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The Minimum Wage and the Great Recession: A Response to Zipperer and Recapitulation of the Evidence

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Abstract:

Clemens and Wither (2014) find that minimum wage increases contributed to employment declines among low-skilled individuals during the Great Recession. Zipperer (2016) argues that Clemens and Wither's estimates are biased. This paper assesses what underlies the difference between Zipperer's estimates and Clemens and Wither's estimates. I first show that Zipperer's control sets significantly attenuate the relationship between Clemens and Wither's "treatment indicator" variables and states' minimum wage rates. Scaling for this dilution of the underlying treatment accounts for nearly half of the difference between Zipperer's estimates and Clemens and Wither's estimates. Second, I show that the within-region variation on which Zipperer focuses attention biases his estimates towards positive values. Employment and income aggregates, as well as housing and construction indicators, reveal that within-region comparisons are prone to considerable upward bias. Florida, for example, experienced a far more severe housing decline than the regional neighbors for which several of Zipperer's specifications use it as the primary control. I show that Zipperer's estimates are quite sensitive to removing states with extreme housing crises from the sample, while the original Clemens and Wither estimates are not. I further show that Zipperer's specifications have implausible implications for the minimum wage's "effects" on employment within high skilled population groups. I conclude by recapitulating the basic facts underlying Clemens and Wither's assessment of the evidence.

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Employment among low-skilled individuals declined dramatically during the Great Recession. Clemens and Wither (2014) and Clemens (2015) investigate whether the federal minimum wage, which concurrently rose from $5.15 to $7.25, contributed to these declines. Using standard program evaluation techniques, these papers estimate that minimum wage increases explain a non-trivial share of the decline in low-skilled groups' employment. A September 2016 revision of Clemens and Wither (2014) consolidates these empirical analyses.[1] Zipperer (2016) critiques selected portions of this September 2016 revision.[2] The current paper assesses Zipperer's comment.

Zipperer's comment is of interest in part because it is representative of the critiques Allegretto, Dube, Reich, and Zipperer (2017) have recently raised against work by Neumark and Wascher (2006), Neumark, Salas, and Wascher (2014b), and Meer and West (2016). The debate involving these authors can be difficult to evaluate, due in part to the nature of the policy variation underlying estimates of the minimum wage's effects. The papers at issue analyze a multi-decade panel that contains more than 200 state and federal minimum wage changes.[3] As shown by Sorkin (2015), the available variation is primarily suitable for estimating short-run responses to temporary minimum wage increases.[4]

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[1]The September 2016 revision can be found here: `http://econweb.ucsd.edu/~j1clemens/pdfs/ClemensWitherMinimumWageGreatRecession.pdf`.

[2]The December 2016 version of Zipperer's analysis was available at the following link as of March 2017: `http://cdn.equitablegrowth.org/wp-content/uploads/2016/12/02155549/120616-WP-comments-on-clemens-and-wither.pdf`. The comprehensiveness of the current paper's response is related in part to the appearance of a variant on Zipperer's empirical analysis in an anonymous review of the original Clemens and Wither analysis. Zipperer and co-authors have referenced his analysis as evidence against the analysis in Clemens and Wither (2014) since as early as a July 2016 revision of Allegretto, Dube, Reich, and Zipperer (2017).

[3]Unsurprisingly given this environment, the analyses have little to say about policy implementation lags. Consequently, they struggle to compellingly distinguish between worrisome "pre-existing trends" and causal anticipation effects. Similarly, they do not distinguish between one time minimum wage changes, multi-phase minimum wage changes, and the increasingly prominent minimum wage changes linked to inflation-indexing provisions (Strain and Brummund, 2016).

[4]Because most historical minimum wage changes were legislated in nominal terms, they have been temporarily binding due to both inflation and real productivity growth.

One benefit of the current setting is that the research designs implemented by Clemens and Wither (2014) are relatively straightforward.[5] This is facilitated by the fact that the analysis involves a single piece of federal minimum wage legislation that more strongly bound the minimum wage rates in one half of the country than in the other. As they pertain to this particular historical episode, one can assess the assumptions underlying alternative empirical specifications with more clarity than is possible in analyses of the multi-decade panel.

As summarized in Zipperer's abstract, he finds that the difference-in-differences estimators in the September 2016 revision of Clemens and Wither (2014) are sensitive to the inclusion of "sectoral or geographic controls." To be more specific, these controls include sets of census region-by-time effects, sets of census division-by-time effects, and interactions between time effects and variables that describe states' industrial composition at baseline. The minimum wage literature's debate over these "sectoral" and "geographic" controls involves two recurring areas of contention, both of which involve a combination of setting-specific issues and issues that are broadly applicable. I show that both recurring areas of contention are relevant to understanding the differences between Zipperer's estimates and the baseline estimates of Clemens and Wither (2014).

The first recurring area of contention involves the question of how dramatically the "sectoral" and "geographic" control variables reduce the policy variation utilized for econometric identification. In the current setting, a salient dimension of this issue can be assessed by estimating the effects of Zipperer's control sets on the underlying "first stage" relationship between Clemens and Wither's policy indicator variables and states' effective minimum wage rates. That is, to what extent do these control sets dilute the treatment underlying estimated changes in employment? The estimates in tables 1 and 2

[5]This is evidenced in part by the fact that Zipperer's (2016) replication of Clemens and Wither's CPS analysis is perfect and that his replication of the prior paper's SIPP analysis is near perfect.

show that Zipperer's specifications substantially reduce the implicit first stage point estimate underlying the Current Population Survey (CPS) analysis in Clemens and Wither's (2014) revision. The estimates in table 3 show that Zipperer's specifications more moderately reduce the first stage implicitly underlying Clemens and Wither's (2014) analysis of the Survey of Income and Program Participation (SIPP).

Accounting for attenuation of the implicit "first stage" is essential for converting estimates into comparable changes in employment per dollar increase in the minimum wage. On average across estimates using the SIPP, re-scaling for attenuation accounts for just under half of the difference between the baseline and Zipperer's estimates. On average across estimates using the CPS, attenuation accounts for just over half of the difference.[6]

A second key question is whether Zipperer's control sets exacerbate bias despite in principle being intended to reduce it. While within-region comparisons may have an intuitive appeal, standard applied econometric insights emphasize that "within" estimators can be more, less, or just as biased as "within and across" estimators. As Neumark and Wascher (2017) observe, these insights can be traced at least as far back as work by Griliches (1977, 1979) on the economic returns to education.

In the current setting, a cursory knowledge of the geography of the housing crisis reveals within-region variation to be problematic. In the South, for example, the majority of the individuals in the "control" group come from Florida. In the Mountain West, the majority come from Arizona. Many readers will recognize Florida and Arizona to be states that experienced relatively extreme housing crises, in particular when compared

[6]One way to place the first stage's relevance into perspective is to average its effects in terms of percentage point changes in the point estimate rather than in terms of the percent of the difference explained. On average across all specifications, Zipperer's point estimates are 2.7 percentage points higher than their respective Clemens and Wither estimates. Similarly averaged across specifications, attenuation of the first stage point estimate accounts for a 1.3 percentage point difference. Expressed in this way, attenuation of the first stage point estimate accounts for just under half of the difference between Zipperer's estimates and Clemens and Wither's estimates.

with their regional neighbors. Indeed, they are states that fail to meet even the most lenient of the matching criteria considered in Clemens (2015). This suggests that weight ought to be shifted *away* from comparisons involving these states rather than *towards* them.

Section 3 develops this point by providing a detailed descriptive look at variations in the severity of the housing crisis both across and within regions. Tables 4, 5, and 6 compare changes in aggregate income, employment, and housing market indicators across the treatment and control groups. Control states experienced more severe recessions than treatment states along each of these dimensions. Consequently, estimates that make no effort to control for variations in the recession's severity across states will be biased towards positive values. Note that Zipperer's conclusions require believing the opposite to be the case.

A key question for assessing Zipperer's specifications is whether within-region variations are more or less exposed to biases due to the severity of the underlying recession than variations that extend both across and within regions. Table 6 reveals that within-region comparisons exacerbate the degree of imbalance between the treatment and control groups. The basic premise underlying a preference for within-region comparisons thus does not hold in this setting.

How empirically relevant are the biases associated with restricting attention to within-region comparisons? A straightforward way to provide evidence on this point is to estimate Zipperer's specifications, along with the Clemens and Wither baseline, on samples that exclude Arizona and Florida. I report these results in tables 7 through 9. The results show that Arizona and Florida significantly shape Zipperer's analysis. On average across specifications in both the SIPP and CPS, comparable estimates are just over one percentage point more negative than the estimates Zipperer reports. Across Zipperer's specifications, the estimated declines in employment per dollar increase in the minimum

wage are, on this sample, economically indistinguishable from the Clemens and Wither baseline. That is, the weight Zipperer's specifications place on Florida and Arizona, coupled with his specifications' attenuation of the implicit first stage, explain nearly the entirety of the difference between Zipperer's estimates and the estimates from Clemens and Wither's (2014) revision.[7]

I push further to provide relatively systematic evidence on the bias Zipperer's control sets introduce. First, I supplement the ad hoc exclusion of Arizona and Florida with the matching exercises that were developed for Clemens and Wither's (2014) revision. That is, I estimate Zipperer's specifications on samples restricted to states that can be matched on the basis of the magnitudes of their housing declines. This analysis thus uses a procedure developed prior to Zipperer's comment to investigate whether his estimates are driven by states that are outside of the common support of the housing decline distribution. The results are presented in tables 10 through 12. On average across the 30 relevant regressions (5 specifications across 3 analysis samples and 2 matching criteria), the estimates are 1.5 percentage points more negative than the estimates Zipperer reports. Coupled with the appropriate scaling for his specifications' attenuation of the implicit first stage, the differences between his estimates and those in the revision of Clemens and Wither (2014) are fully explained.

In additional analysis, I investigate whether Zipperer's specifications predict variations in employment among skill groups for which the minimum wage has no direct effect. I show that they do. In the SIPP analysis, Zipperer's specifications suggest that a \$0.40 increase in the minimum wage generated an 0.8 percentage point increase in em-

[7]On average across the 15 relevant regressions (5 alternative control sets across 3 analysis samples), the difference between Zipperer's $\beta_{\text{Post 2}(t)}$ estimate and the Clemens and Wither baseline is 2.7 percentage points. On average, re-scaling for the first stage accounts for a 1.3 percentage point difference. The re-scaling of the first stage and the percentage point difference associated with the weight Zipperer places on Florida and Arizona thus account for nearly 90 percent of the difference between Zipperer's estimates and the baseline estimates from Clemens and Wither's (2014) revision.

ployment among individuals with average baseline wages in excess of $10.[8] Zipperer's CPS specifications have the implication that a $0.30 increase in the minimum wage increased employment across the upper 90 percent of the skill distribution by a full percentage point.[9] If taken at face value, these estimates suggest that an additional $2 or $3 increase in the minimum wage would have prevented employment from declining by even a single percentage point during the Great Recession.

At this point it is relevant to discuss the portions of Clemens and Wither's (2014) analysis that are omitted from Zipperer's discussion. These include both the aforementioned matching research design and a triple-difference estimator. The triple-difference estimator is of most immediate interest. Algebraically, the triple-difference estimator's state-by-time effects subsume Zipperer's sets of state-by-time varying controls. Indeed, this specification was developed with the debate over geographic controls in mind. That is, it is a specification that is able to rule out some forms of the "geographic controls critique" without simultaneously altering the cross-state comparisons that underlie econometric identification. As Clemens and Strain (2017) clarify, "The inclusion of state-by-time-period effects enables the [triple-difference] specification to control flexibly for economic factors that vary across states and over time. They control for such factors as they manifest themselves through employment changes among the individuals included in the sample as 'within-state control groups."' Because Zipperer leaves readers uninformed of this analysis, he offers a misleadingly broad impression of the forms his critique can plausibly take. Further, the analysis summarized above reveals that Zipperer's estimates are quite sensitive to both the matching and triple-difference approaches.

Zipperer presents a separate exercise that he describes as "an important falsification

[8] $0.40 is the differential change in the minimum wage associated with the implicit first stage for the relevant specifications.

[9] $0.30 is the differential change in the minimum wage associated with the implicit first stage for the relevant specifications.

test." The variation at work in this exercise is quite similar to the within-region variation discussed above. The key detail is that roughly four-fifths of the observations to which Zipperer assigns "placebo treatment status" come from Florida and Arizona. As shown in additional analysis in Clemens (2017), the falsification test is thus biased for the same reason Zipperer's full-sample specifications are biased. That is, the procedure generates negative estimates because it assigns "placebo treatment status" to states that experienced extreme housing crises. Clemens (2017) further shows that test-appropriate confidence intervals are far wider than those Zipperer reports.[10] The "falsification test" is thus uninformative for its intended purpose.

After presenting the analysis discussed above, I conclude by returning to several key facts related to the labor market developments under analysis. First, I briefly recapitulate the basic findings of Clemens and Wither (2014) and Clemens (2015). I then connect the regression estimates to unadjusted data. The facts underlying Clemens and Wither's reading of the evidence can be summarized as follows: in comparisons between "bound" and "unbound" states, employment among low-skilled individuals in "bound" states declined much more than one would predict based on changes their in macroeconomic conditions. Finally, insights from the conceptual framework in Clemens and Wither's (2014) revision are easily overlooked in the debate over program evaluation methods. I thus conclude by summarizing these insights.

