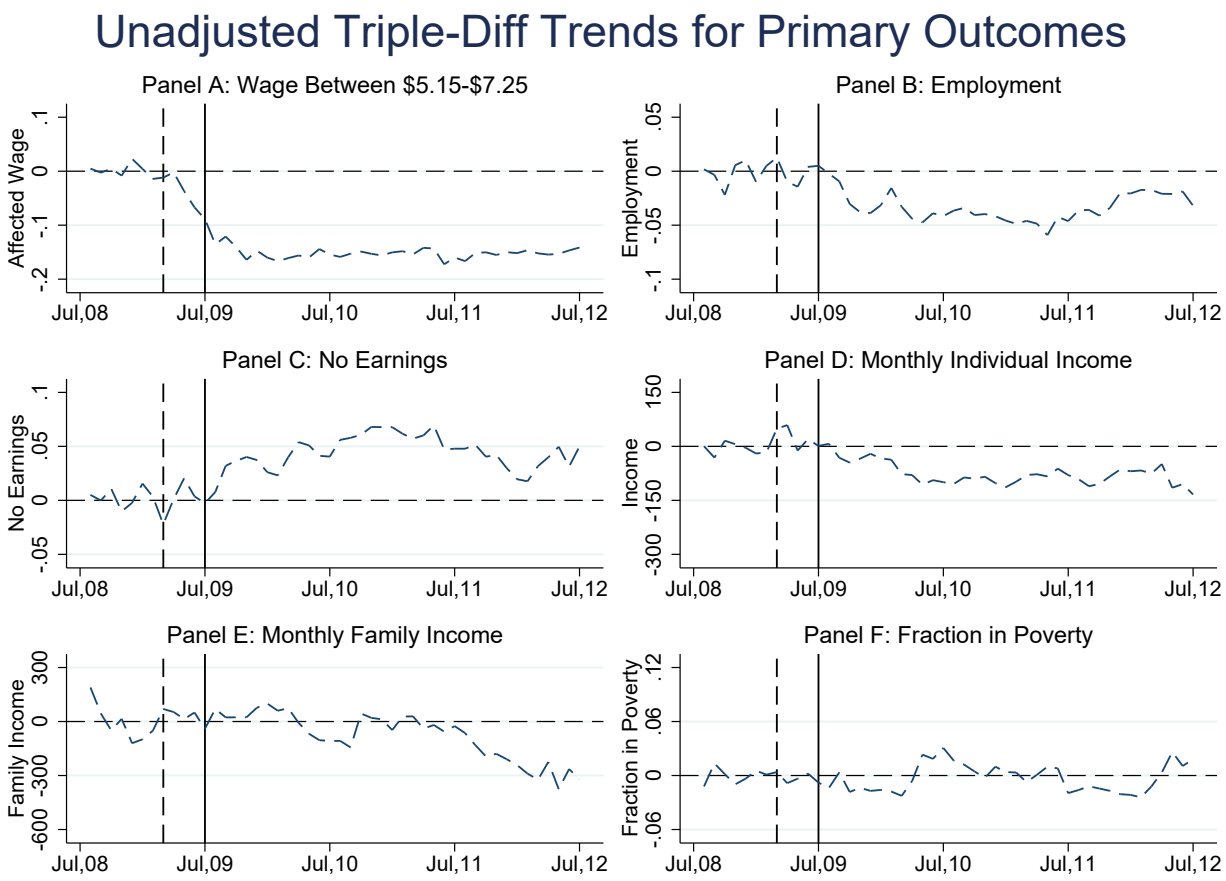



Figure A.2: **Unadjusted Triple-Diff Trends for All Outcomes:**
The figure plots trends in six wage, employment, and earnings related outcomes for groups of low-skilled workers. In all cases the series are constructed by the authors using data from the 2008 panel of the Survey of Income and Program Participation (SIPP). Each series can be described as plotting trends in "unadjusted triple-differences." This involves three steps. First, the series for bound and unbound states are centered relative to the baseline period. Second, the series for unbound states is subtracted from the series for bound states. Third, the series for individuals with average baseline wage rates between $7.50 and $10 are subtracted from the series for individuals with average baseline wages below $7.50 (the "target" group). Series names are provided in the title for each panel in the figure. In each panel, the solid vertical line indicates the timing of the July 2009 increase in the federal minimum wage. The dashed vertical line indicates the April 2009 beginning of the transition of wages out of the range between the old and new federal minimum; the date for the latter designation is driven by the data displayed in this figure's Panel A.

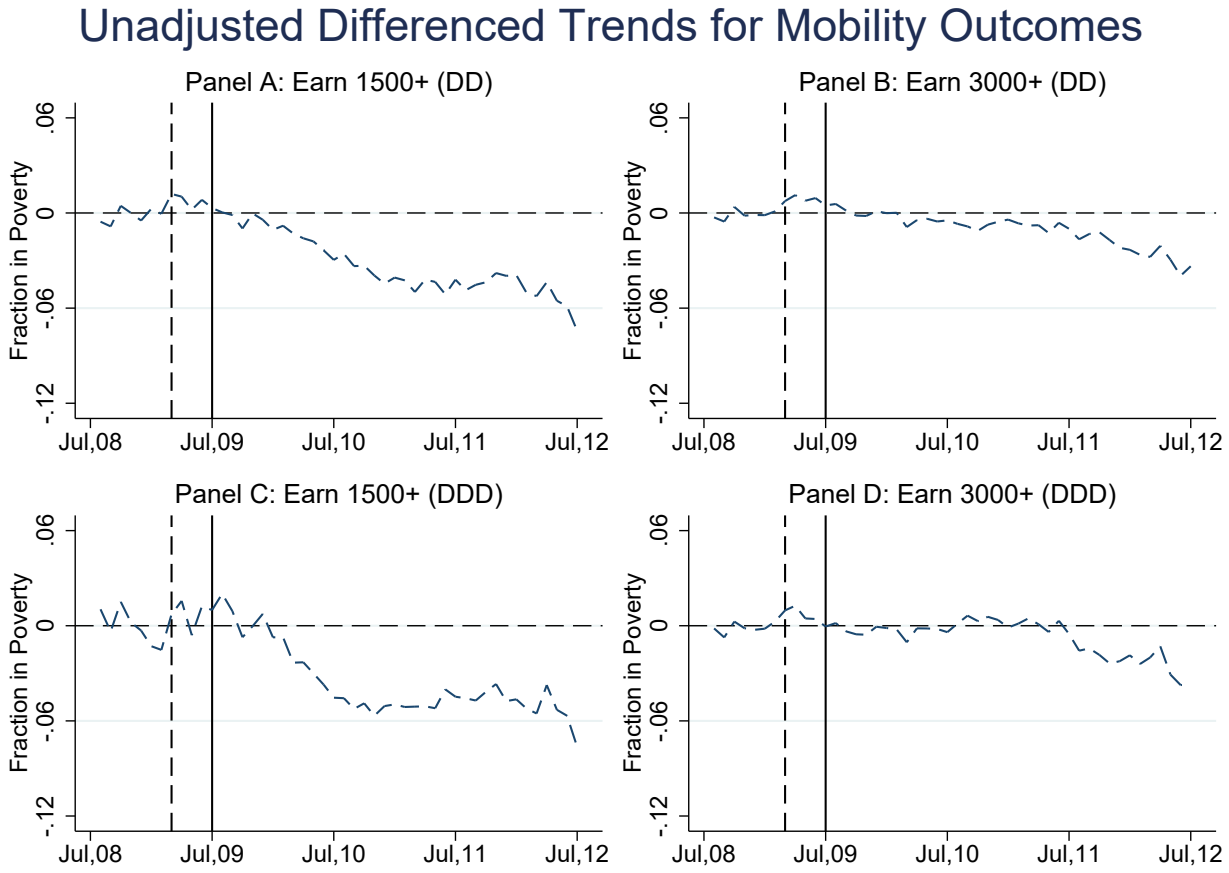



Figure A.3: **Unadjusted and Differenced Trends for Mobility Outcomes:**
The figure plots trends in our economic mobility outcomes for groups of low-skilled workers. In all cases the series are constructed by the authors using data from the 2008 panel of the Survey of Income and Program Participation (SIPP). The series in panels A and B can be described as plotting trends in "unadjusted differences-in-differences." This involves two steps. First, the series for bound and unbound states are centered relative to the baseline period. Second, the series for unbound states is subtracted from the series for bound states. The series in panels C and D can be described as plotting trends in "unadjusted triple-differences." This involves three steps. First, the series for bound and unbound states are centered relative to the baseline period. Second, the series for unbound states is subtracted from the series for bound states. Third, the series for individuals with average baseline wage rates between $7.50 and $10 are subtracted from the series for individuals with average baseline wages below $7.50 (the "target" group). Series names are provided in the title for each panel in the figure. In each panel, the solid vertical line indicates the timing of the July 2009 increase in the federal minimum wage. The dashed vertical line indicates the April 2009 beginning of the transition of wages out of the range between the old and new federal minimum; the date for the latter designation is driven by the data displayed in this figure's Panel A.

