

The Heterogeneous Effects of Large and Small Minimum Wage Changes: Evidence Using a Partially Pre-Committed Analysis Plan

by

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**Abstract**

This paper advances the use of partially pre-committed analysis plans in non-experimental research settings. In a study of recent minimum wage changes, we demonstrate how analyses of longer-run impacts of policy interventions can be pre-specified as extensions to very short-run analyses. Further, our pre-analysis plan includes comparisons of the effects of large vs. small minimum wage increases, which is a theoretically motivated dimension of heterogeneity. We discuss how these use cases harness the strengths of pre-analysis plans while mitigating their weaknesses. This project's initial analyses explored CPS and ACS data from 2011 through 2015. Alongside these analyses, we pre-committed to analyses incorporating CPS and ACS data extending through 2019. Averaging across the specifications in our pre-analysis plan, we estimate that relatively large minimum wage increases reduced employment rates among individuals with low levels of experience and education by just over 2 and a half percentage points during the decade prior to the onset of the Covid-19 pandemic. Our estimates of the effects of relatively small minimum wage increases vary across data sets and specifications but are, on average, both economically and statistically indistinguishable from zero. We estimate that the elasticity of employment with respect to the minimum wage is substantially more negative for large minimum wage increases than for small increases.

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Pre-analysis plans have the potential to increase the transparency and reproducibility of empirical research (Christensen and Miguel, 2018; Janzen and Michler, 2021). They are rare outside of experiments, however, in part because it may be unclear how to operationalize them when analyzing observational data. This paper seeks to illustrate use cases in which pre-analysis plans have high utility when applied to observational data, while also providing a model of how to operationalize such a plan. In addition, the results of our pre-committed analysis advance the contentious minimum-wage literature along potentially fruitful and understudied dimensions.

In this paper, we present the results from a pre-committed analysis of the employment effects of minimum wage changes enacted during the decade preceding the Covid-19 pandemic. We began this project by releasing an NBER working paper in January 2017 in which we analyzed the very-short-run effects of minimum wage changes enacted between 2013 and 2015 using data from the Current Population Survey (CPS) and in which we committed to estimating pre-specified models using future releases of CPS and American Community Survey (ACS) data. In this paper's Section VI and VIII respectively, we present the results from our pre-analysis plan alongside supplemental analyses using modern difference-in-differences methods.

Two features of our pre-analysis plan, which is outlined in Section V, merit discussion at the outset, as they are relevant to understanding key dimensions of our findings. First, our pre-analysis plan includes theoretically motivated comparisons of the effects of large vs. small minimum wage increases. Second, we demonstrate how analyses of relatively long-run impacts of policy interventions can be pre-specified as extensions to short-run analyses. In the following section, we further discuss how these dimensions of our study can help to inform the use of pre-analysis plans in non-experimental settings in future work.

Our analysis was spurred by the fact that the past decade of state and federal minimum

wage policy created an attractive opportunity to analyze the employment effects of minimum wage increases using a pre-analysis plan. After the Great Recession, there was a pause in both state and federal efforts to increase minimum wages, which was followed by considerable divergence in states' policies. Many states legislated and enacted minimum wage changes that varied substantially in their magnitude. From January 2011 to January 2019, for example, Washington, D.C., California, and New York had increased their minimum wages by 61, 50, and 53 percent, respectively. Wage floors rose more moderately in an additional 24 states and were unchanged in the remainder. The past decade thus provided a suitable opportunity to study the effects of both moderate minimum wage changes and historically large minimum wage changes. By contrast, the average increase across the 138 minimum wage increases analyzed by Cengiz *et al.* (2019) averaged just over eight log points.

The results from our pre-analysis plan (see Section VI) are as follows. First, we estimate that relatively large increases in minimum wages reduced employment rates among individuals with low levels of experience and education by just over 2 and a half percentage points. Second, our estimates of the effects of moderate minimum wage increases are centered on zero, as are our estimates of the effects of minimum wage increases linked to inflation-indexing provisions. Finally, we find that the effects of large minimum wage changes have increased in magnitude as time has passed since their enacting legislation. Because large cumulative increases were phased in over a number of years, we emphasize that it is not generally feasible to distinguish between the short-and-medium run effects of the initial increments of the legislated increase, on the one hand, and the contemporaneous effects of a large cumulative increase, on the other hand.

Although our estimation frameworks are pre-committed, it is nonetheless important to assess their internal validity. On this issue, we highlight two developments. Historically, there

have been heated debates over research designs for studying minimum wages. There is broad agreement, however, regarding the validity of the frameworks to which we pre-committed for analyzing precisely the set of minimum wage changes we analyze. Notably, Cengiz *et al.* (2019) argue that a variety of estimators produce unbiased estimates of the effects of minimum wage changes enacted between 1992 and 2016.<sup>2</sup> Additional recent research has supported this assessment more specifically in our context, which focuses on minimum wage increases enacted during the 2010s. Gopalan *et al.* (2021), for example, use event-based difference-in-differences style analyses to estimate wage and employment effects of minimum wage increases using administrative employment records from 2010 to 2015. Clemens, Kahn, and Meer (2021) use similar research designs in analyses of vacancy postings and of the substitution of low-skilled workers for moderately higher-skilled workers using data from 2011 to 2016.

Second, Section VIII presents estimates using the “imputation” estimator of Borusyak, Jaravel, and Spiess (2024), which has attractive properties for our setting. Appendix B further explores results from several alternative event study estimators. We obtain qualitatively similar results, namely null effects of relatively small minimum wage increases and negative effects that rise over time when states enacted large, multi-phase minimum wage increases.

Our analysis contributes to both the minimum wage literature and the broader literature on empirical program evaluation. Our pre-committed analyses are designed to differentiate between the effects of large and small minimum wage increases, as well as between the short- and longer-run effects of the underlying enacting legislation. We emphasize that our empirical interest in these dimensions of heterogeneity is motivated by economic theory. To the best of our knowledge, this makes our study the first to develop a pre-analysis plan with a focus on using

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<sup>2</sup> Cengiz *et al.* (2019) argue that two-way fixed effects analyses of minimum wage changes enacted prior to 1992 are prone to biases, but not recent minimum wage changes.

heterogeneity to examine the predictions of economic models in an analysis of non-experimental data. Our contribution to the minimum wage literature is to provide transparent evidence that large and small minimum wage changes have qualitatively different effects. Employment responds more elastically to large minimum wage increases than to modest increases when both are enacted from a baseline of moderately binding minimum wage levels (as proxied, for example, by the baseline ratio of the minimum wage to the median wage).

While papers on the minimum wage's employment effects have filled volumes,<sup>3</sup> analyses of how elasticities vary with the size of the minimum wage increase are much less common. A related, though distinct, example is Cengiz *et al.*'s (2019) analysis of heterogeneity with respect to the ratio of the minimum wage relative to the median wage at endline. A notable point of comparison is that although even the “small” minimum wage increases we analyze are substantial, they rose essentially on pace with states' median wages, whereas the “large” minimum wage increases we analyze led to substantial increases in the ratio of the minimum wage to the median wage.

Our finding that large minimum wage changes have substantial employment effects while small minimum wage changes have modest effects has implications for policy forecasts. While evidence on this issue is sparse, the idea that large minimum wage changes may have more sharply negative employment effects than small minimum wage changes is motivated by a rich

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<sup>3</sup> Book-length assessments of the effects of minimum wages include Card and Krueger (1995), Neumark and Wascher (2008), and Belman and Wolfson (2014). Recent studies of the minimum wages' effects on employment include papers on minimum wage increases in the United States across recent decades (Meer and West, 2016; Cengiz *et al.*, 2019; Cengiz *et al.*, 2022; Powell 2021); work by Kreiner, Reck, and Skov (2020) and Kabátek (2021) on age-based discontinuities in minimum wages in Denmark and the Netherlands, respectively; two papers studying the effects of minimum wage increases enacted during the Civil Rights era (Derenoncourt and Montialoux, 2021; Bailey, DiNardo, and Stuart, 2021); Clemens and Wither (2019) on the minimum wage increases enacted during the Great Recession; Harastozzi and Lindner (2019) on the effects of large minimum wage increases enacted in Hungary; Jardim *et al.* (2022) on the minimum wage increases enacted by the city of Seattle; Brummund and Strain (2020) on the employment effects of indexing minimum wages to inflation; and multiple papers focused on the short-run effects of the past decade's minimum wage increases (Gopalan *et al.*, 2021; Clemens and Strain, 2018b).

set of theoretical models of labor markets. Specifically, when minimum wages are moderately binding at baseline, large increases are more likely to bring the minimum wage to levels at which it binds negatively on employment (e.g., as in Berger, Herkenhoff, and Mongey, 2022).

In addition to the employment effects of minimum wage increases, this paper contributes to the broader literature on empirical program evaluation. As discussed in the following section, pre-analysis plans are far more common in the context of experiments than in observational studies, reflecting in part that there is far less agreement regarding best-practice protocols outside of experiments. We emphasize that both fully pre-committed analyses and fully flexible analyses have substantial weaknesses outside of experimental settings, the latter due to “p-hacking” and the former due to impracticability. This motivates our exploration of intermediate degrees of pre-commitment, with a goal of preserving some of the core strengths of pre-analysis plans while shedding some of their weaknesses.

Our paper proceeds as follows. Section II discusses the role of pre-analysis plans outside of experimental settings. Section III provides background regarding the minimum wage changes we analyze. Section IV discusses the primary data sources we use. Section V describes our pre-committed estimation frameworks, and Section VI summarizes the results of these pre-committed analyses. Section VII discusses the elasticities implied by the employment and wage impacts we estimate. Section VIII presents estimates from a modern Difference-in-Differences estimator that falls outside of our pre-analysis plan, and Section IX concludes.

## **Section II: The Design of Pre-Analysis Plans Outside of Experimental Settings**

In experimental research, the importance of pre-analysis plans for reducing the risk of p-hacking is widely recognized. While p-hacking poses a substantial threat to the reproducibility of

both experimental and non-experimental research, pre-analysis plans are uncommon outside of experimental settings (Christensen and Miguel, 2018).<sup>4</sup> This is linked in part to the difficulty, which may rise to the level of impracticability, of implementing a “pure” pre-analysis plan for the purpose of estimating long-run effects of non-experimental policy interventions using observational data.<sup>5</sup> In non-experimental settings, then, both fully constrained (i.e., studies with pure pre-analysis plans) and fully flexible (i.e., studies without pre-analysis plans) research face substantial limitations — issues of practicality for the former and p-hacking for the latter. An important methodological question, then, is whether there are dimensions of pure pre-analysis plans that can usefully be retained such that the gains from reductions in p-hacking are large relative to the losses in the practicability of non-experimental research.

Our analysis attempts to make progress in defining the boundaries of intermediate (or partial) degrees of pre-commitment. The key tradeoff is that our we relax the purity of pre-analysis plans from experimental settings by constructing longer-run, pre-committed analyses as extensions to short-run analyses that were not pre-committed.<sup>6</sup> We view the core trade-off we have adopted as attractive for two reasons. First, the pursuit of “pure” pre-analysis plans pushes researchers towards very short-run analyses, which will not tend, by definition, to capture the

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<sup>4</sup> Christensen and Miguel (2018) point out that pre-committed observational studies are quite rare because of their difficulty, as such studies require that researchers be “intimately familiar” with their subject matter. This includes the need for a detailed, forward-looking knowledge of the policy environment. Such studies also require recognizing best-practice research designs for use in a partially unknown research environment. The key advantage of such studies, when implemented successfully, is their potential to reduce concerns related to data mining.

<sup>5</sup> A prior paper by Neumark (2001) and a more recent paper by Neumark and Yen (2022) provide rare examples of the use of pre-analysis plans to analyze the short-run employment effects of minimum wage increases. Notably, both Neumark (2001) and Neumark and Yen (2022) interpret their findings as inconclusive. Neumark’s (2001) abstract attributes this to the possibility that “The limited data to which the pre-specified research design can be applied may preclude finding many significant effects.” Neumark and Yen (2022) conclude that “All told, we view the results as providing neither strong evidence of substantial adverse effects of city minimum wages, nor strong evidence of substantial beneficial effects.”

<sup>6</sup> Readers interested in the development of our pre-analysis plan should turn to the first two papers from our project (Clemens and Strain, 2017, 2018b), as well as the intermediate project milestones in which we incorporated ACS and CPS data from 2016, 2017, and 2018 (Clemens and Strain, 2018a, 2019, 2020). The current paper, which incorporates 2019 data from the ACS and CPS, presents the conclusion of our pre-committed analyses.

long-run policy parameters that are more central for policy evaluation. Second, we observe that conventional, flexible explorations of short-run impacts may be crucial for the development of credible research designs on which pre-committed analyses of long-run effects can be built. Because it takes time for researchers to build consensus regarding which research strategies are appropriate for analyzing a novel setting, it may rapidly become too late to propose a “pure” and “credible” pre-analysis plan for analyzing short-run effects. Consequently, we focus on the use of pre-commitment to reduce p-hacking concerns in the development of longer-run analyses.

A potential threat to any long-run analysis, whether pre-specified or not, is the possibility that economic shocks may create biases that rear their heads in the latter years of an analysis. To the extent that such shocks are standard, e.g., due to normal variations in the strength of contemporaneous macroeconomic fluctuations, we illustrate how robustness checks can be pre-specified. The Covid-19 pandemic illustrates a form of extreme shock that would not be readily addressable through pre-specification. A trade-off we explore in our analysis involves the possibility of pre-specifying narrowly defined degrees of freedom, which relaxes the purity of a pre-analysis plan, but does so in a way that mitigates the risk of a study’s failure due to events that could not plausibly have been foreseen.<sup>7</sup> There are advantages, for example, to allowing some degree of pre-specified flexibility on the control variables an analysis might consider, or to allowing regression specifications to adapt to reflect policy variation that could not have been fully anticipated at the time a pre-analysis plan was written. On the other hand, the more flexibility one introduces, the less the analysis is pre-committed.

To help speak to this methodological trade-off, we present two sets of summary

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<sup>7</sup> Neumark and Yen (2022) question the feasibility of such an approach, writing that “While a PAP could potentially lay out a ‘decision tree’ of how the data will be analyzed and which analyses conducted based on the findings, this can be unwieldy in practice.” Our analysis illustrates how such efforts can be undertaken.



estimates. One strictly adheres to the specifications that were included in the working paper that laid out our pre-analysis plan. The other incorporates refinements that were consistent with dimensions of flexibility we had discussed in our original pre-analysis plan.<sup>8</sup> Reassuringly, the summary estimates from both approaches are very similar. We view the approach of simultaneously presenting estimates from both a rigidly defined pre-analysis plan and a pre-commitment plan that embeds dimensions of flexibility as an attractive approach for executing pre-commitment plans outside of experimental settings.<sup>9</sup>

A key feature of our use case for pre-analysis plans is its emphasis on dimensions of heterogeneous treatment effects that are designed to provide insight into economic models. Theoretically motivated analyses of heterogeneity are a potentially important strength of pre-analysis plans in non-experimental settings because heterogeneity analyses are more prone to data mining concerns than are analyses of overall average treatment effects. This reflects the fact that, absent a pre-analysis plan, researchers can select the subgroup analyses or interaction effects they emphasize on an ex-post, potentially “p-hacked” basis (Gelman and Loken, 2013).

This is an important use case, as heterogeneity analyses are often the primary analyses through which program evaluation research connects to economic theory. Comparisons of minimum wage treatment effects in “high” and “low” concentration labor markets, for example,

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<sup>8</sup> These refinements come along two dimensions. First, the analyses in the published version of our “short run” analyses (Clemens and Strain, 2018b) were refined such that two of the reported difference-in-differences specifications include a control variable, namely the log of state quarterly personal income per capita, which was not included in the original working paper’s specifications. Notably, the potential inclusion of this covariate was itself pre-specified in the initial working paper as an example of the sort of covariate that might ex post be viewed as relevant to the analysis. The second refinement was to incorporate sets of specifications that allow our groupings of states across policy categories to account for policy changes that were enacted after the distribution of our pre-analysis plan.

<sup>9</sup> We note that to guard as strongly against data mining as possible, it is desirable that even the pre-committed dimensions for refinement be resolved as early in a project’s execution as possible. In our case, the refinement of adding an additional control variable to a subset of our difference-in-differences estimators was incorporated as of our first analysis using 2011-2015 ACS data (Clemens and Strain, 2018b). Additionally, the refinements through which we accounted for policy changes that post-dated our pre-commitment were incorporated as of our analysis of data from 2016 (Clemens and Strain, 2018a).

are central to efforts to understand the importance and implications of market power (Okudaira, Takizawa, and Yamanouchi, 2019; Azar *et al.*, 2024). We note that data mining concerns can threaten the validity of the use of subgroup analyses to test the relative importance of alternative economic theories. Pre-analysis plans have the potential to overcome this threat and, by extension, to solidify the scientific basis for theory-testing exercises.

### **Section III: Background on State Minimum Wage Changes Between 2011 and 2019**

During the years following the Great Recession, there was a pause in both state and federal efforts to increase minimum wages. Subsequently, states diverged substantially in their minimum wage policies. This environment offered an opportunity to conduct relatively transparent labor market analyses using standard program evaluation methods.

Our pre-analysis plan divides states into policy groups based on their minimum wage regimes. A key aspect of our pre-analysis plan is that it incorporates heterogeneity in the minimum wage's effects along dimensions that are of long-standing theoretical interest. Specifically, our analysis plan differentiates between the short- and longer-run effects of minimum wage legislation, between the effects of large and small minimum wage changes, and between the effects of newly legislated minimum wage changes and forecastable changes that are driven by inflation-indexing provisions.

We divide states into four groups designed to track several plausibly relevant differences in their minimum wage regimes. The first group consists of states that enacted no minimum wage changes between January 2013 and the later years of our sample. The second group consists of states that enacted minimum wage changes due to prior legislation that calls for indexing the minimum wage for inflation. The third and fourth groups consist of states that have enacted minimum wage changes through relatively recent legislation. We divide the latter set of

states into two groups based on the size of their minimum wage changes and based on how early in our sample they passed the underlying legislation.

Updates to states' minimum wage policies pose challenges to the development of pre-analysis plans. Notably, several of the states that entered our analysis sample with inflation-indexing provisions subsequently enacted minimum wage changes through new statutes. Our approach is thus to present three sets of results. We first present results that hold fixed the policy groupings we adopted in our initial analyses, for which our analysis samples extended through 2015. Second, we present results on samples that exclude states that legislated substantial minimum wage changes after our initial analyses. Third, we present results for which we adjust our groupings of states to account for minimum wage changes enacted as of January 2018.

The maps in Figure 1 present the full divisions of states associated with the policy groupings we use. As shown in the maps, several states shift between the "large" and "small" change groups as we move from the grouping based on changes enacted through January 2015 to the grouping that incorporates changes enacted between January 2015 and January 2018. Figure 2 illustrates the dynamics of the changes in the average effective minimum wage rates across the groupings displayed in Panel A of Figure 1, with Panel A presenting the average level of the nominal minimum wage in each grouping and Panel B presenting the Kaitz Index (i.e., the ratio of the minimum wage to the median wage). While the nominal minimum wage rose non-trivially in our grouping of small increases, the associated Kaitz Index rose modestly (by roughly 0.03) due to contemporaneous growth in median wages. It is only in our grouping of states with large increases that the Kaitz Index rose substantially from baseline to endline (by roughly 0.12). A comparison of Figure 2 with Appendix Figure 2 reveals that updating our groupings of states to reflect minimum wage increases that were passed after the development of our pre-analysis plan

has little impact on the endline differences in the minimum wage increases enacted by our groupings of large increases, small increases, and states with no minimum wage changes.

We note that both the “small” and “large” minimum wage changes we analyze are substantial relative to historical minimum wage changes. In our sample, the longer-run increases (meaning those through January 2019) average roughly 25 log points within our “small” group and 35 log points within our “large” group (see Appendix Figure A1). With respect to the bite of these minimum wage increases, we emphasize, as illustrated in Panel B of Figure 1, that the ratio of the minimum wage to the median wage rose substantially in the latter group but modestly in the former group; even the non-trivial nominal increases enacted in our group of “small” minimum wage increases were roughly on pace with growth in median wages.

#### **Section IV: Data Sources**

Our primary data sources are the American Community Survey (ACS) and the Current Population Survey (CPS).<sup>10</sup> The ACS is the largest publicly available household survey data set containing the information required for our analysis, while the CPS is a common resource for estimating standard employment statistics across geographic areas and demographic groups. Kromer and Howard (2010) document differences in the sampling procedures and survey questions posed in the ACS relative to the smaller and more commonly analyzed CPS.<sup>11</sup>

Table 1 presents summary statistics on the primary ACS samples we analyze (equivalent summary statistics from our CPS samples appear in Appendix Table A2). The first sample,

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<sup>10</sup> The remainder of this section quotes liberally from the text of this project’s previous analyses.

<sup>11</sup> As we have summarized previously, “The sampling universes of the ACS and CPS differ in that the ACS includes individuals residing in institutionalized group quarters while the CPS does not. The inclusion of these individuals in our primary analysis samples does not materially affect our results. Respondents to both surveys answer questions describing their employment status over the course of a reference week. In the ACS, the reference week is the previous calendar week; in the CPS, the reference week is the week containing the 12th day of the month.”

described in Columns 1 and 2, consists of individuals ages 16 to 25 with less than a completed high school education. The second sample, described in Columns 3 and 4, consists of all individuals ages 16 to 21. Columns 1 and 3 present data from 2011 to 2013, while Columns 2 and 4 present data from 2015 to 2019. From the baseline to the later years in our sample, employment rates rose for both groups, as did house prices and aggregate *per capita* incomes.

We supplement the ACS and CPS data with data on macroeconomic covariates. Specifically, we investigate the relevance of departures in economic conditions across our policy groupings, which could bias our estimates, by tracking indicators of the performance of state-level housing markets, state aggregate income *per capita*, and labor markets.

Figure 3 presents time series on median house prices (Panel A) and aggregate income (Panel B) across the policy groups we analyze, namely states that enacted large minimum wage increases, small minimum wage increases, inflation-indexed minimum wage increases, and no minimum wage increases. Table 2 summarizes in sample changes in these macroeconomic covariates, as well as in employment among prime age adults (ages 26–54) and among a group consisting of individuals ages 21–30 with high school degrees and individuals ages 31–64 with less than a completed high school degree. The latter individuals thus have education and/or experience modestly beyond that obtained by most minimum wage workers.

The house price index reveals that the housing recovery following the Great Recession was strong in states that enacted relatively large minimum wage increases. Median house prices rose by roughly 46 percent in this group of states from the 2011–2013 base period through 2019. They rose by 60 percent in states that indexed their minimum wage rates to inflation. Across states that did not increase their minimum wage rates, house prices rose 35 percent, and in states that enacted small minimum wage increases, median house prices rose by 31 percent. The BEA’s

income data show that *per capita* incomes grew roughly \$7,500 more in states that enacted relatively large minimum wage changes than in states that enacted no minimum wage changes.<sup>12</sup> Underlying macroeconomic conditions improved to economically and statistically significantly greater degrees in states that enacted large minimum wage changes than in other states.

The employment series for prime age individuals also suggests that underlying economic conditions were stronger in states that enacted minimum wage increases than in states that did not. From the 2011–2013 baseline through 2019, prime age employment grew by an average of 5.3 percentage points in states that either enacted large minimum wage changes or that indexed their minimum wage rates to inflation. Across states that enacted no minimum wage increases, the prime age employment rate increased by a more modest average of 4.0 percentage points.

Table 2 also presents tabulations of employment rates in our primary analysis samples. Employment among individuals ages 16 to 25 with less than a completed high school education (“Low-Skilled Employment”), as measured in the ACS, expanded 4.0 percentage points less by 2019 in states that enacted large minimum wage changes than in states that enacted no minimum wage change. In the CPS (Table A4), the measured difference was -3.2 percentage points. Among all individuals ages 16 to 21, the difference in the ACS is -1.4 percentage points, while the difference measured in the CPS is -1.1 percentage points.<sup>13</sup>

Employment changes among individuals in states with small minimum wage changes diverge when comparing ACS and CPS data. In the ACS, employment among low-skilled

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<sup>12</sup> Although per capita incomes were not included in this project’s initial analysis, the divergence in per capita incomes across groups was quite apparent when we constructed an early version of Panel B of Figure 3 for our analysis of 2011 to 2015 ACS data (Clemens and Strain, 2018b). This is why, consistent with a pre-specified dimension of refinement to our pre-specified regressions, we incorporated per capita income as a control variable for subsequent analyses. Our initial focus on the FHFA housing price index as a macroeconomic control variable was motivated by analyses of the minimum wage increases enacted during the Great Recession (Clemens and Wither, 2019). In that context, there was a strong mapping between the housing market and overall macroeconomy.

<sup>13</sup> Additional tabulations of interest from ACS data, as well as CPS data, appear in Appendix Tables A3-A8.

individuals rose modestly less in these states relative to individuals in states that enacted no minimum wage changes. In the CPS, by contrast, employment among low-skilled individuals rose nontrivially more in these states than in states that enacted no minimum wage changes.

## Section V: Framework for Estimating the Effects of Minimum Wage Changes

This section presents our regression frameworks for estimating the effects of recent minimum wage increases, following the pre-analysis plan in Clemens and Strain (2017, 2018b). Much of this section’s text is thus largely unchanged from these earlier papers.

Our analysis plan adopts a program evaluation approach in which we divide states into groups based on the minimum wage policy changes they legislated early in the time period we analyze. We estimate standard difference-in-differences and triple-difference specifications to identify differential changes in employment among relatively low-skilled population groups. Our basic difference-in-differences specification is presented in equation (1):

$$Y_{i,s,g(s),t} = \sum_{g(s) \neq 0} \beta_{g(s)} Policy_{g(s)} \times Post_t + \alpha_{1s} State_s + \alpha_{2t} Time_t + X_{i,s,t} \gamma + \varepsilon_{i,s,t}, \quad (1)$$

where  $Y_{i,s,g(s),t}$  is a binary indicator of the employment of individual  $i$ , living in state  $s$ , which falls in policy category  $g(s)$ , in year  $t$ . We estimate equation (1) on samples restricted to the population groups most likely to be affected by the minimum wage, namely young adults (ages 16 to 21) and individuals ages 16 to 25 with less than a completed high school education.