1 Background on the Empirical Setting

This section proceeds as follows. Sub-section 1.1 overviews the minimum wage changes under analysis. Sub-section 1.2 overviews the basic difference-in-differences

[10]The basic issue is that the properties of clustered robust standard errors do not carry over from the baseline regression, in which "treatment" is assigned to 27 of 50 states, to the falsification tests, in which "placebo treatment" is assigned to either 3 or 5 of 23 states.

design deployed in Clemens and Wither's (2014) revision using both the Current Population Survey (CPS) and the 2008 panel of the Survey of Income and Program Participation (SIPP). Sub-section 1.3 overviews both the matching and triple-difference research designs of Clemens and Wither's revision. Sub-section 1.4 overviews Zipperer's critique. Sub-section 1.5 overviews key issues under contention in the recent literature on the minimum wage's employment effects.

Interested readers should look to Clemens (2015) and the September 2016 revision of Clemens and Wither (2014) for relatively detailed discussions of the analysis samples and the federal minimum wage change's legislative history. The discussion below is limited to the material required to understand the critique raised by Zipperer (2016). The current paper thus foregoes discussion of a variety of interesting issues (for example, nuances of the SIPP and CPS data environments' strengths and weaknesses). It is also worth noting that the analyses in Clemens and Wither (2014) and Clemens (2015) report a fuller range of standard robustness checks than are discussed below. Note that sub-sections 1.1, 1.2, and 1.3 draw liberally on text from the methodology sections of Clemens and Wither (2014) and Clemens (2015), where the relevant methods were originally developed. Neither the full text nor the ideas in these sub-sections are original to the current paper.

1.1 Background on Recent Federal Minimum Wage Increases

The minimum wage changes under analysis resulted from federal legislation passed on May 25, 2007. Increases went into effect on July 24th of 2007, 2008, and 2009. In July 2007, the federal minimum rose from $5.15 to $5.85, in July 2008 it rose to $6.55, and in July 2009 it rose to $7.25. The analysis makes use of the fact that these increases were differentially binding across states.

Figure 1 shows Clemens and Wither's (2014) division of states into those that were

fully and partially bound by changes in the federal minimum wage. Figure 2 shows the time paths of the average effective minimum wage rates across these groups of states.[11] On average, the effective minimum across fully bound states rose by \$0.60 to \$0.70 cents more than the average effective minimum across partially bound states. The variation utilized for econometric identification stems from this difference.

1.2 Standard Difference-in-Differences Analysis

In the CPS, the revision of Clemens and Wither (2014) focuses on two samples. The first consists of individuals ages 16 to 30 with less than a completed high school education. The second consists of all young adults ages 16 to 21. The specification used to analyze these samples is reproduced below:

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

The specification controls sets of state fixed effects, State_s, time fixed effects, Time_t, and a vector of controls for variations in states' macroeconomic conditions, $\mathbf{X_{s,t}}$. In Clemens and Wither's (2014) baseline specification, $\mathbf{X_{s,t}}$ includes a median housing price index from the Federal Housing Finance Administration (FHFA), which proxies for the state-level severity of the housing crisis. Within this difference-in-differences framework, the additional robustness analysis presented in Clemens (2015) considers additional variables including aggregate state income per capita and stimulus spending per capita through the American Reinvestment and Recovery Act (ARRA).

[11] Both figure 1 and figure 2 first appeared in Clemens and Wither (2014).

The CPS analysis sample extends from January 2006 through December 2012. The coding of time periods allows estimates to track the full transitional dynamics associated with the law's implementation. May 2007 through July 2009 is the law's implementation period (period $p =$ Transition). Early 2007 and the entirety of 2006 are the base period ($p = 0$). Finally, period Post $1(t)$ corresponds with the first year following the law's implementation and period Post $2(t)$ encompasses subsequent years.

In the 2008 SIPP panel, the analysis in Clemens and Wither (2014) focuses on individuals whose baseline wage histories reveal them to be most likely to be affected by the July 2009 increase in the federal minimum wage. The primary analysis sample consists of individuals whose average wage rates between August 2008 and July 2009 were less than \$7.50. In response to both Zipperer and an anonymous referee, Clemens and Wither (2014) have considered the robustness of their estimates to a range of alternative sample construction procedures.[12]

In the SIPP's longitudinal setting, the difference-in-differences analysis takes the following form:

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

This specification differs from equation (1) in two ways. A difference that is visible in the notation is that the SIPP's longitudinal nature allows the specification to control for individual fixed effects, Individual_i. There is a second difference that is not visible in the notation. This reflects the fact that the 2008 SIPP panel begins during the summer of 2008. Consequently, SIPP samples involve low-wage individuals who had maintained

[12]These results are available upon request.

employment into the year between the federal minimum wage's rise to \$6.55 and its rise to \$7.25. The SIPP thus does not allow the analysis to consider the full transitional dynamics associated with the law's implementation. The transition period thus has a different meaning. Motivated by wage dynamics presented in Clemens and Wither (2014), the specification characterizes May to July 2009 as a period during which reported wages transitioned quite sharply out of the affected range. All other aspects of the specification are the same as before.[13]

For further details on these specifications, I refer readers to Clemens and Wither (2014) and Clemens (2015). The basic features of these specifications are not the subject of Zipperer's critique. Instead, Zipperer's critique is centered on the addition of control sets to the specifications as described above.

1.3 Threats to the Estimation of Causal Effects and Additional Lines of Investigation

There are standard threats to interpreting estimates of $\beta_{p(t)}$ as causal estimates of the effect of binding minimum wage increases on low-skilled groups' employment. The key question is whether the forces underlying the Great Recession would have generated larger, similarly sized, or smaller declines in employment among low-skilled individuals in the bound states relative to the unbound states. Note that this is a setting-specific statement of the parallel trends assumption that is broadly applicable to difference-in-differences analyses. The key assumption is that the treatment and control group would have followed similar paths in the absence of the policy change of interest. This paper is, in large part, an analysis of the magnitudes of the potential sources of bias and of the robustness of alternative approaches to accounting for them.

[13]Clemens (2016) uses the same estimation framework to analyze the relationship between binding minimum wage changes and changes in participation in a variety of low-income support programs.

The September 2016 revision of Clemens and Wither (2014) incorporates two further lines of investigation. The first, which was included in the original working paper, involves a relatively standard triple-difference estimator. The specification appears below:

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

Equation (3) augments equation (2) with the standard components of triple-difference estimation. These include group-by-time-period effects, group-by-state effects, and state-by-time-period effects.

The idea behind the triple-difference estimator is to use individuals whose observable skill levels moderately exceed those of minimum wage workers as a within-state control group. Clemens and Strain (2017) summarize what is and is not accomplished by this approach. They observe that:

> The inclusion of state-by-time-period effects enables the [triple-difference] specification to control flexibly for economic factors that vary across states and over time. They control for such factors as they manifest themselves through employment changes among the individuals included in the sample as 'within-state control groups.'
>
> There is a trade-off in determining how high up the skill distribution one goes to select a within-state control group. Individuals in modestly higher skill groups, for example, may be both directly and indirectly affected by the minimum wage change of interest. Direct effects may arise because individuals in modestly higher skill groups may sometimes work in minimum wage

jobs. Alternatively, their modestly higher pay rates may be benchmarked to the minimum wage to preserve a compensation hierarchy within an employing firm. Indirect effects may arise through bargaining spillovers or through firms' substitution of very low-skilled workers for modestly higher skilled workers. By contrast, high skilled individuals may be poor controls. That is, the employment of high skilled individuals may be too stable for such individuals to provide a reasonable counterfactual for the effects of economic shocks on employment among the low skilled; their labor markets may be too different for the comparison to be meaningful. The choice of a within-state control group is thus a dimension of the research design along which judgment is needed.

An alternative to triple-difference estimation involves estimating sets of difference-in-differences regressions that, across the samples considered, account for the entirety of the working age population. One interpretation of such an exercise is that plausible "within state control groups" are being used in a systematic set of falsification exercises. Difference-in-differences estimates on samples of high skilled individuals can provide evidence on the presence of time varying economic conditions that shape employment. Suppose, for example, that one estimates equation (2) on a sample of high skilled individuals and that the resulting $\beta_{\text{Post 2}(t)}$ is strongly positive. This result would provide evidence that general economic conditions were increasing employment among individuals in "bound" states relative to individuals in "unbound" states. Estimates of the same underlying specification on individuals who are targeted by the minimum wage would thus likely be biased and should be viewed with skepticism. In the analysis below, I estimate such sets of falsification exercises for both the Clemens and Wither baseline and the specifications in which Zipperer includes his most exhaustive control sets.

An additional piece of analysis in the revision of Clemens and Wither (2014) involves

matching states on the basis of their housing declines. Specifically, the analysis matches states on the size of their median house price declines between 2006 and 2012 (with values averaged across all months in these years). The matches are thus based on the extent of the housing decline from the first to the last year of the CPS analysis sample. The procedure applied is nearest neighbor matching without replacement. Clemens and Wither's (2014) revision then restricts samples on the basis of the quality of the resulting matches.

In the September 2016 revision of Clemens and Wither (2014), the matching exercise is used as a check on the robustness of Clemens and Wither's (2014) baseline approach to controlling for variations in the severity of the housing crisis. In the analysis below, I similarly use the matching design to investigate the robustness of Zipperer's analysis. The analysis provides some clarity on the extent to which Zipperer's estimates are driven by the weight his specifications place on comparisons that fall outside of the common support of the distribution of housing declines.

1.4 An Overview of Zipperer's Specifications

Zippperer's critique centers on the sensitivity of estimates of equations (1) and (2) to the inclusion of several control sets. As summarized in Zipperer's abstract, he finds that these estimators are sensitive to the inclusion of "sectoral or geographic controls." The control sets Zipperer includes are captured by notation of the equation below:

$$\begin{aligned} Y_{i,s,t} = & \sum_{p(t)\neq 0} \beta_{p(t)} \text{Bound}_s \times \text{Period}_{p(t)} + \alpha_{1_s} \text{State}_s + \alpha_{2_t} \text{Time}_t + \mathbf{X_{s,t}}\gamma \\ & + \alpha_{3_{r(s),t}} \text{Region}_{r(s)} \times \text{Time}_t + \alpha_{4_t} \text{Ind. Share}_s^{05\text{-}06} \times \text{Time}_t + \varepsilon_{i,s,t}. \end{aligned} \tag{4}$$

The 5 specifications Zipperer estimates involve augmenting equations (1) and (2) with

permutations of the control sets described by $\text{Region}_{r(s)} \times \text{Time}_t$ and Ind. $\text{Share}_s^{05\text{-}06} \times \text{Time}_t$. In words, these control sets consist of census region-by-time effects or census division-by-time effects ($\text{Region}_{r(s)} \times \text{Time}_t$) and interactions between time effects and variables that describe states' industry shares at a baseline defined to be an average across 2005 and 2006 (Ind. $\text{Share}_s^{05\text{-}06} \times \text{Time}_t$). Zipperer constructs these baseline shares using data from the Quarterly Census of Employment and Wages. In some specifications, Ind. $\text{Share}_s^{05\text{-}06}$ is construction's share of private sector employment. In others it is a set of variables describing industry shares at the level of 1-digit NAICS supersectors.[14]

1.5 Discussion of Points of Contention in the Recent Minimum Wage Literature

The minimum wage literature's debate over "sectoral" and "geographic" controls involves two recurring points of contention. This sub-section overviews these issues. The first issue, discussed in sub-section 1.5.1, involves the effect of these controls on the *quantity* of policy variation they leave in play. In the present setting, assessing this issue involves assessing the effect of Zipperer's control sets on the magnitude of the differential minimum wage change predicted by $\text{Bound}_s \times \text{Period}_{p(t)}$. That is, it involves assessing the effect of Zipperer's control sets on the implicit first stage. The second issue, discussed in sub-section 1.5.2, involves the *quality* of the variation that remains in play. That is, this sub-section takes up the question of whether "sectoral" and "geographic" controls introduce or exacerbate bias despite being intended to reduce it.

[14]In the CPS context, the latter control set consists of 84 time indicator variables (monthly indicators across 7 years) each interacted with 9 variables describing the baseline share of private sector of employment in the NAICS supersectors. There are thus 756 variables in this particular control set. There are similarly 756 variables in the control set that interacts each of 9 census division indicators with each of 84 time indicators.

1.5.1 Implications of Sectoral and Geographic Controls for the "First Stage"

The first recurring point of contention involves the question of how dramatically sectoral and geographic controls reduce the variation available for econometric identification. This is a key point of emphasis, for example, in Neumark, Salas, and Wascher (2014b). These authors colorfully describe Allegretto, Dube, Reich, and Zipperer's (2017) approach as "Throwing the Baby out with the Bathwater."

The analyses of Neumark, Salas, and Wascher (2014b) and Allegretto, Dube, Reich, and Zipperer (2017) involve a long panel containing more than 200 distinct state and federal minimum wage changes. In that setting, claims regarding the erosion of identifying variation are difficult to adjudicate. While sectoral and geographic controls clearly reduce the variation used for identification, there is disagreement over whether the resulting specifications should be viewed as "underpowered."

In the current setting, attenuation of the policy variation underlying identification is both straightforwardly relevant and straightforward to assess. Equations (1) and (4) generate "reduced form" estimates. That is, they generate estimates of the relationship between employment and indicators for the onset of differentially binding minimum wage increases. Interpreting this relationship requires information on the underlying "first stage." That is, it requires relating any estimated effect on employment to the underlying change in the minimum wage. This standard scaling of the "reduced form" by the "first stage" is required to obtain comparable estimates of the change in employment per dollar increase in the minimum wage. The key point is that estimates from equations (1) and (4) cannot be informatively compared without information on the implicit first stage point estimates. As shown below, the introduction of $\text{Region}_{r(s)} \times \text{Time}_t$ and Ind. $\text{Share}_s^{05\text{-}06} \times \text{Time}_t$ significantly reduces the first stage relationship between $\text{Bound}_s \times \text{Period}_{p(t)}$ and states' effective minimum wage rates.