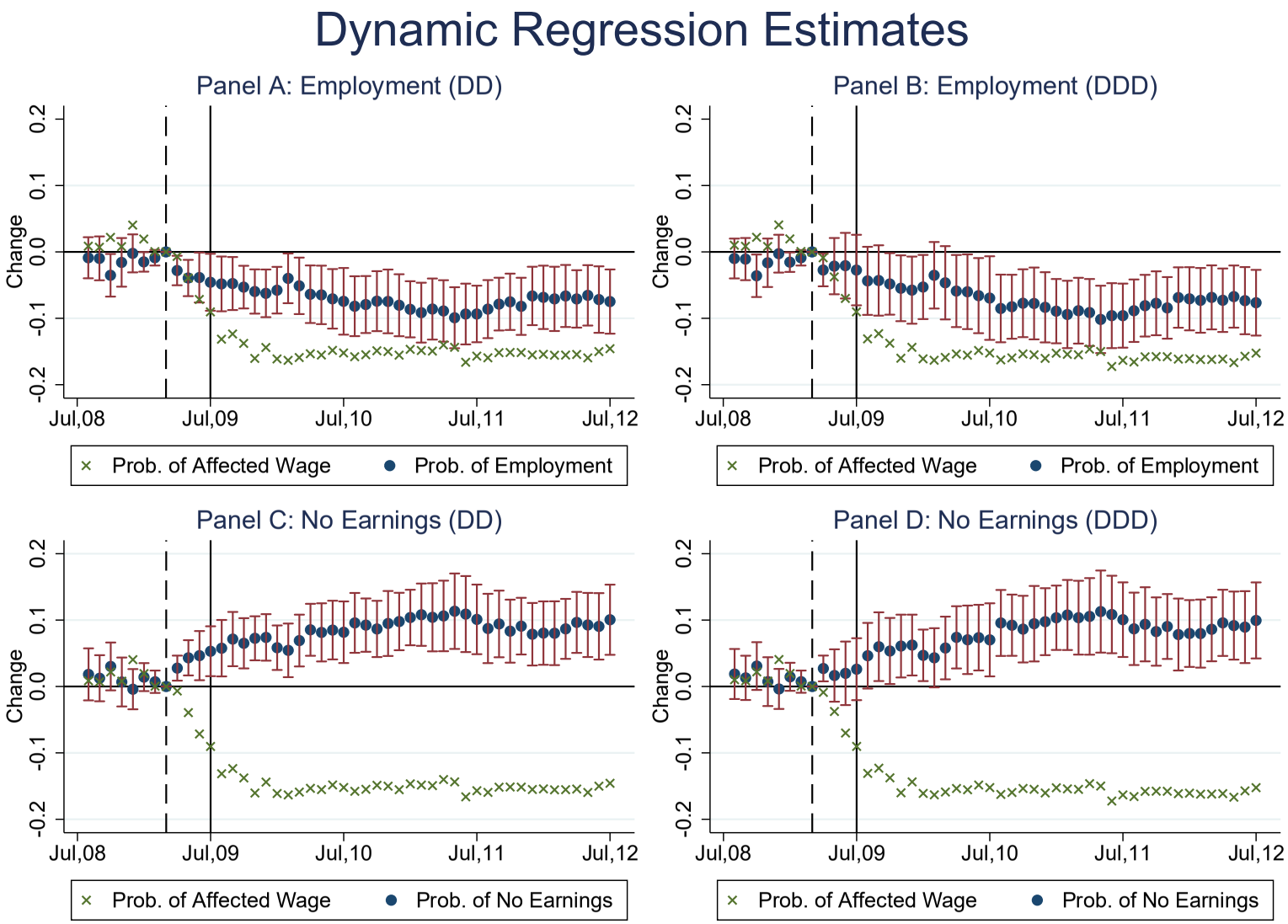



Figure A.4: **Dynamic Regression Estimates:** The figure reports fully dynamic estimates of the minimum wage's effects on the wages and employment of low-skilled workers. Each marker is an estimate of a coefficient of the form $\beta_{p(t)}$ from equations (1) and (2), where the relevant $p(t)$ correspond with individual months and period $p = 0$ is April 2009, the month immediately preceding the transition period. Panels A and C present estimates of the difference-in-differences model of equation (1), while panels B and D present estimates of the triple-difference model of equation (2). In each panel, the green X's are estimates of the effect of binding minimum wage changes on the probability of having a wage between \$5.15 and \$7.25. In panels A and B, the blue dots are estimates of the effect of binding minimum wage increases on the probability of employment (with accompanying 95 percent confidence intervals). In panels C and D, the blue dots are estimates of the effect of binding minimum wage increases on the probability of having no earnings (with accompanying 95 percent confidence intervals).

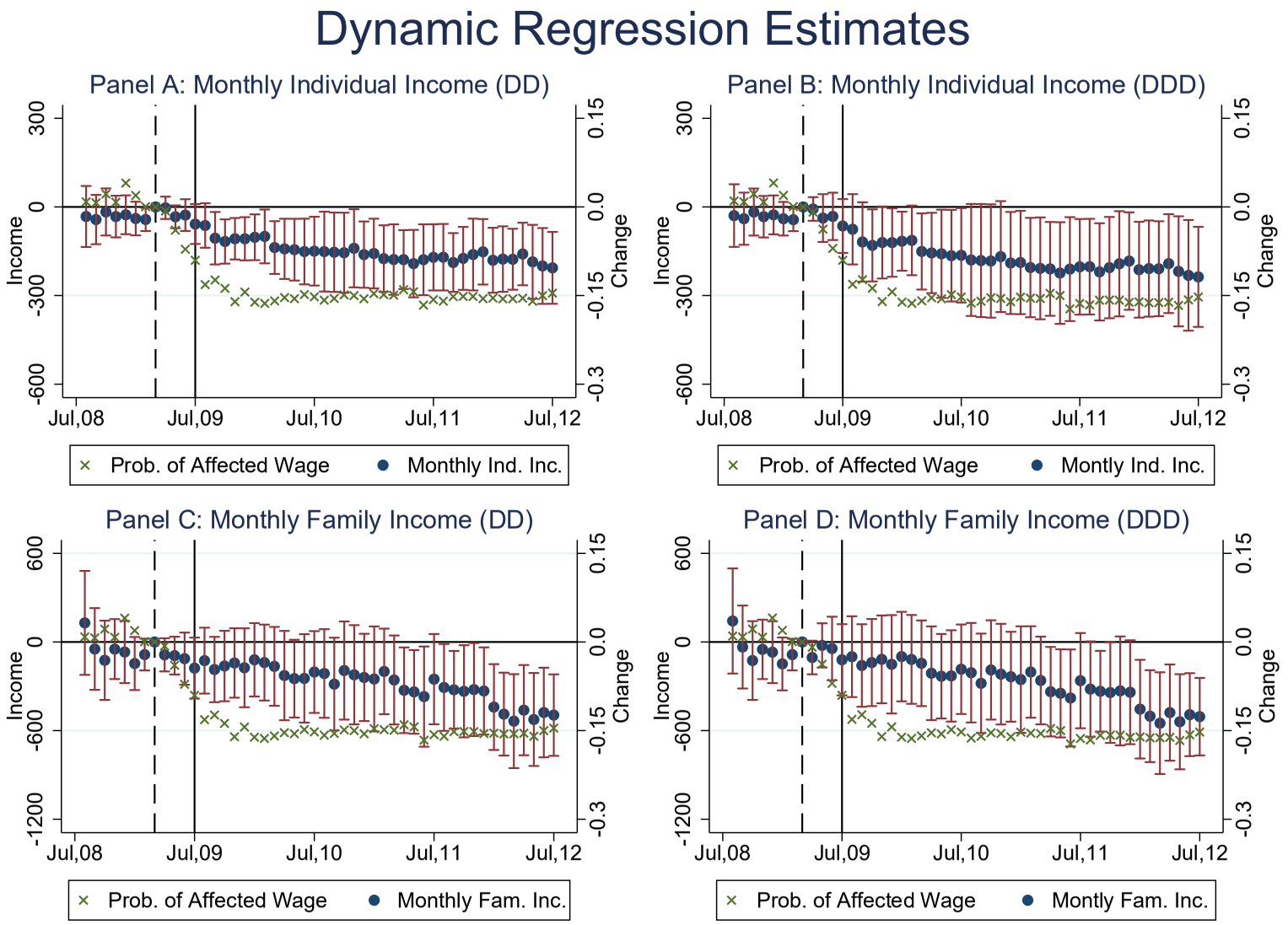



Figure A.5: **Dynamic Regression Estimates:** The figure reports fully dynamic estimates of the minimum wage's effects on the wages and income of low-skilled workers. Each marker is an estimate of a coefficient of the form $\beta_{p(t)}$ from equations (1) and (2), where the relevant $p(t)$ correspond with individual months and period $p = 0$ is April 2009, the month immediately preceding the transition period. Panels A and C present estimates of the difference-in-differences model of equation (1), while panels B and D present estimates of the triple-difference model of equation (2). In each panel, the green X's are estimates of the effect of binding minimum wage changes on the probability of having a wage between \$5.15 and \$7.25. In panels A and B, the blue dots are estimates of the effect of binding minimum wage increases on individual-level monthly income (with accompanying 95 percent confidence intervals). In panels C and D, the blue dots are estimates of the effect of binding minimum wage increases on family-level monthly income (with accompanying 95 percent confidence intervals).