Equation (1) includes standard controls for sets of state and time fixed effects. The vector  $X$  contains sets of control variables that vary across the specifications we estimate. In various specifications, it contains the median house price index, the log of aggregate personal income *per capita*, the employment rate among individuals with moderately higher skill levels than the individuals in the analysis sample, and individual-level demographic characteristics.

$Policy_{g(s)}$  represents binary indicators for whether a state fits into a given policy group. As discussed above, we differentiate among states that increased their minimum wage rates due to inflation-indexing provisions, states that enacted relatively large statutory increases in total, and states that enacted relatively small statutory increases in total. The omitted group is group  $g = 0$ , which represents states that did not increase their minimum wage rates.

The coefficients of interest are the  $\beta_{g(s)}$  on the interaction between  $Policy_{g(s)}$  and  $Post_t$ . For estimates of equation (1), we treat 2014 as a transition year and thus exclude it from the sample. Our initial specifications update the estimates from Clemens and Strain (2017, 2018a, 2018b, 2019, 2020) by simply adding 2019 to the sample. For this analysis,  $Post_t$  is an indicator for observations that occur in 2015, 2016, 2017, 2018, or 2019.  $\beta_{g(s)}$  thus describes differential changes in employment from a base period consisting of 2011, 2012, and 2013 through a post period consisting of 2015–2019 for each policy group. In subsequent analysis, we exclude 2014–2018 from the sample so that  $\beta_{g(s)}$  describes differential changes in employment from the base period through a post period consisting of 2019. For a direct comparison of “short” vs. “longer” run effects, we also report summary estimates for specifications in our project’s initial analyses for which the post period consisted exclusively of 2015.

The coefficient  $\beta_{g(s)}$  is an estimate of the causal effect of states’ minimum wage policy changes on employment under the assumption that employment would, in the absence of minimum wage changes, have evolved similarly across the groups of states. We investigate threats to this assumption in multiple ways. First, guided by our pre-analysis plan, we investigate the robustness of our estimates to changing the variables that proxy for variations in economic conditions. We examine robustness to including no such controls, to controlling for the housing market’s evolution, to controlling for the log of *per capita* income, and to controlling for changes



in employment among individuals in moderately higher-skill groups.

Second, as also in our pre-analysis plan, we estimate the triple-difference model described by equation (2). Notationally, we add the subscript  $d(i)$  for demographic groups, which distinguishes between the within-state control group and the groups that are “targeted” by minimum wages. Equation (2) augments equation (1) with three sets of two-way fixed effects, namely demographic group-by-time-period effects, group-by-state effects, and state-by-time-period effects. These controls account for differential changes in employment across skill groups over time, cross-state differences in the employment of the “target” group relative to other skill groups at baseline, and time-varying differences in states’ economic conditions:

$$\begin{aligned}
Y_{i,d(i),s,g(s),t} = & \sum_{g(s) \neq 0} \beta_{g(s)} Policy_{g(s)} \times Post_t \times Target_{d(i)} + \alpha_{1s} State_s + \alpha_{2t} Time_t \\
& + \alpha_{3d(i)} Target_{d(i)} + \alpha_{4st} State_s \times Time_t + \alpha_{5sd(i)} State_s \times Target_{d(i)} \\
& + \alpha_{6td(i)} Time_t \times Target_{d(i)} + X_{i,s,t} \gamma + \varepsilon_{i,s,t}.
\end{aligned} \tag{2}$$

The implications of the triple-difference model’s state-by-time-period effects depend on which skill groups are included in the sample. The inclusion of state-by-time-period effects enables the specification to control flexibly for economic factors that vary across states and over time. More specifically, they control for such factors as they manifest themselves through employment changes among the individuals included in the sample as “within-state control groups.” In our triple-difference specifications, the within-state control group consists of prime age adults (ages 26 to 54). Note that this implicitly assumes that employment among the “within-state control group” exhibits the same sensitivity to business cycle or other developments as does employment among individuals in the target group. In fact, studies spanning decades have found that employment among teenagers and other low-skilled groups tends to be more sensitive to the business cycle than employment among individuals with greater observable skills (Hoynes,

Miller, and Schaller, 2012). In our setting, where prime age employment enjoyed greater tailwinds in states that enacted large minimum wage increases, this may thus tend to result in estimates of the minimum wage’s effects on employment among members of the target group that are modestly biased towards zero.

Third, we step outside of our pre-analysis plan to implement the “imputation” estimator of Borusyak, Jaravel, and Spiess (2024), which has attractive properties that we discuss more fully in Section VIII. A final methodological note involves confidence intervals. Because the point estimates of interest are averages across the sets of difference-in-differences and triple-difference estimates from our pre-analysis plan, we obtain confidence intervals on these estimates through a bootstrapping procedure, which also enables us to estimate confidence intervals for the labor demand elasticities that are implied by our estimates.<sup>14</sup>

## **Section VI: Regression Estimates of Recent Minimum Wage Changes’ Effects**

This section presents our estimates of the effects of minimum wage changes on employment. The collection of estimates from our pre-committed analyses can be broken down along the following dimensions: (1) ACS or CPS data; (2) analysis samples consisting of individuals ages 16 to 25 with less than a completed high school education (low-skilled workers) or samples consisting of all individuals ages 16 to 21 (young workers); (3) difference-in-differences specifications described by equation (1) or triple-difference specifications described by equation (2); (4) a “post” period consisting of 2015, 2016, 2017, 2018, and 2019 or a “post” period consisting solely of 2019; (5) the barrier between “large” and “small” changes based on

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<sup>14</sup> Each bootstrap replication reproduces the underlying sample structure by drawing states with replacement after stratifying across the policy groupings. We generated 200 replications and observe that the width of the resulting confidence intervals is little changed by extending the number of replications from 100 to 200.

changes enacted through January 2015 or based on changes enacted through January 2018; and (6) including all states in the analysis or omitting states that shift policy categories between January 2015 and January 2019. Results for the full sets of individual specifications can be found in our final project report (Clemens and Strain, 2021)

We summarize two sets of analyses. In Table 4, we summarize estimates that adhere rigidly to the specifications as implemented in Clemens and Strain (2017). In Table 3, we summarize estimates that incorporate refinements along dimensions that were pre-specified in Clemens and Strain (2017).

Our first finding is that large statutory minimum wage changes are, on average, associated with a differential employment decline of 2.65 percentage points across the full set of specifications we estimate, averaging across our primary analysis samples. The estimates are more negative for the sample consisting of individuals ages 16 to 25 (-3.4 percentage points) with less than a completed high school education than for the larger sample of all individuals ages 16 to 21 (-1.9 percentage points).

Second, the results show that the employment declines associated with legislation rise as the increases are phased in over time. As shown in Table 3, employment effects for 16-25 year olds with less than a completed high school education average -1.5 percentage points through an endline consisting solely of 2015 and average -4.2 percentage points estimated through an endline consisting solely of 2019. Equivalent estimates for the “young adult” population ages 16 to 21 are -1.2 and -2.3, respectively. In both instances, these differences are statistically significantly different from zero.

Third, omitting the states that shift policy categories due to minimum wage changes legislated between 2015 and 2018 has modest effects on our results. The point estimate for large statutory increases are slightly smaller and remain statistically distinguishable from zero (see

rows labeled “No Switchers”).

Fourth, estimates of the effects of minimum wage increases linked to inflation-indexing provisions center on 0. We hypothesize that this results from two factors. First, as shown in Figure 2, the Kaitz Index in these states rose little from baseline to endline. Second, firms’ responses to these forecastable minimum wage increases may have occurred closer to the time at which their indexing provisions were first enacted. Fifth, estimates for small statutory minimum wage changes also center on 0, but are highly variable when contrasting estimates from the ACS and CPS, as can be seen in Appendix Table A12.<sup>15</sup> The evidence overall implies that the smaller minimum wage changes in our sample have had no detectable impacts on employment.

For individuals ages 16 to 25 with less than a completed high school education, the employment effects we estimate for both small and indexed increases are both economically much smaller and statistically differentiable from our estimates for large increases. For all individuals ages 16 to 21, the magnitudes of the employment effects differ substantially, but are not as strongly statistically distinguishable across policy groups.

A key final point is that we obtain both qualitatively and quantitatively similar estimates whether we summarize estimates that incorporate pre-specified dimensions of refinement to our sets of specifications or whether we summarize estimates that forego such refinements. This can be seen by comparing the summaries of estimates in Table 3 to those in Table 4.

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<sup>15</sup> Two facts lead us to view the differences we observe in our ACS and CPS analyses for states with small minimum wage increases as likely arising from sampling variations rather than differences in survey design. First, we see no differences when comparing ACS and CPS estimates for either the large or indexed minimum wage increases. Second, we observe no meaningful changes in our ACS estimates if we remove the institutionalized group-quarters population from the sample, which accounts for one of the primary differences between the ACS and CPS sampling universes.

## Section VII: Wage Effects and Implied Elasticities

What do our estimates imply for the elasticity of demand for labor with respect to changes in the minimum wage? Answering this question requires linking the employment effects from the previous section with estimated changes in wages. We estimate wage effects of recent minimum wage changes using the difference-in-differences models we used to estimate employment effects. We summarize these estimates in Appendix Tables A9 and A10. We then combine separately estimated employment and wage effects to obtain both “own-wage” elasticities and elasticities of employment with respect to the minimum wage.

On average across specifications, (see Appendix Table A9), we estimate that large minimum wage changes involved minimum wage increases averaging \$2.91, with corresponding estimates for states with small and inflation indexed minimum wage increases of \$1.90 and \$0.94, respectively.<sup>16</sup> With respect to the wages of individuals ages 16 to 25 with less than a completed high school education, workers in states with large minimum wage increases experienced wage increases averaging \$1.64. The corresponding numbers for states with small and inflation indexed minimum wage increases are \$0.92 and \$0.47, respectively. For individuals ages 16 to 21, the corresponding wage increases were of \$1.34, \$0.70, and \$0.33.

Table 5 and Appendix Table A11 summarize the key inputs for calculating own wage and minimum wage elasticities. We combine our estimated wage and employment impacts with the baseline means of each variable so that we can construct the relevant percent changes. We then compute the elasticities of interest as the percent change in employment divided by the percent change in the relevant wage. Estimates are, once again, quantitatively similar whether we summarize estimates in which we incorporate pre-specified dimensions of refinement to our sets

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<sup>16</sup> Recall that these averages across specifications blend specifications in which the “post” period averages across 2015 to 2019 and specifications in which the “post” period is restricted to 2019 only.

of specifications or summarize estimates that forego such refinements.

We begin by presenting elasticities averaged across the wage and employment effects we estimate for the full set of states that increased their minimum wage rates during our sample period. The average elasticities we estimate (i.e., elasticities that do not distinguish between our “large,” “small,” and “indexer” groupings) are negative. As presented in Table 5, we estimate an own-wage elasticity of -0.265 for individuals ages 16 to 25 with less than a completed high school education and of -0.241 for the sample of all individuals ages 16 to 21. These estimates are close to the -0.17 median of the estimates Dube (2019) reports for U.S.-based studies. The associated elasticities with respect to the minimum wage are -0.122 and -0.082. The latter estimates are close to the median estimate reported in Neumark and Shirley’s (2022) recent meta-analysis. They are also within the range highlighted by the meta-analysis of Wolfson and Belman (2019). The associated elasticities in Appendix Table A11 are qualitatively similar.

We next compare elasticities across policy regimes. We find that the elasticities vary dramatically when we compare large minimum wage increases with minimum wage increases that were small or that were forecastable due to their linkage to inflation indexing provisions. For large minimum wage changes, we estimate an own wage elasticity of -1.02 for individuals ages 16 to 25 with less than a completed high school education and of -0.404 for all individuals ages 16 to 21. For small minimum wage changes, we estimate an own-wage elasticity of 0.481 for individuals ages 16 to 25 with less than a completed high school education and of -0.048 for all individuals ages 16 to 21. For inflation-indexed minimum wage changes, we estimate an own-wage elasticity of 0.162 for individuals ages 16 to 25 with less than a completed high school education and of -0.168 for all individuals ages 16 to 21.

Elasticities of employment with respect to the minimum wage itself follow a quite similar

pattern. We again observe substantial negative elasticities in response to large minimum wage increases and quite modest and sometimes positive elasticities in response to small minimum wage increases and inflation-indexed minimum wage increases.

Appendix Tables A14, A15, and A16 present summaries of our sets of specifications that do not include time-varying covariates, as they may, in some circumstances, complicate the interpretation of difference-in-difference estimates (Caetano *et al.*, 2022). The resulting employment effects and elasticity estimates are similar to those in Tables 3 and 5.

In summary, while the overall elasticities we estimate fall within the consensus range in the literature, we detect economically important heterogeneity with respect to the size of states' minimum wage increases. For large minimum wage changes, we find elasticities that are near the high end or that are more negative than the consensus range, while for smaller minimum wage changes, we find elasticities either within the consensus range or that are more positive.

### **Section VIII: Results Using a Modern Event Study Difference-in-Differences Estimator**

In this section, we present estimates from a modern difference-in-differences estimator that is well suited to our setting. On one level, this analysis can be interpreted as providing the reading of the evidence as we would have developed it if we were executing a fully flexible observational study. Appendix B provides an extensive discussion of the findings we obtain when using a number of alternative event study estimators. Appendix B concludes with a discussion of the value of learning-by-doing through flexible specification search, in particular as it relates to the contemporaneous advances of the modern difference-in-differences literature.

Here we present evidence from the imputation difference-in-differences (DiD) estimator of Borusyak, Jaravel, and Spiess (2024), which involves an intuitive, multi-step procedure. First,

state fixed effects, time effects, and coefficients on time-varying covariates are estimated on “untreated” observations. Second, the counterfactual outcome for each treated observation is “imputed” using the coefficients from the first step. Third, treatment effects are estimated by comparing and aggregating the realized and counterfactual outcomes for treated units. These treatment effects can be aggregated along a variety of dimensions of interest. In our case, the dimensions of interest include distinguishing across categories of treatment (e.g., “small” vs. “large” increases) and distinguishing between short- and longer-run effects, both of which are key components of our pre-committed analyses.

Within the imputation DiD framework, we provide evidence from two standard specification tests. First, we look to changes in employment among groups that are not directly impacted by minimum wages as a conventional falsification test (see Figure B11). Panel A reports estimates from a specification that includes no time varying covariates. The estimates reveal that employment among prime age adults trended more positively in states that enacted large minimum wage increases than in states that enacted no minimum wage increases, as was evident earlier in Table 2. The associated economic tailwinds would thus tend to bias downward the magnitudes of estimates that take no measures to control for macroeconomic conditions. Panel B incorporates the aggregate income and house price controls we include in a number of regressions from our pre-analysis plan, while panel C additionally incorporates the three-year lags of these variables as well as age and education fixed effects. Both of these specifications yield uniformly economic and statistical null estimates for both the small and large minimum wage increases, and thus pass this falsification check. Second, in analyses of our main samples we check for the presence of divergent pre-existing trends and find no evidence of such trends, as can be seen in Figure 4 and Appendix Figure B6.



Figure 4 reports our imputation DiD estimates for the effects of small and large minimum wage changes on employment among individuals ages 16 to 21 and among individuals ages 16 to 25 with less than a completed high school education.<sup>17</sup> As noted above, the estimates to the left of the vertical dashed lines reveal no evidence of concerning divergent pre-existing trends. With respect to subsequent employment effects, the evidence is consistent with the estimates obtained using our pre-analysis plan. We find null effects for the states that enacted small minimum wage increases and negative effects for states with large minimum wage increases. The negative estimates for states that enacted relatively large minimum wage increases begin at roughly -1 percentage point in the year following the enactment of a state's first minimum wage increase. By four years after the enactment of the first increase, the estimate has approached -5 percentage points for individuals ages 16 to 25 with less than a completed high school education and -3 percentage points for the sample of all individuals ages 16 to 21.

For comparison with estimates from Table 3, imputation DiD estimates can be constructed as simple difference-in-differences or triple-difference style averages (rather than being presented as full event study estimates). Averaging across estimates for the 2015-2019 period, the associated estimates for large minimum wage increases are on the order of -3 percentage points for individuals ages 16-25 with less than a completed high school education and -2 percentage points for individuals ages 16-21.

## **Section IX: Discussion and Conclusion**

This paper presents the completed results of a four-year, pre-committed analysis of minimum wage changes enacted during the 2010s. Our pre-analysis plan differentiates between

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<sup>17</sup> Because states with inflation-indexing regimes were implementing minimum wage increases from the onset of our sample, this and other event study analyses exclude states categorized as indexers in Figure 1 Panel A.

the employment effects of large and small minimum wage increases, as well as between their short- and longer-run effects. To our knowledge, this study is the first to execute a pre-analysis plan with a focus on using heterogeneity to examine the predictions of economic models in an analysis of non-experimental data.

During the time period we study, we estimate that relatively large minimum wage increases had substantial, negative effects on employment rates among individuals with low levels of experience and education. By contrast, our estimates of the effects of relatively small minimum wage increases are centered on zero. Relative to existing research on the employment effects of minimum wages, our estimates imply elasticities that are near the high end or larger than the consensus range in response to large minimum wage increases. Our estimates are either within the consensus range or more positive than the consensus range in response to small minimum wage increases.

The minimum wage increases we analyze relate quite closely to the \$10/hour and \$12/hour policy options that were analyzed by the Congressional Budget Office towards the end of the decade we study (CBO, 2019). We find that the smaller increases in our sample have had employment effects that are more modest than the demand elasticities assumed by CBO would have led us to project. For the larger increases, however, we estimate elasticities that are either larger in magnitude or near the high end of the consensus range from the literature. Altogether, our results thus suggest that forecasts should allow for substantial nonlinearities in the minimum wage's effects, which can imply qualitative differences in the employment effects of large minimum wage increases relative to small minimum wage increases.

How do our analyses connect to the broader literature on the economics of the minimum wage? The divergence we estimate between the effects of large and small minimum wage

increases maps quite readily into theoretical models in which labor market frictions create room for minimum wages to increase earnings without reducing employment. In most, if not all, such models, there is a point beyond which the minimum wage's effects on employment become negative. This applies, for example, to textbook monopsony models, models of monopsonistic competition (Bhaskar and To, 1999), equilibrium search models (Engbom and Moser, 2018), equilibrium models of labor markets described by oligopsony (Berger, Herkenhoff, and Mongey, 2022), and models that incorporate search frictions and firms' choices over capital as well as labor (Hurst *et al.*, 2022). Adjustment along margins other than employment, including evasion, worker effort, and fringe benefits, can also lead to a divergence between the employment effects of large and small minimum wage increases (Clemens, 2021).

These theoretical considerations can make sense of a broad set of findings in the recent minimum wage literature. First, analyses of historical U.S. variation in minimum wages tend to find quite modest employment effects (Cengiz *et al.*, 2019, 2022). Second, during economic expansions, firms appear to adjust employment by reducing hiring rather than by increasing firing (Gopalan *et al.*, 2021; Caliendo, Wittbrodt, and Schröder, 2019). Third, minimum wage increases appear to have had sharper than usual effects during the Great Recession (Clemens and Wither, 2019). Fourth, long-standing discontinuities in age-based minimum wages appear to have relatively large employment effects (Kreiner, Reck, and Skov, 2020; Kabátek, 2021). Fifth, the city of Seattle's initial minimum wage increase appears to have had much more modest effects than its subsequent minimum wage increases (Jardim *et al.*, 2022). Models that incorporate both labor market search frictions and costs to firms' adjustments to their production technologies (Hurst *et al.*, 2022) can make sense of the full set of findings described above.

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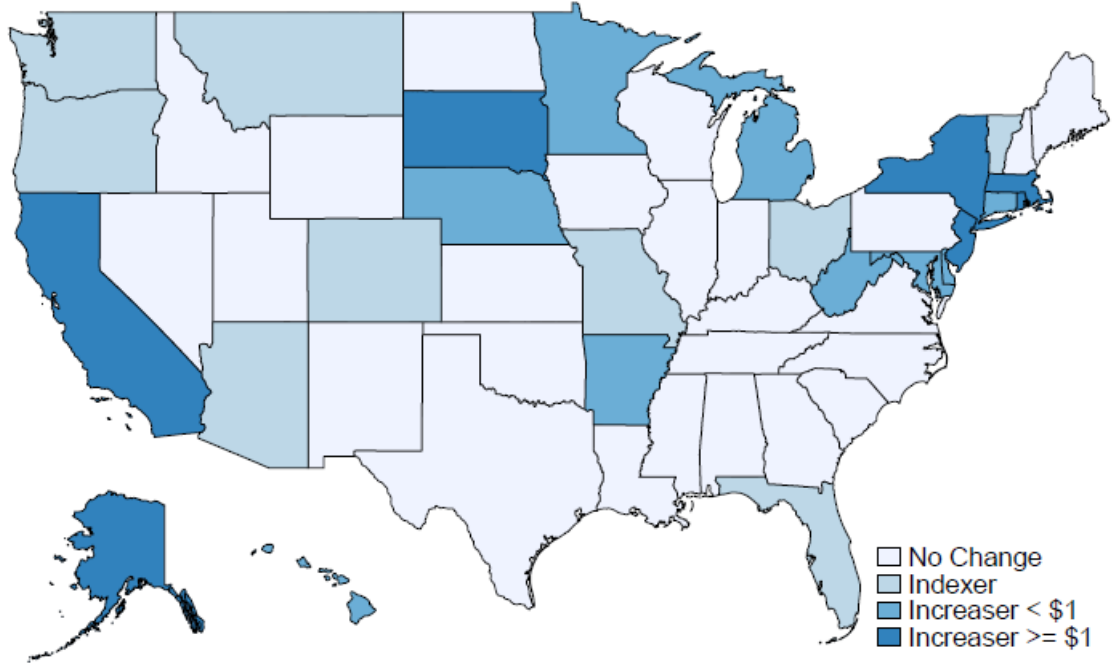
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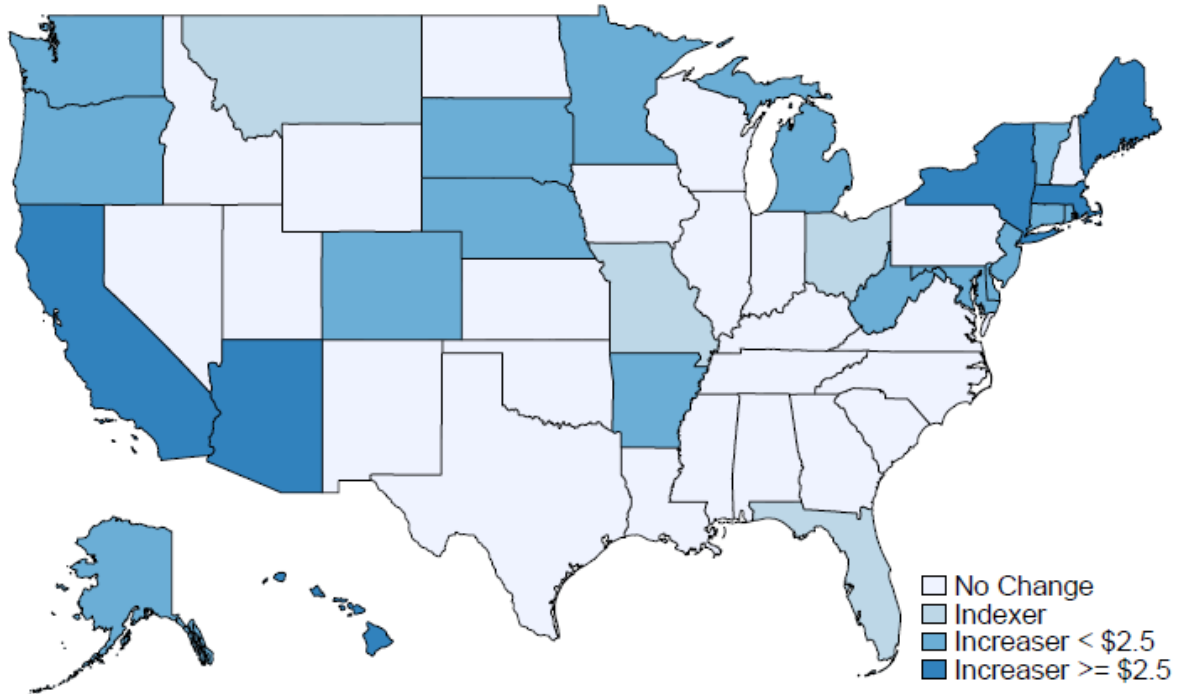
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## Figures and Tables