1.5.2 Do Sectoral and Geographic Controls Reduce Bias or Exacerbate It?

A second key question is whether Zipperer's geographic and sectoral control sets introduce or exacerbate bias despite being intended to reduce it. There are several relevant issues here. A first set of issues involves the question of whether within-region variation is less prone to bias than variation that extends both across and within regions. A second set of issues involves concerns that arise when controlling for trends in analyses of policies that have dynamic effects.

While geographic comparisons may have an intuitive appeal, a cursory knowledge of the geography of the housing crisis reveals within-region variation to be problematic. In the South, for example, the majority of the individuals in the "control" group come from Florida. In the Mountain West, the majority come from Arizona. Many readers will recognize Florida and Arizona to be states that experienced relatively extreme housing crises, in particular when compared with their regional neighbors. Because the experiences of these states were quite unlike the experiences of their neighbors, the basic premise underlying within-region comparisons is dubious in this setting.

I save a detailed presentation of descriptive evidence on the pitfalls of within-region variation for section 3. Sections 4.2 and 4.3 present systematic regression evidence on the relevance of these pitfalls for Zipperer's estimates. The remainder of this section focuses on a separate methodological issue.

In any program evaluation context, it is important to understand when introducing a new control set risks exacerbating biases despite being intended to correct for potential biases. It is increasingly appreciated, for example, that controlling for state-specific time trends can downwardly bias estimates when true causal effects unfold dynamically. Applications of this point include Wolfers's (2006) analysis of divorce law and Baum-Snow and Lutz's (2011) analysis of school desegregation. In the minimum wage context, this point was made through simulations that appear in the working paper version of Meer

and West's (2013) analysis.

When a policy change's effects unfold dynamically, state-specific time trends and correlated covariate sets will also be correlated with the policy change's causal effect. Controlling for such covariates will tend to bias estimates towards 0 unless the policy variables in the regression specification perfectly capture the dynamics of the policy change's effects.[15] Basic "pre-post" difference-in-differences specifications are thus particularly exposed to this potential problem. A related issue applies most directly to the inclusion of control sets like Zipperer's "baseline industry" controls. The issue involves a straightforward correlation between the policy effects of interest and the control set. If states' minimum wage policies are correlated with their industrial composition, then interactions between time effects and measures of baseline industrial composition will be correlated with minimum wage changes' causal effects. The inclusion of such control sets will thus tend to bias estimated employment effects towards 0.

2 Evidence on the Effects of the Sectoral and Geographic Controls on the "First Stage"

Tables 1 through 3 present evidence on the effect of Zipperer's sectoral and geographic controls on the first stage relationship between Clemens and Wither's (2014) policy indicator variables and states' minimum wage rates. The tables are organized as follows. Table 1 presents estimates associated with CPS samples of individuals ages 16 to 30 with less than a completed high school education. Table 2 presents estimates associated with CPS samples of all young adults ages 16 to 21. Table 3 presents estimates associated with SIPP samples of individuals who reported average wage rates less than

[15]This will not generally be possible if the relevant dynamics exhibit heterogeneity within the set of treatment states due to unobservable factors.

$7.50 during the period extending from August 2008 through July 2009.

Within each table, column 1 presents estimates from the baseline specification of Clemens and Wither's (2014) September 2016 revision. Columns 2 through 6 present specifications that include the 5 alternative control sets considered by Zipperer. In column 2, the control set consists of time effects interacted with the construction share of private sector employment as measured using the average of 2005 and 2006 data in the Quarterly Census of Employment and Wages. Column 3 involves a broader set of variables describing baseline industry shares. This control set includes full sets of time effects interacted with variables corresponding with the share of each state's employment that comes from each of the 1-digit supersectors in the NAICS industry coding scheme. In column 4, the control set includes full sets of time effects interacted with separate indicators for each of the 4 census regions. In column 5, the control set includes full sets of time effects interacted with separate indicators for each of the 9 census divisions. Column 6 combines the control sets from columns 2 and 5.

I present first stage results in panel A of each table. For both CPS samples, Zipperer's specifications dramatically reduce the first stage point estimate.[16] On average across the CPS samples and specifications, the coefficient $\beta_{\text{Post 2}(t)}$ for the first stage regression is reduced by nearly 50 percent.

On the SIPP sample, the declines in the first stage point estimates have substantive implications, but are less dramatic. On average across the 5 SIPP specifications, the coefficient $\beta_{\text{Post 2}(t)}$ for the first stage regression is reduced by 17 percent. In the SIPP specifications there is a divide between the effects of the industry share interactions and the time varying geographic fixed effects. The latter 3 control sets reduce the first stage

[16]Note that because there are modest discrepancies across existing minimum wage databases, alternative databases may yield modestly different first stage point estimates. The database used here comes from the replication materials of Meer and West (2016), but with a correction for the minimum wage rate applicable in Michigan between October 2006 and October 2007.

point estimate by an average of 23 percent while the former 2 control sets reduce the first stage point estimate by an average of 8 percent.

Across the 15 specifications of interest, the average decline in the first stage point estimate is nearly 40 percent. Comparing Clemens and Wither's (2014) estimates with Zipperer's estimates requires re-scaling the Clemens and Wither baseline for attenuation of the underlying first stage. On average across all 15 specifications, this re-scaling accounts for nearly half of the difference between Clemens and Wither's (2014) estimates and Zipperer's estimates.

Because Zipperer does not present these "implicit first stage" estimates, his analysis provides a misleading impression of the factors underlying differences between his estimates and estimates from Clemens and Wither's (2014) revision. Zipperer attributes the difference to bias. This section straightforwardly shows that nearly half of the difference is due to the fact that the estimates as presented are not directly comparable. The empirical analysis below explores the remainder.

3 A Descriptive Look at the Biases to Which the Baseline and Within-Region Estimators Are Exposed

3.1 Sources of Bias in the Baseline

Figure 5, which first appeared in Clemens and Wither (2014), presents the evolution of several macroeconomic indicators in the treatment and control group. Table 4 summarizes the evolution of these and additional economic indicators as they align with the baseline and post-implementation periods in Clemens and Wither's (2014) CPS analysis. The data consistently show that states in the control group were exposed to recessions of greater severity than were the treatment states. From the baseline through

the post-implementation period, aggregate income per capita declined by $1,160 more in the control states than in the treatment states. The overall employment rate declined by 0.5 percentage point more in the control states, employment among prime aged adults declined by 0.8 percentage point more in the control states, and the unemployment rate rose by 1.08 percentage points more in the control states. Median house prices declined by a fairly dramatic $84,000 more in the control states than in the treatment states.

The macroeconomic indicators in figure 5 and table 4 point uniformly to the conclusion that the Great Recession was more severe in the states that comprise Clemens and Wither's (2014) control group than in the states that comprise their treatment group. Estimates are thus at risk of being biased towards zero. Zipperer asserts the opposite. He writes that Clemens and Wither's estimates are biased *away* from zero because "Bound and unbound states were not comparable prior to the Great Recession and indeed faced different employment shocks during the downturn."

Zipperer presents a cross-sectional fact that he describes as "direct evidence" of the shocks to which he refers. Specifically, he notes that construction's share of employment was higher in 2006 in bound states than in unbound states. Zipperer infers that bound states were thus exposed to relatively large construction employment shocks.

The data in figure 3 provide evidence that Zipperer's inference is incorrect. Construction's share of employment evolved similarly in bound states relative to unbound states. The "shock" for which Zipperer claims to provide evidence simply did not materialize. Construction accounted for a larger share of employment in the bound states than in the unbound states by roughly the same amount in each year from 2002 through 2015. The fact that the 2006 construction share was higher in bound states than in unbound states appears immaterially related to construction's cycle of boom and bust.[17]

[17]The parallel movement in states' construction shares from 2002 to 2015 suggests that bound states' "steady state" construction shares are higher than the construction shares in unbound states. The fact that "bound" states are disproportionately rural may point to reasons why this is the case.

Figure 4 presents additional evidence that construction shocks were, if anything, the opposite of what Zipperer infers. The figure presents BEA data on construction output, which shows that construction declined far more in the control states than in the treatment states.[18] Zipperer's inference is thus inconsistent with both the construction output series and the median house price series.

While cross-sectional differences may be indicative of exposure to differential shocks, they are not direct evidence of such shocks. The data discussed above provide evidence that the "shocks" Zipperer infers from baseline construction shares lack empirical relevance. That is, they leave no trace in the observed changes in prime aged employment, income per capita, construction output, or even construction employment itself.

3.2 Sources of Bias When Restricting To Within-Region Variations

The premise underlying Zipperer's (2016) use of region-by-time fixed effects is that within-region comparisons are less prone to bias than comparisons that extend both within and across regions. Knowledge of the housing decline's geography (in particular of its within-region variations) casts doubt on this premise as it applies to the current setting. Most strikingly, within-region and within-division variation will emphasize comparisons of Florida to the remainder of the South and of Arizona to the remainder of the Mountain West. Such comparisons are dubious because these states experienced housing crises far more extreme than their neighbors. The remainder of this section presents systematic descriptive evidence on the variations to which within-region estimation shifts attention.

Table 6 presents regional variations in the set of macroeconomic series summarized

[18] An important detail underlying the BEA's state-level construction series is that the price adjustment BEA uses to infer "quantities" is national rather than local. State variations in the series are thus not driven by variations in home prices. This is easily confirmed in the data. Averages across the bound and unbound states' "quantity" and "quantity times price" series diverge from one another by the same amount and with the same pattern over time.

previously in table 4. Column 1 presents changes from the baseline period to the post-implementation period for the control group. Column 2 presents changes for the treatment group. Column 3 presents the difference between column 1 and column 2. Panels A through D present these differences for the 4 census regions, while panel E presents averages across census regions. The averages are weighted according to each region's share of the population in the CPS analysis of individuals ages 16 to 30 with less than a completed high school education.

The data in panel E reveal that within-region variations in the severity of the Great Recession are even less balanced across the treatment and control states than are variations across the country as a whole. The imbalance in the decline in aggregate income rises from the $1,160 per capita reported in table 4 to the $1,570 reported in table panel E of table 6. The imbalance in the decline in aggregate employment rises from 0.5 to 0.8 percentage point, while the imbalance in the decline in prime aged employment rises from 0.8 percentage point to 1.0 percentage point.

The data in table 6 reveal that restricting attention to within-region variations *increases* the propensity for estimates to be biased by differences in the severity of the Great Recession.[19] In the meantime, as shown in section 2, it substantially attenuates the minimum wage variation underlying econometric identification. The following section provides regression evidence that, together, these factors explain the entirety of the difference between Zipperer's estimates and the Clemens and Wither baseline.

[19]The data thus support Neumark, Salas, and Wascher's (2014a) analogy between within-region variation in minimum wage policy and the use of "twins" or other "within-family" approaches to estimating the returns to education. If regional neighbors are so similar, it becomes crucial to ask why they adopted different minimum wage policies. As in the case of twins, within-region comparisons can generate estimates that are either more or less subject to bias than comparisons that extend both across and within regions. In the present case, states like Arizona, Florida, and New Hampshire may have different minimum wage policies than their neighbors for a variety of reasons. Some of these reasons may make their populations' employment trajectories poor counterfactuals for their neighbors' populations. In both the present case and the twins case, estimates involving the narrow, superficially attractive comparison can ultimately be more biased than broader comparisons.

4 Regression Evidence on Bias

This section presents regression evidence related to several features of both the baseline specifications of Clemens and Wither's (2014) revision and the 5 specifications estimated by Zipperer. The first set of regressions involves the relatively ad hoc exclusion of Florida and Arizona from the sample. The second set of regressions involves the more systematic sample selection procedures implemented in Clemens (2015) and the revision of Clemens and Wither (2014). The third set of regressions involves the systematic set of regressions in which I estimate the baseline equations of both Clemens and Wither's (2014) revision and Zipperer's (2016) analysis on sub-samples of skill groups that fully partition the working age population.

4.1 Evidence from the Ad Hoc Exclusion of Florida and Arizona from the Sample

This section presents estimates in which I exclude Florida and Arizona from the analysis sample. As shown in section 3, the within-region variation to which Zipperer restricts attention is less intuitively appealing than one might initially expect. Notably, it shifts weight towards comparisons of Florida to the remainder of the South and of Arizona to the remainder of the Mountain West. Excluding these states from the sample thus provides a very direct, though ad hoc, approach to investigating the hypothesis that Zipperer's results are driven by the weight they place on these comparisons. The results support this view.

Tables 7 through 9 are organized in roughly the same fashion as tables 1 through 3. Table 7 involves CPS samples of individuals ages 16 to 30 with less than a completed high school education. Table 8 involves all CPS young adults ages 16 to 21. Table 3 involves the SIPP sample. Panel A presents the full sample estimates of both the Clemens and

Wither baseline and the 5 Zipperer specifications. Panel B presents the same sets of specifications estimated on samples that exclude Florida and Arizona.

The results in tables 7 through 9 reveal that Zipperer's estimates are significantly shaped by comparisons involving Florida and Arizona. On average across specifications in both the SIPP and CPS, the relevant estimates are just over a full percentage point more negative than the estimates Zipperer presents. Consistent with the bias suggested by section 3's descriptive analysis, the specifications that include Zipperer's region-by-time and division-by-time effects are more sensitive to Florida and Arizona's inclusion than the specifications involving baseline industry controls.[20]

Between attenuation of first stage point estimates and weight shifted to dubious comparisons, the differences between Zipperer's estimates and Clemens and Wither's estimates are nearly fully explained. On average across the 15 relevant specifications (5 alternative control sets across 3 analysis samples), the difference between Zipperer's $\beta_{\text{Post 2}(t)}$ estimate and the Clemens and Wither baseline is 2.7 percentage points. On average, re-scaling for the first stage accounts for a 1.3 percentage point difference. The exclusion of Florida and Arizona shifts Zipperer's estimates downward by an average of 1.1 percentage points. The re-scaling of the first stage and the role of Florida and Arizona thus account for nearly 90 percent (2.4 out of 2.7 percentage points) of the difference between Zipperer's estimates and the baseline estimates from Clemens and Wither's (2014) revision.