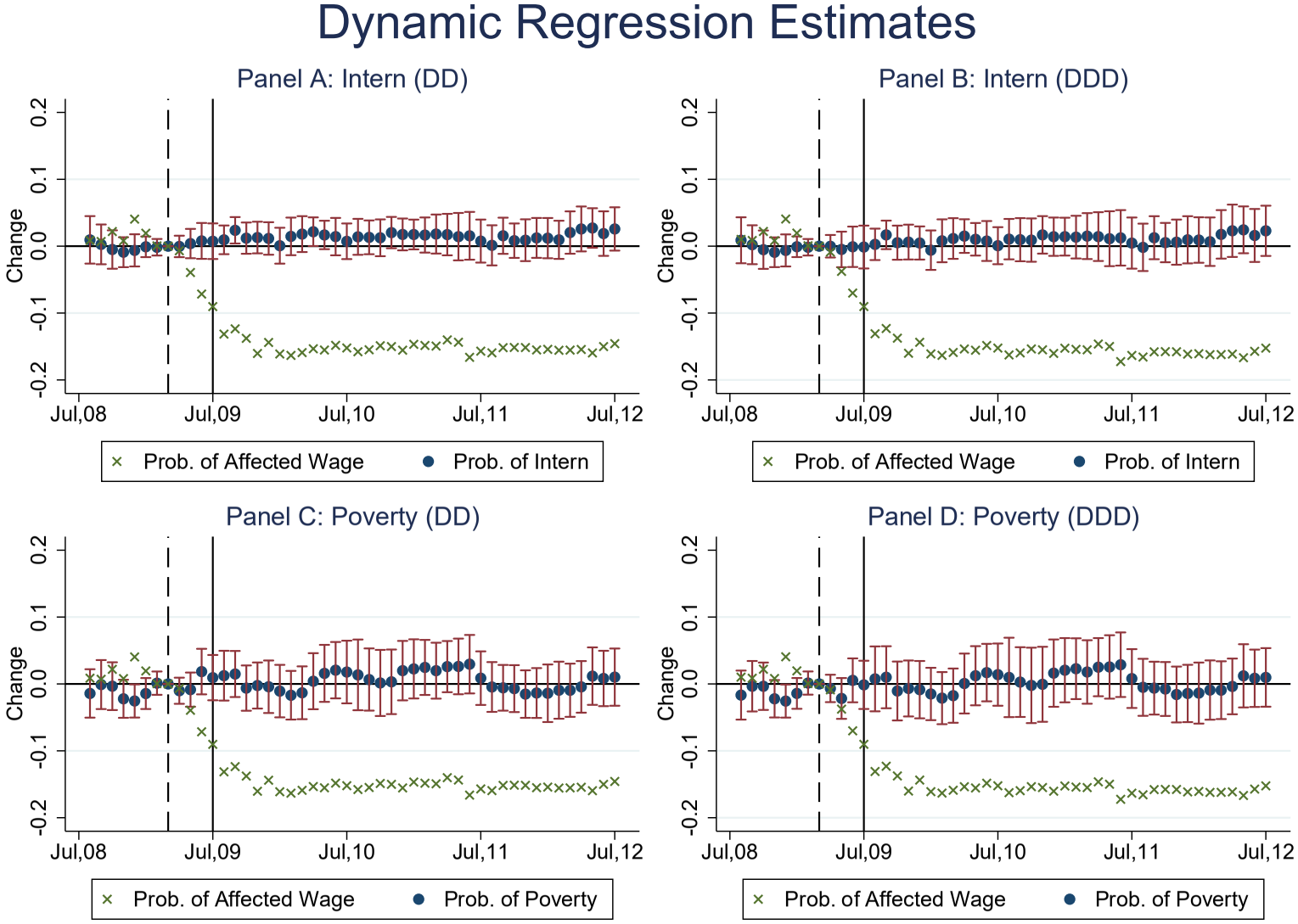



Figure A.6: **Dynamic Regression Estimates:** The figure reports fully dynamic estimates of the minimum wage's effects on the wages and income of low-skilled workers. Each marker is an estimate of a coefficient of the form $\beta_{p(t)}$ from equations (1) and (2), where the relevant $p(t)$ correspond with individual months and period $p = 0$ is April 2009, the month immediately preceding the transition period. Panels A and C present estimates of the difference-in-differences model of equation (1), while panels B and D present estimates of the triple-difference model of equation (2). In each panel, the green X's are estimates of the effect of binding minimum wage changes on the probability of having a wage between \$5.15 and \$7.25. In panels A and B, the blue dots are estimates of the effect of binding minimum wage increases on individual-level probabilities of working without pay (with accompanying 95 percent confidence intervals). In panels C and D, the blue dots are estimates of the effect of binding minimum wage increases on family-level probabilites of having income below the federal poverty line (with accompanying 95 percent confidence intervals).

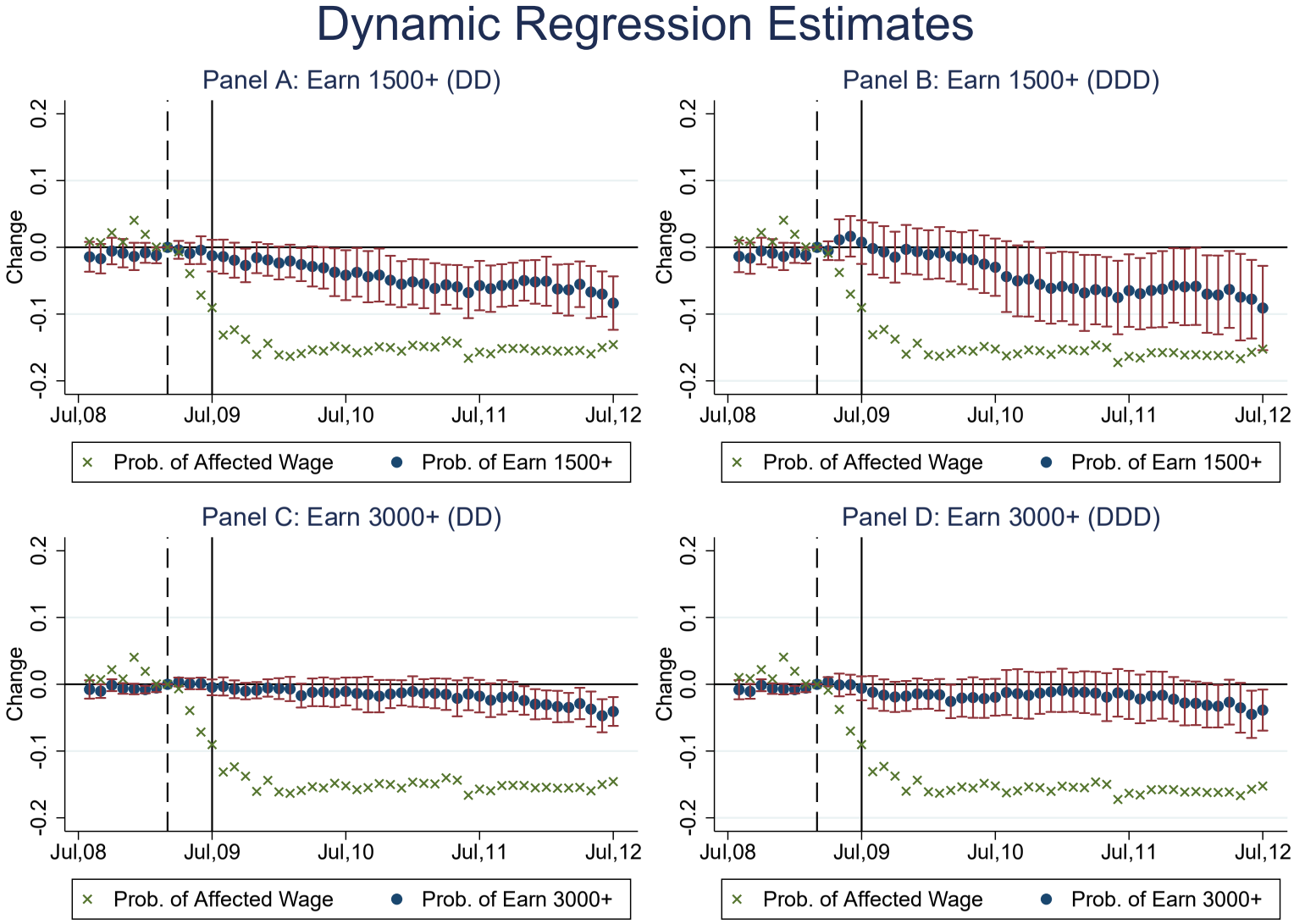



Figure A.7: **Dynamic Regression Estimates:** The figure reports fully dynamic estimates of the minimum wage's effects on the wages and income of low-skilled workers. Each marker is an estimate of a coefficient of the form $\beta_{p(t)}$ from equations (1) and (2), where the relevant $p(t)$ correspond with individual months and period $p = 0$ is April 2009, the month immediately preceding the transition period. Panels A and C present estimates of the difference-in-differences model of equation (1), while panels B and D present estimates of the triple-difference model of equation (2). In each panel, the green X's are estimates of the effect of binding minimum wage changes on the probability of having a wage between \$5.15 and \$7.25. In panels A and B, the blue dots are estimates of the effect of binding minimum wage increases on individual-level probabilities of having monthly earnings in excess of \$1,500 (with accompanying 95 percent confidence intervals). In panels C and D, the blue dots are estimates of the effect of binding minimum wage increases individual-level probabilities of having monthly earnings in excess of \$3,000 (with accompanying 95 percent confidence intervals).