Panel A: Original Policy Categories

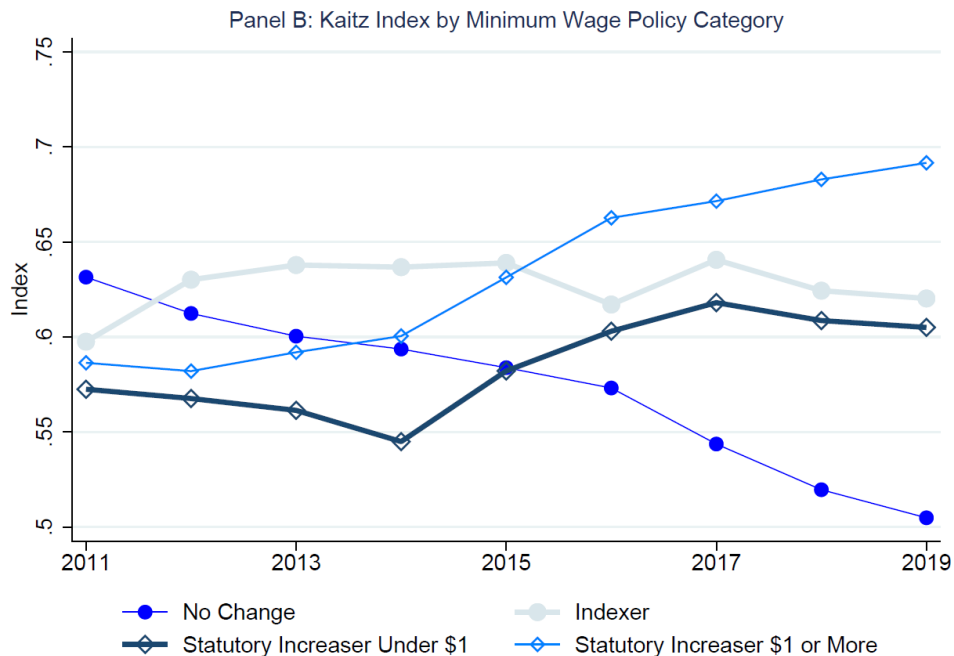
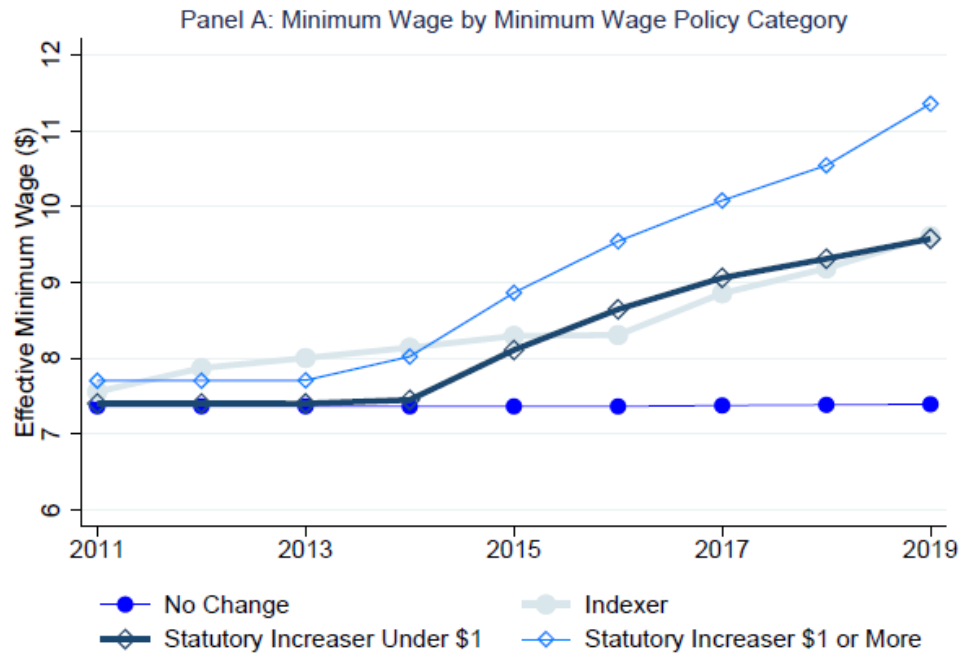


Panel B: New Policy Categories

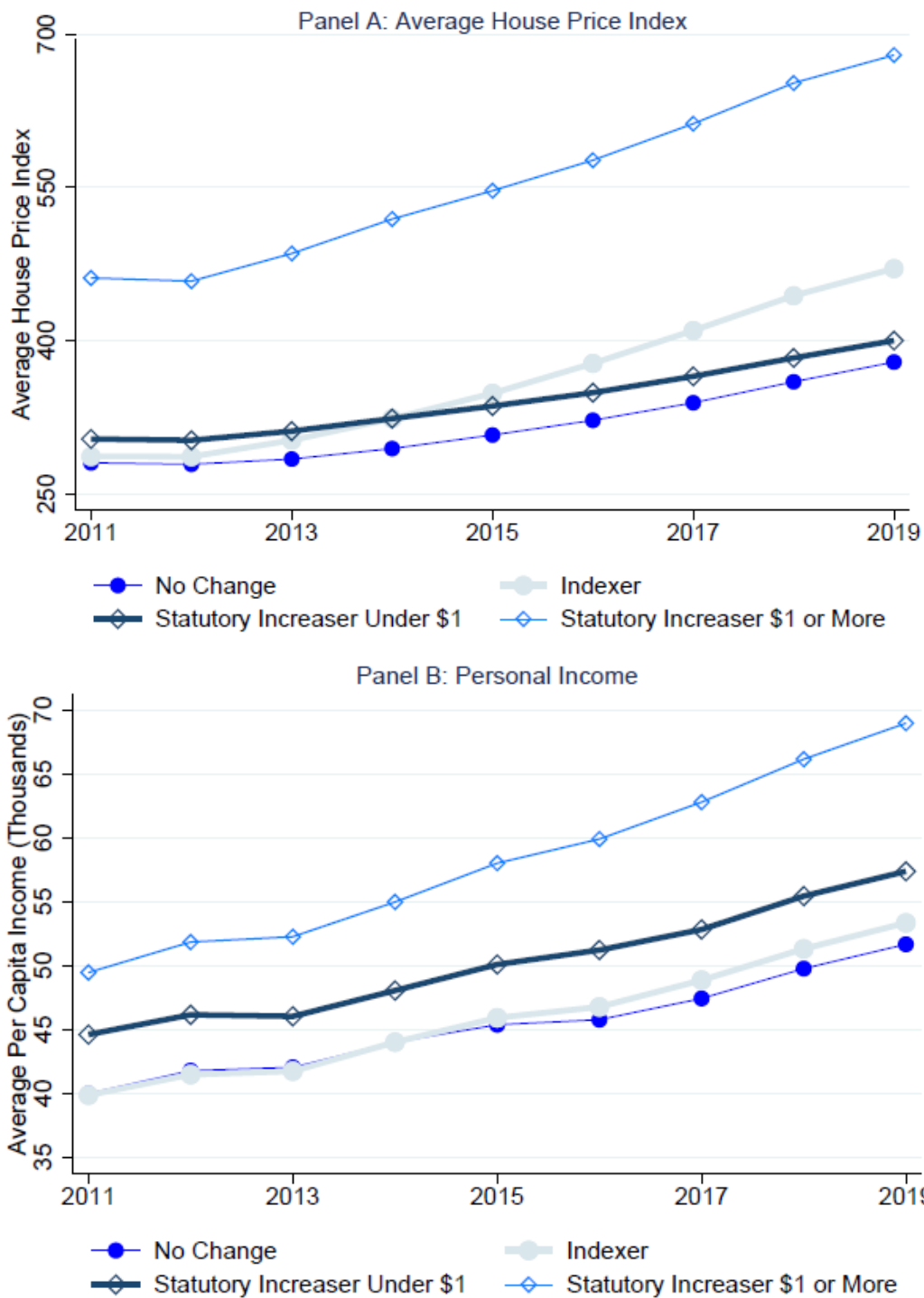


**Figure 1. States in Original and New Minimum Wage Policy Categories:** Panel A displays the states in our original policy categories defined using minimum wage changes between 2013 and 2015. Panel B displays the states in our original policy categories defined using indexed and statutory minimum wage increases between January 2013 and January 2018. Indexers are states that index their minimum wage to inflation. Data on minimum wage indexing provisions come from the National Council of State Legislatures. Data on minimum wage changes come from the U.S. Department of Labor.

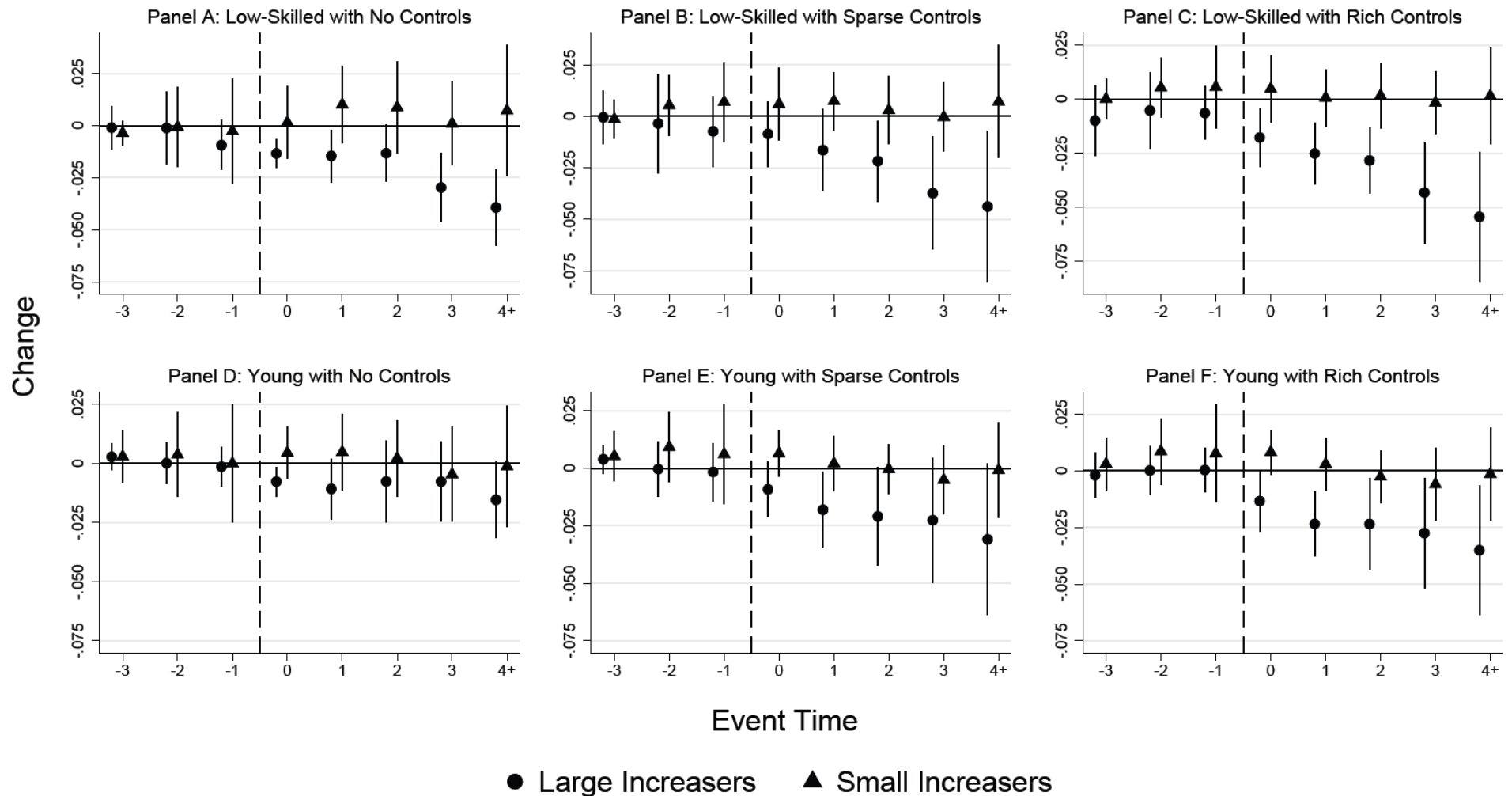




**Figure 2. Average Minimum Wage and Kaitz Index Across Policy Categories:** Panel A plots the average annual effective minimum wage for states in each of our four policy categories from January 2011 to January 2019. Panel B plots the average Kaitz index for states in each of our four policy categories from January 2011 to January 2019. We calculate median hourly wages from the CPS ORG using all employed individuals ages 16 and over who do not have imputed wage rates. For individuals paid by the hour, we use the reported hourly wage. For individuals not paid by the hour, we calculate an hourly wage using their reported weekly earnings divided by their reported usual hours worked per week. States are defined as statutory increasers under \$1 if the combined statutory increase in their minimum wage between January 2013 and January 2015 was under \$1. States are defined as statutory increasers of \$1 or more if the combined statutory increase in their minimum wage was \$1 or greater. Indexers are states that index their minimum wage to inflation. The effective minimum wage is defined as the maximum of the state and federal minimum wage. Data on minimum wage rates come from the U.S. Department of Labor. Data on minimum wage policies come from the National Conference of State Legislatures. Averages are weighted by population.



**Figure 3. Macroeconomic Time Series Across Policy Categories:** Panel A plots the average housing price index variable for each of our four policy categories from 2011 to 2019. Housing price index data come from the Federal Housing Finance Agency. Panel B plots average *per capita* income for each of our four policy categories from 2011 to 2019. Data on average *per capita* income come from the Bureau of Economic Analysis. States are defined as statutory increasers under \$1 if the combined statutory increase in their minimum wage between January 2013 and January 2015 was under \$1. States are defined as statutory increasers of \$1 or more if the combined statutory increase in their minimum wage was \$1 or greater. Indexers are states that index their minimum wage to inflation. Averages are weighted by population



**Figure 4. Event Studies of Changes in Employment Following Large and Small Statutory Minimum Wage Increases Using the BJS Imputation Estimator:** This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2024) (BJS). For the BJS estimator, we code the first treatment year as the year in which a state’s first statutory minimum wage increase took effect. We compare estimates for large vs. small increasers as defined in the main text. Panels A, B, and C plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels D, E, and F plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual average *per capita* income and the annual average state house price index used in our main regressions. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of log *per capita* income and the house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.

**Table 1. Sample Summary Statistics: ACS and Supplemental Data for 2011–2013 and 2015–2019**

	(1)	(2)	(3)	(4)
Years	2011–2013	2015–2019	2011–2013	2015–2019
Skill Groups	Ages 16 to 25 w/ < High School		Ages 16 to 21	
Employment	0.225 (0.417)	0.257 (0.437)	0.374 (0.484)	0.422 (0.494)
Age	17.90 (2.444)	17.63 (2.253)	18.58 (1.704)	18.54 (1.703)
Black	0.166 (0.372)	0.155 (0.362)	0.153 (0.360)	0.147 (0.354)
High School Degree	0 (0)	0 (0)	0.343 (0.475)	0.358 (0.479)
Some College Education	0 (0)	0 (0)	0.247 (0.431)	0.242 (0.428)
House Price Index	325.9 (99.86)	413.3 (133.1)	330.4 (101.6)	419.8 (135.9)
Income <i>per capita</i> (\$1,000s)	43.81 (6.270)	51.82 (8.524)	44.04 (6.364)	52.24 (8.665)
Effective Minimum Wage (\$)	7.531 (0.422)	8.398 (1.341)	7.536 (0.424)	8.450 (1.371)
Observations	346,135	519,374	774,438	1,235,967

Note: This table reports summary statistics for our two sample groups. Columns 1 and 2 report the means and standard deviations (in parentheses) of each variable for our subsample of low-skilled individuals, defined as individuals ages 16 to 25 with less than a high school education. Columns 3 and 4 report means and standard deviations (in parentheses) of each variable for our subsample of young adult individuals, defined as individuals ages 16 to 21. Entries for employment, age, race, and education summarize data from the American Community Survey (ACS). The house price index variable uses data from the Federal Housing Finance Agency (FHFA). The income *per capita* variable uses data from the Bureau of Economic Analysis (BEA). The effective minimum wage variable uses data from the Department of Labor.

**Table 2. Unadjusted Differences Across Policy Regimes Using ACS Data and \$1 Cutoff**

	(1)	(2)	(3)	(4)
	2011-2013	2019	Change	Change Relative to Non-Increasers
<b>Young Adult Employment</b>				
Non-Increasers	0.385	0.456	0.071	
Indexers	0.384	0.461	0.077	0.006
Increase < \$1	0.415	0.471	0.056	-0.015
Increase >= \$1	0.330	0.387	0.057	-0.014
<b>Low-Skilled Employment</b>				
Non-Increasers	0.239	0.293	0.054	
Indexers	0.222	0.291	0.069	0.015
Increase < \$1	0.246	0.291	0.045	-0.009
Increase >= \$1	0.188	0.202	0.014	-0.040
<b>Prime Age Employment</b>				
Non-Increasers	0.751	0.791	0.040	
Indexers	0.746	0.797	0.051	0.011
Increase < \$1	0.768	0.812	0.044	0.004
Increase >= \$1	0.748	0.802	0.054	0.014
<b>Mid-Skilled Employment</b>				
Non-Increasers	0.576	0.640	0.064	
Indexers	0.583	0.666	0.083	0.019
Increase < \$1	0.576	0.644	0.068	0.004
Increase >= \$1	0.590	0.655	0.065	0.001
<b>House Price Index</b>				
Non-Increasers	279.8	377.3	97.5	
Indexers	291.1	466.6	175.5	78.0
Increase < \$1	303.6	397.1	93.5	-4.0
Increase >= \$1	465.6	679.2	213.6	116.1
<b>Income per Capita (\$1000s)</b>				
Non-Increasers	41.21	51.50	10.29	
Indexers	40.96	53.10	12.14	1.85
Increase < \$1	45.44	57.23	11.79	1.50
Increase >= \$1	51.04	68.86	17.82	7.53

Notes: This table reports employment rates for each of our four policy groups (non-increasers, indexers, increase < \$1, and increase >= \$1) broken out across four types of individuals: young adults, low-skilled, prime age, and mid-skill. Young adults are defined as individuals ages 16 to 21. Low-skilled adults are those ages 16 to 25 without a completed high school education. Prime age adults are defined as individuals between the ages of 26 and 54. Mid-skilled individuals are those ages 22 to 30 years old with a high school degree, or high school dropouts between the ages of 31 and 64. This table also reports mean values of economic control variables (house price index and income per capita) for each of our four policy groups calculated using our sample of young adults. The employment variables are constructed using ACS data, the income per capita variable uses BEA data, and the house price index variable uses FHFA data. Data sources are more fully described in the note to Table 2. Column 1 reports the average value between 2011 and 2013 for each row, column 2 reports the average value in 2019, and column 3 reports the difference between the two. Column 4 reports the change in the average value for each row relative to the relevant non-increaser value. Averages are weighted by state population.

**Table 3. Summary of Employment Regression Results**

Panel A: Low-Skilled Workers				
	(1)	(2)	(3)	(4)
Sample	All	All	All	All
Policy Group	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	-0.003	-0.029	0.013	0.006
	[-.017,.007]	[-.042,-.012]	[-.015,.029]	[-.007,.018]
\$1 Cutoff; Post Period 2015	-0.001	-0.015	0.003	0.010
	[-.012,.008]	[-.024,-.005]	[-.014,.018]	[-.003,.019]
\$1 Cutoff; Post Period 2019	-0.007	-0.042	0.017	0.002
	[-.027,.009]	[-.060,-.019]	[-.025,.050]	[-.017,.018]
\$1 Cutoff; No Switchers; Post Period 2015-2019	-0.004	-0.028	0.013	0.003
	[-.019,.008]	[-.041,-.009]	[-.014,.032]	[-.008,.016]
\$1 Cutoff; No Switchers; Post Period 2019	-0.008	-0.041	0.017	-0.001
	[-.029,.008]	[-.058,-.021]	[-.027,.051]	[-.019,.019]
\$2.5 Cutoff; Post Period 2019	-0.009	-0.031	0.004	-0.001
	[-.028,.010]	[-.053,.009]	[-.028,.032]	[-.021,.016]
Overall Average Effects	-0.006	-0.034	0.013	0.002
	[-.026,.008]	[-.057,-.005]	[-.024,.041]	[-.017,.017]
Panel B: Young Workers				
	(1)	(2)	(3)	(4)
Sample	All	All	All	All
Policy Group	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	-0.005	-0.017	-0.001	0.004
	[-.018,.003]	[-.037,-.008]	[-.020,.012]	[-.006,.013]
\$1 Cutoff; Post Period 2015	-0.005	-0.012	-0.002	0.000
	[-.015,.003]	[-.030,.001]	[-.022,.012]	[-.011,.010]
\$1 Cutoff; Post Period 2019	-0.007	-0.023	0.001	0.000
	[-.022,.001]	[-.046,-.012]	[-.020,.017]	[-.013,.013]
\$1 Cutoff; No Switchers; Post Period 2015-2019	-0.007	-0.018	-0.001	-0.003
	[-.018,.001]	[-.035,-.006]	[-.019,.013]	[-.011,.020]
\$1 Cutoff; No Switchers; Post Period 2019	-0.010	-0.024	0.001	-0.008
	[-.024,.000]	[-.045,-.010]	[-.020,.018]	[-.020,.026]
\$2.5 Cutoff; Post Period 2019	-0.008	-0.011	-0.007	-0.005
	[-.021,.005]	[-.027,.011]	[-.028,.009]	[-.016,.015]
Overall Average Effects	-0.008	-0.019	-0.002	-0.002
	[-.021,.003]	[-.041,-.000]	[-.022,.014]	[-.015,.016]

Notes: This table presents averages across estimates from the regression analyses in our pre-analysis plan. The underlying estimates are estimates of  $\beta$  (g(s)) from either equation (1) or equation (2). They are thus estimates of the change in the employment rate among individuals in our analysis samples from states that increased their minimum wages relative to individuals in states that did not increase their minimum wages. The numbers in brackets below each average are 95 percent confidence intervals generated by bootstrapping the estimated average. The key dimensions along which we average the estimates (e.g., contrasting time periods, contrasting the “Low-Skilled” and “Young” samples, or contrasting the effects of “Large” increases, “Small” increases, and the inflation-indexed minimum wage changes enacted by the “Indexer” group) are clearly labeled in the body of the table. The grouping of states we describe as “\$1 Cutoff” corresponds with the grouping in Panel A of Figure 1, which is the grouping from our original pre-analysis plan. The grouping of states we describe as “\$2.5 Cutoff” corresponds with the grouping in Panel B of Figure 1, which reflects minimum wage changes enacted after we developed our pre-analysis plan. (Note that the inclusion of estimates involving updated groupings was, itself, specified in our pre-analysis plan.) The “\$1 Cutoff Post Period 2015” results were not part of the pre-analysis plan and are thus not included in the “Overall Average Effects” calculations. Panel A includes individuals 16 to 25 with less than a completed high school education and Panel B includes all individuals ages 16 to 21.

**Table 4. Summary of Employment Regression Results Using Specifications from Clemens and Strain (2017)**

Panel A: Low-Skilled Workers				
	(1)	(2)	(3)	(4)
Sample	All	All	All	All
Policy Group	All Change	Large	Small	Indexer
Original Categories				
Post Period 2015-2019	-0.002 [-.016,.010]	-0.025 [-.041,-.010]	0.013 [-.015,.031]	0.007 [-.008,.020]
Post Period 2015	0.000 [-.012,.009]	-0.015 [-.025,-.003]	0.004 [-.017,.021]	0.010 [-.003,.021]
Post Period 2019	-0.006 [-.027,.010]	-0.038 [-.060,-.015]	0.018 [-.025,.050]	0.003 [-.021,.019]
Overall Average Effects	-0.004 [-.025,.010]	-0.032 [-.058,-.011]	0.016 [-.021,.041]	0.005 [-.019,.019]
Panel B: Young Workers				
	(1)	(2)	(3)	(4)
Sample	All	All	All	All
Policy Group	All Change	Large	Small	Indexer
Original Categories				
Post Period 2015-2019	-0.005 [-.019,.002]	-0.017 [-.037,-.007]	-0.001 [-.020,.012]	0.003 [-.007,.013]
Post Period 2015	-0.006 [-.016,.002]	-0.014 [-.030,-.000]	-0.002 [-.022,.011]	-0.002 [-.013,.008]
Post Period 2019	-0.008 [-.024,.001]	-0.024 [-.047,-.012]	0.001 [-.019,.016]	-0.002 [-.016,.013]
Overall Average Effects	-0.007 [-.021,.002]	-0.020 [-.043,-.008]	0.000 [-.020,.013]	0.000 [-.015,.013]

Notes: This table presents averages across estimates from the regression analyses in our pre-analysis plan. The underlying estimates are calculated from regression equation (3) from Clemens and Strain (2017). They are thus estimates of the change in the employment rate among individuals in our analysis samples from states that increased their minimum wages relative to individuals in states that did not increase their minimum wages. The numbers in brackets below each average are 95 percent confidence intervals generated by bootstrapping the estimated average. The key dimensions along which we average the estimates (e.g., contrasting time periods, contrasting the “Low-Skilled” and “Young” samples, or contrasting the effects of “Large” increases, “Small” increases, and the inflation-indexed minimum wage changes enacted by the “Indexer” group) are clearly labeled in the body of the table. The grouping of states we describe as “Original” corresponds with the grouping in Panel A of Figure 1, which is the grouping from our original pre-analysis plan. The “Post Period 2015” results were not part of the pre-analysis plan and are thus not included in the “Overall Average Effects” calculations. Panel A includes individuals 16 to 25 with less than a completed high school education and Panel B includes all individuals ages 16 to 21.