While the difference between Zipperer's estimates and Clemens and Wither's estimates has plausibly been explained, the exclusion of Florida and Arizona from the sample may seem excessively ad hoc. In the following sub-sections, I thus pursue a

[20]Consistent with section 1.5.2's discussion of sources of bias, the industry-controls specifications yield particularly strong and implausible correlations between minimum wage increases and increases in employment among high-experience, high-education, and high-wage individuals. This analysis appears in tables 11 and 12 and is discussed in section 4.3.

more systematic analysis of potential bias.

4.2 Evidence from Matches Based on the Severity of States' Housing Declines

Tables 10 through 12 present estimates on samples that apply the matching criteria developed for Clemens (2015) and the revision of Clemens and Wither (2014). These criteria were thus developed prior to Zipperer's comment. Zipperer's comment omits discussion of this aspect of Clemens and Wither's (2014) analysis. This is unfortunate, as it sheds light on the relevance of extreme housing crisis experiences for both Zipperer's specifications and Clemens and Wither's baseline estimates.

Tables 10 through 12 present three sets of estimates. Panel A again presents the full sample estimates of Clemens and Wither's baseline alongside Zipperer's 5 specifications. In panel B, the sample is limited to states that could be matched to a state with a median house price decline no more than $10,000 different than its own decline. In panel C, the sample is limited to states that could be matched to a state with a median house price decline no more than $20,000 different than its own decline.[21]

The estimates are quite similar to the estimates reported in tables 7 through 9. On average across the 30 relevant specifications (5 specifications across 3 analysis samples and 2 matching criteria), the estimates of Zipperer's specifications are 1.55 percentage points more negative than the estimates Zipperer reports. Coupled with the appropriate re-scaling for his specifications' attenuation of the first stage, the difference between Zipperer's estimates and those in the revision of Clemens and Wither (2014) is fully

[21]Sorted by state postal codes, the states dropped when using the $20,000 criterion are: AL, AZ, CA, DE, FL, LA, MA, MI, MT, ND, NM, NV, RI, WA, IN, KY, NE, SC, and UT. The states dropped by the $10,000 criterion are: AL, AR, AZ, CA, CT, DE, FL, GA, HI, KS, LA, MA, ME, MI, MO, MS, MT, ND, NM, NV, OK, RI, SD, TN, VA, WA, and WI. Restricting samples towards the common support of the distribution of house price declines involves dropping unbound states with unusually large housing declines and bound states with relatively smooth housing market experiences.

explained.

The exclusion of states with housing crises outside the common support connecting the treatment and control groups substantially affects Zipperer's estimates. More specifically, it makes them systematically more negative. In contrast, the matching design has modest effects on the estimates from Clemens and Wither's (2014) revision.

4.3 Evidence from Higher Skill Groups

Tables 13 and 14 present estimates of equations (1) and (4) on sub-samples of skill groups that systematically partition the working age population. The estimates in these tables are organized differently than the estimates in earlier tables. Table 13 presents estimates from the CPS. Columns 1 through 3 present estimates across samples that fully partition the working age population on the basis of both age and education. Columns 4 through 6 similarly present estimates across samples that partition the working age population on the basis of age alone. Table 14 presents estimates from the SIPP. The working age population is again fully partitioned, in this case on the basis of baseline wage histories.

The estimates in panel A are of the baseline specifications from Clemens and Wither's (2014) revision. Each of the estimates in panel A of both table 13 and 14 appear in the September 2016 revision of Clemens and Wither (2014). The estimates demonstrate that the differential employment declines in states that were fully bound by this period's minimum wage changes occurred exclusively among skill groups that are plausibly affected by the minimum wage. In the CPS, this includes young adults ages 16 to 21 and individuals ages 16 to 30 with less than a completed high school education. In the SIPP, this includes individuals with average baseline wage rates below $7.50 as well as individuals who were not employed throughout the baseline period. Estimates for all other skill groups are statistically indistinguishable from 0 and generally economically quite

close to 0.

Panels B and C investigate whether Zipperer's specifications pass this same systematic set of checks. Panel B presents the specification in which Zipperer includes his most detailed set of industry share-by-time controls. Panel C presents the specification in which Zipperer includes the relatively extensive set of census division by time effects.

In Zipperer's specifications, minimum wage increases predict substantial increases in employment among skill groups for which the minimum wage has no direct effect. In the SIPP analysis, Zipperer's specifications suggest that a $0.40 increase in the minimum wage (see the first stage estimates from table 3) generated an 0.8 percentage point increase in employment among individuals with average baseline wages in excess of $10. The estimates implausibly suggest that economy-wide employment would have gone up during the Great Recession had the minimum wage risen by an additional $3.

In the CPS regressions, estimates of Zipperer's specifications involving higher skill groups (columns 2,3, 5, and 6) are uniformly positive, though mixed with regards to statistical significance. In the industry-shares specification, results for high skill groups are uniformly quite strongly statistically significant. On average across the two sets of specifications, the results suggest that a $0.30 minimum wage increase (see the first stage estimates from tables 1 and 2) led to a 1 percentage point increase in employment among adults with significant experience and/or at least a high school degree.[22] In the CPS, Zipperer's estimates thus suggest that employment would have increased during the Great Recession had the minimum wage risen by just $2 more.

It has become fashionable to speculate that minimum wage increases might increase

[22]Recall that the "target" samples of young adults and young, low-education individuals account for an average of roughly 10 percent of the working age population. A percentage point of employment among the remaining 90 percent of the working age population thus corresponds with the same number of jobs as a nearly 10 percentage point change in employment among the smaller low-skilled groups. Recalling further that Zipperer's specifications suggest little to no employment loss among the low-skilled groups, employment increases among the higher skilled cannot plausibly be interpreted as a substitution effect.

employment through "stimulus" effects. It is thus necessary to be clear that such effects cannot plausibly explain the results described above. In Zipperer's CPS specifications, the estimates imply that a $1 increase in low-skilled workers' wage bills predicts an increase of roughly $50 in higher skilled individuals' wage bills.[23] This is well over an order of magnitude beyond what the "minimum wage stimulus" argument can plausibly imply.

5 Recapitulation of Basic Facts and Findings

In this section, I return to the basic facts underlying Clemens and Wither's (2014) assessment of the evidence in their September 2016 revision. The basic facts are as follows. Across low-skilled groups in states with similarly sized housing declines, employment declined far more when the state was fully bound by this period's minimum wage increases than when the state was only partially bound. Among individuals ages 16 to 30 with less than a completed high school education, the differential decline was 3 and a half percentage points. Among young adults ages 16 to 21, the differential was 2 and a half percentage points. Among the more targeted sample of low-skilled workers in the SIPP, the differential was 6 and a half percentage points.

Tables 4 and 5 enable readers to generate their preferred estimate by working directly from unadjusted data. Employment among low-skilled groups declined modestly more in fully bound states than in partially bound states in spite of the fact that the recessions

[23]Recall that the first stage in Zipperer's specification predicts a $0.30 differential increase in states' minimum wage rates. The affected population works an average of around 1000 hours per year, so that the differential increase in annual income would be $300. This applies to several percent of the working age population, suggesting that the earnings gains amount to roughly $10 per working age individual. By way of comparison, the employment gains predicted by Zipperer's specification suggest an employment increase of roughly 1 percentage point across skill groups that account for 90 percent of the working age population. Average earnings within this group are on the order of $50,000 per year. A 1 percentage point increase in the prevalence of such earnings thus amounts to $500 per working age individual. There is thus roughly $500 in additional earnings among the "middle" and "high" skilled per $10 in mechanical earnings gain among minimum wage workers. The implied "earnings multiplier" is thus roughly 50.

in fully bound states were less severe than the recessions in partially bound states. In the unadjusted CPS data, the differential employment decline from the baseline through the post-implementation period was just over half of a percentage point among young adults and just over 1 percentage point among individuals ages 16 to 30 with less than a completed high school education (see panel B of table 4).[24] In the more targeted SIPP sample, the unadjusted differential decline was nearly 4 percentage points (see panel B of table 5).

Readers can use the macroeconomic and housing market indicators from panel A of tables 4 and 5 to adjust for variations in the severity of the Great Recession as they see fit. One way to arrive at the baseline estimates of Clemens and Wither is to assume that employment among low-skilled groups was between 2 and 3 times as sensitive to the forces underlying the Great Recession as was employment among prime aged adults. Our baseline estimates are also what one obtains by applying external evidence from Charles, Hurst, and Notowidigdo (2016).

Three facts underlie my application of Charles, Hurst, and Notowidigdo's (2016) estimates. First, on a population weighted basis, the average housing decline in partially bound states was roughly 1.15 standard deviations larger than the average housing decline in fully bound states. Second, for a broad group containing all individuals ages 21 to 55, Charles, Hurst, and Notowidigdo (2016) estimate that a 1 standard deviation difference in the housing bubble's magnitude predicts a differential employment change of 0.9 percentage point. Third, the groups analyzed by Clemens and Wither (2014) exhibit roughly twice the sensitivity to the housing decline as this broad group of primarily prime-aged individuals.[25] The implied bias is thus on the order of 2 to 2.5 percentage

[24]Note that these are the results one obtains if one runs the simplest possible difference-in-differences specification in which there are no macroeconomic covariates, the set of state dummy variables is replaced with a "Treatment" indicator, the set of time dummy variables is replaced with a "Post" indicator, and the sample excludes the transition period between the law's legislation and full implementation.

[25]This assessment is based on differences in the coefficients on the house price index in estimates

points depending on the group under analysis.[26] These adjustments correspond almost exactly to the difference between the unadjusted CPS data and Clemens and Wither's baseline regression estimates.

The remainder of Clemens and Wither's analysis explores potential threats to the magnitude of these estimates and to the case for causal identification. In both the SIPP and CPS, the estimates are robust to the inclusion of a range of macroeconomic covariates as controls. The estimates are further robust to restricting the sample to states that can be matched on the basis of their housing declines. Finally, the differential employment declines occur entirely among skill groups that are directly affected by the minimum wage. These features of Clemens and Wither's analysis underlie their assessment that the baseline estimate is the most plausible reading of the evidence.

As shown above, the specifications Zipperer puts forth lack several of the properties underlying this assessment. Consistent with descriptive evidence on the pitfalls of within-region variation, Zipperer's estimates are quite sensitive to the exclusion of states with housing declines outside the common support connecting the treatment and control groups. Excluding these states from the sample makes his estimates systematically more negative, by an average of roughly 1.5 percentage points. Second, Zipperer's specifications have the implausible implication that employment among high-skilled groups would have expanded during the Great Recession had the minimum wage, which does not affect these groups directly, been increased by an additional \$2 or \$3. Finally, Zipperer's specifications significantly dilute the treatment (that is, they attenuate the implicit first stage) underlying estimated employment changes. Their reduction in the implicit first stage, which his analysis does not report, accounts for roughly half of the difference

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involving samples restricted to the relevant skill groups.

[26]For the sample of 21 to 55 year olds, the adjustment would have been $1.15 \times 0.9 \approx 1$. Because the skill groups Clemens and Wither (2014) analyze have employment that is twice or just more than twice as sensitive to house price declines, the adjustment for these groups is between 2 and 2.5 percentage points.

between his estimates and the Clemens and Wither baseline.

6 Conclusion

Clemens and Wither (2014) use a straightforward conceptual framework to place their empirical analysis in context. As similarly discussed by Clemens and Strain (2017), the framework draws on Bound and Johnson's (1992) descriptive division of wage determinants into competitive and institutional forces. This dichotomy is ideal for conveying the economic implications of the minimum wage, which hinge on whether low-skilled individuals' wages are low due primarily to competitive market forces or to shortcomings in their bargaining power. Unintended job loss will tend to be large when low wages reflect low competitive market valuations of low-skilled workers' output. Conversely, intended wage gains can be large when weak bargaining positions generate large wedges between individuals' wages and their competitively valued marginal products.

The dichotomy between market and institutional forces delivers a further, closely related insight. The minimum wage's effectiveness as a response to changes in inequality can depend crucially on the causes of slow wage growth at the wage distribution's lower tail. If slow wage growth reflects a weakening of bargaining positions, then the wedge between wages and marginal products would be expanding. The capacity for the minimum wage to generate wage gains without reducing employment would thus be increasing. By contrast, if slow wage growth reflects the competitive effects of trade and technology driven developments (Katz and Murphy, 1992; Autor, Katz, and Kearney, 2008; Autor, Dorn, and Hanson, 2013), the job loss linked to a given real value of the minimum wage may, if anything, be rising.[27]

[27]The arrival of lower cost production possibilities reduces prices in competitive product markets. In both the framework of Clemens and Wither's (2014) revision and in standard instructional diagrams, this reduces the value of low-skilled workers' output, or their "marginal revenue product." Note that downward shifts in the marginal revenue product curve reduce the efficient wage in both the monopsony

This line of reasoning connects to two empirical insights regarding the minimum wage's effects on employment. A first point is that the minimum wage is an institution that mediates how the labor market responds to economic shocks. More specifically, it is a source of rigidity that, in both neoclassical and new Keynesian models, will increase the employment loss linked to declines in demand for goods and, by extension, for labor. This point is easily overlooked in standard program evaluation analyses because it involves the mediation of shocks to labor demand rather than the effects of changes to the minimum wage itself. Such effects will not be readily detected using standard program evaluation techniques. This highlights that standard program evaluation techniques can be insufficient to generate evidence on key dimensions of the minimum wage's effects.