Table A.1: **First Stage and Employment Effects for All Adults**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Panel A*: | | Dependent Variable: Affected Wage | | | |
| Bound x Post 1 | 0.001 | -0.160*** | -0.034** | -0.008* | -0.000 |
| | (0.001) | (0.021) | (0.010) | (0.004) | (0.001) |
| Bound x Post 2 | 0.001 | -0.163*** | -0.042*** | -0.005 | 0.000 |
| | (0.001) | (0.024) | (0.009) | (0.005) | (0.001) |
| *N* | 523,086 | 147,459 | 102,193 | 122,786 | 1,076,148 |
| Mean of Dep. Var. | 0.000 | 0.302 | 0.057 | 0.026 | 0.004 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| *Panel B*: | | Dependent Variable: Employment | | | |
| Bound x Post 1 | -0.009+ | -0.044* | 0.004 | -0.008 | 0.002 |
| | (0.005) | (0.019) | (0.021) | (0.012) | (0.004) |
| Bound x Post 2 | -0.022* | -0.066** | -0.026 | -0.002 | -0.003 |
| | (0.009) | (0.020) | (0.021) | (0.013) | (0.005) |
| *N* | 523,086 | 147,459 | 102,193 | 122,786 | 1,076,148 |
| Mean of Dep. Var. | 0.241 | 0.702 | 0.757 | 0.836 | 0.936 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| Sample | Unemp. at Base | Under $7.50 | $7.50-$8.50 | $8.50-$10.00 | Over $10.00 |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. In panel A the dependent variable in this panel is an indicator equal to 1 if an individual reports a wage between $5.15 and $7.25 in the relevant month. In panel B the dependent variable is an indicator equal to 1 if an individual is employed. In both panels, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (1), where the relevant $p(t)$ corresponds with the period beginning in July 2009 and extending through July 2010. The estimates in row 2 are of the coefficient $\beta_{p(t)}$ from equation (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. The samples used across columns 1 through 5 fully partition the set of all individuals aged 16 to 64 for whom the relevant earnings and employment data were available for at least 36 months between August 2008 and July 2012. Standard errors are clustered at the state level.

Table A.2: **Robustness of Estimated Effects on Employment**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dependent Variable | | | | Employed | | | |
| *Panel A*: | | | | Difference-in-Differences Specifications | | | |
| Bound x Post 1 | -0.044* | -0.033 | -0.058* | -0.043* | -0.043* | -0.053* | -0.043* |
| | (0.019) | (0.021) | (0.022) | (0.019) | (0.021) | (0.022) | (0.019) |
| Bound x Post 2 | -0.066** | -0.051* | -0.085** | -0.063** | -0.063** | -0.077** | -0.063** |
| | (0.020) | (0.019) | (0.029) | (0.019) | (0.019) | (0.026) | (0.021) |
| *N* | 147,459 | 147,459 | 147,459 | 147,459 | 121,763 | 124,698 | 144,499 |
| Mean of Dep. Var. | 0.702 | 0.702 | 0.702 | 0.702 | 0.706 | 0.699 | 0.702 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| *Panel B*: | | | | Triple Difference Specifications | | | |
| Bound x Post 1 x Target | -0.039 | -0.025 | -0.038 | -0.040 | -0.035 | -0.047 | -0.041 |
| | (0.026) | (0.028) | (0.026) | (0.026) | (0.026) | (0.029) | (0.026) |
| Bound x Post 2 x Target | -0.068** | -0.048* | -0.067** | -0.073** | -0.062* | -0.074** | -0.066** |
| | (0.022) | (0.023) | (0.022) | (0.022) | (0.024) | (0.024) | (0.022) |
| *N* | 270,245 | 270,245 | 270,245 | 270,245 | 223,669 | 231,943 | 264,499 |
| Mean of Dep. Var. | 0.763 | 0.763 | 0.763 | 0.763 | 0.767 | 0.763 | 0.763 |
| Estimation Framework | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D |
| Macro Covariates | Yes | No | Yes | Yes | Yes | Yes | Yes |
| State Trends | No | No | Yes | No | No | No | No |
| Trends In Demographics | No | No | No | Yes | No | No | No |
| Minimum Sample Inclusion | 3 yrs | 3 yrs | 3 yrs | 3 yrs | 3.5 yrs | 3 yrs | 3 yrs |
| Excluded States | None | None | None | None | None | N.B.<$7.00 | B.>$6.55 |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Panel A reports estimates of the minimum wage's short and medium run effects on an indicator for whether or not an individual is employed. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. Panel B reports analogous estimates of $\beta_{p(t)}$ from equation (2), namely our triple-difference specification. In Panel A the sample consists exclusively of individuals with average baseline wages less than $7.50. In Panel B the sample is augmented to include individuals whose average baseline wages are between $8.50 and $10.00 as a within-state control group. The columns explore our baseline results' (column 1) robustness to a variety of specification changes, which are further described in the main text and within the table itself. Standard errors are clustered at the state level.

Table A.3: **Robustness of Estimated Effects on Working Without Pay**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dependent Variable | | | | Unpaid Work | | | |
| *Panel A*: | | | | Difference-in-Differences Specifications | | | |
| Bound x Post 1 | 0.015+ | 0.015+ | 0.014 | 0.014 | 0.019* | 0.017 | 0.014 |
| | (0.009) | (0.009) | (0.011) | (0.008) | (0.008) | (0.010) | (0.009) |
| Bound x Post 2 | 0.017+ | 0.018+ | 0.012 | 0.012 | 0.020* | 0.014 | 0.015 |
| | (0.010) | (0.010) | (0.016) | (0.009) | (0.009) | (0.011) | (0.010) |
| *N* | 147,459 | 147,459 | 147,459 | 147,459 | 121,763 | 124,698 | 144,499 |
| Mean of Dep. Var. | 0.125 | 0.125 | 0.125 | 0.125 | 0.125 | 0.121 | 0.126 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| *Panel B*: | | | | Triple Difference Specifications | | | |
| Bound x Post 1 x Target | 0.008 | 0.009 | 0.008 | 0.007 | 0.008 | 0.014 | 0.007 |
| | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.012) | (0.011) |
| Bound x Post 2 x Target | 0.014 | 0.014 | 0.014 | 0.010 | 0.012 | 0.015 | 0.011 |
| | (0.012) | (0.013) | (0.012) | (0.012) | (0.013) | (0.015) | (0.012) |
| *N* | 270,245 | 270,245 | 270,245 | 270,245 | 249,365 | 254,704 | 267,459 |
| Mean of Dep. Var. | 0.089 | 0.089 | 0.089 | 0.089 | 0.093 | 0.092 | 0.090 |
| Estimation Framework | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D |
| Macro Covariates | Yes | No | Yes | Yes | Yes | Yes | Yes |
| State Trends | No | No | Yes | No | No | No | No |
| Trends In Demographics | No | No | No | Yes | No | No | No |
| Minimum Sample Inclusion | 3 yrs | 3 yrs | 3 yrs | 3 yrs | 3.5 yrs | 3 yrs | 3 yrs |
| Excluded States | None | None | None | None | None | N.B.<$7.00 | B.>$6.55 |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Panel A reports estimates of the minimum wage's short and medium run effects on the probability that an individual works without pay. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. Panel B reports analogous estimates of $\beta_{p(t)}$ from equation (2), namely our triple-difference specification. In Panel A the sample consists exclusively of individuals with average baseline wages less than $7.50. In Panel B the sample is augmented to include individuals whose average baseline wages are between $8.50 and $10.00 as a within-state control group. Standard errors are clustered at the state level.

Table A.4: **Robustness of Estimated Effects on Having No Earnings**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dependent Variable | No Earnings | | | | | | |
| *Panel A*: | Difference-in-Differences Specifications | | | | | | |
| Bound x Post 1 | 0.059** | 0.049* | 0.071*** | 0.056** | 0.062** | 0.070** | 0.057** |
| | (0.019) | (0.020) | (0.019) | (0.019) | (0.020) | (0.020) | (0.019) |
| Bound x Post 2 | 0.082*** | 0.068** | 0.098*** | 0.076*** | 0.083*** | 0.091** | 0.078*** |
| | (0.021) | (0.021) | (0.025) | (0.021) | (0.018) | (0.027) | (0.021) |
| *N* | 147,459 | 147,459 | 147,459 | 147,459 | 121,763 | 124,698 | 144,499 |
| Mean of Dep. Var. | 0.422 | 0.422 | 0.422 | 0.422 | 0.419 | 0.422 | 0.424 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| *Panel B*: | Triple Difference Specifications | | | | | | |
| Bound x Post 1 x Target | 0.047* | 0.034 | 0.047* | 0.047+ | 0.043+ | 0.060* | 0.049* |
| | (0.023) | (0.026) | (0.023) | (0.024) | (0.023) | (0.025) | (0.023) |
| Bound x Post 2 x Target | 0.082*** | 0.062* | 0.081*** | 0.083** | 0.079** | 0.090*** | 0.077** |
| | (0.022) | (0.024) | (0.022) | (0.024) | (0.023) | (0.025) | (0.023) |
| *N* | 270,245 | 270,245 | 270,245 | 270,245 | 249,365 | 254,704 | 267,459 |
| Mean of Dep. Var. | 0.326 | 0.326 | 0.326 | 0.326 | 0.334 | 0.333 | 0.327 |
| Estimation Framework | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D |
| Macro Covariates | Yes | No | Yes | Yes | Yes | Yes | Yes |
| State Trends | No | No | Yes | No | No | No | No |
| Trends In Demographics | No | No | No | Yes | No | No | No |
| Minimum Sample Inclusion | 3 yrs | 3 yrs | 3 yrs | 3 yrs | 3.5 yrs | 3 yrs | 3 yrs |
| Excluded States | None | None | None | None | None | N.B.<$7.00 | B.>$6.55 |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Panel A reports estimates of the minimum wage's short and medium run effects on the probability that an individual has no earnings. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. Panel B reports analogous estimates of $\beta_{p(t)}$ from equation (2), namely our triple-difference specification. In Panel A the sample consists exclusively of individuals with average baseline wages less than $7.50. In Panel B the sample is augmented to include individuals whose average baseline wages are between $8.50 and $10.00 as a within-state control group. Standard errors are clustered at the state level.