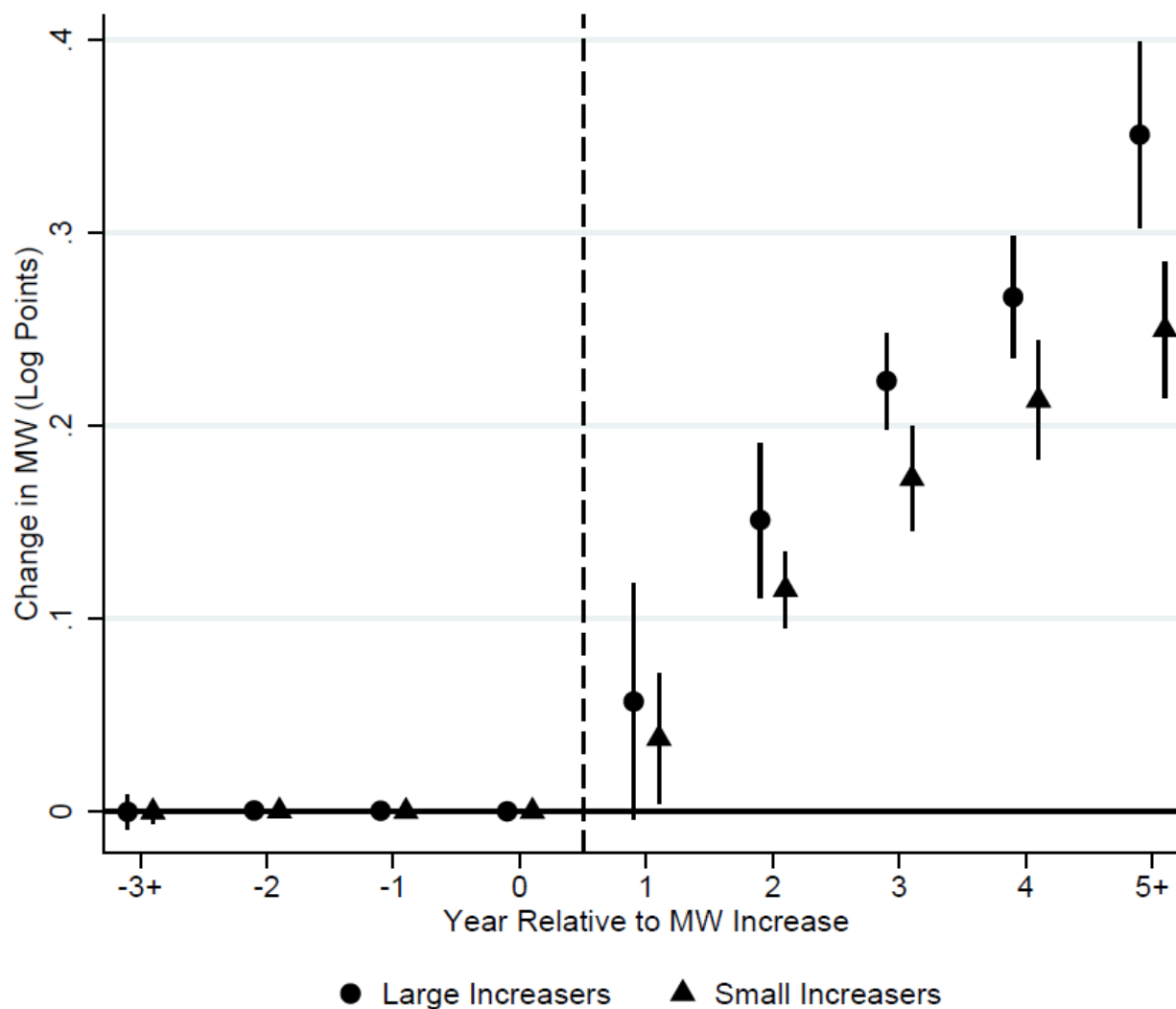
**Table 5. Summary of Elasticities**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Skill Group	Low-Skilled	Low-Skilled	Low-Skilled	Low-Skilled	Young	Young	Young	Young
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
<b>Panel A: Employment</b>								
Overall Average Effects	-0.006	-0.034	0.013	0.002	-0.008	-0.019	-0.002	-0.002
Mean in 2011-2013 Baseline	0.212	0.188	0.246	0.222	0.366	0.330	0.415	0.384
Change from Baseline (%)	-3.046	-18.205	5.246	0.886	-2.061	-5.679	-0.374	-0.612
<b>Panel B: Minimum Wages</b>								
Overall Average Effects	1.917	2.912	1.898	0.941	1.921	2.915	1.898	0.935
Mean in 2011-2013 Baseline	7.689	7.721	7.407	7.804	7.685	7.713	7.411	7.810
Change from Baseline (%)	24.933	37.712	25.627	12.061	25.001	37.797	25.811	12.051
<b>Panel C: Hourly Wages</b>								
Overall Average Effects	1.009	1.641	0.921	0.466	0.788	1.339	0.697	0.327
Mean in 2011-2013 Baseline	8.769	9.192	8.448	8.549	9.197	9.535	8.963	8.978
Change from Baseline (%)	11.509	17.849	10.900	5.454	8.567	14.046	7.778	3.645
Elasticity of Hourly Wage w.r.t Minimum Wage	0.462	0.473	0.425	0.452	0.343	0.372	0.30	0.30
<b>Panel D Elasticities</b>								
Own Wage	-0.265	-1.020	0.481	0.162	-0.241	-0.404	-0.048	-0.168
	[-1.36,0.31]	[-2.09,-0.16]	[-1.50,1.45]	[-5.16,4.32]	[-1.00,0.07]	[-1.52,0.01]	[-1.10,0.54]	[-5.26,5.68]
Minimum Wage	-0.122	-0.483	0.205	0.073	-0.082	-0.150	-0.015	-0.051
	[-0.49,0.16]	[-0.94,-0.07]	[-0.37,0.67]	[-0.76,0.77]	[-0.25,0.03]	[-0.43,-0.00]	[-0.22,0.15]	[-0.44,0.28]

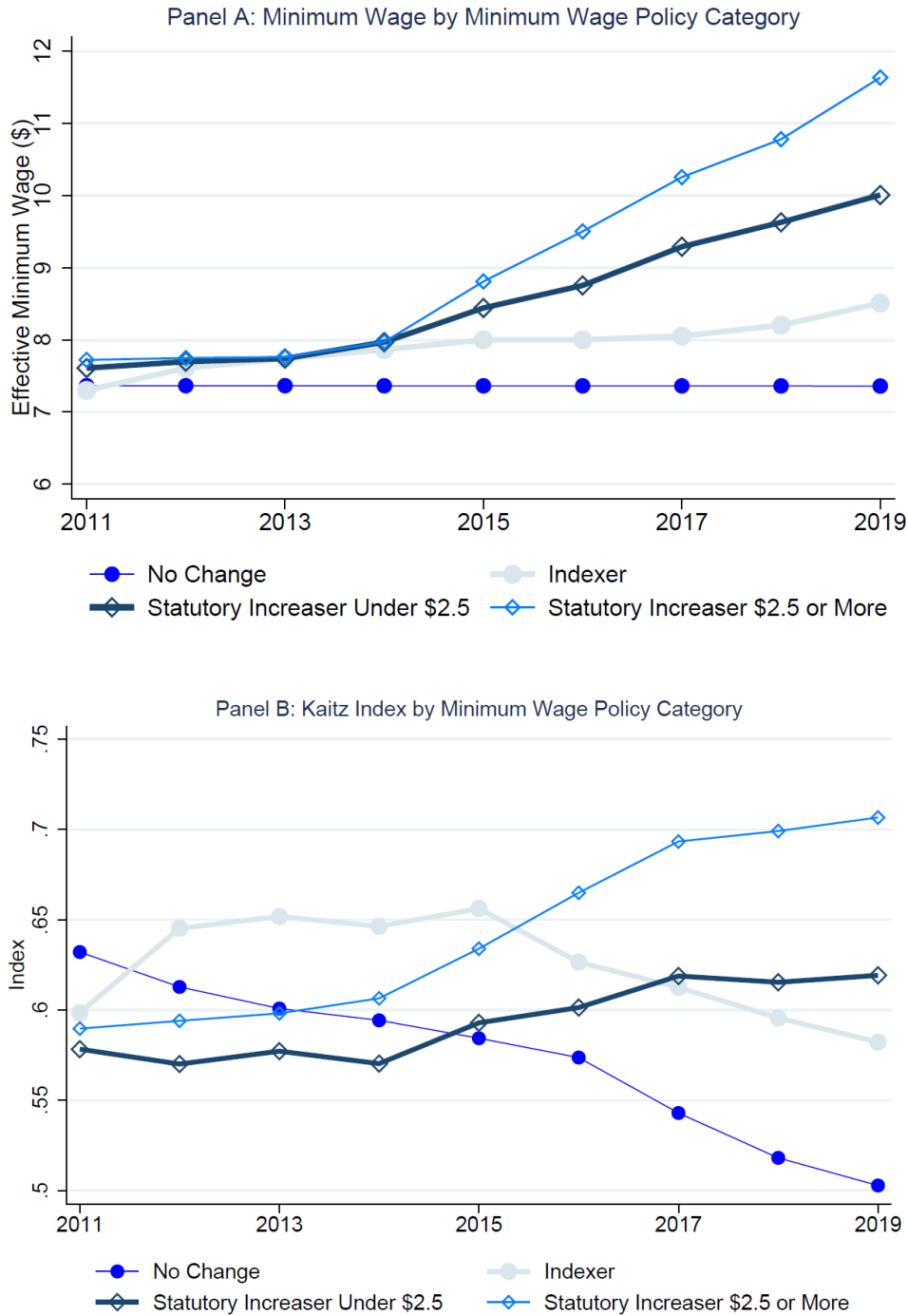
Notes: This table reports average employment and wage effects for each minimum wage policy group and skill group along with own-wage and minimum wage elasticities. The Own Wage elasticity is calculated as the ratio of the bottom row of Panel A (i.e., the percent change in employment from baseline) divided by the bottom row of Panel C (i.e., the percent change in hourly wages from baseline), while the Minimum Wage elasticity is calculated as the ratio of the bottom row of Panel A (i.e., the percent change in employment from baseline) divided by the bottom row of Panel B (i.e., the percent change in the statutory minimum wage from baseline). The numbers in brackets below each elasticity are 95 percent confidence intervals generated by bootstrapping the estimated elasticity. The baseline mean for the employment panel comes from the ACS and the overall average effects on employment are calculated from regression estimates on data from the ACS and CPS. The baseline mean and estimated overall average effects on minimum wages come from the basic monthly CPS. The baseline mean and estimated overall average effects on hourly wages come from the CPS ORG. Averages in the “mean in 2011-2013 baseline” rows are calculated using our original policy categories, while those in the “overall average effects rows” use results generated on both the original and new policy categories. Low-Skilled individuals are ages 16 to 25 with less than a completed high school education and young individuals are ages 16 to 21. Average effects for employment (panel A), minimum wages (panel B), and hourly wages (panel C) are taken from Tables 3 and A9. The hourly wage elasticity with respect to the minimum wage is the percentage change in average hourly wages from the baseline period of 2011-2013 divided by the percentage change in minimum wages from 2011-2013.



### Online Appendix A: Additional Tables and Figures



**Figure A1. Changes in State Effective Minimum Wage Following Initial Statutory Minimum Wage Increases:** This figure displays coefficients from the “stacked event study” estimator described by equation (5). The dependent variable is the log of the minimum wage. Event Time is defined such that year “1” corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. We compare estimates for large vs. small increasers as defined in Panel A of Figure 1. Regressions include state and year fixed effects. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.



**Figure A2. Average Minimum Wage and Kaitz Index Across New Policy Categories:** Panel A plots the average annual effective minimum wage for states in each of our four policy categories from January 2011 to January 2019. Panel B plots the average Kaitz index for states in each of our four policy categories from January 2011 to January 2019. We calculate median hourly wages from the CPS ORG using all employed individuals ages 16 and over who do not have imputed wage rates. For individuals paid by the hour, we use the reported hourly wage. For individuals not paid by the hour, we calculate an hourly wage using their reported weekly earnings divided by their reported usual hours worked per week. States are defined as statutory increasers under \$2.5 if the combined statutory increase in their minimum wage between January 2013 and January 2018 was under \$2.5. States are defined as statutory increasers of \$2.5 or more if the combined statutory increase in their minimum wage was \$2.5 or greater. Indexers are states that index their minimum wage to inflation. The effective minimum wage is defined as the maximum of the state and federal minimum wage. Data on minimum wage rates come from the U.S. Department of Labor. Data on minimum wage policies come from the National Conference of State Legislatures. Averages are weighted by state population.

**Table A1. Sample Summary Statistics: ACS and Supplemental Data for 2011–2013 and 2019**

	(1)	(2)	(3)	(4)
Years	2011–2013	2019	2011–2013	2019
Skill Groups	Ages 16 to 25 w/ < High School		Ages 16 to 21	
Employment	0.225 (0.417)	0.273 (0.445)	0.374 (0.484)	0.442 (0.497)
Age	17.90 (2.444)	17.53 (2.155)	18.58 (1.704)	18.54 (1.696)
Black	0.166 (0.372)	0.148 (0.355)	0.153 (0.360)	0.146 (0.353)
High School Degree	0 (0)	0 (0)	0.343 (0.475)	0.368 (0.482)
Some College Education	0 (0)	0 (0)	0.247 (0.431)	0.240 (0.427)
House Price Index	325.9 (99.86)	460.9 (143.2)	330.4 (101.6)	466.9 (146.1)
Income Per Capita (\$1,000s)	43.81 (6.270)	56.10 (8.965)	44.04 (6.364)	56.45 (9.118)
Effective Minimum Wage	7.531 (0.422)	8.899 (1.812)	7.536 (0.424)	8.960 (1.837)
Observations	346,135	98,302	774,438	243,315

Notes: This table reports summary statistics for our two sample groups. Columns 1 and 2 report averages and standard deviations (in parentheses) of each of the variables for our subsample of low-skilled individuals, defined as individuals ages 16 to 25 with less than a high school education. Columns 3 and 4 report averages and standard deviations (in parentheses) for our subsample of young adult individuals, defined as individuals ages 16 to 21. Entries for employment, age, race, and education summarize data from the American Community Survey (ACS). The house price index variable uses data from the Federal Housing Finance Agency (FHFA). The income *per capita* variable uses data from the Bureau of Economic Analysis (BEA). The effective minimum wage variable uses data from the Department of Labor.

**Table A2. Sample Summary Statistics: CPS and Supplemental Data for 2011-2013 and 2015-2019**

	(1)	(2)	(3)	(4)
Years	2011-2013	2015-2019	2011-2013	2015-2019
Skill Groups	Ages 16 to 25 w/ < High School		Ages 16 to 21	
Employment	0.234 (0.424)	0.261 (0.439)	0.360 (0.480)	0.398 (0.490)
Age	17.97 (2.423)	17.73 (2.243)	18.50 (1.730)	18.47 (1.734)
Black	0.164 (0.370)	0.156 (0.363)	0.155 (0.362)	0.150 (0.357)
High School Degree	0 (0)	0 (0)	0.223 (0.416)	0.234 (0.424)
Some College Education	0 (0)	0 (0)	0.299 (0.458)	0.290 (0.454)
House Price Index	327.8 (100.8)	413.9 (132.5)	331.8 (102.5)	419.9 (135.0)
Income Per Capita (\$1000s)	43.91 (6.338)	51.88 (8.513)	44.15 (6.420)	52.30 (8.597)
Effective Minimum Wage (\$)	7.535 (0.423)	8.416 (1.344)	7.541 (0.426)	8.461 (1.366)
Observations	197,386	287,097	365,354	546,414

Notes: This table reports summary statistics for our two sample groups. Columns 1 and 2 report averages and standard errors (in parenthesis) of each of the variables for our subsample of low-skilled individuals, defined as individuals ages 16 to 25 with less than a high school education. Columns 3 and 4 report averages and standard errors (in parenthesis) for our subsample of young adult individuals, defined as individuals ages 16 to 21. Entries for employment, age, race, and education summarize data from the Current Population Survey (CPS). The house price index variable uses data from the Federal Housing Finance Agency (FHFA). The income per capita variable uses data from the Bureau of Economic Analysis (BEA). The effective minimum wage variable uses data from the Department of Labor.

**Table A3. Sample Summary Statistics: CPS and Supplemental Data for 2011–2013 and 2019**

	(1)	(2)	(3)	(4)
Years	2011–2013	2019	2011–2013	2019
Skill Groups	Ages 16 to 25 w/ < High School		Ages 16 to 21	
Employment	0.234 (0.424)	0.266 (0.442)	0.360 (0.480)	0.410 (0.492)
Age	17.97 (2.423)	17.62 (2.118)	18.50 (1.730)	18.47 (1.729)
Black	0.164 (0.370)	0.153 (0.360)	0.155 (0.362)	0.149 (0.356)
High School Degree	0 (0)	0 (0)	0.223 (0.416)	0.239 (0.427)
Some College Education	0 (0)	0 (0)	0.299 (0.458)	0.291 (0.454)
House Price Index	327.8 (100.8)	460.9 (143.2)	331.8 (102.5)	465.6 (144.8)
Income Per Capita (\$1,000s)	43.91 (6.338)	56.14 (8.962)	44.15 (6.420)	56.40 (9.044)
Effective Minimum Wage	7.535 (0.423)	8.919 (1.810)	7.541 (0.426)	8.971 (1.826)
Observations	197,386	51,409	365,354	101,036

Note: This table reports summary statistics for our two sample groups. Columns 1 and 2 report averages and standard deviations (in parentheses) of each of the variables for our subsample of low-skilled individuals, defined as individuals ages 16 to 25 with less than a high school education. Columns 3 and 4 report averages and standard deviations (in parentheses) for our subsample of young adult individuals, defined as individuals ages 16 to 21. Entries for employment, age, race, and education summarize data from the Current Population Survey (CPS). The house price index variable uses data from the Federal Housing Finance Agency (FHFA). The income *per capita* variable uses data from the Bureau of Economic Analysis (BEA). The effective minimum wage variable uses data from the Department of Labor.

**Table A4. Unadjusted Differences Across Policy Regimes Using CPS Data and \$1 Cutoff**

	(1)	(2)	(3)	(4)
	2011-2013	2019	Change	Change Relative to Non-increasers
<b>Young Adult Employment</b>				
Non-Increasers	0.377	0.423	0.046	
Indexers	0.373	0.429	0.056	0.010
Increase < \$1	0.400	0.466	0.066	0.020
Increase >= \$1	0.304	0.339	0.035	-0.011
<b>Low-Skilled Employment</b>				
Non-Increasers	0.250	0.282	0.032	
Indexers	0.240	0.273	0.033	0.001
Increase < \$1	0.238	0.326	0.088	0.056
Increase >= \$1	0.198	0.198	0.000	-0.032
<b>Prime-Age Employment</b>				
Non-Increasers	0.761	0.800	0.039	
Indexers	0.757	0.808	0.051	0.012
Increase < \$1	0.774	0.819	0.045	0.006
Increase >= \$1	0.745	0.794	0.049	0.010
<b>Mid-Skilled Employment</b>				
Non-Increasers	0.591	0.655	0.064	
Indexers	0.589	0.675	0.086	0.022
Increase < \$1	0.583	0.642	0.059	-0.005
Increase >= \$1	0.579	0.634	0.055	-0.009
<b>House Price Index</b>				
Non-Increasers	279.6	376.7	97.1	
Indexers	291.2	469.1	177.9	80.8
Increase < \$1	303.8	396.1	92.3	-4.8
Increase >= \$1	465.6	675.6	210	112.9
<b>Income Per Capita (\$1000s)</b>				
Non-Increasers	41.20	51.54	10.34	
Indexers	41.01	53.17	12.16	1.82
Increase < \$1	45.54	57.05	11.51	1.17
Increase >= \$1	51.07	68.77	17.70	7.36

Notes: This table reports employment rates for each our of our four policy groups (non-increasers, indexers, increase < \$1, and increase >= \$1) broken out across four types of individuals: young adults, low-skilled, prime-age, and mid-skill. Young adults are defined as individuals ages 16 to 21. Low-skilled adults are those ages 16 to 25 without a completed high school education. Prime age adults are defined as individuals between the ages of 26 and 54. Mid-skilled individuals are those ages 22 to 30 years old with a high school degree, or high school dropouts between the ages of 31 and 64. This table also reports mean values of economic control variables (house price index and income per capita) for each of our four policy groups calculated using our sample of young adults. The employment variables are constructed using CPS data, the income per capita variable uses BEA data, and the house price index variable uses FHFA data. Data sources are more fully described in the note to Table 2. Column 1 reports the average value between 2011 and 2013 for each row, column 2 reports the average value in 2019, and column 3 reports the difference between the two. Column 4 reports the change in the average value for each row relative to the relevant non-increaser value. Averages are weighted by state population.

**Table A5. Unadjusted Differences Across Policy Regimes Using ACS Data and \$1 Cutoff**

	(1)	(2)	(3)	(4)
	2011-2013	2015-2019	Change	Change Relative to Non-Increasers
<b>Young Adult Employment</b>				
Non-Increasers	0.385	0.434	0.049	
Indexers	0.384	0.442	0.058	0.009
Increase < \$1	0.415	0.459	0.044	-0.005
Increase >= \$1	0.330	0.368	0.038	-0.011
<b>Low-Skilled Employment</b>				
Non-Increasers	0.239	0.272	0.033	
Indexers	0.222	0.273	0.051	0.018
Increase < \$1	0.246	0.282	0.036	0.003
Increase >= \$1	0.188	0.198	0.010	-0.023
<b>Prime Age Employment</b>				
Non-Increasers	0.751	0.779	0.028	
Indexers	0.746	0.783	0.037	0.009
Increase < \$1	0.768	0.800	0.032	0.004
Increase >= \$1	0.748	0.786	0.038	0.010
<b>Mid-Skilled Employment</b>				
Non-Increasers	0.576	0.621	0.045	
Indexers	0.583	0.640	0.057	0.012
Increase < \$1	0.576	0.627	0.051	0.006
Increase >= \$1	0.590	0.632	0.042	-0.003
<b>House Price Index</b>				
Non-Increasers	279.8	339.9	60.1	
Indexers	291.1	407.5	116.4	56.3
Increase < \$1	303.6	363.0	59.4	-0.7
Increase >= \$1	465.6	612.3	146.7	86.6
<b>Income per Capita (\$1000s)</b>				
Non-Increasers	41.21	47.85	6.64	
Indexers	40.96	49.06	8.10	1.46
Increase < \$1	45.44	53.18	7.74	1.10
Increase >= \$1	51.04	63.00	11.96	5.32

Notes: This table reports employment rates for each our of our four policy groups (non-increasers, indexers, increase < \$1, and increase >= \$1) broken out across four types of individuals: young adults, low-skill, prime age, and mid-skilled. Young adults are defined as individuals ages 16 to 21. Low-skilled adults are those ages 16 to 25 without a completed high school education. Prime age adults are defined as individuals between the ages of 26 and 54. Mid-skilled individuals are those ages 22 to 30 years old with a high school degree, or high school dropouts between the ages of 31 and 64. This table also reports mean values of economic control variables (house price index and income per capita) for each of our four policy groups calculated using our sample of young adults. The employment variables are constructed using ACS data, the income per capita variable uses BEA data, and the house price index variable uses FHFA data. Data sources are more fully described in the note to Table 2. Column 1 reports the average value between 2011 and 2013 for each row, column 2 reports the average value between 2015 and 2019, and column 3 reports the difference between the two. Column 4 reports the change in the average value for each row relative to the relevant non-increaser value. Averages are weighted by state population.

**Table A6. Unadjusted Differences Across Policy Regimes Using CPS Data and \$1 Cutoff**

	(1)	(2)	(3)	(4)
	2011-2013	2015-2019	Change	Change Relative to Non-increasers
<b>Young Adult Employment</b>				
Non-Increasers	0.377	0.413	0.036	
Indexers	0.373	0.416	0.043	0.007
Increase < \$1	0.400	0.443	0.043	0.007
Increase >= \$1	0.304	0.334	0.030	-0.006
<b>Low-Skilled Employment</b>				
Non-Increasers	0.250	0.278	0.028	
Indexers	0.240	0.270	0.030	0.002
Increase < \$1	0.238	0.300	0.062	0.034
Increase >= \$1	0.198	0.199	0.001	-0.027
<b>Prime Age Employment</b>				
Non-Increasers	0.761	0.788	0.027	
Indexers	0.757	0.792	0.035	0.008
Increase < \$1	0.774	0.805	0.031	0.004
Increase >= \$1	0.745	0.779	0.034	0.007
<b>Mid-Skilled Employment</b>				
Non-Increasers	0.591	0.632	0.041	
Indexers	0.589	0.651	0.062	0.021
Increase < \$1	0.583	0.630	0.047	0.006
Increase >= \$1	0.579	0.623	0.044	0.003
<b>House Price Index</b>				
Non-Increasers	279.6	339.9	60.3	
Indexers	291.2	407.4	116.2	55.9
Increase < \$1	303.8	364.2	60.4	0.1
Increase >= \$1	465.6	608.5	142.9	82.6
<b>Income Per Capita (\$1000s)</b>				
Non-Increasers	41.20	47.96	6.76	
Indexers	41.01	49.06	8.05	1.29
Increase < \$1	45.54	53.20	7.66	0.90
Increase >= \$1	51.07	62.86	11.79	5.03

Notes: This table reports employment rates for each our of our four policy groups (non-increasers, indexers, increase < \$1, and increase >= \$1) broken out across four types of individuals: young adults, low-skilled, prime age, and mid-skill. Young adults are defined as individuals ages 16 to 21. Low-skilled adults are those ages 16 to 25 without a completed high school education. Prime age adults are defined as individuals between the ages of 26 and 54. Mid-skilled individuals are those ages 22 to 30 years old with a high school degree, or high school dropouts between the ages of 31 and 64. This table also reports mean values of economic control variables (house price index and income per capita) for each of our four policy groups calculated using our sample of young adults. The employment variables are constructed using CPS data, the income per capita variable uses BEA data, and the house price index variable uses FHFA data. Data sources are more fully described in the note to Table 2. Column 1 reports the average value between 2011 and 2013 for each row, column 2 reports the average value between 2015 and 2019, and column 3 reports the difference between the two. Column 4 reports the change in the average value for each row relative to the relevant non-increaser value. Averages are weighted by state population.