The second insight is that the potential magnitude and duration of a minimum wage change's bite can be gauged using a combination of wage and productivity data. As shown in Clemens and Wither's (2014) revision, baseline wage data provide insight into the depth of a minimum wage increase's bite into low-skilled groups' wage distributions. Subsequent inflation and productivity data provide further insight into that bite's duration. This final insight leads to the analysis underlying figure 6, which appears in Clemens and Wither's (2014) revision.

Figure 6 presents data on low-skilled groups' wage distributions for the periods surrounding the federal minimum wage increases of both the 1990s and the 2000s. The key point is that these historical episodes differ dramatically. During the 1990s, the federal minimum wage rose by 21 percent, inflation was moderate, and productivity growth was rapid. During the late 2000s, the federal minimum wage rose by 41 percent, inflation was modest, and productivity growth was quite slow.[28] Both the depth and duration of the

diagram and the diagram associated with competitive firms. At any wage rate above the efficient wage, this shift would similarly increase the job loss associated with any given value of the minimum wage.

[28]This is particularly true when headline productivity statistics are adjusted for changes in the labor market's composition (Bosler, Daly, Fernald, and Hobijn, 2016). Such composition effects were particularly prominent during this historical episode because employment declined dramatically among low-

latter minimum wage increase's bite was thus far greater than that of the earlier increase. It would be truly surprising, whether the low-skilled labor market is perfectly or imperfectly competitive, if these minimum wage changes' employment effects did not differ accordingly.

Early data on the effects of minimum wage changes enacted since 2012 appear consistent with the general point that macroeconomic and broader labor market conditions matter. Through 2016, employment among young adults in states that have increased their minimum wage rates has kept up with, if not modestly outpaced, employment in states that have not. Disentangling the effects of this more recent period's minimum wage changes from variations in the robustness of the economic expansion will be an important task for future work. Future data may also shed light on the dynamics with which the responses of both firms and individuals unfold.

education, low-experience individuals.

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Figures and Tables

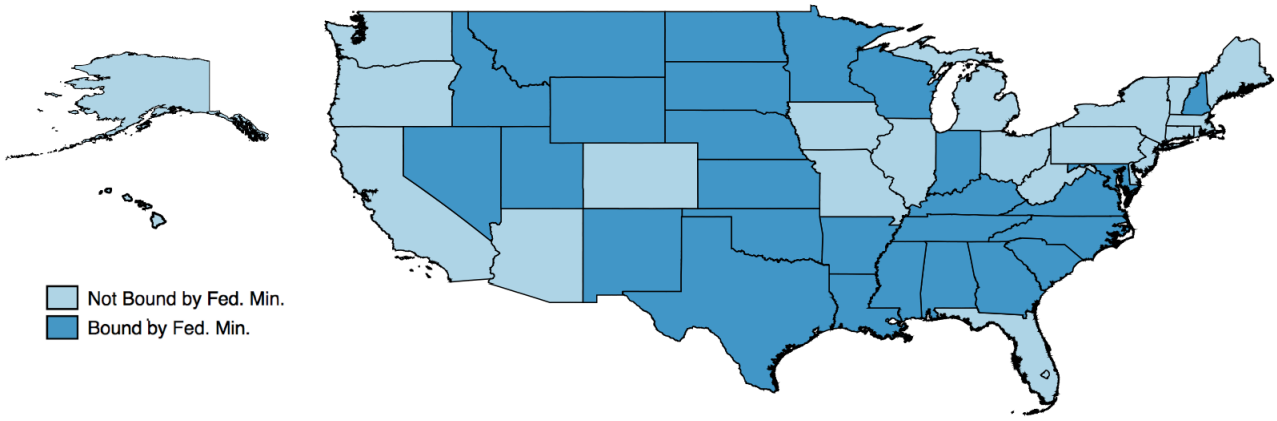


Figure 1: **States Bound by the 2008 and 2009 Federal Minimum Wage Increases:**
The map differentiates states on the basis of whether they were fully or partially bound by the July 2007, 2008 and 2009 increases in the federal minimum wage. I define states as fully bound if their January 2008 minimum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

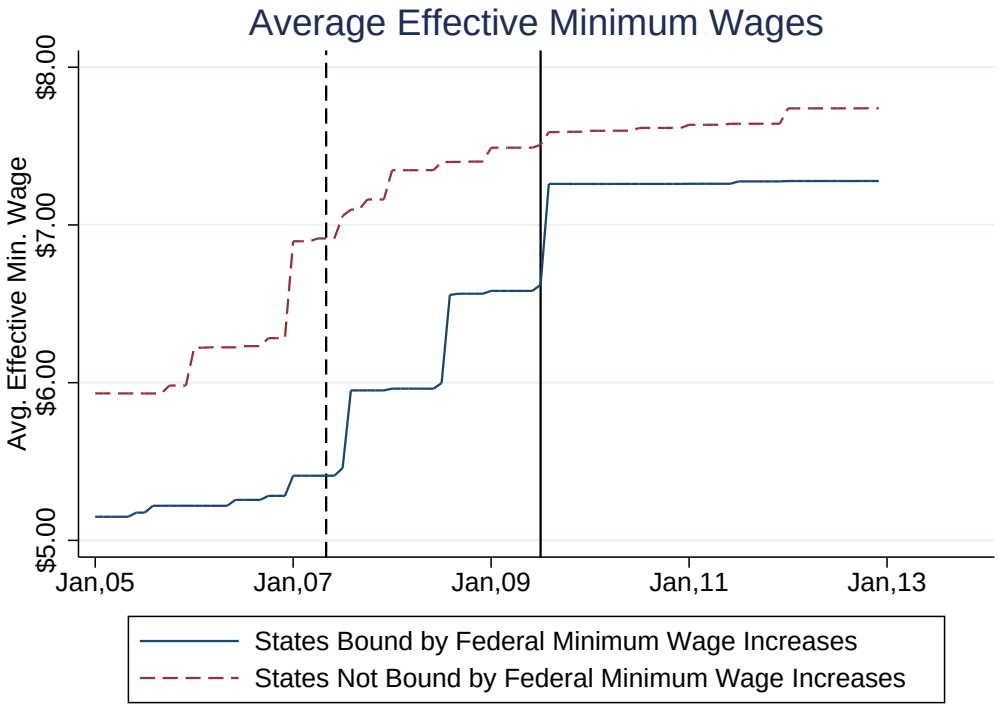


Figure 2: **Evolution of the Average Minimum Wage in Bound and Unbound States:**
As in the previous figure, states are defined as fully bound if they were reported by the Bureau of Labor Statistics (BLS) to have had a minimum wage less than $6.55 in January 2008. Such states were at least partially bound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

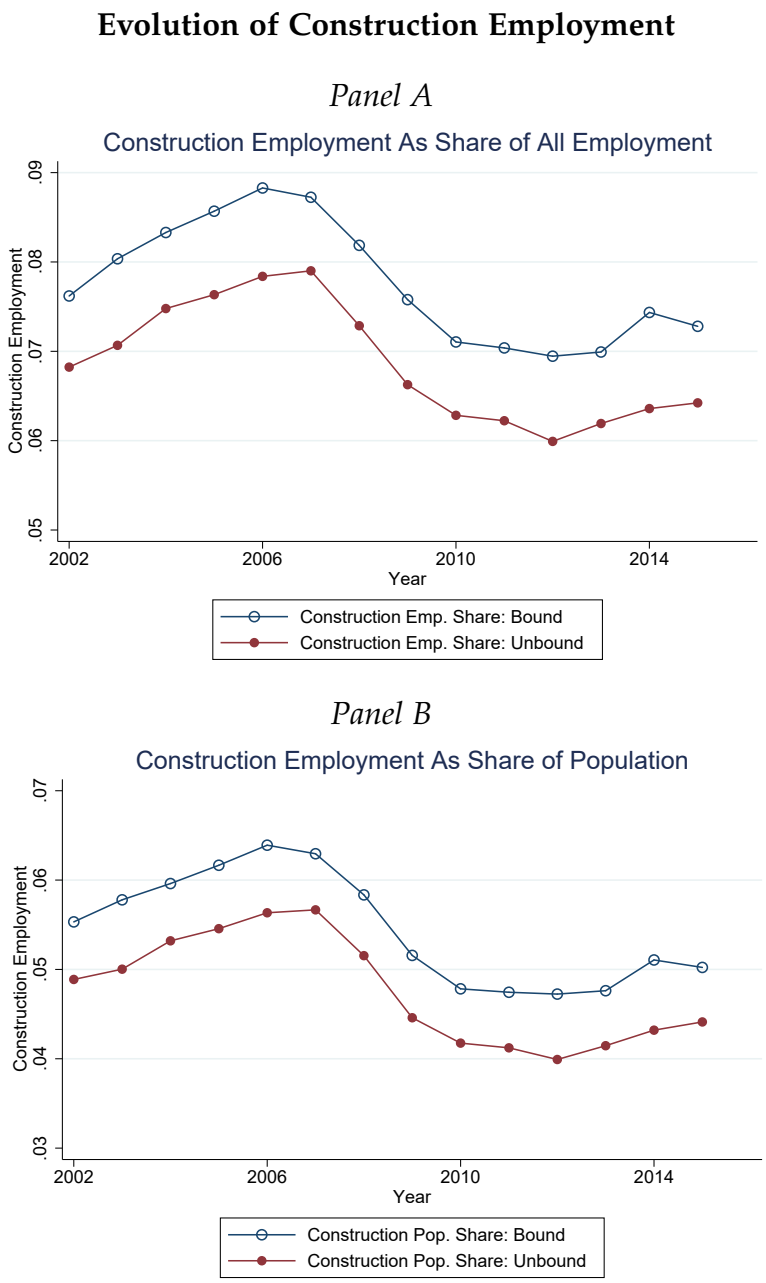
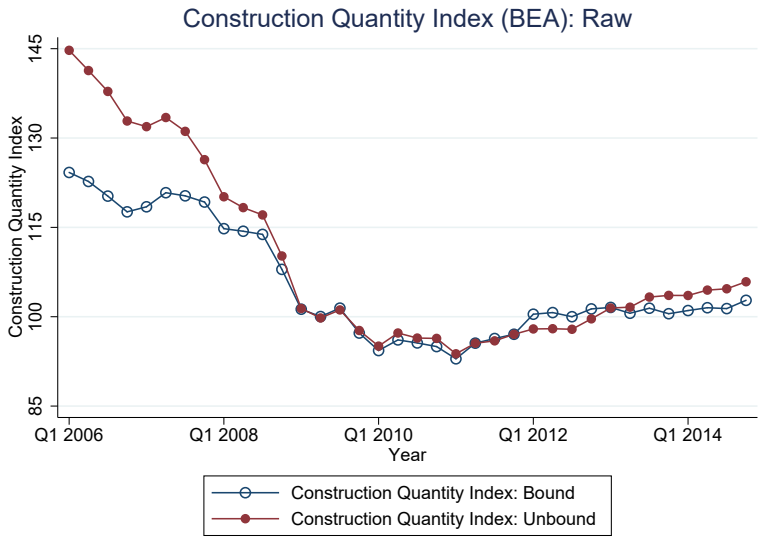


Figure 3: **Evolution of Construction Employment:**
Note: The figure present the evolution of construction employment as estimated using data from the basic CPS. The series in panel A reports construction employment as a fraction of total employment. The series in panel B reports construction employment as a fraction of the working age population.

Evolution of Construction Output

Panel A



Panel B

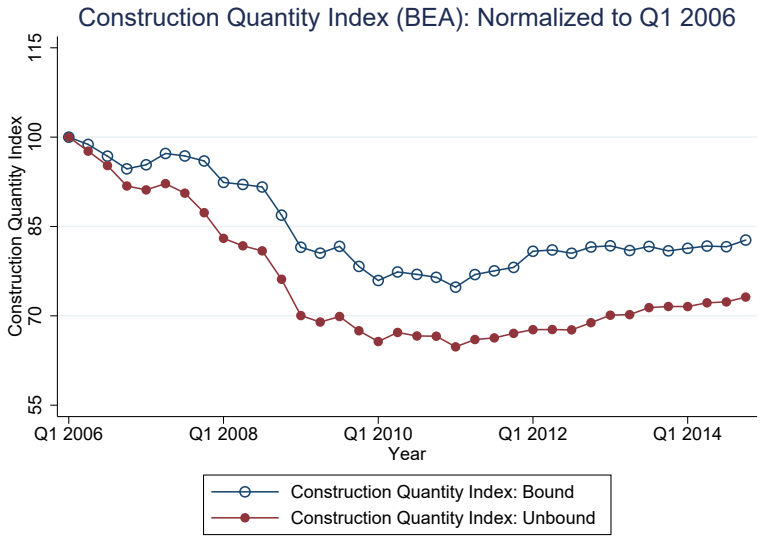


Figure 4: **Evolution of Construction Employment:**
Note: The figure present the evolution of construction output as reported by the Bureau of Economic Analysis (BEA). The series in panel A are population weighted averages of the BEA's regional accounts construction quantity index. The series in panel B renormalizes the indices to take a value of 100 in the first quarter of 2006.