Table A.5: **Robustness of Estimated Effects on Average Income**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dependent Variable | Average Individual Income | | | | | | |
| *Panel A*: | Difference-in-Differences Specifications | | | | | | |
| Bound x Post 1 | -92.087* | -81.512* | -81.505 | -83.528* | -95.688* | -132.883** | -88.845* |
| | (36.474) | (36.007) | (51.270) | (37.526) | (37.780) | (42.963) | (37.056) |
| Bound x Post 2 | -144.042** | -128.897** | -103.013 | -116.174* | -149.823*** | -189.034** | -135.469** |
| | (44.748) | (42.904) | (81.985) | (45.446) | (36.653) | (54.550) | (45.570) |
| *N* | 147,459 | 147,459 | 147,459 | 147,459 | 121,763 | 124,698 | 144,499 |
| Mean of Dep. Var. | 748.459 | 748.459 | 748.459 | 748.459 | 755.041 | 753.408 | 744.122 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| *Panel B*: | Triple Difference Specifications | | | | | | |
| Bound x Post 1 x Target | -105.029+ | -82.630 | -105.980+ | -107.071+ | -111.376* | -123.873+ | -116.252* |
| | (55.231) | (56.567) | (55.513) | (56.414) | (54.396) | (64.723) | (55.462) |
| Bound x Post 2 x Target | -174.255* | -141.453+ | -175.021* | -175.867* | -193.630* | -211.826* | -176.081* |
| | (73.107) | (71.392) | (73.827) | (72.953) | (79.356) | (85.291) | (74.685) |
| *N* | 270,245 | 270,245 | 270,245 | 270,245 | 249,365 | 254,704 | 267,459 |
| Mean of Dep. Var. | 995.515 | 995.515 | 995.515 | 995.515 | 971.849 | 977.143 | 991.559 |
| Estimation Framework | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D |
| Macro Covariates | Yes | No | Yes | Yes | Yes | Yes | Yes |
| State Trends | No | No | Yes | No | No | No | No |
| Trends In Demographics | No | No | No | Yes | No | No | No |
| Minimum Sample Inclusion | 3 yrs | 3 yrs | 3 yrs | 3 yrs | 3.5 yrs | 3 yrs | 3 yrs |
| Excluded States | None | None | None | None | None | N.B.<$7.00 | B.>$6.55 |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Panel A reports estimates of the minimum wage's short and medium run effects on monthly income. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. Panel B reports analogous estimates of $\beta_{p(t)}$ from equation (2), namely our triple-difference specification. In Panel A the sample consists exclusively of individuals with average baseline wages less than $7.50. In Panel B the sample is augmented to include individuals whose average baseline wages are between $8.50 and $10.00 as a within-state control group. Standard errors are clustered at the state level.

Table A.6: **Robustness of Estimated Effects on Transitions into Lower Middle Class Earnings**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dependent Variable | Probability of Earning $1500+ | | | | | | |
| *Panel A*: | Difference-in-Differences Specifications | | | | | | |
| Bound x Post 1 | -0.016 | -0.013 | -0.007 | -0.014 | -0.011 | -0.022 | -0.017 |
| | (0.011) | (0.011) | (0.017) | (0.011) | (0.012) | (0.013) | (0.011) |
| Bound x Post 2 | -0.047*** | -0.043** | -0.021 | -0.041** | -0.051*** | -0.053** | -0.046*** |
| | (0.013) | (0.013) | (0.025) | (0.013) | (0.014) | (0.015) | (0.013) |
| *N* | 147,459 | 147,459 | 147,459 | 147,459 | 121,763 | 124,698 | 144,499 |
| Mean of Dep. Var. | 0.206 | 0.206 | 0.206 | 0.206 | 0.203 | 0.222 | 0.206 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| *Panel B*: | Triple Difference Specifications | | | | | | |
| Bound x Post 1 x Target | -0.003 | 0.001 | -0.003 | -0.007 | -0.024 | -0.004 | -0.009 |
| | (0.016) | (0.015) | (0.017) | (0.017) | (0.018) | (0.019) | (0.017) |
| Bound x Post 2 x Target | -0.054* | -0.048* | -0.054* | -0.061** | -0.073** | -0.048 | -0.059* |
| | (0.024) | (0.021) | (0.024) | (0.023) | (0.027) | (0.030) | (0.024) |
| *N* | 270,245 | 270,245 | 270,245 | 270,245 | 249,365 | 254,704 | 267,459 |
| Mean of Dep. Var. | 0.282 | 0.282 | 0.282 | 0.282 | 0.277 | 0.272 | 0.282 |
| Estimation Framework | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D |
| Macro Covariates | Yes | No | Yes | Yes | Yes | Yes | Yes |
| State Trends | No | No | Yes | No | No | No | No |
| Trends In Demographics | No | No | No | Yes | No | No | No |
| Minimum Sample Inclusion | 3 yrs | 3 yrs | 3 yrs | 3 yrs | 3.5 yrs | 3 yrs | 3 yrs |
| Excluded States | None | None | None | None | None | N.B.<$7.00 | B.>$6.55 |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Panel A reports estimates of the minimum wage's short and medium run effects on the probability that an individual has earnings greater than $1500 in a month. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. Panel B reports analogous estimates of $\beta_{p(t)}$ from equation (2), namely our triple-difference specification. In Panel A the sample consists exclusively of individuals with average baseline wages less than $7.50. In Panel B the sample is augmented to include individuals whose average baseline wages are between $8.50 and $10.00 as a within-state control group. Standard errors are clustered at the state level.

## B.2 Further Analysis of the Minimum Wage's Effect on Employment

This appendix presents further analysis of the minimum wage's effect on employment. We begin with a presentation of further analysis of our baseline result's robustness, with emphasis on the potential relevance of alternative strategies for controlling for heterogeneity in macroeconomic conditions. We next consider our baseline's robustness to alternative approaches to selecting our analysis sample using baseline wage data. Finally, we present estimates in which we replace our treatment group with groups selected using the demographic and industrial proxies used regularly in the literature. The latter analysis facilitates a comparison of our approach with alternative research designs.

### B.2.1 Further Checks on the Robustness of Our Baseline Estimates

Appendix Table B.1 provides additional evidence regarding the relevance of controls for differences in the severity of the Great Recession in bound and unbound states. Columns 1 and 2 replicate columns 1 and 2 from panel A of Table A.2. As an alternative to controlling for the housing price index, column 3 adds controls for state level income and employment per capita. Column 4 adds controls for stimulus spending per capita and two additional variables. The first, "Predicted State Income," is a projection of state-specific changes in aggregate output that are predictable on the basis of each state's historical relationship with the national business cycle. The second, "Predicted State Employment," is a projected change in employment based on each states' baseline industrial composition and subsequent industry-specific employment growth at the national level (Bartik, 1991).

The inclusion of alternative macroeconomic control variables increases the estimated effect of binding minimum wage increases relative to specifications that include no such controls. When these variables are included alongside the housing price index, the esti-

mates are essentially unchanged from the baseline. The housing price index consistently emerges as a stronger predictor of employment among low-skilled individuals than the alternative macroeconomic control variables. The specifications in columns 6 and 7 incorporate state-specific trends, the full sets of trends in various demographic characteristics, and trends specific to each individual's modal industry of employment at baseline. In both of these specifications, we estimate that binding minimum wage increases resulted in eight and a half percentage point declines in the employment of low-skilled workers.

Appendix Table B.2 presents estimates in which we restrict our SIPP analysis samples to states that could be matched on the basis of their housing declines. The specifications are the same set of specifications presented in table A.2's robustness analysis. The estimates in table B.2 and A.2 are quite similar, providing evidence that our analysis is robust to the adoption of a simple matching framework. Tables B.3 and B.4 show that estimates on the sample matched on house price declines are largely insensitive to whether the specification controls directly for the house price index.

### B.2.2 Alternative Approaches to Sample Construction

The analysis presented in this subsection considers 5 distinct definitions of the "target" group on which we estimate equation (1). The first is our baseline analysis sample. 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 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.[17]

#### B.2.2.1 Summary Statistics on the Alternative "Target" Samples

Table B.5 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.[18] Because differences between sub-minimum wage workers and near minimum

[17]In practice, 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.

[18]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.

#### B.2.2.2 Regression Analysis

This section presents several sets of regression estimates. The first set reports estimates of equation (1) on each of the 5 "target" group samples discussed above. The outcomes we consider include the "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. The second set reports estimates that, using our 5 approaches to dividing the population across skill groups, explore the relationship

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their employment trajectories.

between binding minimum wage increases and employment across the entirety of the working age population. The third set reports evidence on the sensitivity of the baseline estimates to changes in the panel balance criterion we apply.

We begin this section by presenting estimates of equation (1) on the 5 "target" group samples discussed above. Table B.6 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 intensely 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 intensely treated by the minimum wage change than is the baseline group.

Panel A of Table B.7 presents estimates of the effect of binding minimum wage increases on employment. Column 1 reports our baseline estimates. It reveals that, conditional on the magnitude of states' house price declines, employment declined 6.6 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 intensely 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 B.6, was the most intensely treated. Adjusted for the intensity of treatment observed in table B.6, the estimates are thus quite similar across approaches to assembling the target group.

Panel B of Table B.7 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 6.0 percentage points to 8.3 percentage points.

Tables B.8 and B.9 present estimates that involve two additional alterations to the sample's composition. First, the estimates in Table B.8 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.[19] The application of sample weights has little effect on our results.