**Table A7. Unadjusted Differences Across Policy Regimes Using ACS Data and \$2.5 Cutoff**

	(1)	(2)	(3)	(4)
	2011-2013	2019	Change	Change Relative to Non-Increasers
<b>Young Adult Employment</b>				
Non-Increasers	0.388	0.458	0.070	
Indexers	0.387	0.461	0.074	0.003
Increase < \$2.5	0.405	0.467	0.061	-0.009
Increase >= \$2.5	0.333	0.395	0.062	-0.009
<b>Low-Skilled Employment</b>				
Non-Increasers	0.246	0.298	0.052	
Indexers	0.228	0.296	0.068	0.016
Increase < \$2.5	0.244	0.295	0.051	-0.002
Increase >= \$2.5	0.189	0.207	0.019	-0.034
<b>Prime-Age Employment</b>				
Non-Increasers	0.763	0.804	0.041	
Indexers	0.755	0.808	0.053	0.012
Increase < \$2.5	0.776	0.822	0.045	0.005
Increase >= \$2.5	0.752	0.806	0.054	0.013
<b>Mid-Skilled Employment</b>				
Non-Increasers	0.596	0.661	0.065	
Indexers	0.587	0.673	0.086	0.021
Increase < \$2.5	0.610	0.683	0.073	0.008
Increase >= \$2.5	0.602	0.669	0.067	0.002
<b>House Price Index</b>				
Non-Increasers	278.4	375.8	97.4	
Indexers	265.2	406.0	140.8	43.4
Increase < \$2.5	341.3	476.4	135.1	37.7
Increase >= \$2.5	451.6	675.6	224.0	126.5
<b>Income per Capita (\$1000s)</b>				
Non-Increasers	41.23	51.50	10.27	
Indexers	40.31	51.06	10.75	0.48
Increase < \$2.5	46.66	60.04	13.38	3.10
Increase >= \$2.5	49.13	66.36	17.23	6.95

Notes: This table reports employment rates for each our of our four policy groups (non-increasers, indexers, increase < \$2.5, and increase >= \$2.5) broken out across four types of individuals: young adults, low-skilled, prime-age, and mid-skilled. Young adults are defined as individuals ages 16 to 21. Low-skilled adults are those ages 16 to 25 without a completed high school education. Prime age adults are defined as individuals between the ages of 26 and 54. Mid-skilled individuals are those ages 22 to 30 years old with a high school degree, or high school dropouts between the ages of 31 and 64. This table also reports mean values of economic control variables (house price index and income per capita) for each of our four policy groups calculated using our sample of young adults. The employment variables are constructed using ACS data, the income per capita variable uses BEA data, and the house price index variable uses FHFA data. Data sources are more fully described in the note to Table 2. Column 1 reports the average value between 2011 and 2013 for each row, column 2 reports the average value in 2019, and column 3 reports the difference between the two. Column 4 reports the change in the average value for each row relative to the relevant non-increaser value. Averages are weighted by state population.

**Table A8. Unadjusted Differences Across Policy Regimes Using CPS Data and \$2.5 Cutoff**

	(1)	(2)	(3)	(4)
	2011-2013	2019	Change	Change Relative to Non-Increasers
<b>Young Adult Employment</b>				
Non-Increasers	0.376	0.422	0.046	
Indexers	0.379	0.421	0.042	-0.004
Increase < \$2.5	0.384	0.435	0.051	0.005
Increase >= \$2.5	0.304	0.357	0.053	0.007
<b>Low-Skilled Employment</b>				
Non-Increasers	0.250	0.281	0.031	
Indexers	0.243	0.263	0.020	-0.011
Increase < \$2.5	0.239	0.296	0.057	0.026
Increase >= \$2.5	0.197	0.209	0.012	-0.019
<b>Prime-Age Employment</b>				
Non-Increasers	0.761	0.800	0.039	
Indexers	0.755	0.804	0.049	0.010
Increase < \$2.5	0.773	0.822	0.049	0.010
Increase >= \$2.5	0.740	0.789	0.049	0.010
<b>Mid-Skilled Employment</b>				
Non-Increasers	0.591	0.655	0.064	
Indexers	0.584	0.649	0.065	0.000
Increase < \$2.5	0.603	0.671	0.068	0.003
Increase >= \$2.5	0.570	0.636	0.066	0.001
<b>House Price Index</b>				
Non-Increasers	278.1	375.1	97.0	
Indexers	263.9	407.6	143.7	46.7
Increase < \$2.5	342.6	477.2	134.6	37.6
Increase >= \$2.5	452.3	670.6	218.3	121.3
<b>Income Per Capita (\$1000s)</b>				
Non-Increasers	41.21	51.54	10.33	
Indexers	40.31	51.10	10.79	0.46
Increase < \$2.5	46.86	60.05	13.19	2.86
Increase >= \$2.5	49.15	65.96	16.81	6.48

Notes: This table reports employment rates for each of our four policy groups (non-increasers, indexers, increase < \$2.5, and increase >= \$2.5) broken out across four types of individuals: young adults, low-skill, prime-age, and mid-skill. Young adults are defined as individuals ages 16 to 21. Low-skilled adults are those ages 16 to 25 without a completed high school education. Prime age adults are defined as individuals between the ages of 26 and 54. Mid-skilled individuals are those ages 22 to 30 years old with a high school degree, or high school dropouts between the ages of 31 and 64. This table also reports mean values of economic control variables (house price index and income per capita) for each of our four policy groups calculated using our sample of young adults. The employment variables are constructed using CPS data, the income per capita variable uses BEA data, and the house price index variable uses FHFA data. Data sources are more fully described in the note to Table 2. Column 1 reports the average value between 2011 and 2013 for each row, column 2 reports the average value in 2019, and column 3 reports the difference between the two. Column 4 reports the change in the average value for each row relative to the relevant non-increaser value. Averages are weighted by state population.

**Table A9. Summary of Wage Regression Results**

Panel A: Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	OW	OW	OW	OW	MW	MW	MW	MW
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	0.85 [0.54,1.05]	1.21 [0.87,1.49]	0.75 [0.38,0.95]	0.60 [0.30,0.87]	1.46 [1.29,1.65]	2.02 [1.64,2.31]	1.50 [1.30,1.69]	0.85 [0.56,1.27]
\$1 Cutoff; Post Period 2015	0.72 [0.20,1.24]	1.30 [0.02,2.79]	0.35 [-0.10,0.64]	0.51 [0.22,0.85]	0.79 [0.71,0.88]	1.18 [1.01,1.44]	0.71 [0.56,0.78]	0.49 [0.44,0.55]
\$1 Cutoff; Post Period 2019	1.19 [0.60,1.64]	1.76 [0.87,2.24]	0.94 [0.33,1.30]	0.86 [0.11,1.71]	2.33 [1.92,2.72]	3.28 [2.21,3.76]	2.10 [1.78,2.46]	1.60 [0.96,2.53]
\$1 Cutoff; No Switchers; Post Period 2015-2019	0.81 [0.53,0.95]	1.27 [0.92,1.51]	0.77 [0.38,0.96]	0.39 [0.14,0.53]	1.37 [1.21,1.50]	2.09 [1.71,2.38]	1.51 [1.32,1.71]	0.52 [0.36,0.64]
\$1 Cutoff; No Switchers; Post Period 2019	1.03 [0.48,1.35]	1.91 [1.01,2.37]	0.97 [0.31,1.35]	0.20 [-0.22,0.51]	2.12 [1.78,2.36]	3.42 [2.34,3.90]	2.13 [1.79,2.53]	0.81 [0.61,0.95]
\$2.5 Cutoff; Post Period 2019	1.17 [0.77,1.50]	2.05 [1.47,2.48]	1.17 [0.68,1.64]	0.28 [-0.13,0.67]	2.30 [2.13,2.48]	3.74 [3.44,3.93]	2.24 [1.94,2.68]	0.92 [0.75,1.31]
Overall Average Effects	1.01 [0.56,1.49]	1.64 [0.92,2.35]	0.92 [0.37,1.50]	0.47 [-0.11,1.29]	1.92 [1.25,2.55]	2.91 [1.77,3.85]	1.90 [1.36,2.52]	0.94 [0.43,2.05]
Panel B: Young Workers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	OW	OW	OW	OW	MW	MW	MW	MW
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	0.61 [0.41,0.76]	0.91 [0.57,1.05]	0.51 [0.29,0.77]	0.39 [0.14,0.66]	1.46 [1.29,1.66]	2.02 [1.64,2.32]	1.51 [1.30,1.70]	0.85 [0.55,1.28]
\$1 Cutoff; Post Period 2015	0.41 [0.22,0.66]	0.77 [0.39,1.25]	0.14 [-0.25,0.56]	0.33 [0.11,0.50]	0.79 [0.72,0.88]	1.18 [1.03,1.44]	0.71 [0.56,0.77]	0.49 [0.43,0.57]
\$1 Cutoff; Post Period 2019	0.96 [0.51,1.28]	1.48 [0.64,1.84]	0.77 [0.35,1.17]	0.64 [0.08,1.20]	2.33 [1.92,2.72]	3.28 [2.24,3.75]	2.13 [1.77,2.49]	1.58 [0.95,2.52]
\$1 Cutoff; No Switchers; Post Period 2015-2019	0.57 [0.34,0.72]	0.95 [0.65,1.10]	0.52 [0.28,0.76]	0.22 [-0.15,0.52]	1.37 [1.20,1.50]	2.09 [1.71,2.39]	1.52 [1.33,1.72]	0.51 [0.35,0.64]
\$1 Cutoff; No Switchers; Post Period 2019	0.84 [0.49,1.13]	1.59 [0.82,1.96]	0.79 [0.27,1.27]	0.14 [-0.31,0.81]	2.13 [1.80,2.37]	3.43 [2.36,3.90]	2.16 [1.79,2.55]	0.81 [0.60,0.95]
\$2.5 Cutoff; Post Period 2019	0.97 [0.68,1.23]	1.76 [1.37,2.07]	0.89 [0.45,1.29]	0.24 [-0.10,0.71]	2.31 [2.13,2.49]	3.75 [3.45,3.94]	2.25 [1.95,2.69]	0.92 [0.75,1.32]
Overall Average Effects	0.79 [0.40,1.19]	1.34 [0.65,1.95]	0.70 [0.30,1.22]	0.33 [-0.15,0.93]	1.92 [1.25,2.55]	2.92 [1.76,3.85]	1.91 [1.36,2.54]	0.94 [0.42,2.02]

Notes: This table presents averages across estimates from the regression analyses in our pre-analysis plan. The underlying estimates are, in each case, estimates of  $\beta_{(g(s))}$  from equation (1). They are thus estimates of the change in the hourly wage (OW) or minimum wage (MW) among individuals in our analysis samples from states that increased their minimum wages relative to individuals in states that did not increase their minimum wages. The numbers in brackets below each average are 95 percent confidence intervals generated by bootstrapping the estimated average. The key dimensions along which we average the estimates (e.g., contrasting time periods, contrasting the “Low-Skilled” and “Young” samples, or contrasting the effects of “Large” increases, “Small” increases, and the inflation-indexed minimum wage changes enacted by the “Indexer” group) are clearly labeled in the body of the table. For estimated effects on hourly wages, we use data from the CPS ORG and for estimated effects on minimum wages we use data from the basic monthly CPS. The grouping of states we describe as “\$1 Cutoff” corresponds with the grouping in Panel A of Figure 1, which is the grouping from our original pre-analysis plan. The grouping of states we describe as “\$2.5 Cutoff” corresponds with the grouping in Panel B of Figure 1, which reflects minimum wage changes enacted after we developed our pre-analysis plan. (Note that the inclusion of estimates involving updated groupings was, itself, specified in our pre-analysis plan.) The “\$1 Cutoff Post Period 2015” results were not part of the pre-analysis plan and are thus not included in the “Overall Average Effects” calculations. Panel A includes individuals 16 to 25 with less than a completed high school education and Panel B includes all individuals ages 16 to 21.

**Table A10. Summary of Wage Regression Results Using Specifications from Clemens and Strain (2017)**

Panel A: Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	OW	OW	OW	OW	MW	MW	MW	MW
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
Original Categories								
Post Period 2015-2019	0.80	1.10	0.77	0.54	1.37	1.85	1.52	0.73
	[0.45,1.01]	[0.68,1.46]	[0.39,0.99]	[0.21,0.79]	[1.15,1.59]	[1.39,2.26]	[1.31,1.72]	[0.39,1.11]
Post Period 2015	0.82	1.48	0.37	0.60	0.80	1.20	0.71	0.50
	[0.24,1.25]	[0.03,2.82]	[-0.13,0.66]	[0.24,1.06]	[0.71,0.88]	[1.01,1.43]	[0.56,0.78]	[0.39,0.58]
Post Period 2019	1.12	1.64	0.97	0.76	2.22	3.09	2.13	1.45
	[0.48,1.60]	[0.78,2.17]	[0.34,1.37]	[0.02,1.59]	[1.82,2.65]	[2.16,3.68]	[1.79,2.50]	[0.85,2.40]
Overall Average Effects	0.96	1.37	0.87	0.65	1.79	2.47	1.82	1.09
	[0.35,1.47]	[0.36,2.60]	[-0.02,1.27]	[0.16,1.27]	[0.73,2.58]	[1.03,3.62]	[0.60,2.40]	[0.41,1.96]
Panel B: Young Workers								
Outcome	OW	OW	OW	OW	MW	MW	MW	MW
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
Original Categories								
Post Period 2015-2019	0.55	0.81	0.52	0.33	1.37	1.86	1.52	0.72
	[0.34,0.74]	[0.46,1.02]	[0.31,0.78]	[0.06,0.58]	[1.16,1.59]	[1.40,2.27]	[1.31,1.74]	[0.37,1.12]
Post Period 2015	0.45	0.84	0.15	0.35	0.80	1.20	0.71	0.50
	[0.21,0.65]	[0.36,1.22]	[-0.22,0.56]	[0.11,0.51]	[0.71,0.88]	[1.01,1.43]	[0.56,0.78]	[0.40,0.58]
Post Period 2019	0.91	1.38	0.79	0.56	2.22	3.10	2.15	1.42
	[0.46,1.23]	[0.64,1.83]	[0.36,1.24]	[0.03,1.09]	[1.83,2.66]	[2.18,3.68]	[1.78,2.53]	[0.83,2.40]
Overall Average Effects	0.73	1.09	0.66	0.45	1.80	2.48	1.84	1.07
	[0.36,1.18]	[0.50,1.72]	[0.31,1.18]	[0.05,1.01]	[1.17,2.57]	[1.48,3.65]	[1.33,2.45]	[0.45,2.03]

Notes: This table presents averages across estimates from the regression analyses in our pre-analysis plan. The underlying estimates are calculated from regression equation (3) from Clemens and Strain (2017). They are thus estimates of the change in either the hourly wage (OW) or in the applicable minimum wage (MW) among individuals in our analysis samples from states that increased their minimum wages relative to individuals in states that did not increase their minimum wages. The numbers in brackets below each average are 95 percent confidence intervals generated by bootstrapping the estimated average. The key dimensions along which we average the estimates (e.g., contrasting time periods, contrasting the “Low-Skilled” and “Young” samples, or contrasting the effects of “Large” increases, “Small” increases, and the inflation-indexed minimum wage changes enacted by the “Indexer” group) are clearly labeled in the body of the table. For estimated effects on hourly wages, we use data from the CPS ORG and for estimated effects on minimum wages we use data from the basic monthly CPS. The grouping of states we describe as “Original” corresponds with the grouping in Panel A of Figure 1, which is the grouping from our original pre-analysis plan. The "Post Period 2015" results were not part of the pre-analysis plan and are thus not included in the "Overall Average Effects" calculations. Panel A includes employed individuals 16 to 25 with less than a completed high school education and Panel B includes employed individuals ages 16 to 21.

**Table A11. Summary of Elasticities Using Regression Specifications from Clemens and Strain (2017)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Skill Group	Low-Skilled	Low-Skilled	Low-Skilled	Low-Skilled	Young	Young	Young	Young
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
<u>Panel A: Employment</u>								
Overall Average Effects	-0.004	-0.032	0.016	0.005	-0.007	-0.020	0.000	0.000
Mean in 2011-2013 Baseline	0.212	0.188	0.246	0.222	0.366	0.330	0.415	0.384
Change from Baseline (%)	-1.777	-16.831	6.333	2.147	-1.841	-6.185	-0.014	0.061
<u>Panel B: Minimum Wages</u>								
Overall Average Effects	1.794	2.473	1.821	1.087	1.796	2.476	1.837	1.074
Mean in 2011-2013 Baseline	7.689	7.721	7.407	7.804	7.685	7.713	7.411	7.810
Change from Baseline (%)	23.330	32.024	24.591	13.932	23.368	32.107	24.787	13.746
<u>Panel C: Hourly Wages</u>								
Overall Average Effects	0.962	1.368	0.869	0.648	0.732	1.093	0.655	0.447
Mean in 2011-2013 Baseline	8.769	9.192	8.448	8.549	9.197	9.535	8.963	8.978
Change from Baseline (%)	10.968	14.879	10.291	7.582	7.958	11.464	7.312	4.981
Elasticity of Hourly Wage w.r.t Minimum Wage	0.470	0.465	0.418	0.544	0.341	0.357	0.295	0.362
<u>Panel D Elasticities</u>								
Own Wage	-0.166	-1.113	0.607	0.299	-0.239	-0.543	-0.002	0.013
	[-1.47,0.39]	[-2.88,-0.34]	[-1.38,1.63]	[-1.79,1.09]	[-1.18,0.08]	[-1.72,-0.22]	[-1.05,0.51]	[-2.00,0.85]
Minimum Wage	-0.077	-0.513	0.251	0.164	-0.080	-0.192	-0.001	0.005
	[-0.46,0.23]	[-1.09,-0.17]	[-0.33,0.65]	[-0.57,0.83]	[-0.36,0.03]	[-0.50,-0.07]	[-0.43,0.20]	[-0.48,0.30]

Notes: This table reports average employment and wage effects for each minimum wage policy group and skill group along with own-wage and minimum wage elasticities using the regression specifications from Clemens and Strain (2017). The Own Wage elasticity is calculated as the ratio of the bottom row of Panel A (i.e., the percent change in employment from baseline) divided by the bottom row of Panel C (i.e., the percent change in hourly wages from baseline), while the Minimum Wage elasticity is calculated as the ratio of the bottom row of Panel A (i.e., the percent change in employment from baseline) divided by the bottom row of Panel B (i.e., the percent change in the statutory minimum wage from baseline). The numbers in brackets below each elasticity are 95 percent confidence intervals generated by bootstrapping the estimated elasticity. The baseline mean for the employment panel comes from the ACS and the overall average effects on employment are calculated from regression estimates on data from the ACS and CPS. The baseline mean and estimated overall average effects on minimum wages come from the basic monthly CPS. The baseline mean and estimated overall average effects on hourly wages come from the CPS ORG. Averages in the “mean in 2011-2013 baseline” rows are calculated using our original policy categories, while those in the “overall average effects rows” use results generated on both the original and new policy categories. Low-Skilled individuals are ages 16 to 25 with less than a completed high school education and young individuals are ages 16 to 21. Average effects for employment (panel A), minimum wages (panel B), and hourly wages (panel C) are taken from Tables 4 and A10.

**Table A12. Summary of Employment Regression Results for ACS and CPS Samples**

Panel A: Low-Skilled Workers								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	ACS	ACS	ACS	ACS	CPS	CPS	CPS	CPS
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	-0.005 [-.023,.005]	-0.028 [-.040,-.014]	-0.001 [-.032,.016]	0.012 [-.006,.026]	-0.001 [-.017,.012]	-0.029 [-.048,-.009]	0.027 [.003,.045]	0.000 [-.017,.013]
\$1 Cutoff; Post Period 2015	0.003 [-.010,.012]	-0.015 [-.030,-.003]	0.011 [-.015,.025]	0.014 [-.001,.025]	-0.005 [-.018,.009]	-0.015 [-.027,-.000]	-0.004 [-.025,.023]	0.005 [-.017,.024]
\$1 Cutoff; Post Period 2019	-0.017 [-.040,-.003]	-0.046 [-.061,-.030]	-0.012 [-.049,.012]	0.007 [-.018,.025]	0.002 [-.021,.024]	-0.037 [-.062,-.007]	0.046 [-.001,.088]	-0.003 [-.027,.023]
\$1 Cutoff; No Switchers; Post Period 2015-2019	-0.006 [-.025,.009]	-0.027 [-.040,-.008]	-0.001 [-.033,.019]	0.011 [-.010,.037]	-0.002 [-.016,.010]	-0.029 [-.045,-.011]	0.027 [-.001,.046]	-0.005 [-.024,.020]
\$1 Cutoff; No Switchers; Post Period 2019	-0.016 [-.045,.002]	-0.046 [-.061,-.024]	-0.011 [-.051,.017]	0.009 [-.039,.044]	0.000 [-.025,.021]	-0.036 [-.061,-.011]	0.046 [-.006,.085]	-0.011 [-.030,.022]
\$2.5 Cutoff; Post Period 2019	-0.015 [-.036,.007]	-0.044 [-.061,-.007]	-0.009 [-.037,.017]	0.009 [-.023,.043]	-0.004 [-.031,.019]	-0.019 [-.048,.026]	0.018 [-.023,.053]	-0.011 [-.035,.004]
Overall Average Effects	-0.012 [-.037,.006]	-0.038 [-.060,-.013]	-0.007 [-.045,.017]	0.010 [-.021,.039]	-0.001 [-.022,.019]	-0.030 [-.058,.013]	0.033 [-.012,.074]	-0.006 [-.029,.018]
Panel B: Young Workers								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	ACS	ACS	ACS	ACS	CPS	CPS	CPS	CPS
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	-0.008 [-.021,-.001]	-0.020 [-.036,-.009]	-0.008 [-.031,.005]	0.003 [-.007,.012]	-0.002 [-.016,.008]	-0.014 [-.040,-.002]	0.005 [-.012,.018]	0.005 [-.009,.018]
\$1 Cutoff; Post Period 2015	-0.003 [-.016,.005]	-0.015 [-.034,-.001]	0.002 [-.020,.016]	0.002 [-.009,.010]	-0.006 [-.020,.005]	-0.009 [-.028,.005]	-0.006 [-.027,.012]	-0.003 [-.022,.016]
\$1 Cutoff; Post Period 2019	-0.014 [-.029,-.005]	-0.025 [-.039,-.015]	-0.015 [-.044,.002]	-0.003 [-.017,.008]	-0.001 [-.021,.013]	-0.022 [-.055,-.001]	0.016 [-.002,.031]	0.003 [-.020,.032]
\$1 Cutoff; No Switchers; Post Period 2015-2019	-0.008 [-.022,.001]	-0.020 [-.035,-.009]	-0.007 [-.027,.009]	0.003 [-.010,.018]	-0.006 [-.018,.006]	-0.015 [-.039,-.000]	0.005 [-.013,.018]	-0.008 [-.017,.032]
\$1 Cutoff; No Switchers; Post Period 2019	-0.013 [-.029,-.002]	-0.025 [-.040,-.015]	-0.015 [-.042,.005]	0.001 [-.016,.022]	-0.008 [-.025,.011]	-0.023 [-.056,-.004]	0.016 [-.004,.033]	-0.017 [-.035,.042]
\$2.5 Cutoff; Post Period 2019	-0.013 [-.026,-.001]	-0.021 [-.034,-.009]	-0.014 [-.032,.003]	-0.003 [-.023,.017]	-0.002 [-.022,.020]	-0.001 [-.023,.035]	0.001 [-.027,.021]	-0.006 [-.026,.043]
Overall Average Effects	-0.011 [-.027,-.001]	-0.022 [-.038,-.010]	-0.012 [-.038,.005]	0.000 [-.018,.018]	-0.004 [-.021,.013]	-0.015 [-.049,.017]	0.009 [-.018,.028]	-0.005 [-.028,.034]

Notes: This table presents averages across estimates from the regression analyses in our pre-analysis plan. The underlying estimates are, in each case, estimates of  $\beta_{(g(s))}$  from equation (1) or equation (2). They are thus estimates of the change in the employment rate among individuals in our analysis samples from states that increased their minimum wages relative to individuals in states that did not. The numbers in brackets below each average are 95 percent confidence intervals generated by bootstrapping the estimated average. The key dimensions along which we average the estimates (e.g., contrasting time periods, contrasting analyses using ACS vs. CPS data, contrasting the “Low-Skilled” and “Young” samples, or contrasting the effects of “Large” increases, “Small” increases, and the inflation-indexed minimum wage changes enacted by the “Indexer” group) are labeled in the table. The grouping of states we describe as “\$1 Cutoff” corresponds with Panel A of Figure 1, which is the grouping from our original pre-analysis plan. The grouping of states we describe as “\$2.5 Cutoff” corresponds with Panel B of Figure 1, reflecting minimum wage changes enacted after we developed our pre-analysis plan. (Note that the inclusion of estimates involving updated groupings was, itself, specified in our pre-analysis plan.) The “\$1 Cutoff Post Period 2015” results were not part of the pre-analysis plan and are thus not included in the “Overall Average Effects” calculations. Panel A includes individuals 16 to 25 with less than a completed high school education and Panel B includes all individuals ages 16 to 21.