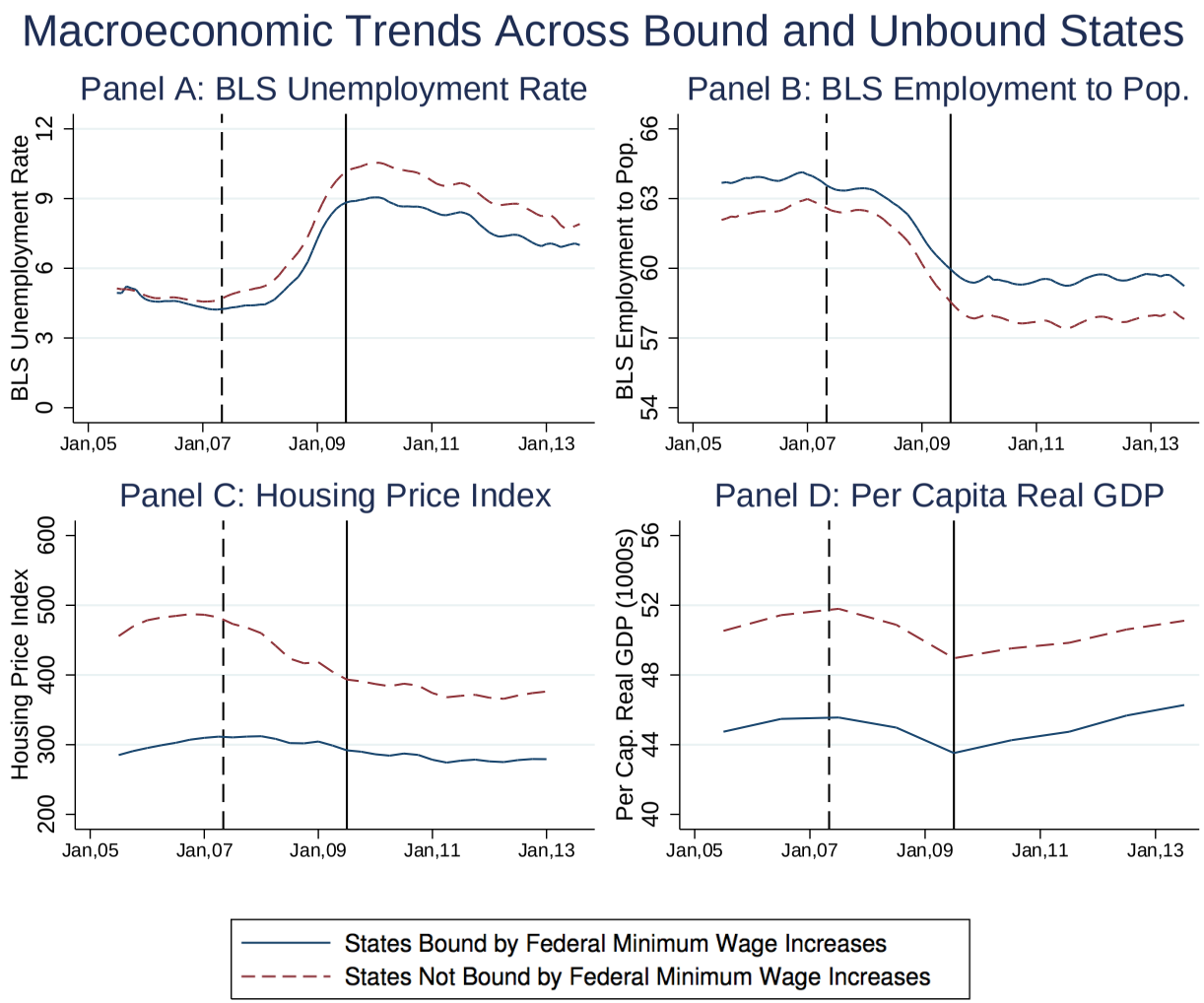


Figure 5: **Macroeconomic Trends in Bound and Unbound States:**
Note: Bound and unbound states are defined as in previous figures. This figure's panels plot the evolution of macroeconomic indicators over the course of the housing boom and bust. All series are weighted by state population so as to reflect the weighting applied in the regression analysis. Panel A plots the average monthly unemployment rate, as reported by the BLS. Panel B plots the average monthly employment to population ratio, also as reported by the BLS. Panel C plots the average of the quarterly Federal Housing Finance Agency's all-transactions median housing price index. Panel D plots the average of annual real per capita Gross State Product, as reported by the Bureau of Economic Analysis (BEA). In each panel, the dashed vertical line indicates the May 2007 passage of the federal minimum wage increases, while the solid vertical line indicates the timing of the July 2009 implementation of the final increase from $6.55 to $7.25.

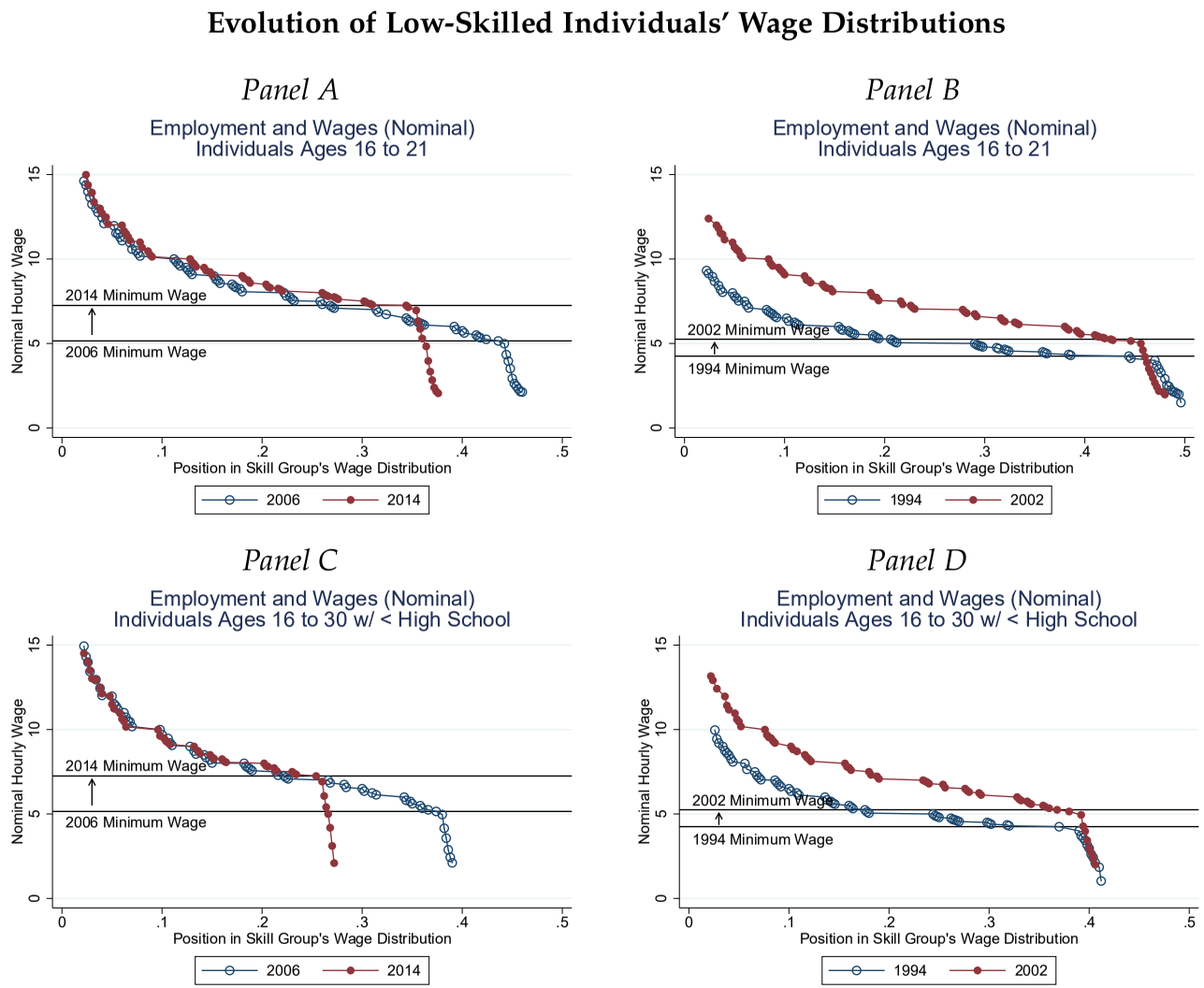


Figure 6: **Low-Skilled Individuals' Wage Distributions Surrounding Federal Minimum Wage Increases**
Note: The panels of the figure present wage distributions constructed using data from the NBER's CPS-MORG files for 1994, 2002, 2006, and 2014. The presented series describe the wage distributions of young adults ages 16 to 21 (panels A and B) and of individuals ages 30 and under with less than a completed high school education (panels C and D). The samples consist of individuals residing in states we categorize as "bound" or "fully bound" by the most recent federal minimum wage increases, as these states have historically maintained minimum wage rates quite close to the federal minimum. When available, individual-level wages are taken to be the reported values of the variable "earnhre" divided by 100. When "earnhre" is missing, individual-level wages are estimated as "earnwke/hours." Workers were sorted according to their wage rates, with unemployed individuals assigned wage rates of 0. The wage rates for each year were then divided into 500 quantiles with application of the CPS's population weights. The markers indicate all positive wage quantiles outside of the top 2 percentiles of each distribution.

Table 1: **First Stage and Reduced Form Estimates in the CPS: Least Skilled Group**

	(1)	(2)	(3)	(4)	(5)	(6)
	Clemens & Wither (Column 1)		Zipperer Specifications (Columns 2 through 6)			
Panel A:	Dependent Variable: Min. Wage (First Stage)					
Bound x Post 1	0.734***	0.557***	0.385**	0.550**	0.478*	0.461*
	(0.176)	(0.147)	(0.143)	(0.185)	(0.184)	(0.186)
Bound x Post 2	0.656**	0.446**	0.242+	0.435*	0.352+	0.338+
	(0.190)	(0.157)	(0.133)	(0.195)	(0.181)	(0.187)
Panel B:	Dependent Variable: Employed (Reduced Form)					
Bound x Post 1	-0.037**	-0.022	-0.020	-0.012	-0.014	-0.013
	(0.011)	(0.013)	(0.013)	(0.017)	(0.014)	(0.013)
Bound x Post 2	-0.037**	-0.022	-0.015	-0.003	0.001	0.002
	(0.011)	(0.013)	(0.013)	(0.016)	(0.015)	(0.013)
N	580,248	580,248	580,248	580,248	580,248	580,248
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Base Const. by Time	No	Yes	No	No	No	Yes
Base Ind. by Time	No	No	Yes	No	No	No
Region by Time	No	No	No	Yes	No	No
Division by Time	No	No	No	No	Yes	Yes
Matching Criterion	None	None	None	None	None	None
Dataset	CPS	CPS	CPS	CPS	CPS	CPS
Skill Group	Target	Target	Target	Target	Target	Target

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. This table presents estimates of equations (1) and (4) on samples that include all individuals in the Current Population Survey who are between 16 and 30 years of age and have less than a completed high school education. Column 1 contains the baseline estimate of equation (1) from Clemens and Wither's original analysis. Columns 2 through 6 contain estimates of equation (4) with the 5 alternative control sets considered by Zipperer. In Panel A, the dependent variable is the effective minimum wage. Estimates in Panel A thus report the "first stage" relationship. In Panel B, the dependent variable is an indicator for employment. Panel A is thus informative regarding the extent to which Zipperer's coefficients can be explained by the extent to which his controls sets erode the first stage. Across the specifications in this table, erosion of the first stage accounts for 62 percent of the difference. Standard errors allow for correlation clusters across errors at the state level.

Table 2: **First Stage and Reduced Form Estimates in the CPS: Young Adults**

	(1)	(2)	(3)	(4)	(5)	(6)
	Clemens & Wither (Column 1)		Zipperer Specifications (Columns 2 through 6)			
Panel A:	Dependent Variable: Min. Wage (First Stage)					
Bound x Post 1	0.703***	0.516**	0.350*	0.492*	0.418*	0.401+
	(0.180)	(0.153)	(0.147)	(0.196)	(0.199)	(0.200)
Bound x Post 2	0.628**	0.406*	0.211	0.379+	0.295	0.280
	(0.194)	(0.162)	(0.138)	(0.203)	(0.195)	(0.199)
Panel B:	Dependent Variable: Employed (Reduced Form)					
Bound x Post 1	-0.027**	-0.013	0.005	-0.007	-0.009	-0.010
	(0.009)	(0.009)	(0.008)	(0.013)	(0.014)	(0.012)
Bound x Post 2	-0.023*	-0.008	0.001	0.003	0.005	0.005
	(0.010)	(0.009)	(0.008)	(0.010)	(0.010)	(0.009)
N	894,384	894,384	894,384	894,384	894,384	894,384
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Base Const. by Time	No	Yes	No	No	No	Yes
Base Ind. by Time	No	No	Yes	No	No	No
Region by Time	No	No	No	Yes	No	No
Division by Time	No	No	No	No	Yes	Yes
Matching Criterion	None	None	None	None	None	None
Dataset	CPS	CPS	CPS	CPS	CPS	CPS
Skill Group	Target	Target	Target	Target	Target	Target

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. This table presents estimates of equations (1) and (4) on samples that include all individuals in the Current Population Survey who are between 16 and 21 years of age. Column 1 contains the baseline estimate of equation (1) from Clemens and Wither's original analysis. Columns 2 through 6 contain estimates of equation (4) with the 5 alternative control sets considered by Zipperer. In Panel A, the dependent variable is the effective minimum wage. Estimates in Panel A thus report the "first stage" relationship. In Panel B, the dependent variable is an indicator for employment. Panel A is thus informative regarding the extent to which Zipperer's coefficients can be explained by the extent to which his controls sets erode the first stage. Across the specifications in this table, erosion of the first stage accounts for 48 percent of the difference. Standard errors allow for correlation clusters across errors at the state level.