Second, the estimates in table B.9 involve two changes to our sequence of data cleaning operations. The first change effectively alters the panel balance criterion. Specifically, 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 B.9, we effectively omitted this step in the data cleaning

[19]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.

process. The results in table B.9 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.

We now present 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 B.10, which replicates our baseline estimates. 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 3. 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 4 includes 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 B.11, B.12, B.13, and B.14 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 B.10 through B.14 provide evidence on two points of inter-

est. First, a potential criticism of our baseline analysis is that it is 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 B.10 through B.14 provide evidence that this is not the case. Binding minimum wage increases were associated with declines in employment entry among individuals who were not employed at baseline. Combining this sample with each table's "target" sample generates the results one would tend to expect based on the results we see for each sample 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 employment 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.

Table B.15 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 B.15 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 B.1 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 B.1 correspond with the differential evolution of employment (panel A) or the "no earnings" outcome (panel B). The green 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.[20]

The estimates in figure B.1 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 earn-

[20]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.

ings" 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.

### B.2.3 Contrasting Approaches To Evaluating the Minimum Wage

In further analysis, we estimate the minimum wage's effects on the employment of populations studied frequently in the literature, namely teenagers and food service workers. More specifically, we estimate equation (1) on a sample selected to include individuals who were teenagers or for whom food service was the modal industry of employment during the baseline period. Appendix Figure B.2 and Table B.16 characterize the bite of binding minimum wage increases on the wage distributions of groups of workers that are commonly analyzed in the literature. Figure B.2's Panels A and B display the wage distributions of teenagers and food service workers. As summarized in Table B.16, the minimum wage's bite on these groups' wage distributions is just over half the size of its bite on the distribution for workers with average baseline wages below $7.50. Relative to our analysis of workers with average baseline wages in the affected range, analyses of these groups will thus have an attenuated ability to detect any effects of minimum wage increases on employment.

The histograms in Figures 3 and B.2 display our approach's suitability for identifying both targeted workers and workers who were low-skilled but unaffected, making them attractive as within-state controls. As desired, the baseline wage distribution for workers with average baseline wages less than $7.50 has significant mass between $6.50 and $7.50. Our within-state control group has a baseline wage distribution tightly clustered between $8.00 and $10.00. As illustrated in Figure B.2's panel C, comparison samples drawn based on manufacturing industries will tend to contain many much higher

skilled, and thus less directly comparable, individuals. Figure B.2's panels A and B show that analysis samples of teenagers and food service workers similarly have baseline wage distributions more diffuse than that of our target sample.

Column 5 of Appendix Table B.16 reports our estimate that binding minimum wage increases reduced teenager and food service workers' medium-run employment by 4 percentage points. Column 6 reports an estimate near 0 for the minimum wage increase's effect on the employment of manufacturing workers, whose wage distribution was unaffected. Our specification thus passes the primary falsification test emphasized in Dube, Lester, and Reich (2010). Tables B.17 and B.18 present similar analyses of the probability of working without pay and having no earnings.

We estimate that the wage distribution of our target sample was nearly twice as affected as the wage distribution of teenagers and food service workers. Our estimates of the minimum wage increase's effects on these groups' employment were similarly proportioned. It is thus important to note that, all else equal, estimates of a minimum wage increase's effects on relatively untargeted groups will be attenuated and, as a result, more prone to type II error.

Appendix Table B.19 provides a further line of comparison between our results and the findings of industry-specific analyses of the minimum wage. In our baseline analysis and our analysis of teenagers and food service workers, we estimate the minimum wage's effects on the employment of low-skilled *individuals*. By contrast, analyses of industry-level data estimate the minimum wage's effects on total employment in low-skill-intensive *industries*. In Table B.19 we present estimates of the minimum wage's effect on the probability that any given individual is employed in the food service sector. For the full sample of individuals aged 16 to 64, the estimated effect on food service employment is economically negligible and statistically indistinguishable from 0. As revealed in column 2, this masks a 3 percentage point decline in food service employment

among individuals with average baseline wages below $7.50. Column 3 reports an offsetting increase in the food service employment of workers with higher baseline wage rates.[21]

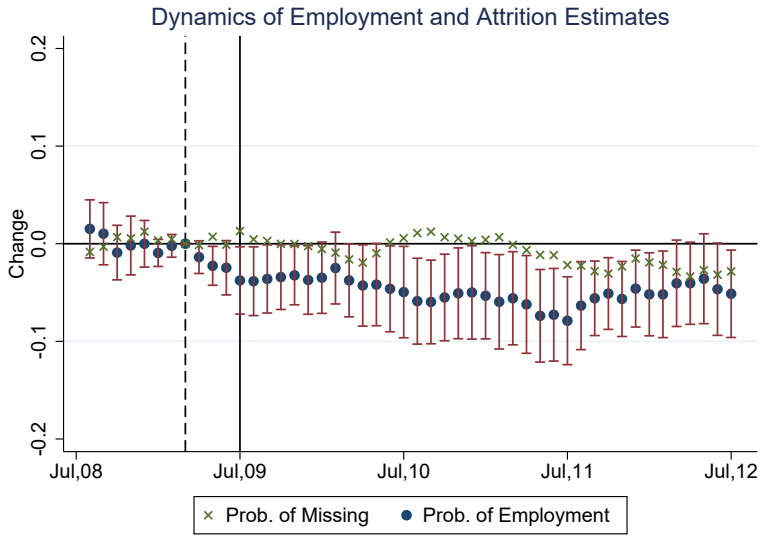
We draw two additional lessons from this analysis. First, we note that the minimum wage's effects may vary significantly across industries, making it difficult to extrapolate from industry-specific estimates to aggregate employment. In a standard model, the determinants of an industry's adaptation to a minimum wage change include its ability to substitute between low-skilled workers, high-skilled workers, and capital, as well as the elasticity of demand for its output. The results in Table B.19 suggest that, during the period we study, food-service employers had significant scope for substituting between low- and high-skilled workers.

Second, the results in Table B.19 highlight that substitution between low- and high-skilled workers can complicate efforts to evaluate the minimum wage's effects using data on industry-level wage bills and employment. In such data, substitution between low and high-skilled workers would be indistinguishable from an outcome in which an increase in the minimum wage non-trivially increased per-worker earnings and had minimal effects on employment. In the setting we analyze, this mistaken interpretation would leave the impression that the minimum wage had achieved its objective of increasing low-skilled workers' incomes at little cost.

[21] Because the sample in column 3 is roughly 10 times the size of the sample in column 2, the -0.03 employment effect from column 2 is essentially fully offset by the estimate of 0.003 from column 3.

## Evidence on Sample Attrition

*Panel A*




*Panel B*

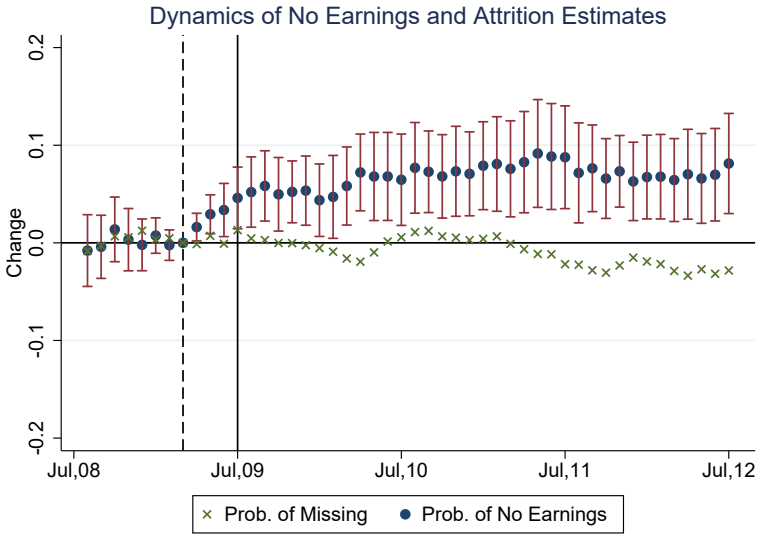



Figure B.1: **Evidence on Sample Attrition:**
Note: The figure reports fully dynamic estimates of the minimum wage's effects on employment alongside estimates of differential sample attrition. The green X's correspond with the attrition estimates of the probability that a person-month observation was 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).