**Table A13. Summary of Employment Regression Results Using Specifications from Clemens and Strain (2017) for Separate ACS and CPS Samples**

Panel A: Low-Skilled Workers								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	ACS	ACS	ACS	ACS	CPS	CPS	CPS	CPS
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
Original Categories								
Post Period 2015-2019	-0.004 [-.021,.007]	-0.024 [-.038,-.009]	-0.001 [-.029,.016]	0.013 [-.007,.027]	0.000 [-.016,.013]	-0.027 [-.046,-.008]	0.027 [.001,.048]	0.001 [-.018,.014]
Post Period 2015	0.004 [-.012,.014]	-0.012 [-.030,.005]	0.009 [-.025,.025]	0.016 [-.003,.028]	-0.005 [-.019,.009]	-0.018 [-.030,-.003]	-0.001 [-.023,.026]	0.005 [-.018,.025]
Post Period 2019	-0.015 [-.039,.001]	-0.041 [-.061,-.023]	-0.011 [-.048,.015]	0.008 [-.022,.027]	0.003 [-.022,.027]	-0.035 [-.065,-.002]	0.046 [-.001,.089]	-0.003 [-.029,.027]
Overall Average Effects	-0.009 [-.036,.006]	-0.032 [-.058,-.013]	-0.006 [-.048,.016]	0.011 [-.020,.027]	0.002 [-.019,.023]	-0.031 [-.061,-.004]	0.037 [-.000,.080]	-0.001 [-.027,.025]
Panel B: Young Workers								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	ACS	ACS	ACS	ACS	CPS	CPS	CPS	CPS
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
Original Categories								
Post Period 2015-2019	-0.008 [-.021,.000]	-0.019 [-.035,-.008]	-0.007 [-.030,.006]	0.002 [-.009,.012]	-0.002 [-.017,.008]	-0.015 [-.040,-.002]	0.005 [-.012,.019]	0.003 [-.010,.018]
Post Period 2015	-0.004 [-.016,.004]	-0.015 [-.034,-.000]	0.002 [-.019,.016]	0.001 [-.011,.010]	-0.008 [-.021,.003]	-0.012 [-.029,.002]	-0.007 [-.028,.011]	-0.005 [-.024,.014]
Post Period 2019	-0.014 [-.030,-.004]	-0.024 [-.039,-.013]	-0.014 [-.043,.003]	-0.005 [-.022,.008]	-0.002 [-.022,.012]	-0.024 [-.056,-.004]	0.016 [-.002,.032]	0.000 [-.024,.029]
Overall Average Effects	-0.011 [-.029,-.000]	-0.022 [-.039,-.009]	-0.011 [-.039,.005]	-0.001 [-.020,.011]	-0.002 [-.020,.010]	-0.019 [-.051,-.002]	0.011 [-.010,.029]	0.002 [-.020,.023]

Notes: This table presents averages across estimates from the regression analyses in our pre-analysis plan. The underlying estimates are, in each case, estimates of either equation (1) or equation (2). They are thus estimates of the change in the employment rate among individuals in our analysis samples from states that increased their minimum wages relative to individuals in states that did not increase their minimum wages. The numbers in brackets below each average are 95 percent confidence intervals generated by bootstrapping the estimated average. The key dimensions along which we average the estimates (e.g., contrasting time periods, contrasting analyses using ACS vs. CPS data, contrasting the “Low-Skilled” and “Young” samples, or contrasting the effects of “Large” increases, “Small” increases, and the inflation-indexed minimum wage changes enacted by the “Indexer” group) are clearly labeled in the body of the table. The grouping of states we describe as “Original” corresponds with the grouping in Panel A of Figure 1, which is the grouping from our original pre-analysis plan. The “Post Period 2015” results were not part of the pre-analysis plan and are thus not included in the “Overall Average Effects” calculations. Panel A includes individuals 16 to 25 with less than a completed high school education and Panel B includes all individuals ages 16 to 21.

**Table A14. Summary of Employment Regression Results From Specifications With No Time-Varying Covariates**

Panel A: Low-Skilled Workers				
	(1)	(2)	(3)	(4)
Sample	All	All	All	All
Policy Group	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	-0.004 [-.018,.008]	-0.028 [-.044,-.009]	0.012 [-.017,.029]	0.005 [-.010,.019]
\$1 Cutoff; Post Period 2015	-0.002 [-.013,.007]	-0.017 [-.028,.002]	0.003 [-.020,.019]	0.009 [-.004,.019]
\$1 Cutoff; Post Period 2019	-0.008 [-.024,.011]	-0.039 [-.058,-.014]	0.015 [-.025,.045]	0.002 [-.014,.019]
\$1 Cutoff; No Switchers; Post Period 2015-2019	-0.005 [-.021,.009]	-0.028 [-.041,-.007]	0.012 [-.014,.031]	0.001 [-.011,.014]
\$1 Cutoff; No Switchers; Post Period 2019	-0.009 [-.029,.010]	-0.039 [-.057,-.017]	0.015 [-.028,.042]	-0.002 [-.021,.020]
\$2.5 Cutoff; Post Period 2019	-0.010 [-.025,.013]	-0.031 [-.051,.014]	0.003 [-.027,.035]	-0.003 [-.020,.012]
Overall Average Effects	-0.007 [-.025,.010]	-0.033 [-.054,-.004]	0.011 [-.024,.039]	0.000 [-.017,.018]
Panel B: Young Workers				
	(1)	(2)	(3)	(4)
Sample	All	All	All	All
Policy Group	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	-0.003 [-.017,.003]	-0.014 [-.035,-.005]	-0.001 [-.023,.012]	0.005 [-.005,.016]
\$1 Cutoff; Post Period 2015	-0.004 [-.014,.004]	-0.010 [-.027,-.001]	-0.002 [-.022,.012]	0.001 [-.010,.011]
\$1 Cutoff; Post Period 2019	-0.006 [-.021,.002]	-0.019 [-.047,-.008]	0.000 [-.022,.017]	0.002 [-.011,.016]
\$1 Cutoff; No Switchers; Post Period 2015-2019	-0.006 [-.017,.001]	-0.016 [-.037,-.004]	0.000 [-.017,.013]	-0.003 [-.010,.018]
\$1 Cutoff; No Switchers; Post Period 2019	-0.009 [-.021,.001]	-0.019 [-.050,-.008]	0.000 [-.020,.017]	-0.008 [-.018,.025]
\$2.5 Cutoff; Post Period 2019	-0.007 [-.018,.007]	-0.009 [-.023,.013]	-0.006 [-.024,.014]	-0.006 [-.015,.015]
Overall Average Effects	-0.006 [-.020,.003]	-0.015 [-.040,.000]	-0.002 [-.022,.014]	-0.002 [-.015,.016]

Notes: This table presents averages across estimates from the regression analyses in our pre-analysis plan not including time-varying covariates and are estimates of  $\beta_{(g(s))}$  from either equation (1) or equation (2). They are thus estimates of the change in the employment rate among individuals in our analysis samples from states that increased their minimum wages relative to individuals in states that did not increase their minimum wages. The numbers in brackets below each average are 95 percent confidence intervals generated by bootstrapping the estimated average. The key dimensions along which we average the estimates (e.g., contrasting time periods, contrasting the “Low-Skilled” and “Young” samples, or contrasting the effects of “Large” increases, “Small” increases, and the inflation-indexed minimum wage changes enacted by the “Indexer” group) are clearly labeled in the body of the table. The grouping of states we describe as “\$1 Cutoff” corresponds with the grouping in Panel A of Figure 1, which is the grouping from our original pre-analysis plan. The grouping of states we describe as “\$2.5 Cutoff” corresponds with the grouping in Panel B of Figure 1, which reflects minimum wage changes enacted after we developed our pre-analysis plan. (Note that the inclusion of estimates involving updated groupings was, itself, specified in our pre-analysis plan.) The “\$1 Cutoff Post Period 2015” results were not part of the pre-analysis plan and are thus not included in the “Overall Average Effects” calculations. Panel A includes individuals 16 to 25 with less than a completed high school education and Panel B includes all individuals ages 16 to 21.



**Table A15. Summary of Wage Regression Results From Specifications With No Time-Varying Covariates**

Panel A: Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	OW	OW	OW	OW	MW	MW	MW	MW
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	0.96 [0.64,1.22]	1.43 [0.95,1.63]	0.75 [0.39,0.98]	0.70 [0.39,1.14]	1.62 [1.41,1.83]	2.32 [1.68,2.49]	1.51 [1.36,1.70]	1.03 [0.62,1.46]
\$1 Cutoff; Post Period 2015	0.65 [0.25,1.32]	1.17 [0.29,2.95]	0.33 [-0.04,0.61]	0.44 [0.16,0.71]	0.78 [0.71,0.89]	1.15 [1.02,1.44]	0.70 [0.56,0.78]	0.48 [0.45,0.52]
\$1 Cutoff; Post Period 2019	1.36 [0.67,1.83]	2.11 [1.09,2.61]	0.93 [0.27,1.31]	1.03 [0.30,1.99]	2.50 [1.98,2.86]	3.59 [2.24,3.94]	2.11 [1.82,2.44]	1.80 [0.95,2.69]
\$1 Cutoff; No Switchers; Post Period 2015-2019	0.87 [0.61,1.01]	1.44 [0.99,1.60]	0.76 [0.28,0.96]	0.41 [0.19,0.56]	1.49 [1.23,1.58]	2.33 [1.48,2.51]	1.52 [1.38,1.73]	0.60 [0.58,0.62]
\$1 Cutoff; No Switchers; Post Period 2019	1.10 [0.47,1.40]	2.14 [0.83,2.55]	0.95 [0.26,1.27]	0.22 [-0.26,0.51]	2.21 [1.66,2.41]	3.62 [1.90,3.95]	2.13 [1.83,2.52]	0.89 [0.89,0.90]
\$2.5 Cutoff; Post Period 2019	1.29 [0.90,1.69]	2.33 [1.74,2.71]	1.24 [0.71,1.83]	0.29 [-0.18,0.71]	2.37 [2.21,2.59]	3.88 [3.58,3.98]	2.28 [2.00,2.82]	0.96 [0.89,1.29]
Overall Average Effects	1.12 [0.61,1.65]	1.89 [0.98,2.61]	0.93 [0.35,1.61]	0.53 [-0.15,1.58]	2.04 [1.38,2.70]	3.15 [1.86,3.96]	1.91 [1.39,2.60]	1.06 [0.59,2.29]
Panel B: Young Workers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	OW	OW	OW	OW	MW	MW	MW	MW
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
\$1 Cutoff; Post Period 2015-2019	0.72 [0.44,0.90]	1.12 [0.68,1.30]	0.53 [0.30,0.83]	0.50 [0.20,0.88]	1.63 [1.42,1.84]	2.33 [1.68,2.50]	1.52 [1.36,1.71]	1.03 [0.62,1.48]
\$1 Cutoff; Post Period 2015	0.39 [0.19,0.64]	0.71 [0.41,1.25]	0.13 [-0.26,0.56]	0.31 [0.10,0.49]	0.78 [0.71,0.89]	1.15 [1.02,1.44]	0.71 [0.56,0.77]	0.48 [0.45,0.52]
\$1 Cutoff; Post Period 2019	1.09 [0.56,1.44]	1.75 [0.88,2.15]	0.77 [0.33,1.20]	0.76 [0.16,1.47]	2.50 [1.98,2.87]	3.60 [2.27,3.94]	2.13 [1.82,2.46]	1.77 [0.95,2.68]
\$1 Cutoff; No Switchers; Post Period 2015-2019	0.64 [0.44,0.79]	1.12 [0.55,1.30]	0.54 [0.28,0.82]	0.26 [0.01,0.51]	1.49 [1.22,1.59]	2.34 [1.47,2.52]	1.54 [1.38,1.73]	0.60 [0.58,0.62]
\$1 Cutoff; No Switchers; Post Period 2019	0.91 [0.52,1.19]	1.78 [0.59,2.10]	0.79 [0.26,1.30]	0.17 [-0.12,0.68]	2.23 [1.67,2.42]	3.63 [1.90,3.95]	2.16 [1.84,2.54]	0.89 [0.89,0.91]
\$2.5 Cutoff; Post Period 2019	1.04 [0.68,1.34]	1.93 [1.50,2.22]	0.95 [0.46,1.43]	0.24 [-0.02,0.66]	2.38 [2.22,2.59]	3.88 [3.60,3.98]	2.29 [2.00,2.83]	0.96 [0.89,1.30]
Overall Average Effects	0.88 [0.46,1.30]	1.54 [0.69,2.14]	0.72 [0.30,1.28]	0.39 [-0.04,1.06]	2.04 [1.39,2.69]	3.16 [1.87,3.96]	1.93 [1.41,2.61]	1.05 [0.59,2.29]

Notes: This table presents averages across estimates from the regression analyses in our pre-analysis plan not including time-varying covariates. The underlying estimates are, in each case, estimates of  $\beta_{(g(s))}$  from either equation (1) or equation (2). They are thus estimates of the change in the hourly wage or minimum wage rate among individuals in our analysis samples from states that increased their minimum wages relative to individuals in states that did not increase their minimum wages. The numbers in brackets below each coefficient are 95 percent confidence intervals generated by bootstrapping the estimated coefficient. The key dimensions along which we average the estimates (e.g., contrasting time periods, contrasting the “Low-Skilled” and “Young” samples, or contrasting the effects of “Large” increases, “Small” increases, and the inflation-indexed minimum wage changes enacted by the “Indexer” group) are clearly labeled in the body of the table. For estimated effects on hourly wages, we use data from the CPS ORG, and for estimated effects on minimum wages we use data from the basic monthly CPS. The grouping of states we describe as “\$1 Cutoff” corresponds with the grouping in Panel A of Figure 1, which is the grouping from our original pre-analysis plan. The grouping of states we describe as “\$2.5 Cutoff” corresponds with the grouping in Panel B of Figure 1, which reflects minimum wage changes enacted after we developed our pre-analysis plan. (Note that the inclusion of estimates involving updated groupings was, itself, specified in our pre-analysis plan.) The “\$1 Cutoff Post Period 2015” results were not part of the pre-analysis plan and are thus not included in the “Overall Average Effects” calculations. Panel A includes individuals ages 25 and younger with less than a completed high school education and Panel B includes all individuals ages 16 to 21.

**Table A16. Summary of Wage Regression Elasticities From Specifications With No Time-Varying Covariates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Skill Group	Low-Skilled	Low-Skilled	Low-Skilled	Low-Skill	Young	Young	Young	Young
Policy Group	All Change	Large	Small	Indexer	All Change	Large	Small	Indexer
<u>Panel A: Employment</u>								
Overall Average Effects	-0.007	-0.033	0.011	0.000	-0.006	-0.015	-0.002	-0.002
Mean in 2011-2013 Baseline	0.212	0.188	0.246	0.222	0.366	0.330	0.415	0.384
Change from Baseline (%)	-3.361	-17.540	4.529	0.216	-1.733	-4.597	-0.410	-0.563
<u>Panel B: Minimum Wages</u>								
Overall Average Effects	2.038	3.147	1.912	1.055	2.045	3.157	1.926	1.052
Mean in 2011-2013 Baseline	7.689	7.721	7.407	7.804	7.685	7.713	7.411	7.810
Change from Baseline (%)	26.510	40.764	25.811	13.525	26.612	40.927	25.990	13.469
<u>Panel C: Hourly Wages</u>								
Overall Average Effects	1.116	1.891	0.926	0.530	0.882	1.539	0.717	0.388
Mean in 2011-2013 Baseline	8.769	9.192	8.448	8.549	9.197	9.535	8.963	8.978
Change from Baseline (%)	12.726	20.577	10.967	6.199	9.586	16.143	8.005	4.322
Elasticity of Hourly Wage w.r.t Minimum Wage	0.480	0.505	0.425	0.458	0.360	0.394	0.308	0.321
<u>Panel D Elasticities</u>								
Own Wage	-0.264	-0.852	0.413	0.035	-0.181	-0.285	-0.051	-0.130
	[-1.26,0.35]	[-1.85,-0.11]	[-1.79,1.37]	[-5.72,3.47]	[-0.90,0.08]	[-1.39,0.00]	[-1.28,0.44]	[-6.40,3.67]
Minimum Wage	-0.127	-0.430	0.175	0.016	-0.065	-0.112	-0.016	-0.042
	[-0.45,0.17]	[-0.81,-0.05]	[-0.39,0.62]	[-0.66,0.63]	[-0.22,0.03]	[-0.41,0.00]	[-0.21,0.15]	[-0.36,0.24]

Notes: This table reports average employment and wage effects for each minimum wage policy group and skill group along with own-wage and minimum wage elasticities not including time-varying covariates. The numbers in brackets below each elasticity are 95 percent confidence intervals generated by bootstrapping the estimated elasticity. The baseline mean for the employment panel comes from the ACS and the overall average effects on employment are calculated from regression estimates on data from the ACS and CPS. The baseline mean and estimated overall average effects on minimum wages come from the basic monthly CPS. The baseline mean and estimated overall average effects on hourly wages come from the CPS ORG. Averages in the “mean in 2011-2013 baseline” rows are calculated using our original policy categories, while those in the “overall average effects rows” use results generated on both the original and new policy categories. Low-Skilled individuals are ages 16-25 with less than a completed high school education and young individuals are ages 16 to 21. Average effects for employment (panel A), minimum wages (panel B) and hourly wages (panel C) are taken from Tables A14 and A15. The hourly wage elasticity with respect to the minimum wage is the percentage change in average hourly wages from the baseline period of 2011-2013 divided by the percentage change in minimum wages from 2011-2013. The own-wage elasticity for each policy-skill group is the percentage change in employment divided by the percentage change in average hourly wages from the baseline period of 2011-2013 and the minimum wage elasticity is the percentage change in employment divided by the percentage change in the minimum wage from 2011-2013.

## Online Appendix B: Additional Analyses Outside of Our Pre-commitment Plan

In this appendix, we present a set of analyses using ACS data that are outside of our pre-analysis plan but that provide additional evidence on the validity and economic implications of our findings.

For example, we further investigate the dynamics with which our estimated effects unfold. Additionally, we implement estimators recommended by recent applied econometrics papers that have shed new light on best-practice methods for difference-in-differences settings characterized by staggered treatment adoption and heterogeneous treatment effects (De Chaisemartin and d'Haultfoeuille, 2020; Baker, Larcker, and Wang, 2022; Borusyak, Jaravel, and Spiess, 2024; Goodman-Bacon, 2021). To improve our ability to explore pre-treatment trends, we add data from 2010 to the samples for these analyses.

### *Additional Estimation Frameworks*

In this section, we present four additional pieces of analysis. First, we increment modestly from our pre-committed research designs to present estimates that track employment dynamically in calendar time across our original policy groupings as defined in Panel A of Figure 1:

$$Y_{i,s,g,t} = \sum_{g \neq 0} \sum_{t \neq 2013} \beta_{g,t} Policy_{g(s)} \times Time_t + \alpha_{1s} State_s + \alpha_{2t} Time_t + X_{i,s,t} \gamma + \varepsilon_{i,s,t}. \quad (3)$$

Equation (3) has both strengths and drawbacks. A strength is that because all estimates are constructed relative to a base year of 2013, the estimator can track dynamics without being subject to critiques raised in the methodological papers referenced above. Absent time varying controls, for example, it is not subject to concerns associated with negative weights, which can arise when treatment is assigned to different observations at different points in time (Goodman-

Bacon, 2021). On the other hand, because equation (3) does not encode variations in the timing with which states enacted their first minimum wage changes, the estimates are not fully informative regarding the evolution of employment with “time since treatment.”

We next present estimates using what is commonly known as an event study framework, as described by equation (4):

$$Y_{i,s,g,t,p(s,t)} = \sum_{g \neq 0} \sum_{p(s,t) \neq 0} \beta_{g,p(s,t)} Policy_{g(s)} \times Time_{p(s,t)} + \alpha_{1s} State_s + \alpha_{2t} Time_t + X_{i,s,t} \gamma + \varepsilon_{i,s,t}, \quad (4)$$

in which  $p(s,t)$  describes when each calendar year falls relative to the year immediately before a state implemented its first minimum wage change due to new legislation, and where  $Time_{p(s,t)}$  is a set of dummy variables associated with each value of  $p(s,t)$ . The first treatment year for each state with a statutory increase is the first year that an increase mandated by new legislation goes into effect.<sup>18</sup> A full list of states and the associated year of first statutory increase is in Table B1. Our assignment of “large” and “small” increases uses the original policy groupings defined in Panel A of Figure 1.<sup>19</sup> Relative to equation (3), equation (4) has the benefit of tracking the relationship between employment and minimum wages in a way that captures time since

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<sup>18</sup> For example, California signed legislation to increase the state minimum wage on September 25, 2013. The first increase associated with the new legislation was on July 1, 2014. Therefore, we assign “year 1” for California to be 2014. Missouri indexed the state minimum wage to inflation until January of 2019. Missouri citizens approved a ballot initiative on November 6, 2018 to increase the state minimum wage to \$12 by 2023. The first increase in this series occurred on January 1, 2019. We thus assign “year 1” for Missouri to be 2019. A slight tweak on this assignment rule involves the state of New York. New York passed legislation in March 2013 to increase its minimum wage on December 31, 2013. We assign the year of first statutory increase to 2014, reflecting that 2014 was the first year during which the increase was in effect.

<sup>19</sup> We omit all indexer states (based on Panel A of Figure 1) from event study regressions based on time relative to first statutory increase because these states had minimum wage increases from these indexing provisions prior to enacting any statutory increase.

treatment. As documented in the methodological papers referenced above, however, the implicit weightings underlying the event study framework’s estimates may lead to misleading concerns regarding “pre-trends”, may fail to describe any treatment effects of genuine interest, and may even carry the opposite sign of the underlying effects of interest.

To resolve these issues and to shed additional light on the validity of our estimates, we provide evidence from two proposed solutions to the econometric problems that can arise in settings with staggered treatment rollouts and treatment effect heterogeneity. First, we implement a design described by Baker, Larcker, and Wang (2022) as the “stacked regression estimator.” This estimator has gained traction in the context of minimum wage analyses through its use by Cengiz *et al.* (2019) in their study of a long panel of historical minimum wage changes:

$$Y_{i,s,g,c,t,p(s,t)} = \sum_{g \neq 0} \sum_{p(s,t) \neq 0} \beta_{g,p(s,t)} Policy_{g(s)} \times Time_{p(s,t)} + \alpha_{1s,c} State_{s,c} + \alpha_{2t} Time_t + X_{i,s,t} \gamma + \varepsilon_{i,s,t}. \quad (5)$$

The stacked event study estimator is described by equation (5). The equation is estimated on a data set constructed through the following steps. First, we create separate, event-by-cohort-specific data sets for each policy cohort, by which we refer to the group of states that implemented their first minimum wage increase during a particular year. Each cohort-specific data set consists of the relevant policy cohort plus the set of control states that implemented no minimum wage changes across the duration of our sample. Within each cohort-specific data set, time is specified in “event time” with respect to the number of years relative to the year in which the policy cohort implemented its first statutory minimum wage changes. We then append (or “stack”) these policy-cohort data sets on top of one another. The stacked data set thus contains

replicates of the observations associated with the control groups. As discussed by Baker, Larcker, and Wang (2022), a relevant change in equation (5) relative to equation (4) is the inclusion of a set of cohort-by-state effects to account for the multiple appearances of observations from the never-treated control states, in which the observations from these states are associated with different time periods,  $p(s,t)$ , relative to the year in which a given policy cohort implemented its minimum wage increases.

Baker, Larcker, and Wang (2022) provide additional discussion of why the stacked event study estimator eliminates the problem of negative weights. For intuition on why this is the case, note that the specification produces estimates equivalent to what one would obtain by estimating a separate regression for each of the policy cohorts, then weighting across those estimates. Recall that the problem of negative weights can arise due to the presence of staggered treatment timing. Now note that staggered treatment timing is eliminated if separate regressions are run for each policy cohort. In effect, the stacked event study rearranges the data so that treatment events are coded as though they occur simultaneously. It is thus straightforward to see that this estimator resolves a key driver of the negative weights problem by effectively eliminating staggered treatment timing.

Finally, we implement a design developed by Borusyak, Jaravel, and Spiess (2024), which is well suited to our setting. The “imputed causal effects” approach of Borusyak, Jaravel, and Spiess involves an intuitive, multi-step procedure. First, state effects, time effects, and coefficients on time-varying covariates are estimated on untreated observations. Then, the counterfactual outcome for each treated observation is “imputed” using the coefficients from the first step.<sup>20</sup> In the final step, treatment effects are estimated by comparing and aggregating the

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<sup>20</sup> As discussed by Borusyak, Jaravel, and Spiess, the presence of never-treated states is essential for the implementation of this step to generate counterfactual estimates for all treated observations. For this purpose, the

realized and counterfactual outcomes for treated units. These treatment effects can be aggregated along a variety of dimensions of interest. In our case, the dimensions of interest include distinguishing across categories of treatment and distinguishing between short- and longer-run effects, both of which are key components of our pre-committed analyses. Borusyak, Jaravel, and Spiess provide a complementary approach to checking for the potential relevance of divergent pre-existing trends through estimates that rely exclusively on untreated observations.

In the analysis below, we present estimates of equations (3), (4), and (5), as well as estimates from the “imputed causal effects” approach of Borusyak, Jaravel, and Spiess. In each case, we present estimates of dynamic causal effects using standard event-study figures. We present estimates using a “sparse” set of controls and a “rich” set of controls. The sparse control set consists of the log of personal income per capita and the median house price index. The rich set of controls adds sets of age and education fixed effects, as well as three-year changes in the log of personal income per capita and the median house price index, the rationale for which we discuss below. We additionally present a set of falsification checks in which we run this same set of analyses on samples that consist of the full prime age population.<sup>21</sup>

#### *Results from Supplemental Analyses Using Recently Proposed Estimation Frameworks*

Figure B1 presents estimates of equation (3), which allows us to track the calendar time dynamics of the employment changes that occurred in states that increased their minimum wages relative to states that did not. The estimates for years prior to 2013 provide an indication of whether there were divergent trends in the treatment states relative to the control states during

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presence of many never-treated states is a strength of our empirical setting.

<sup>21</sup> Maine had its first statutory minimum wage increase in 2017 and no minimum wage changes from 2011-2015. Since it is a “no change” state according to the definition from Figure 1 Panel A, but did have a large statutory minimum wage increase (as shown in Figure 1 Panel B), we include Maine only in the “Large Increasers” estimates.

the years preceding the enactment of new minimum wage changes. Estimates for subsequent years provide evidence on the full dynamics of employment's evolution as minimum wage changes went into effect.

Focusing first on estimates for years preceding 2013, none of the estimates are statistically significantly distinguishable from zero. This is reassuring with respect to concerns related to divergent pre-existing trends. Focusing on estimates for the large increaser states, the pre-2013 time profile is almost perfectly flat for the sample of individuals ages 16 to 21. For the sample of individuals ages 16 to 25 with less than a completed high school education, one could arguably see signs of a modest negative trend.<sup>22</sup> This leads us to consider what factors might differ between our control states and states that enacted large minimum wage changes.

A potentially relevant feature of the time period and labor markets we analyze is that states that enacted large minimum wage changes included states that experienced particularly large shocks due to the housing crisis that precipitated the 2008 global financial crisis and Great Recession. This leads us to consider whether medium-run changes in housing prices and aggregate income might be relevant to the relative changes in these states' employment rates among low-skilled individuals. We investigate this possibility by incorporating three-year changes in both of these variables into the specifications we label as having "rich controls."