Table 3: **First Stage and Reduced Form Estimates in the SIPP: Target Group**

	(1)	(2)	(3)	(4)	(5)	(6)
	Clemens & Wither (Column 1)		Zipperer Specifications (Columns 2 through 6)			
Panel A:	Dependent Variable: Min. Wage (First Stage)					
Bound x Post 1	0.542***	0.517***	0.519***	0.456***	0.451***	0.450***
	(0.046)	(0.050)	(0.058)	(0.052)	(0.051)	(0.046)
Bound x Post 2	0.495***	0.454***	0.458***	0.395***	0.372***	0.371***
	(0.057)	(0.057)	(0.062)	(0.061)	(0.061)	(0.054)
Panel B:	Dependent Variable: Employed (Reduced Form)					
Bound x Post 1	-0.044*	-0.030	-0.041*	-0.017	-0.012	-0.014
	(0.019)	(0.020)	(0.019)	(0.025)	(0.024)	(0.023)
Bound x Post 2	-0.066**	-0.051*	-0.043*	-0.047*	-0.030	-0.030
	(0.020)	(0.020)	(0.021)	(0.021)	(0.020)	(0.020)
N	147,459	147,459	147,459	147,459	147,459	147,459
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Base Const. by Time	No	Yes	No	No	No	Yes
Base Ind. by Time	No	No	Yes	No	No	No
Region by Time	No	No	No	Yes	No	No
Division by Time	No	No	No	No	Yes	Yes
Matching Criterion	None	None	None	None	None	None
Dataset	SIPP	SIPP	SIPP	SIPP	SIPP	SIPP
Skill Group	Target	Target	Target	Target	Target	Target

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. This table presents estimates of equation (2) and the SIPP equivalent of equation (4) on samples that include individuals in the Survey of Income and Program Participation whose average wage between August 2008 and July 2009 was less than $7.50. Column 1 contains the baseline estimate of equation (2) from Clemens and Wither's original analysis. Columns 2 through 6 contain estimates of the SIPP equivalent of (4) with the 5 alternative control sets considered by Zipperer. In Panel A, the dependent variable is the effective minimum wage. Estimates in Panel A thus report the "first stage" relationship. In Panel B, the dependent variable is an indicator for employment. Panel A is thus informative regarding the extent to which Zipperer's coefficients can be explained by the extent to which his controls sets erode the first stage. Across the specifications in this table, erosion of the first stage accounts for 36 percent of the difference. Standard errors allow for correlation clusters across errors at the state level.

Table 4: **Variations Underlying Regression Estimates: CPS Sample**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Control		Treatment		Control	Treatment	Difference
	Baseline	Post	Baseline	Post	Change	Change	
Panel A:	Macroeconomic and Housing Covariates						
Income Per Cap. (1000s)	56.01	54.26	50.29	49.70	-1.75	-0.59	1.16
Employment Rate	0.626	0.578	0.638	0.595	-0.048	-0.043	0.005
Unemployment Rate	4.701	9.495	4.498	8.210	4.794	3.712	-1.082
Med. House Price (Millions)	0.484	0.378	0.303	0.281	-0.106	-0.022	0.084
Const. Output Index (BEA)	109.1	84.25	113.7	101.2	-24.85	-12.5	12.35
Prime Aged Employment	0.796	0.748	0.800	0.760	-0.048	-0.04	0.008
Panel B:	Employment among Low-Skilled Groups						
Young Adult Employment	0.437	0.349	0.469	0.373	-0.088	-0.096	-0.008
Low-Skilled Employment	0.385	0.276	0.421	0.300	-0.109	-0.121	-0.012

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

Table 5: **Variations Underlying Regression Estimates: SIPP Sample**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Control		Treatment		Control	Treatment	Difference
	Baseline	Post	Baseline	Post	Change	Change	
Panel A:	Macroeconomic and Housing Covariates						
Income Per Cap. (1000s)	54.84	54.17	50.34	49.76	-0.67	-0.58	0.09
Employment Rate	0.606	0.581	0.618	0.597	-0.025	-0.021	0.004
Unemployment Rate	7.923	9.570	6.897	8.316	1.647	1.419	-0.228
Med. House Price (Millions)	0.419	0.380	0.309	0.287	-0.039	-0.022	0.017
Const. Output Index (BEA)	107.9	96.21	106.4	97.41	-11.69	-8.99	2.7
Prime Aged Employment	0.774	0.748	0.779	0.760	-0.026	-0.019	0.007
Panel B:	Employment among Low-Skilled Groups						
Low-Skilled Employment	0.672	0.683	0.714	0.683	0.011	-0.031	-0.042

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

Table 6: **Within Region Comparisons of the Recession's Severity**

	(1)	(2)	(3)
	Change from Baseline to Post		
	Bound	Unbound	Diff.
Panel A:		Midwest	
Income Per Cap. (1000s)	-1.01	0.52	1.53
Employment Rate	-0.05	-0.04	0.01
Unemployment Rate	4.043	2.934	-1.109
Med. House Price (Millions)	-0.043	-0.020	0.023
Const. Output Index (BEA)	-19.38	-6	16.31
Prime Aged Emp. Rate	-0.048	-0.033	0.015
Panel B:		Northeast	
Income Per Cap. (1000s)	-0.13	-0.06	0.07
Employment Rate	-0.034	-0.027	0.007
Unemployment Rate	3.886	2.194	-1.692
Med. House Price (Millions)	-0.063	-0.078	-0.015
Const. Output Index (BEA)	-5.8	-23.23	-24.09
Prime Aged Emp. Rate	-0.035	-0.03	0.005
Panel C:		South	
Income Per Cap. (1000s)	-4.03	-0.58	3.45
Employment Rate	-0.06	-0.041	0.019
Unemployment Rate	6.045	3.775	-2.27
Med. House Price (Millions)	-0.161	-0.016	0.145
Const. Output Index (BEA)	-44.83	-11.8	54.1
Prime Aged Emp. Rate	-0.066	-0.039	0.027
Panel D:		West	
Income Per Cap. (1000s)	-2.98	-3.25	-0.27
Employment Rate	-0.056	-0.065	-0.009
Unemployment Rate	5.691	5.096	-0.595
Med. House Price (Millions)	-0.174	-0.071	0.103
Const. Output Index (BEA)	-39.32	-33.4	13.62
Prime Aged Emp. Rate	-0.052	-0.068	-0.016
Panel E:		Averages across Regions	
Income Per Cap. (1000s)			1.572
Employment Rate			0.008
Unemployment Rate			-1.517
Med. House Price (Millions)			0.083
Const. Output Index (BEA)			23.379
Prime Aged Emp. Rate			0.010

Note: This table presents summary statistics on changes in the income per capita, aggregate employment rates, aggregate unemployment rates, median house prices, construction output, and prime aged employment across regions. The weights applied to obtain weighted averages across regions correspond with each region's share of the CPS least-skilled sample. Data come from BEA, BLS, and FHFA.

Table 7: **Employment Estimates in the CPS: Robustness to the Exclusion of FL and AZ (Least Skilled)**

	(1)	(2)	(3)	(4)	(5)	(6)
	Clemens & Wither (Column 1)		Zipperer Specifications (Columns 2 through 6)			
Panel A:	Dependent Variable: Employed — Sample: Full					
Bound x Post 1	-0.037**	-0.022	-0.020	-0.012	-0.014	-0.013
	(0.011)	(0.013)	(0.013)	(0.017)	(0.014)	(0.013)
Bound x Post 2	-0.037**	-0.022	-0.015	-0.003	0.001	0.002
	(0.011)	(0.013)	(0.013)	(0.016)	(0.015)	(0.013)
N	580,248	580,248	580,248	580,248	580,248	580,248
Panel B:	Dependent Variable: Employed — Sample: No FL or AZ					
Bound x Post 1	-0.040***	-0.033*	-0.023*	-0.037**	-0.034*	-0.031*
	(0.011)	(0.013)	(0.011)	(0.012)	(0.014)	(0.015)
Bound x Post 2	-0.041***	-0.032**	-0.020*	-0.028**	-0.022*	-0.021+
	(0.010)	(0.010)	(0.009)	(0.009)	(0.010)	(0.011)
N	550,558	550,558	550,558	550,558	550,558	550,558
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Base Const. by Time	No	Yes	No	No	No	Yes
Base Ind. by Time	No	No	Yes	No	No	No
Region by Time	No	No	No	Yes	No	No
Division by Time	No	No	No	No	Yes	Yes
Dataset	CPS	CPS	CPS	CPS	CPS	CPS
Skill Group	Target	Target	Target	Target	Target	Target

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. This table presents estimates of equations (1) and (4) on samples that include all individuals in the Current Population Survey who are between 16 and 30 years of age and have less than a completed high school education. Column 1 contains the baseline estimate of equation (1) from Clemens and Wither's original analysis. Columns 2 through 6 contain estimates of equation (4) with the 5 alternative control sets considered by Zipperer. Panels differ with regards to the states included. Panel A includes the full sample of states. Panel B excludes observations from Florida and Arizona. A comparison of the results in Panels A and B reveals the extent to which estimates are sensitive to the exclusion of observations from states that had unusually severe housing crises. The results reveal that Clemens and Wither's baseline is not sensitive to this exclusion. By contrast, Zipperer's estimates are quite sensitive. This provides evidence that his estimates are driven in part by the weight his specifications place on comparisons that, upon informed inspection, are prone to significant bias. Standard errors allow for correlation clusters across errors at the state level.

Table 8: **Employment Estimates in the CPS: Robustness to the Exclusion of FL and AZ (Young Adults)**

	(1)	(2)	(3)	(4)	(5)	(6)
	Clemens & Wither (Column 1)		Zipperer Specifications (Columns 2 through 6)			
Panel A:	Dependent Variable: Employed — Sample: Full					
Bound x Post 1	-0.027**	-0.013	0.005	-0.007	-0.009	-0.010
	(0.009)	(0.009)	(0.008)	(0.013)	(0.014)	(0.012)
Bound x Post 2	-0.023*	-0.008	0.001	0.003	0.005	0.005
	(0.010)	(0.009)	(0.008)	(0.010)	(0.010)	(0.009)
N	894,384	894,384	894,384	894,384	894,384	894,384
Panel B:	Dependent Variable: Employed — Sample: No FL or AZ					
Bound x Post 1	-0.029**	-0.017	0.002	-0.021	-0.024	-0.022
	(0.009)	(0.010)	(0.008)	(0.013)	(0.016)	(0.016)
Bound x Post 2	-0.025*	-0.010	-0.003	-0.008	-0.008	-0.008
	(0.010)	(0.009)	(0.007)	(0.008)	(0.008)	(0.008)
N	851,795	851,795	851,795	851,795	851,795	851,795
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Base Const. by Time	No	Yes	No	No	No	Yes
Base Ind. by Time	No	No	Yes	No	No	No
Region by Time	No	No	No	Yes	No	No
Division by Time	No	No	No	No	Yes	Yes
Dataset	CPS	CPS	CPS	CPS	CPS	CPS
Skill Group	Target	Target	Target	Target	Target	Target

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. This table presents estimates of equations (1) and (4) on samples that include all individuals in the Current Population Survey who are between 16 and 21 years of age. Column 1 contains the baseline estimate of equation (1) from Clemens and Wither's original analysis. Columns 2 through 6 contain estimates of equation (4) with the 5 alternative control sets considered by Zipperer. Panels differ with regards to the states included. Panel A includes the full sample of states. Panel B excludes observations from Florida and Arizona. A comparison of the results in Panels A and B reveals the extent to which estimates are sensitive to the exclusion of observations from states that had unusually severe housing crises. The results reveal that Clemens and Wither's baseline is not sensitive to this exclusion. By contrast, Zipperer's estimates are quite sensitive. This provides evidence that his estimates are driven in part by the weight his specifications place on comparisons that, upon informed inspection, are prone to significant bias. Standard errors allow for correlation clusters across errors at the state level.

Table 9: **Employment Estimates in the SIPP: Robustness to the Exclusion of FL and AZ**

	(1)	(2)	(3)	(4)	(5)	(6)
	Clemens & Wither (Column 1)		Zipperer Specifications (Columns 2 through 6)			
Panel A:	Dependent Variable: Employed — Sample: Full					
Bound x Post 1	-0.044*	-0.030	-0.041*	-0.017	-0.012	-0.014
	(0.019)	(0.020)	(0.019)	(0.025)	(0.024)	(0.023)
Bound x Post 2	-0.066**	-0.051*	-0.043*	-0.047*	-0.030	-0.030
	(0.020)	(0.020)	(0.021)	(0.021)	(0.020)	(0.020)
N	147,459	147,459	147,459	147,459	147,459	147,459
Panel B:	Dependent Variable: Employed — Sample: No FL or AZ					
Bound x Post 1	-0.058**	-0.051*	-0.047*	-0.048+	-0.042	-0.041
	(0.018)	(0.023)	(0.021)	(0.027)	(0.030)	(0.029)
Bound x Post 2	-0.072**	-0.058*	-0.040	-0.062*	-0.038	-0.036
	(0.021)	(0.022)	(0.025)	(0.024)	(0.025)	(0.025)
N	137,213	137,213	137,213	137,213	137,213	137,213
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Base Const. by Time	No	Yes	No	No	No	Yes
Base Ind. by Time	No	No	Yes	No	No	No
Region by Time	No	No	No	Yes	No	No
Division by Time	No	No	No	No	Yes	Yes
Dataset	SIPP	SIPP	SIPP	SIPP	SIPP	SIPP
Skill Group	Target	Target	Target	Target	Target	Target

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. This table presents estimates of equation (2) and the SIPP equivalent of equation (4) on samples that include individuals in the Survey of Income and Program Participation whose average wage between August 2008 and July 2009 was less than $7.50. Column 1 contains the baseline estimate of equation (1) from Clemens and Wither's original analysis. Columns 2 through 6 contain estimates of equation (4) with the 5 alternative control sets considered by Zipperer. Panels differ with regards to the states included. Panel A includes the full sample of states. Panel B excludes observations from Florida and Arizona. A comparison of the results in Panels A and B reveals the extent to which estimates are sensitive to the exclusion of observations from states that had unusually severe housing crises. The results reveal that Clemens and Wither's baseline is not sensitive to this exclusion. By contrast, Zipperer's estimates are quite sensitive. This provides evidence that his estimates are driven in part by the weight his specifications place on comparisons that, upon informed inspection, are prone to significant bias. Standard errors allow for correlation clusters across errors at the state level.