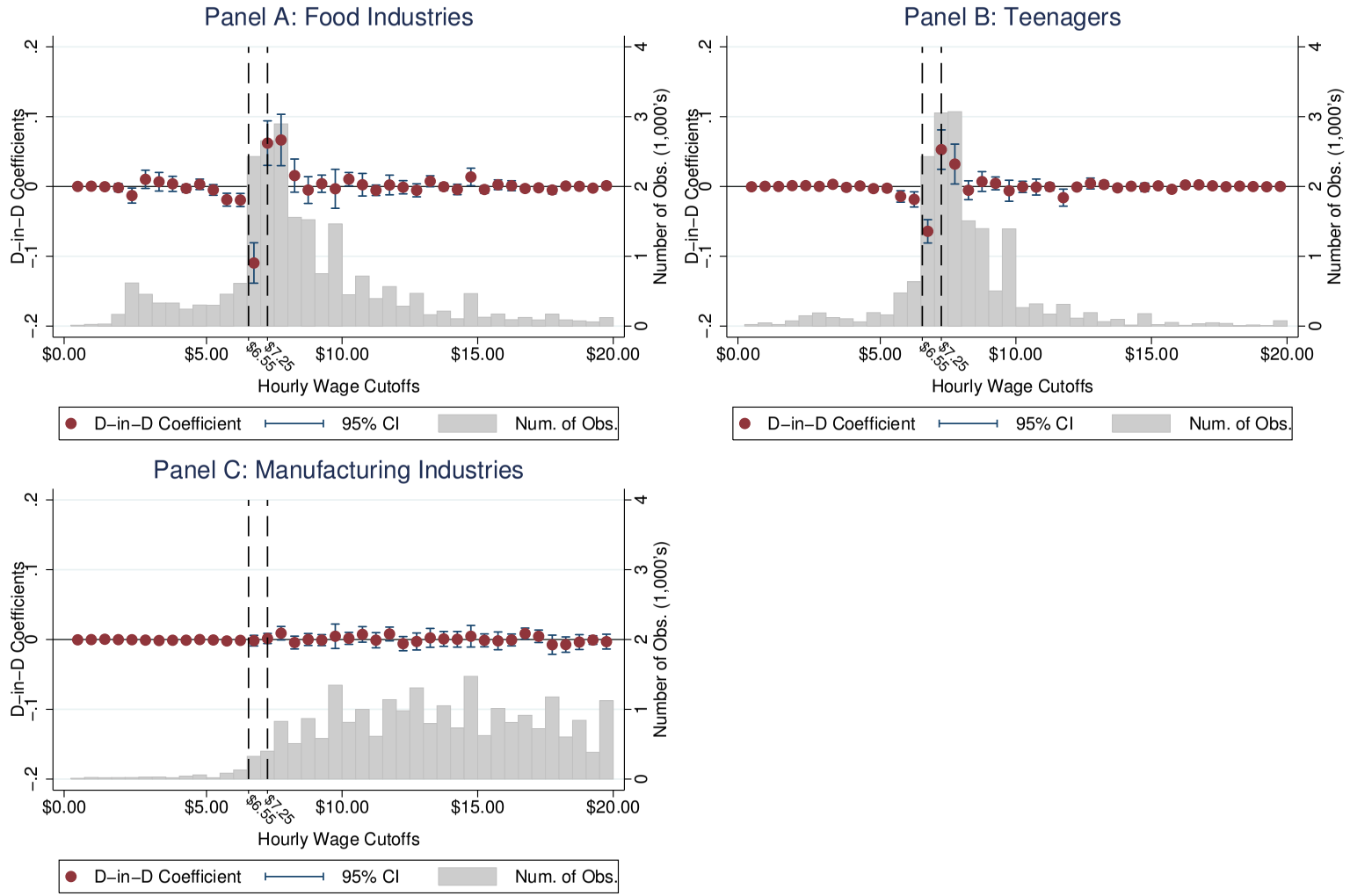



Figure B.2: **Estimated Effects of the Minimum Wage on Hourly Wage Distributions:** The figure reports estimates of binding minimum wage increase's medium run effects on the wage distributions of three groups of low-skilled earners. More specifically, each dot is an estimate of the coefficient $\beta_p(t)$ from equation (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. The dependent variables in each specification take the form $Y^j_{i,s,t} = 1\{W^{j-1} < \text{Hourly Wage}_{i,s,t} < W^j\}$. These $Y_{i,s,t}$ are indicators equal to 1 if an individual's hourly wage is in the band between $W^{j-1}$ and $W^j$, where each band is a 50 cent interval. The results can thus be described as estimates of the minimum wage's effect on the wage distribution's probability mass function. In Panel A, the sample consists of individuals whose modal industry of employment over the baseline was food service. In Panel B, the sample consists of individuals who were teenagers at the beginning of the sample period. In Panel C, the sample consists of individuals whose modal industry of employment over the baseline was manufacturing. In the background of each panel is a histogram characterizing the frequency distribution of hourly wages during the sample's baseline period.

Table B.1: **Further Robustness of the Estimated Employment Effects**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dependent Variable | | | | Employed | | | |
| Bound x Post 1 | -0.044* | -0.033 | -0.034 | -0.041+ | -0.049* | -0.059** | -0.059* |
| | (0.019) | (0.021) | (0.021) | (0.024) | (0.021) | (0.022) | (0.023) |
| Bound x Post 2 | -0.066** | -0.051* | -0.053** | -0.059* | -0.068** | -0.086** | -0.085** |
| | (0.020) | (0.019) | (0.020) | (0.022) | (0.021) | (0.028) | (0.029) |
| Housing Price Index | 0.755* | | | | 0.652+ | 1.749* | 1.785** |
| | (0.323) | | | | (0.355) | (0.724) | (0.636) |
| State Employment Rate | | | 0.489 | 0.401 | 0.121 | | 0.700 |
| | | | (0.706) | (0.739) | (0.757) | | (0.859) |
| State Inc. Per Cap. (1000s) | | | 0.004 | 0.004 | 0.002 | | 0.005 |
| | | | (0.004) | (0.004) | (0.003) | | (0.004) |
| Stimulus Per Cap. (1000s) | | | | -0.033 | -0.032 | | 0.010 |
| | | | | (0.037) | (0.037) | | (0.034) |
| Predicted State Income | | | | 0.499 | 0.228 | | -0.774 |
| | | | | (0.529) | (0.536) | | (1.426) |
| Predicted State Employment | | | | -0.597 | -0.759 | | -0.709 |
| | | | | (0.707) | (0.680) | | (0.770) |
| *N* | 147,459 | 147,459 | 147,459 | 147,459 | 147,459 | 146,256 | 146,256 |
| Mean of Dep. Var. | 0.702 | 0.702 | 0.702 | 0.702 | 0.702 | 0.702 | 0.702 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| State Trends | No | No | No | No | No | Yes | Yes |
| Trends In Demographics | No | No | No | No | No | Yes | Yes |
| Trends In Baseline Ind. | No | No | No | No | No | Yes | Yes |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Each column reports estimates of the minimum wage's short and medium run effects on employment. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. The stimulus spending variable was taken from Chodorow-Reich, Feiveson, Liscow, and Woolston (2012). "Predict State Income" is a projection of state-specific changes in aggregate output that are predictable on the basis of each state's historical relationship with the national business cycle. "Predicted State Employment" is a projected change in employment based on each states' baseline industrial composition and subsequent industry-specific employment growth at the national level (Bartik, 1991). Standard errors are clustered at the state level.

Table B.2: **Further Robustness of the Estimated Employment Effects: Matched Sample**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dependent Variable | | | | Employed | | | |
| *Panel A*: | | | | Difference-in-Differences Specifications | | | |
| Bound x Post 1 | -0.064** | -0.061** | -0.068** | -0.065*** | -0.067** | -0.054* | -0.065*** |
| | (0.018) | (0.018) | (0.023) | (0.017) | (0.021) | (0.020) | (0.018) |
| Bound x Post 2 | -0.073** | -0.069** | -0.069+ | -0.072*** | -0.080*** | -0.068* | -0.075** |
| | (0.021) | (0.020) | (0.035) | (0.020) | (0.020) | (0.026) | (0.021) |
| *N* | 98,932 | 98,932 | 98,932 | 98,932 | 81,425 | 90,925 | 98,664 |
| Mean of Dep. Var. | 0.705 | 0.705 | 0.705 | 0.705 | 0.708 | 0.707 | 0.705 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| *Panel B*: | | | | Triple Difference Specifications | | | |
| Bound x Post 1 x Target | -0.070** | -0.066* | -0.069* | -0.072** | -0.070* | -0.054+ | -0.070** |
| | (0.025) | (0.025) | (0.025) | (0.026) | (0.028) | (0.027) | (0.025) |
| Bound x Post 2 x Target | -0.082** | -0.078** | -0.081** | -0.089** | -0.087** | -0.070* | -0.084** |
| | (0.024) | (0.023) | (0.024) | (0.025) | (0.028) | (0.028) | (0.024) |
| *N* | 170,020 | 170,020 | 170,020 | 170,020 | 140,833 | 157,766 | 169,400 |
| Mean of Dep. Var. | 0.764 | 0.764 | 0.764 | 0.764 | 0.770 | 0.767 | 0.764 |
| Estimation Framework | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D | D-in-D-in-D |
| Macro Covariates | Yes | No | Yes | Yes | Yes | Yes | Yes |
| State Trends | No | No | Yes | No | No | No | No |
| Trends In Demographics | No | No | No | Yes | No | No | No |
| Minimum Sample Inclusion | 3 yrs | 3 yrs | 3 yrs | 3 yrs | 3.5 yrs | 3 yrs | 3 yrs |
| Excluded States | None | None | None | None | None | N.B.<$7.00 | B.>$6.55 |

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Each column reports estimates of the minimum wage's short and medium run effects on employment. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. The stimulus spending variable was taken from Chodorow-Reich, Feiveson, Liscow, and Woolston (2012). "Predict State Income" is a projection of state-specific changes in aggregate output that are predictable on the basis of each state's historical relationship with the national business cycle. "Predicted State Employment" is a projected change in employment based on each states' baseline industrial composition and subsequent industry-specific employment growth at the national level (Bartik, 1991). All samples are restricted to individuals in states with housing declines that could be matched to within $20,000. Standard errors are clustered at the state level.