Panels B and D of Figure B1 reveal that the inclusion of the richer set of covariates has

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<sup>22</sup> Roth (2022) demonstrates that this kind of tasseography based on event study plots can be scientifically counterproductive. Specifically, he demonstrates that pre-testing on the basis of pre-treatment estimates in event study frameworks can result in biased treatment effect estimates. Nonetheless, pre-testing of this sort remains quite common in many program evaluation literatures, including research on minimum wages. Fortunately, the treatment effects we estimate are not ultimately sensitive to whether we adapt our specifications in response to the pre-treatment evidence we observe in our event-study plots. More importantly, our pre-committed research designs, which generate quite similar estimates to the estimators we consider in the current section, were not selected based on pre-testing of this sort. As discussed in the first entries of this project (Clemens and Strain, 2017, 2018b), our covariates were selected on the basis of observable proxies for labor market and other macroeconomic shocks (e.g., shocks to housing prices or to aggregate economy income) that might plausibly give rise to biases in our estimates.



essentially no effect on estimates for years after 2013. That is, the treatment effects we estimate are robust to the inclusion of these additional controls. Importantly, this is true across the full set of estimators we utilize. At the same time, the inclusion of these covariates leads estimates for years prior to 2013 to hew more closely to zero. In these specifications, the estimates associated with both samples and all three of the treatment groups could not plausibly be viewed as providing evidence of a divergent trend preceding the implementation of minimum wage increases.

We now turn to estimates for years after 2013, which track the relationship between employment and the implementation of minimum wage changes. As in our pre-committed analyses, we find a divergence in the experience of states that implemented large minimum wage increases relative to the control group of states that implemented no minimum wage changes. By contrast, states that implemented small or inflation-indexed minimum wage changes experienced modest differential employment changes when compared with states that enacted no minimum wage increases.

Relative to individuals in our control states, low-skilled individuals in states that enacted large minimum wage increases experienced employment declines that accumulated steadily over time. As of 2015, our low-skilled samples in states that enacted large minimum wage increases had experienced an employment decline of just over 1 percentage point relative to states that enacted no minimum wage changes. This estimate is on the margins of being statistically distinguishable from zero at the 0.05 level. By 2019, the differential decline in employment for the low-skilled sample in states with large increases had grown to nearly 5 percentage points. Further, the 2019 estimate is strongly statistically distinguishable from the 2015 estimate. It is also statistically distinguishable from the 2019 estimate for individuals in states with small

increases and individuals in states with inflation-indexed increases. These dimensions of heterogeneity are less pronounced, though still present, for the samples that include all individuals ages 16 to 21. For the latter samples, the estimates for individuals in states with large minimum wage increases grow from just under 2 percentage points in 2015 to roughly 4 percentage points in 2019. For the latter sample, the difference in the estimated effects of large minimum wage increases relative to small minimum wage increases is persistent at roughly 2 percentage points from 2015 to 2019.

Figures B2 and B3 present the basic “event study” estimates described by equation (4). The medium-run estimates in Figure B2 appear to track quite closely with what one might have inferred from the estimates of equation (3); they exhibit non-trivial employment declines in states that enacted minimum wage increases, in particular when those minimum wage increases were large. Note that in this and subsequent analyses, we omit all indexer states (based on Panel A of Figure 1) from event study regressions based on time relative to first statutory increase because these states had minimum wage increases from these indexing provisions prior to enacting any statutory increase. In Panel A of Figure B3, one might argue that, while none of the pre-event estimates are statistically distinguishable from 0, there is a possibility of a divergent, pre-policy change trend in states that enacted relatively large minimum increases. Traditional event study estimates, however, are now known to be prone to biases. More reliable estimates can be obtained through the “stacked event study” and “imputation” estimators, to which we now turn.

We next present estimates of the “stacked event study” estimator described by equation (5). Importantly, the stacked event study estimator is not subject to the problem of negative weights, which can adversely impact the interpretability of estimates from equation (4). We

present estimates of equation (5) in Figures B4 and B5. Interestingly, the pre-event estimates of equation (5) hew modestly more closely to 0 and provide even less indication of potentially concerning divergent trends over the years preceding the implementation of minimum wage increases than did estimates of equation (4).

Over the years following the enactment of minimum wage increases, estimated employment effects become increasingly negative over time. Across the full set of states that enacted minimum wage changes due to new legislation, we estimate employment declines quite close to zero in the first year following the implementation of a state's first minimum wage change. But by the fourth year following the increase, the estimate for the population ages 16 to 21 is marginally greater than -2 percentage points, while the estimate for the low-skilled population (i.e., those ages 16 to 25 with less than a completed high school education) is around -3 percentage points.

In Figure B5, we present estimates of equation (5) in which we differentiate between large minimum wage increases and small minimum wage increases. The estimates are quite striking. By the fourth year following the implementation of a state's first new statutory minimum wage increase, there is a very modest decline, less than 0.5 percentage points and statistically indistinguishable from zero, for states that enacted small minimum wage increases. In the states that enacted large minimum wage increases, by contrast, the estimated impact in year 4 and beyond is just over -4 percentage points for the low-skilled sample. Also in states with large minimum wage increases, the estimate for the population ages 16 to 21 is around -3 percentage points in years 4 and beyond. Both of these estimates are quite strongly statistically distinguishable from both zero and from the estimate for the states that enacted small minimum wage increases. By contrast, the year 1 estimates for states with large minimum wage increases

relative to states with small minimum wage increases differ modestly in economic terms and are not uniformly statistically distinguishable from one another.

Figures B6 and B7 present estimates comparable to those in B4 and B5, but using the multistep “imputation” procedure proposed by Borusyak, Jaravel, and Spiess (2024) (BJS).<sup>23</sup> A cosmetic difference between the figures presenting results from the BJS procedure and our other figures is that the imputation procedure codes the year of a state’s first enacted minimum wage change as “year 0.” In addition, the BJS procedure does not have a base period in the same sense as the traditional event study estimates. The dynamics of the estimated treatment effects are thus shifted by one year relative to the previous figures.

These cosmetic differences aside, the estimates in Figures B6 and B7 are largely indistinguishable from those in Figures B4 and B5. Estimates for the full set of states that implemented minimum wage increases through new legislation are modest over the initial years following the increases, but rise in magnitude to around -2.5 percentage points in our young adult sample and between -3 and -4 percentage points in our low-skilled sample. When we differentiate states that enacted large increases from states that enacted small minimum wage increases, we find null effects for the latter group and quite large, negative effects for states with large minimum wage increases. The evidence reveals, once again, that employment rates in the states that enacted minimum wage changes moved in parallel with employment rates in states that did not increase their minimum wages during the years preceding the minimum wage changes of interest.

Finally, Figures B8 through B11 present a set of falsification checks in which we investigate whether our specifications predict employment changes in a group that is almost

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<sup>23</sup> Because these are our preferred modern DiD event study estimates for large and small increasers, Figure B7 is also discussed in the main text and appears there as Figure 4.

entirely unaffected by the minimum wage, namely the full population of prime age adults. In these figures, the specifications that include no controls for macroeconomic conditions (panel A in each figure) reveals that employment among prime age adults trended more positively in states that enacted large minimum wage increases than in states that enacted no minimum wage increases, as was evident earlier in Table 2. The associated economic tailwinds would thus tend to bias downward the magnitudes of estimates that take no measures to control for macroeconomic conditions. Subsequent panels reveal that employment among prime age adults moved almost perfectly in parallel when comparing our treatment and control groups in both our “sparse controls” and “rich controls” specifications and in both the “stacked event study” and “imputation” estimators. These specifications thus pass both the pre-trend tests and falsification checks emphasized as the key determinants of a specification’s credibility in a number of recent contributions to the minimum wage literature (Reich, 2019; Cengiz et al., 2019; Clemens, Kahn, and Meer, 2021).<sup>24</sup> Further, the analysis in this section has shown that the effects we estimate are robust to the adoption of specifications that resolve concerns raised in recent applied econometrics research on difference-in-differences settings with staggered treatment rollouts and heterogeneous treatment effects.

### *Summary Discussion*

This appendix has presented evidence from specifications including a traditional event study estimator, a stacked event study estimator, and an imputed causal effects event study

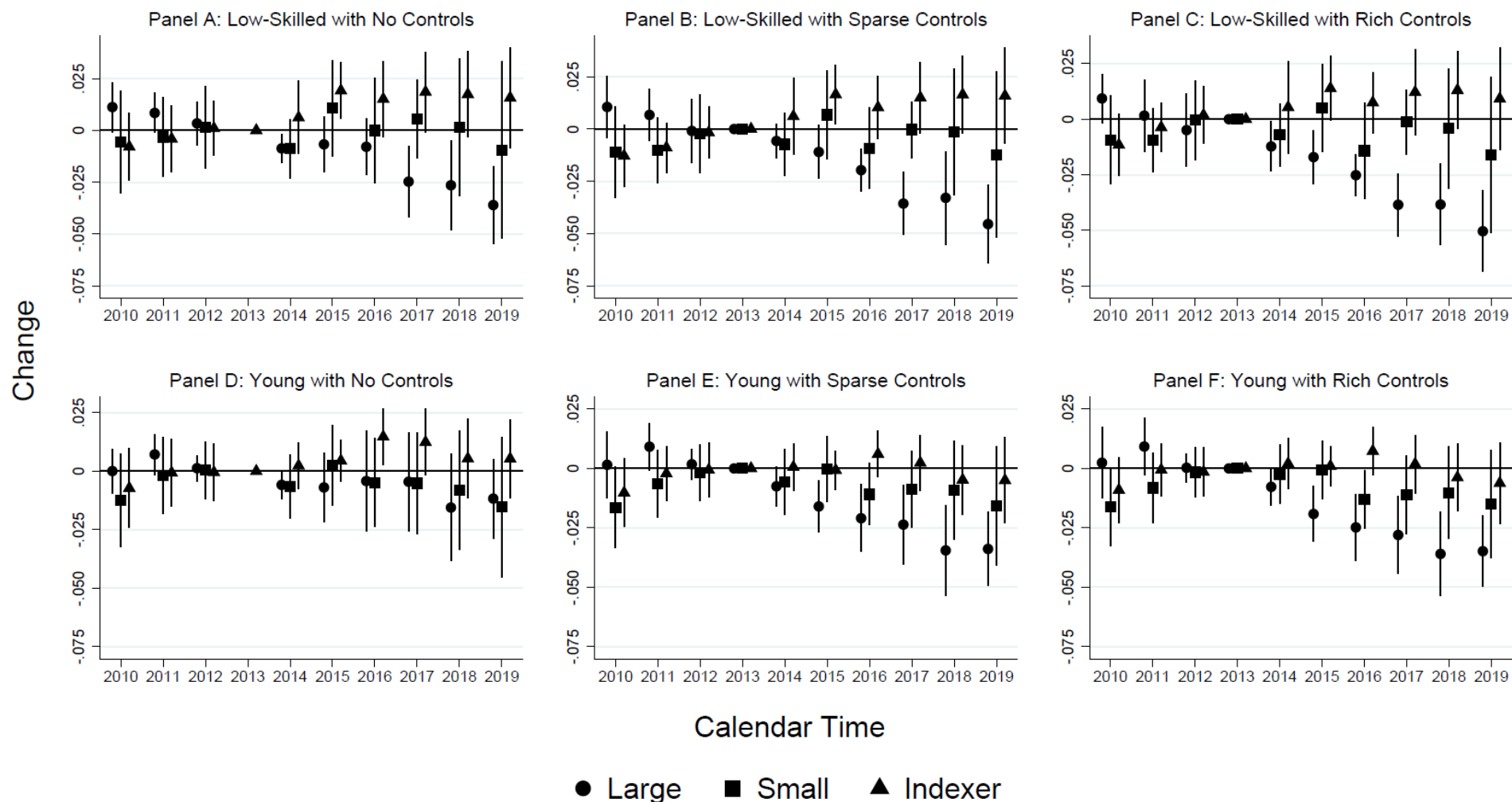
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<sup>24</sup> In a written supplement to his February 7, 2019, testimony to Congress, for example, Reich writes that “our most credible evidence comes from studies that carefully check that their treatment and control groups exhibited similar trends prior to the minimum wage policy treatment, that their effects on pay line up with the size of the mandated increases, and that the methods do not find results where they should not—such as among the college-educated or in high-paying industries.”

estimator. Notably, the estimates from all three of these estimators are both qualitatively and quantitatively quite similar. In light of insights developed in the new difference-in-differences literature, it is of interest to consider how the traditional event study estimator would have been assessed by diagnostics for the potential relevance of concerns associated with negatively weighted treatment effects. Our implementation of the diagnostics proposed by De Chaisemartin, and d'Haultfoeuille (2020) reveals that the sum of the negative weights for observations to which negative weights are assigned is less than  $-0.11$  across all of the specifications reported in Figures B2 and B3. Our assessment is that the standard deviation across treatment effects that would be required for the true estimates to take the opposite sign of our estimates is quite large relative to the actual point estimate for this group. Ultimately, our assessment of this issue's potential relevance is driven primarily by the fact that we obtain similar estimates whether we run the traditional event estimator, the stacked event study estimator, or the imputation difference-in-differences estimator.

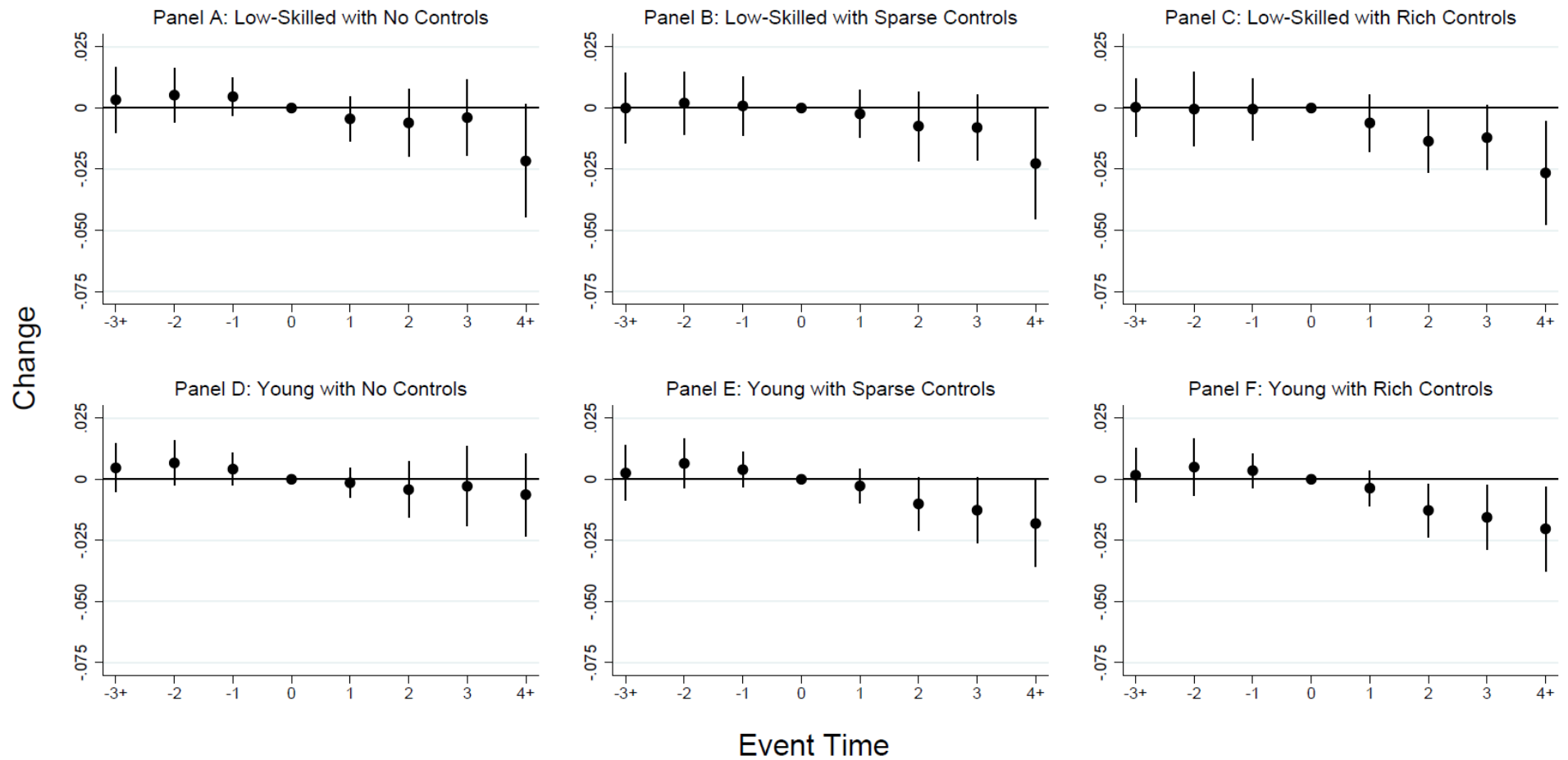
We conclude by discussing the modern difference-in-differences literature's implications for the value of learning-by-doing through specification search. As a general point, the possibility that best practice may evolve poses a potential threat to pre-committed research designs. In our application, however, we note that the traditional event study models that are prone to the most severe potential biases highlighted by the modern difference-in-differences literature were not among the specifications to which we had precommitted. Our pre-commitment focused on more basic event-driven difference-in-differences designs that avoid the pitfalls of staggered rollout designs. While this may in part have been fortuitous, a potential lesson from this experience is that simpler and more transparent research designs may have a tendency to be more resilient. That said, as noted earlier, our capacity to explore dynamics was

constrained by the structure our pre-committed analyses, which highlights the value of learning-by-doing through flexible specification search. In our view, this makes the pairing of pre-committed analyses with analyses that allow for flexible specification search attractive in that it generates an evidence base blending an approach that reduces p-hacking concerns with an approach that maximizes practicability.



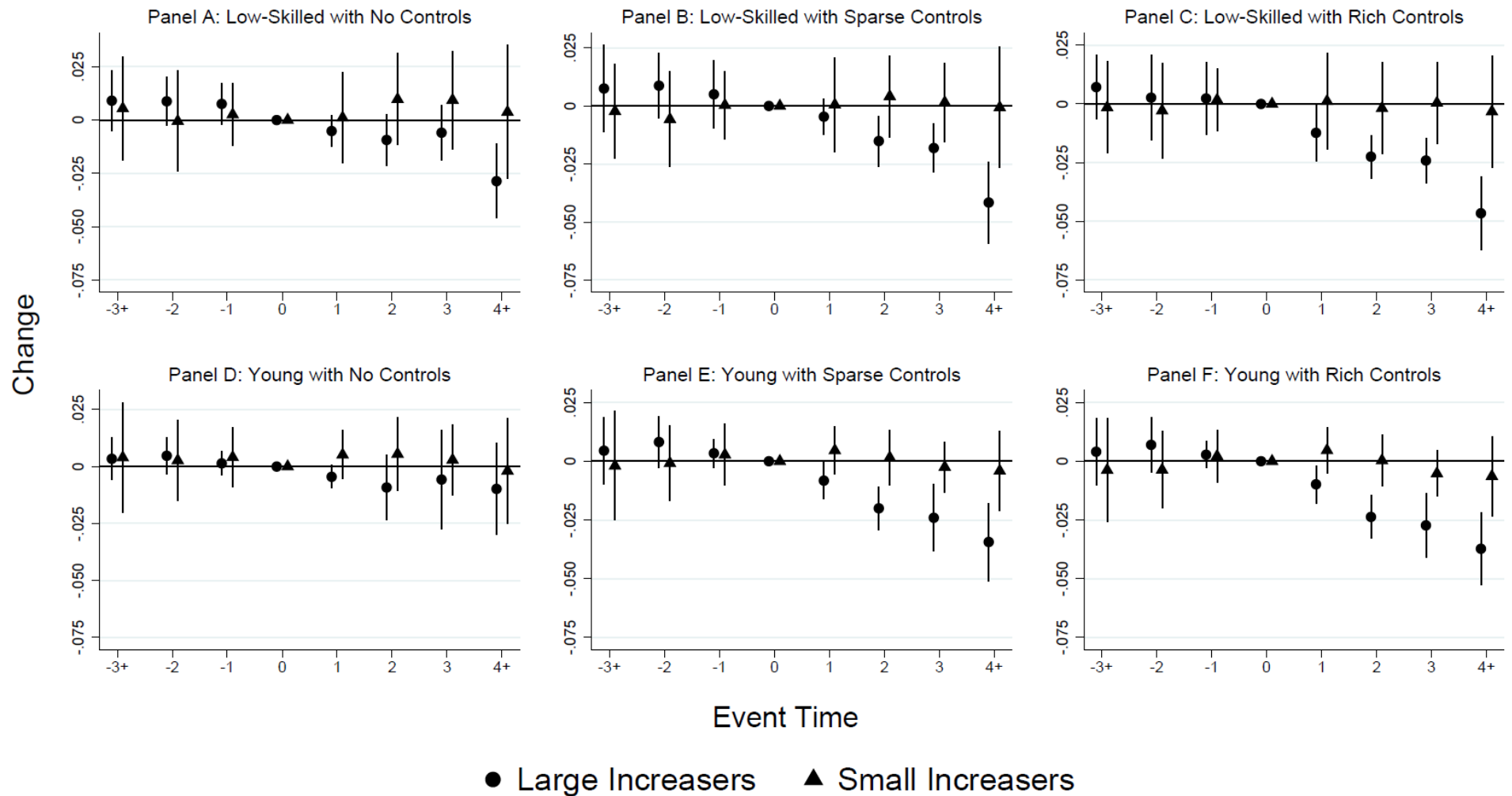
**Figure B1. Event Studies of the Change in Employment Relative to 2013:** This figure displays coefficients from event study regressions described by equation (3). All coefficients are estimates relative to a base year of 2013. States are divided into the large, small, and indexer groupings defined in Panel A of Figure 1. Panels A, B, and C plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels D, E, and F plot coefficients from regressions for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual *per capita* income and the annual average of the median house price index. Regressions with “rich controls” include all sparse controls plus the three-year lag of both the log of annual *per capita* income and the annual average of the median house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.