Table 10: **Employment Estimates in the CPS: Robustness to Applying Matching Criteria (Least Skilled)**

	(1)	(2)	(3)	(4)	(5)	(6)
	Clemens & Wither (Column 1)		Zipperer Specifications (Columns 2 through 6)			
Panel A:	Dependent Variable: Employed — Sample: Full					
Bound x Post 1	-0.037**	-0.022	-0.020	-0.012	-0.014	-0.013
	(0.011)	(0.013)	(0.013)	(0.017)	(0.014)	(0.013)
Bound x Post 2	-0.037**	-0.022	-0.015	-0.003	0.001	0.002
	(0.011)	(0.013)	(0.013)	(0.016)	(0.015)	(0.013)
N	580,248	580,248	580,248	580,248	580,248	580,248
Panel B:	Dependent Variable: Employed — Sample: Matched within $10K					
Bound x Post 1	-0.032*	-0.018	0.004	-0.029+	-0.041**	-0.048**
	(0.015)	(0.018)	(0.014)	(0.015)	(0.013)	(0.015)
Bound x Post 2	-0.045**	-0.031*	-0.012	-0.031**	-0.032*	-0.037*
	(0.013)	(0.012)	(0.010)	(0.010)	(0.011)	(0.014)
N	284,957	284,957	284,957	284,957	284,957	284,957
Panel C:	Dependent Variable: Employed — Sample: Matched within $20K					
Bound x Post 1	-0.037**	-0.034*	-0.029*	-0.018	-0.021	-0.021
	(0.013)	(0.014)	(0.013)	(0.014)	(0.018)	(0.019)
Bound x Post 2	-0.043**	-0.035**	-0.024*	-0.018	-0.012	-0.013
	(0.012)	(0.012)	(0.010)	(0.012)	(0.012)	(0.012)
N	358,272	358,272	358,272	358,272	358,272	358,272
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Base Const. by Time	No	Yes	No	No	No	Yes
Base Ind. by Time	No	No	Yes	No	No	No
Region by Time	No	No	No	Yes	No	No
Division by Time	No	No	No	No	Yes	Yes
Dataset	CPS	CPS	CPS	CPS	CPS	CPS
Skill Group	Target	Target	Target	Target	Target	Target

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Estimates involve the same sample and organization across columns as the estimates reported in table 5. Panel A includes the full sample of states. Panel B excludes observations from states that could not be matched to another state that had a housing decline within $10,000. Panel C applies a similarly motivated matching criterion of $20,000. A comparison of the across panels reveals the extent to which estimates are sensitive to the exclusion of observations that fail the systematic matching criteria just described. The results reveal that Clemens and Wither's baseline is not sensitive to the application of these criteria. By contrast, Zipperer's estimates are quite sensitive. This provides evidence that his estimates are driven in part by the weight his specifications place on comparisons that, upon informed inspection, are prone to significant bias. Standard errors allow for correlation clusters across errors at the state level.

Table 11: **Employment Estimates in the CPS: Robustness to Applying Matching Criteria (Young Adults)**

	(1)	(2)	(3)	(4)	(5)	(6)
	Clemens & Wither (Column 1)		Zipperer Specifications (Columns 2 through 6)			
Panel A:	Dependent Variable: Employed — Sample: Full					
Bound x Post 1	-0.027**	-0.013	0.005	-0.007	-0.009	-0.010
	(0.009)	(0.009)	(0.008)	(0.013)	(0.014)	(0.012)
Bound x Post 2	-0.023*	-0.008	0.001	0.003	0.005	0.005
	(0.010)	(0.009)	(0.008)	(0.010)	(0.010)	(0.009)
N	894,384	894,384	894,384	894,384	894,384	894,384
Panel B:	Dependent Variable: Employed — Sample: Matched within $10K					
Bound x Post 1	-0.028*	-0.016	0.012	-0.029	-0.044*	-0.043+
	(0.014)	(0.017)	(0.011)	(0.020)	(0.019)	(0.022)
Bound x Post 2	-0.028*	-0.016	-0.005	-0.021*	-0.015	-0.022
	(0.013)	(0.012)	(0.006)	(0.008)	(0.010)	(0.015)
N	442,124	442,124	442,124	442,124	442,124	442,124
Panel C:	Dependent Variable: Employed — Sample: Matched within $20K					
Bound x Post 1	-0.021*	-0.013	0.000	-0.002	-0.003	-0.001
	(0.009)	(0.009)	(0.008)	(0.010)	(0.010)	(0.009)
Bound x Post 2	-0.024*	-0.014	-0.005	-0.007	0.000	-0.000
	(0.012)	(0.011)	(0.008)	(0.009)	(0.010)	(0.010)
N	564,164	564,164	564,164	564,164	564,164	564,164
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Base Const. by Time	No	Yes	No	No	No	Yes
Base Ind. by Time	No	No	Yes	No	No	No
Region by Time	No	No	No	Yes	No	No
Division by Time	No	No	No	No	Yes	Yes
Dataset	CPS	CPS	CPS	CPS	CPS	CPS
Skill Group	Target	Target	Target	Target	Target	Target

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Estimates involve the same sample and organization across columns as the estimates reported in table 6. Panel A includes the full sample of states. Panel B excludes observations from states that could not be matched to another state that had a housing decline within $10,000. Panel C applies a similarly motivated matching criterion of $20,000. A comparison of the across panels reveals the extent to which estimates are sensitive to the exclusion of observations that fail the systematic matching criteria just described. The results reveal that Clemens and Wither's baseline is not sensitive to the application of these criteria. By contrast, Zipperer's estimates are quite sensitive. This provides evidence that his estimates are driven in part by the weight his specifications place on comparisons that, upon informed inspection, are prone to significant bias. Standard errors allow for correlation clusters across errors at the state level.

Table 12: **Employment Estimates in the SIPP: Robustness to Applying Matching Criteria**

	(1)	(2)	(3)	(4)	(5)	(6)
	Clemens & Wither (Column 1)		Zipperer Specifications (Columns 2 through 6)			
Panel A:	Dependent Variable: Employed — Sample: Full					
Bound x Post 1	-0.044*	-0.030	-0.041*	-0.017	-0.012	-0.014
	(0.019)	(0.020)	(0.019)	(0.025)	(0.024)	(0.023)
Bound x Post 2	-0.066**	-0.051*	-0.043*	-0.047*	-0.030	-0.030
	(0.020)	(0.020)	(0.021)	(0.021)	(0.020)	(0.020)
N	147,459	147,459	147,459	147,459	147,459	147,459
Panel B:	Dependent Variable: Employed — Sample: Matched within $10K					
Bound x Post 1	-0.050+	-0.026	-0.054	-0.037	-0.046	-0.029
	(0.026)	(0.040)	(0.035)	(0.038)	(0.044)	(0.048)
Bound x Post 2	-0.056+	-0.023	-0.063*	-0.047	-0.059+	-0.046
	(0.029)	(0.040)	(0.030)	(0.032)	(0.032)	(0.040)
N	72,443	72,443	72,443	72,443	72,443	72,443
Panel C:	Dependent Variable: Employed — Sample: Matched within $20K					
Bound x Post 1	-0.064**	-0.056**	-0.061**	-0.089***	-0.094***	-0.090***
	(0.018)	(0.020)	(0.019)	(0.021)	(0.025)	(0.025)
Bound x Post 2	-0.073**	-0.054*	-0.047*	-0.081**	-0.084***	-0.079***
	(0.021)	(0.023)	(0.023)	(0.022)	(0.021)	(0.021)
N	98,932	98,932	98,932	98,932	98,932	98,932
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Base Const. by Time	No	Yes	No	No	No	Yes
Base Ind. by Time	No	No	Yes	No	No	No
Region by Time	No	No	No	Yes	No	No
Division by Time	No	No	No	No	Yes	Yes
Dataset	SIPP	SIPP	SIPP	SIPP	SIPP	SIPP
Skill Group	Target	Target	Target	Target	Target	Target

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Estimates involve the same sample and organization across columns as the estimates reported in table 7. Panel A includes the full sample of states. Panel B excludes observations from states that could not be matched to another state that had a housing decline within $10,000. Panel C applies a similarly motivated matching criterion of $20,000. A comparison of the across panels reveals the extent to which estimates are sensitive to the exclusion of observations that fail the systematic matching criteria just described. The results reveal that Clemens and Wither's baseline is not sensitive to the application of these criteria. By contrast, Zipperer's estimates are quite sensitive. This provides evidence that his estimates are driven in part by the weight his specifications place on comparisons that, upon informed inspection, are prone to significant bias. Standard errors allow for correlation clusters across errors at the state level.

Table 13: **Employment Estimates in the CPS: All Skill Groups**

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Employed					
Skill Group:	Least	Middle	Highest	Young	Middle	Old
Panel A:	Specification: Clemens & Wither					
Bound x Post 1	-0.037**	-0.010	-0.004	-0.027**	-0.004	-0.004
	(0.011)	(0.008)	(0.005)	(0.009)	(0.006)	(0.005)
Bound x Post 2	-0.037**	-0.006	0.001	-0.023*	0.001	-0.001
	(0.011)	(0.008)	(0.005)	(0.010)	(0.006)	(0.006)
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B:	Specification: Zipperer with Industry x Time Controls					
Bound x Post 1	-0.020	0.016**	0.010**	0.005	0.010*	0.008*
	(0.013)	(0.006)	(0.003)	(0.008)	(0.005)	(0.004)
Bound x Post 2	-0.015	0.013+	0.014***	0.001	0.012*	0.012**
	(0.013)	(0.006)	(0.003)	(0.008)	(0.005)	(0.004)
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry by Time	Yes	Yes	Yes	Yes	Yes	Yes
Panel C:	Specification: Zipperer with Division x Time Controls					
Bound x Post 1	-0.014	0.005	0.004	-0.009	0.003	0.006
	(0.014)	(0.008)	(0.005)	(0.014)	(0.006)	(0.006)
Bound x Post 2	0.001	0.008	0.004	0.005	0.006	0.003
	(0.015)	(0.008)	(0.004)	(0.010)	(0.005)	(0.005)
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Division by Time	Yes	Yes	Yes	Yes	Yes	Yes
N	580,248	1,814,333	4,905,828	894,384	3,515,120	2,890,905
Dataset	CPS	CPS	CPS	CPS	CPS	CPS

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Each column in each panel presents estimates from equation (1) or (4). Estimates in columns 1 through 3 partition the CPS across age by education categories. The Least skilled are individuals ages 16 to 30 with less than a completed high school education. The Middle skilled include older high school dropouts and high school graduates ages 16 to 30. The Highest skilled include the remainder of the working age population. Columns 4 through 6 similarly partition the population by age. The three groups are ages 16 to 21, ages 22 to 45, and ages 46 to 65. Panel A reports estimates of the Clemens and Wither baseline. It shows that the differential employment declines estimated by Clemens and Wither load exclusively onto the Least skilled. By contrast, panels B and C show that Zipperer's estimates implausibly suggest that minimum wage increases led to substantial increases in employment among high skilled groups. This is particularly true in the Panel B specification, which includes time effects interacted with baseline industry share variables. This is the most economically motivated of Zipperer's control sets, and the obvious implausibility of the estimates it generates reveals his "controls" to be a source of substantial bias. Standard errors allow for correlation clusters across errors at the state level.

Table 14: **Employment Estimates in the SIPP: All Skill Groups**

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Employed				
Skill Group:	Unemp. at Base	Under $7.50	$7.50-$8.49	$8.50-$9.99	Over $10.00
Panel A:	Specification: Clemens & Wither				
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)
State and Time FE	Yes	Yes	Yes	Yes	Yes
Panel B:	Specification: Zipperer with Industry x Time Controls				
Bound x Post 1	-0.011*	-0.041*	0.045*	-0.005	0.008*
	(0.004)	(0.019)	(0.022)	(0.017)	(0.003)
Bound x Post 2	-0.021*	-0.043*	0.016	0.008	0.010+
	(0.008)	(0.021)	(0.028)	(0.016)	(0.005)
State and Time FE	Yes	Yes	Yes	Yes	Yes
Industry by Time	Yes	Yes	Yes	Yes	Yes
Panel C:	Specification: Zipperer with Division x Time Controls				
Bound x Post 1	-0.009	-0.012	0.041	-0.006	0.008*
	(0.006)	(0.024)	(0.030)	(0.020)	(0.004)
Bound x Post 2	-0.011	-0.030	0.008	0.016	0.008+
	(0.010)	(0.020)	(0.029)	(0.022)	(0.004)
State and Time FE	Yes	Yes	Yes	Yes	Yes
Division by Time	Yes	Yes	Yes	Yes	Yes
N	523,086	147,459	102,193	122,786	1,076,148
Dataset	SIPP	SIPP	SIPP	SIPP	SIPP

Note: +, *, **, and *** indicate statistical significance at the 0.10, 0.05, 0.01, and 0.001 levels respectively. Each panel presents estimates from equation (2) or the SIPP equivalent of equation (4). Across the 5 columns the samples partition the working age SIPP population according to baseline employment status and wage rates. The sample in each column is described in the column heading. Panel A reports estimates of the Clemens and Wither baseline. It shows that the differential employment declines estimated by Clemens and Wither load exclusively onto a combination of the lowest wage group and individuals who lacked employment throughout the baseline. By contrast, panels B and C show that Zipperer's estimates implausibly suggest that minimum wage increases led to substantial increases in employment among high skilled groups. The implausibility of these estimates reveal that the inclusion of Zipperer's "controls" sets introduce substantial bias. Indeed, if interpreted at face value, his estimates suggest that increasing the minimum wage by an additional $3 would have prevented employment from declining by even a single percentage point during the Great Recession. Standard errors allow for correlation clusters across errors at the state level.