Table B.3: **Effects on Wage Distributions and Employment by Average Baseline Wages**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent Variable | Wage between $5.15 and $7.25 | | | Employed | | |
| Bound x Post 1 | -0.148*** | -0.037* | -0.002 | -0.061** | -0.006 | 0.008 |
| | (0.028) | (0.014) | (0.005) | (0.018) | (0.022) | (0.017) |
| Bound x Post 2 | -0.143*** | -0.048*** | 0.000 | -0.069** | -0.048* | 0.008 |
| | (0.028) | (0.011) | (0.006) | (0.020) | (0.020) | (0.014) |
| Housing Price Index | | | | | | |
| $N$ | 98,932 | 64,848 | 71,088 | 98,932 | 64,848 | 71,088 |
| Mean of Dep. Var. | 0.315 | 0.065 | 0.031 | 0.705 | 0.771 | 0.848 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| Weighted | No | No | No | No | No | No |
| Individual Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | Under $7.50 | $7.50-$8.49 | $8.50-$9.99 | Under $7.50 | $7.50-$8.49 | $8.50-$9.99 |

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 (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. In columns 1-3, the dependent variable is an indicator for whether an individual's hourly wage is between $5.15 and $7.25. In columns 4-6, the dependent variable is an indicator for whether an individual is employed. In Columns 1 and 4, the sample consists of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In Columns 2 and 5, the sample consists of individuals whose average baseline wages are between $7.50 and $8.50. In Columns 3 and 6, the sample consists of individuals whose average baseline wages are between $8.50 and $10.00. All samples are restricted to individuals in states with housing declines that could be matched to within $20,000. Standard errors are clustered at the state level.

Table B.4: **Effects on Wage Distributions and Employment by Average Baseline Wages**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent Variable | Wage between $5.15 and $7.25 | | | Employed | | |
| Bound x Post 1 | -0.143*** | -0.035* | -0.004 | -0.064** | -0.005 | 0.009 |
| | (0.025) | (0.014) | (0.005) | (0.018) | (0.023) | (0.017) |
| Bound x Post 2 | -0.137*** | -0.045*** | -0.002 | -0.073** | -0.047* | 0.009 |
| | (0.025) | (0.010) | (0.006) | (0.021) | (0.022) | (0.014) |
| Housing Price Index | -0.861 | -0.364+ | 0.303+ | 0.511 | -0.080 | -0.136 |
| | (0.523) | (0.190) | (0.166) | (0.386) | (0.522) | (0.404) |
| $N$ | 98,932 | 64,848 | 71,088 | 98,932 | 64,848 | 71,088 |
| Mean of Dep. Var. | 0.315 | 0.065 | 0.031 | 0.705 | 0.771 | 0.848 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| Weighted | No | No | No | No | No | No |
| Individual Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | Under $7.50 | $7.50-$8.49 | $8.50-$9.99 | Under $7.50 | $7.50-$8.49 | $8.50-$9.99 |

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 (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. In columns 1-3, the dependent variable is an indicator for whether an individual's hourly wage is between $5.15 and $7.25. In columns 4-6, the dependent variable is an indicator for whether an individual is employed. In Columns 1 and 4, the sample consists of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In Columns 2 and 5, the sample consists of individuals whose average baseline wages are between $7.50 and $8.50. In Columns 3 and 6, the sample consists of individuals whose average baseline wages are between $8.50 and $10.00. All samples are restricted to individuals in states with housing declines that could be matched to within $20,000. Standard errors are clustered at the state level.

Table B.5: **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 B.6: **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 B.5. 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 B.7: **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 B.5. 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 B.8: **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 B.5. 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 B.9: **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 B.5. 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 B.10: **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 B.5. Standard errors allow for correlation clusters across errors at the state level.

Table B.11: **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 B.5. Standard errors allow for correlation clusters across errors at the state level.

Table B.12: **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 B.5. Standard errors allow for correlation clusters across errors at the state level.

Table B.13: **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 B.5. Standard errors allow for correlation clusters across errors at the state level.

Table B.14: **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 B.5. Standard errors allow for correlation clusters across errors at the state level.

Table B.15: **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.

Table B.16: **Cross-Sample Comparison of Effects on Wages and Employment**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent Variable | Wage between $5.15 and $7.25 | | | Employed | | |
| Bound x Post 1 | -0.160*** | -0.082*** | -0.005 | -0.044* | -0.024+ | 0.004 |
| | (0.021) | (0.014) | (0.004) | (0.019) | (0.013) | (0.009) |
| Bound x Post 2 | -0.163*** | -0.089*** | -0.007 | -0.066** | -0.044*** | 0.005 |
| | (0.024) | (0.016) | (0.005) | (0.020) | (0.012) | (0.011) |
| $N$ | 147,459 | 275,130 | 166,662 | 147,459 | 275,130 | 166,662 |
| Mean of Dep. Var. | 0.302 | 0.103 | 0.014 | 0.702 | 0.514 | 0.917 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| Sample | Under $7.50 | Food Service and Teens | Manufac-turing | Under $7.50 | Food Service and Teens | Manufac-turing |

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 (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. In columns 1-3, the dependent variable is an indicator for whether an individual's hourly wage is between $5.15 and $7.25. In columns 4-6, the dependent variable is an indicator for whether an individual was employed. In Columns 1 and 4, the sample consists of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In Columns 2 and 5, the sample consists of individuals who were teenagers or whose modal industry was food service when employed at baseline. In Columns 3 and 6, the sample consists of individuals whose modal industry was manufacturing when employed at baseline. Standard errors are clustered at the state level.

Table B.17: **Cross-Sample Comparison of Effects on Wages and the Probability of Working Without Pay**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent Variable | Wage between $5.15 and $7.25 | | | Unpaid Work | | |
| Bound x Post 1 | -0.160*** | -0.082*** | -0.005 | 0.015+ | 0.002 | 0.002 |
| | (0.021) | (0.014) | (0.004) | (0.009) | (0.004) | (0.005) |
| Bound x Post 2 | -0.163*** | -0.089*** | -0.007 | 0.017+ | 0.002 | -0.004 |
| | (0.024) | (0.016) | (0.005) | (0.010) | (0.004) | (0.005) |
| $N$ | 147,459 | 275,130 | 166,662 | 147,459 | 275,130 | 166,662 |
| Mean of Dep. Var. | 0.302 | 0.103 | 0.014 | 0.125 | 0.039 | 0.025 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| Sample | Under $7.50 | Food Service and Teens | Manufac-turing | Under $7.50 | Food Service and Teens | Manufac-turing |

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 (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. In columns 1-3, the dependent variable is an indicator for whether an individual's hourly wage is between $5.15 and $7.25. In columns 4-6, the dependent variable is an indicator for whether an individual worked with no pay. In Columns 1 and 4, the sample consists of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In Columns 2 and 5, the sample consists of individuals who were teenagers or whose modal industry was food service when employed at baseline. In Columns 3 and 6, the sample consists of individuals whose modal industry was manufacturing when employed at baseline. Standard errors are clustered at the state level.

Table B.18: **Cross-Sample Comparison of Effects on Wages and the Probability of Having Positive Earnings**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent Variable | Wage between $5.15 and $7.25 | | | No Earnings | | |
| Bound x Post 1 | -0.160*** | -0.082*** | -0.005 | 0.059** | 0.026* | -0.001 |
| | (0.021) | (0.014) | (0.004) | (0.019) | (0.012) | (0.010) |
| Bound x Post 2 | -0.163*** | -0.089*** | -0.007 | 0.082*** | 0.046*** | -0.010 |
| | (0.024) | (0.016) | (0.005) | (0.021) | (0.012) | (0.012) |
| $N$ | 147,459 | 275,130 | 166,662 | 147,459 | 275,130 | 166,662 |
| Mean of Dep. Var. | 0.302 | 0.103 | 0.014 | 0.422 | 0.525 | 0.108 |
| Estimation Framework | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D | D-in-D |
| Sample | Under $7.50 | Food Service and Teens | Manufac-turing | Under $7.50 | Food Service and Teens | Manufac-turing |

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 (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. In columns 1-3, the dependent variable is an indicator for whether an individual's hourly wage is between $5.15 and $7.25. In columns 4-6, the dependent variable is an indicator for whether an individual has no earnings, characterized as either being unemployed or employed without pay. In Columns 1 and 4, the sample consists of individuals whose average baseline wages (meaning wages when employed between August 2008 and July 2009) are less than $7.50. In Columns 2 and 5, the sample consists of individuals who were teenagers or whose modal industry was food service when employed at baseline. In Columns 3 and 6, the sample consists of individuals whose modal industry was manufacturing when employed at baseline. Standard errors are clustered at the state level.

Table B.19: **Effects on Food Service Employment**

| | (1) | (2) | (3) |
|---|---|---|---|
| | | Food Service | |
| Bound x Post 1 | -0.000 | -0.023* | 0.002+ |
| | (0.001) | (0.011) | (0.001) |
| Bound x Post 2 | -0.001 | -0.033* | 0.003* |
| | (0.002) | (0.013) | (0.001) |
| $N$ | 1,971,672 | 147,459 | 1,824,213 |
| Mean of Dep. Var. | 0.047 | 0.216 | 0.033 |
| Estimation Framework | D-in-D | D-in-D | D-in-D |
| Weighted | No | No | No |
| Individual Fixed Effects | Yes | Yes | Yes |
| Sample | Full Population | Under $7.50 | All Other |

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 probability of working in the food service sector. More specifically, the estimates in row 1 are of the coefficient $\beta_{p(t)}$ from equation (1), 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 (1), where the relevant $p(t)$ corresponds with the period beginning one year after the July 2009 increase in the federal minimum wage. In column 1, the sample contains all individuals aged 16 to 64 for whom the relevant earnings and employment data were available for at least 36 months between August 2008 and July 2012. In column 2, the sample consists of individuals from the sample in column 1 whose average baseline wages (meaning wages when employed between August 2008 and July 2009) were less than $7.50. The sample in column 3 is the complement of the sample in column 2. Standard errors are clustered at the state level.