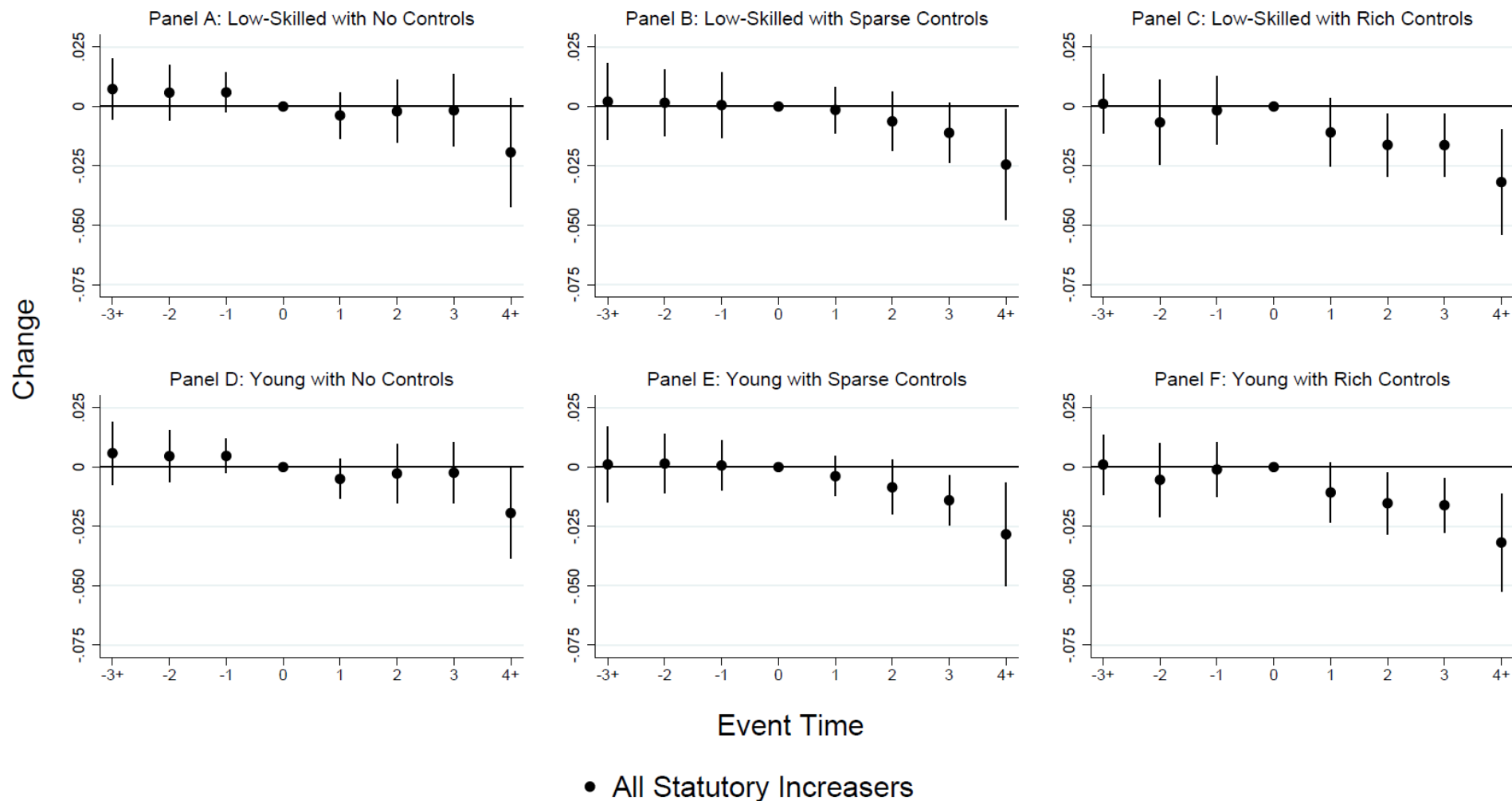


• All Statutory Increases

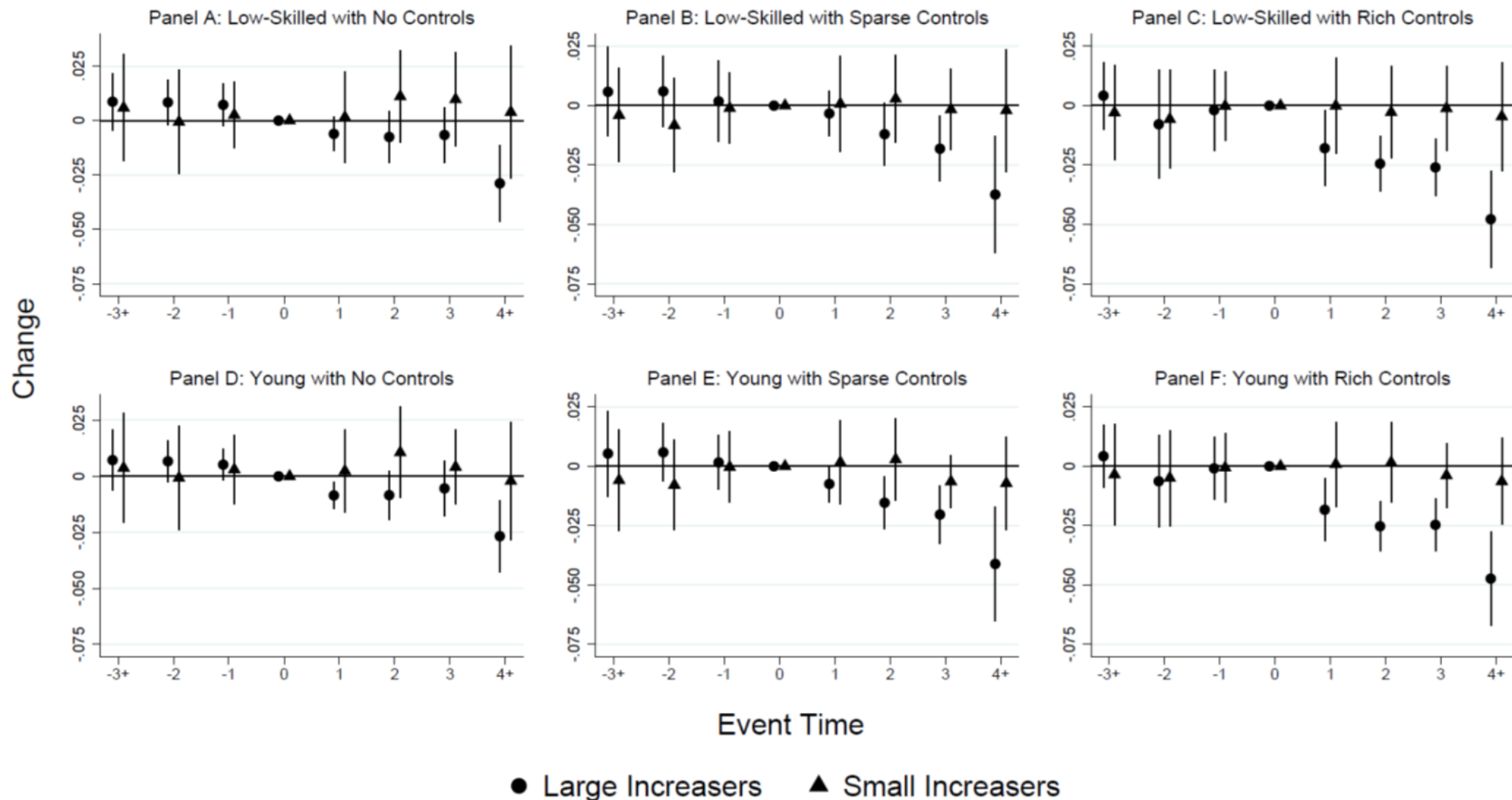
**Figure B2. Event Studies of Changes in Employment Following Statutory Minimum Wage Increases:** This figure displays coefficients from event study regressions described by equation (4). Event Time is defined such that year “1” corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. Panels A, B, and C plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels D, E, and F plot coefficients from regressions for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects and no time-varying covariates. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual *per capita* income and the annual average of the median house price index. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of both the log of annual *per capita* income and the annual average of the median house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.



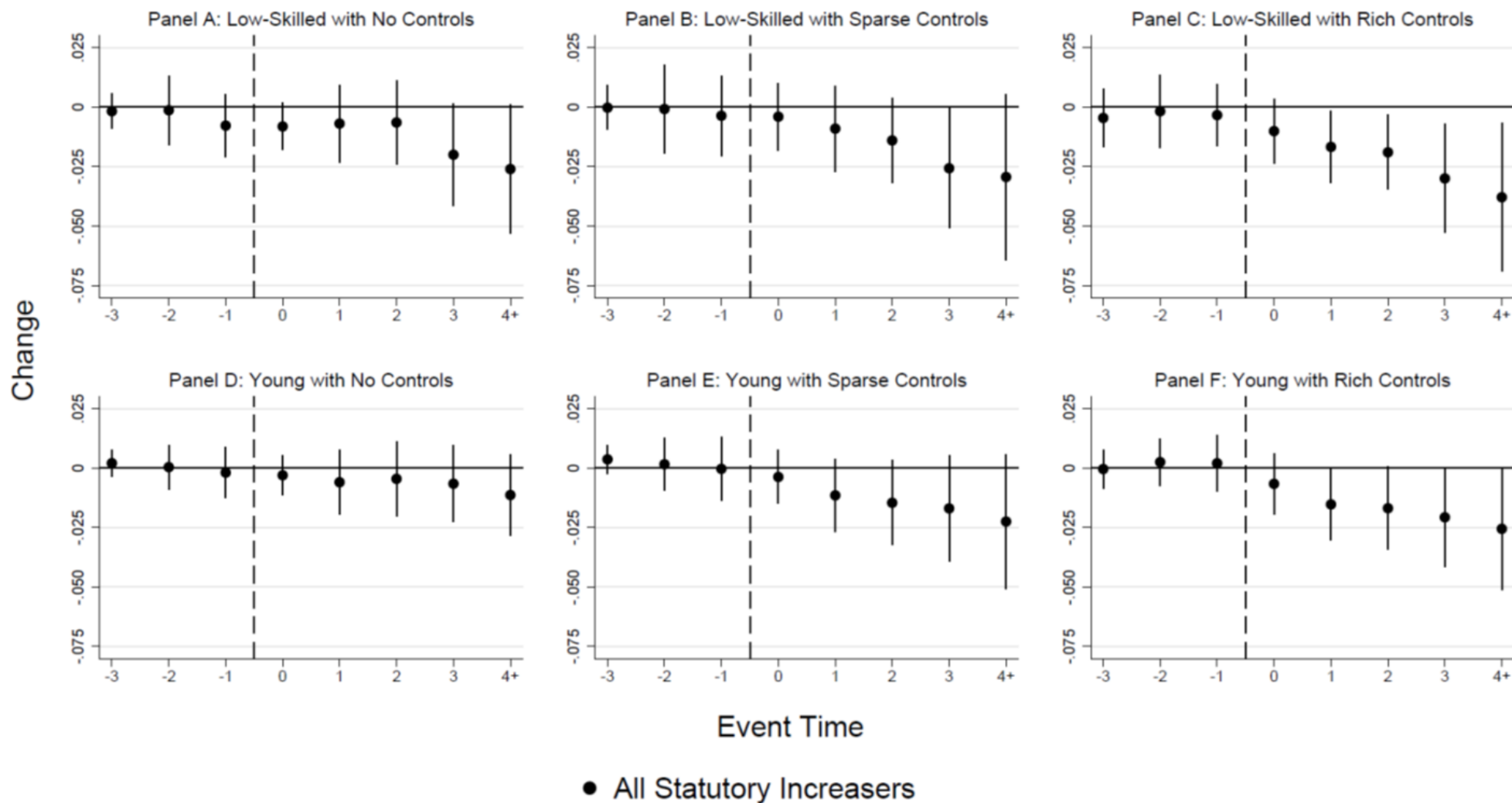
**Figure B3. Event Studies of Changes in Employment Following Large and Small Statutory Minimum Wage Increases:** This figure displays coefficients from event study regressions described by equation (4). Event Time is defined such that year “1” corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. We compare estimates for large vs. small increasers as defined in the main text. Panels A, B, and C plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels D, E, and F plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects and no time-varying covariates. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual *per capita* income and the annual average of the median house price index. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of both the log of annual *per capita* income and the annual average of the median house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.



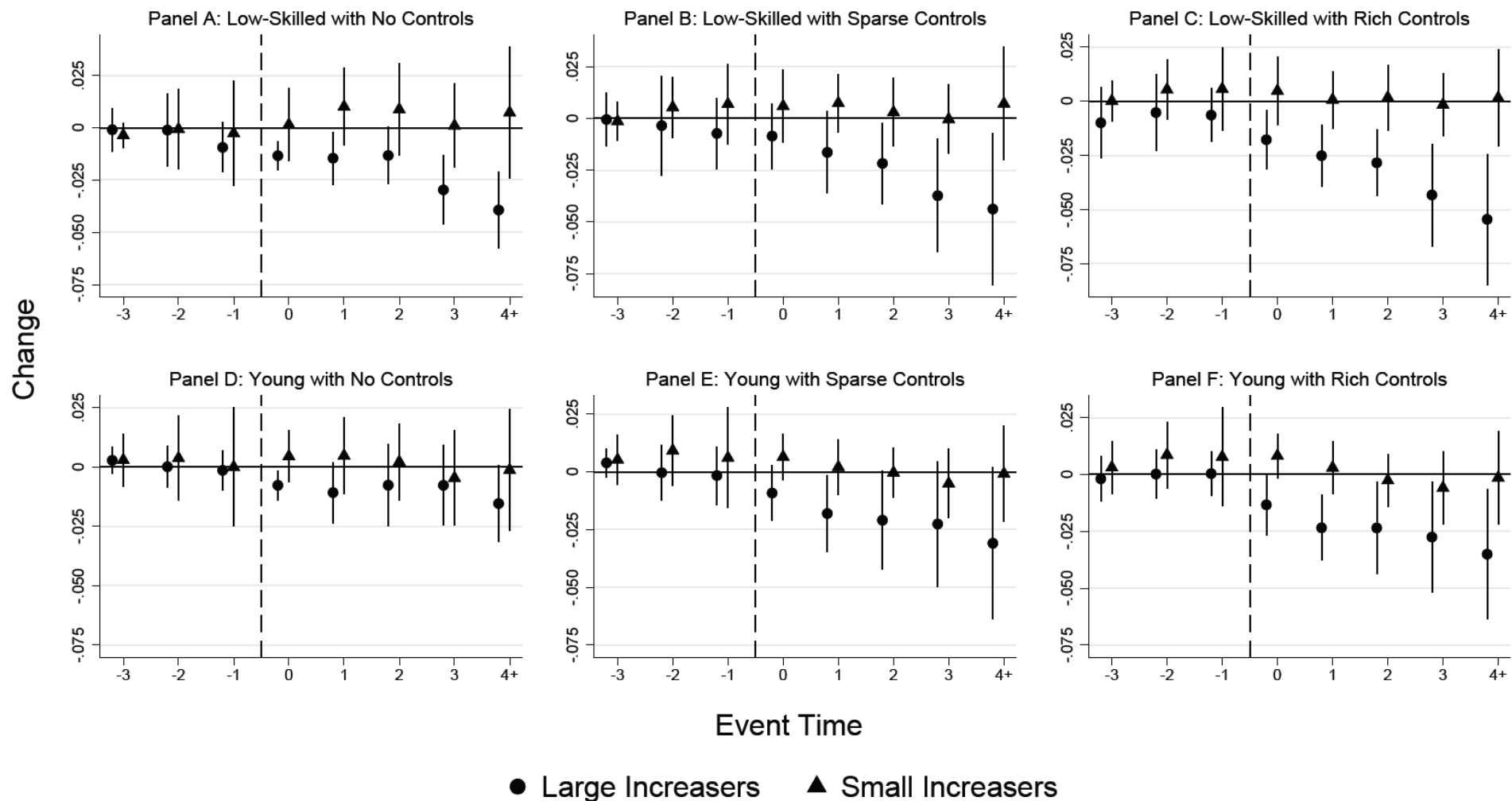
**Figure B4. Stacked Event Studies of Changes in Employment Following Statutory Minimum Wage Increases:** This figure displays coefficients from the “stacked event study” estimator described by equation (5). Event Time is defined such that year “1” corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. Panels A, B, and C plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels D, E, and F plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects and no time-varying covariates. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual *per capita* income and the annual average of the median house price index. Regressions with “rich controls” include all sparse controls plus the three-year lag of both the log of annual *per capita* income and the annual average of the median house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.



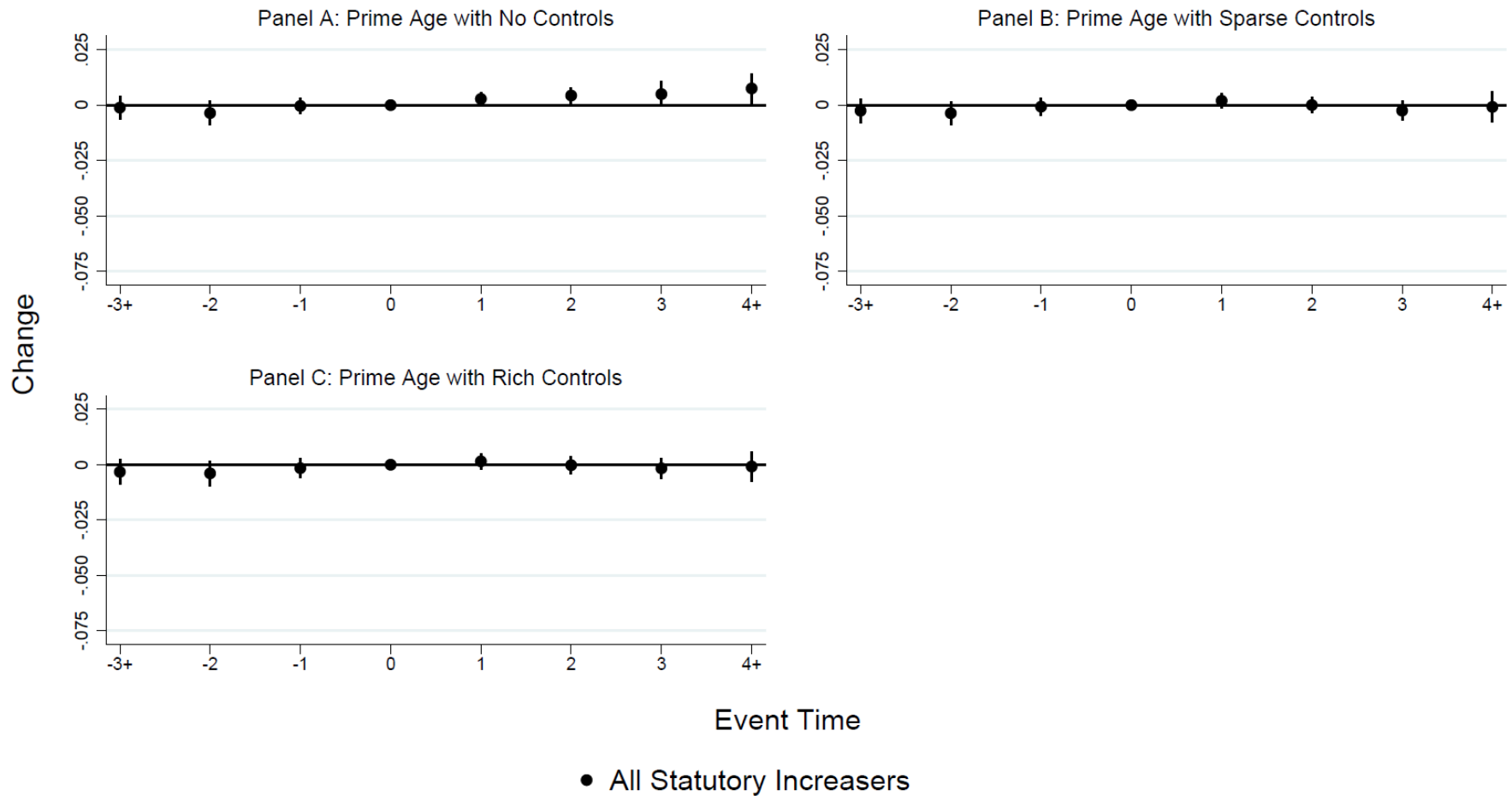
**Figure B5. Stacked Event Studies of Changes in Employment Following Large and Small Statutory Minimum Wage Increases:** This figure displays coefficients from the “stacked event study” estimator described by equation (5). Event Time is defined such that year “1” corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. We compare estimates for large vs. small increasers as defined in the main text. Panels A, B, and C plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels D, E, and F plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects and no time-varying covariates. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual *per capita* income and the annual average of the median house price index. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of both the log of annual *per capita* income and the annual average of the median house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.



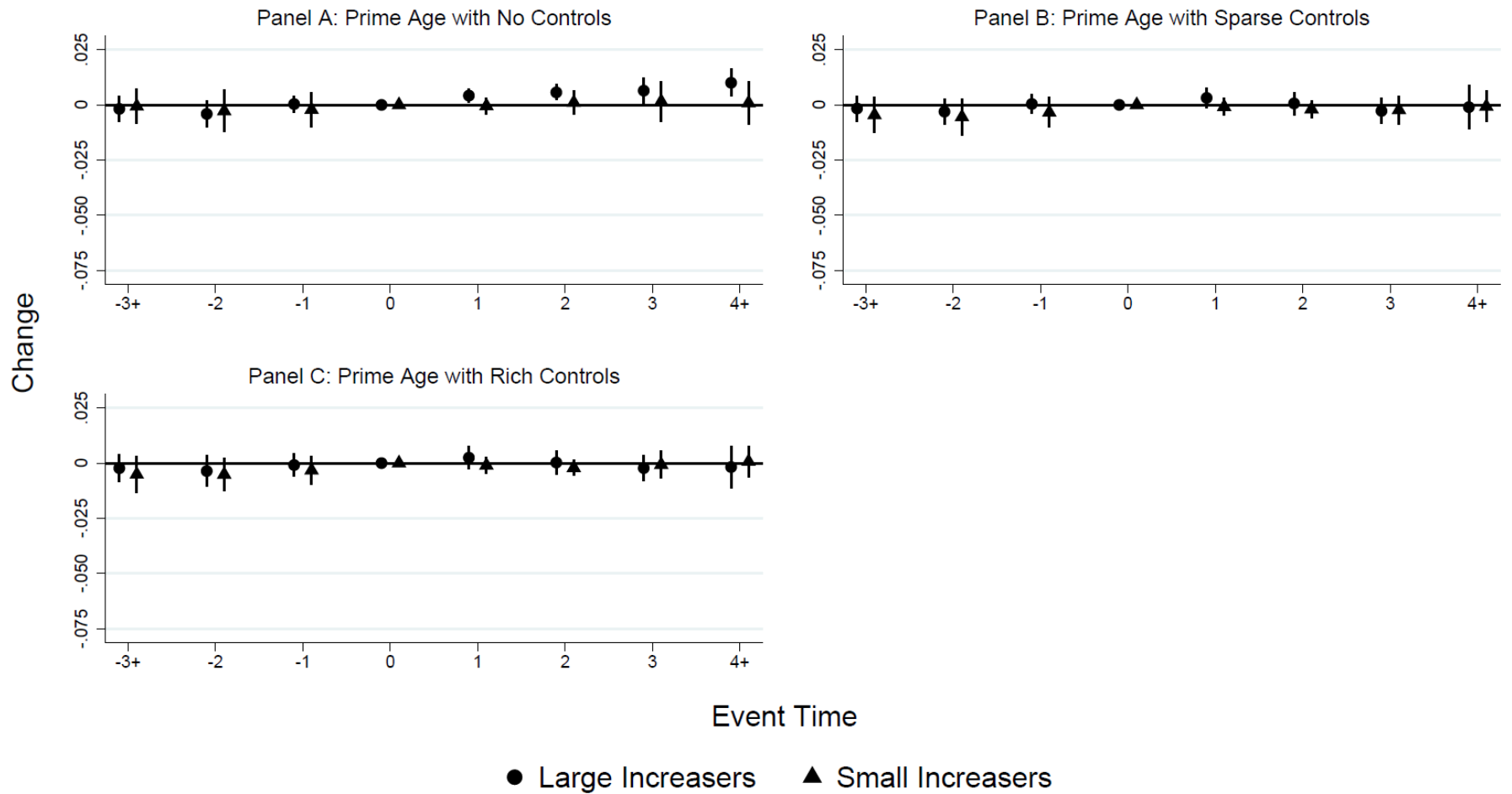
**Figure B6. Event Studies of Changes in Employment Following Statutory Minimum Wage Increases Using the BJS Imputation Estimator:** This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2024) (BJS). For the BJS estimator, we code the first treatment year as the year in which a state’s first statutory minimum wage increase took effect. Panels A, B, and C plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels D, E, and F plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual average *per capita* income and the annual average state house price index used in our main regressions. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of log *per capita* income and the house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.



**Figure B7. Event Studies of Changes in Employment Following Large and Small Statutory Minimum Wage Increases Using the BJS Imputation Estimator:** This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2024) (BJS). For the BJS estimator, we code the first treatment year as the year in which a state’s first statutory minimum wage increase took effect. We compare estimates for large vs. small increasers as defined in the main text. Panels A, B, and C plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels D, E, and F plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual average *per capita* income and the annual average state house price index used in our main regressions. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of log *per capita* income and the house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.

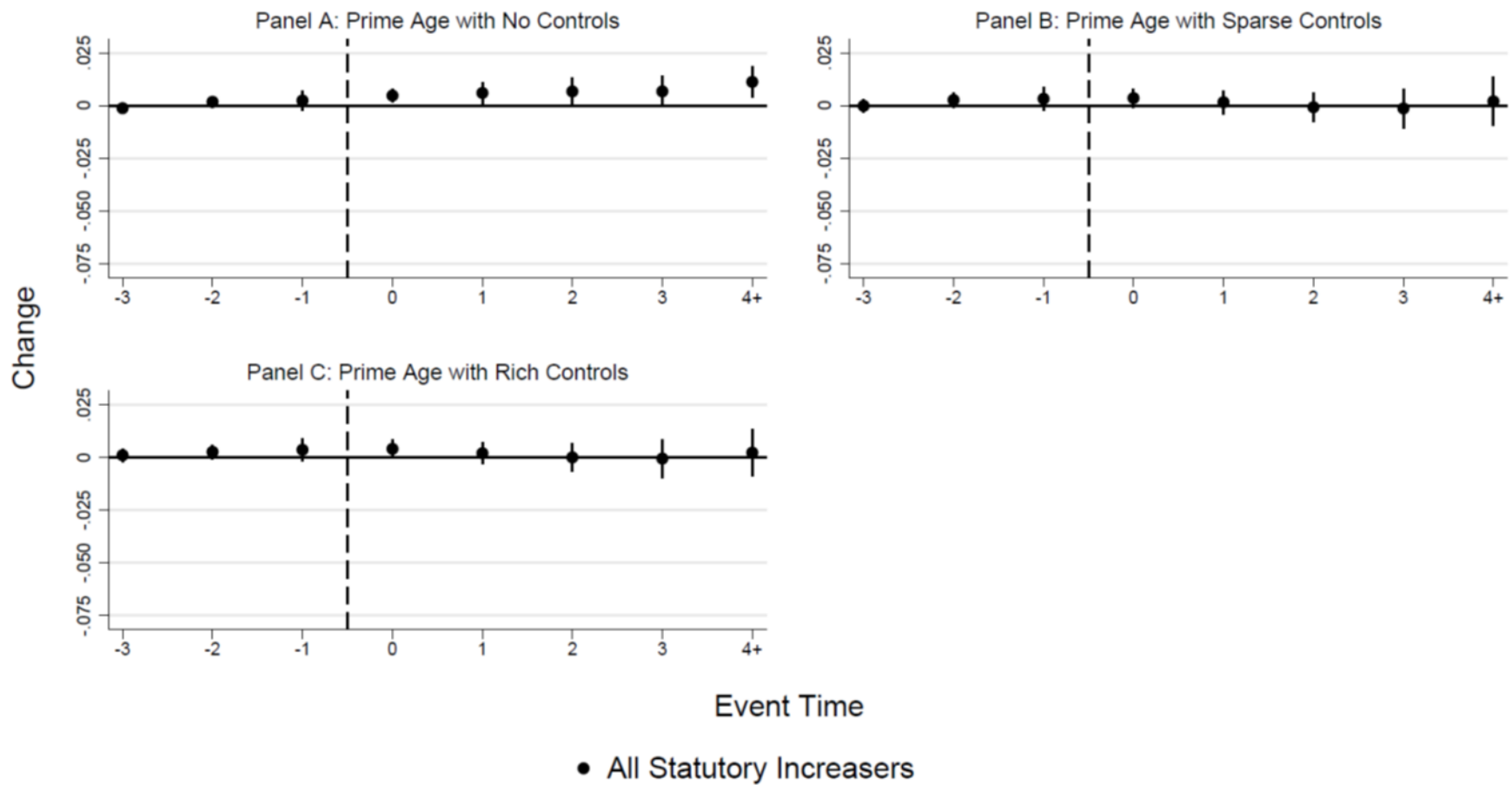


**Figure B8. Stacked Event Studies of Changes in Prime Age Employment Following Statutory Minimum Wage Increases:** This figure displays coefficients from the “stacked event study” estimator described by equation (5). Event Time is defined such that year “1” corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. Panels A, B, and C plot coefficients for prime age individuals defined as individuals ages 26–54. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects and no time-varying covariates. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual average *per capita* income and the annual average state house price index used in our main regressions. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of log *per capita* income and the house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.

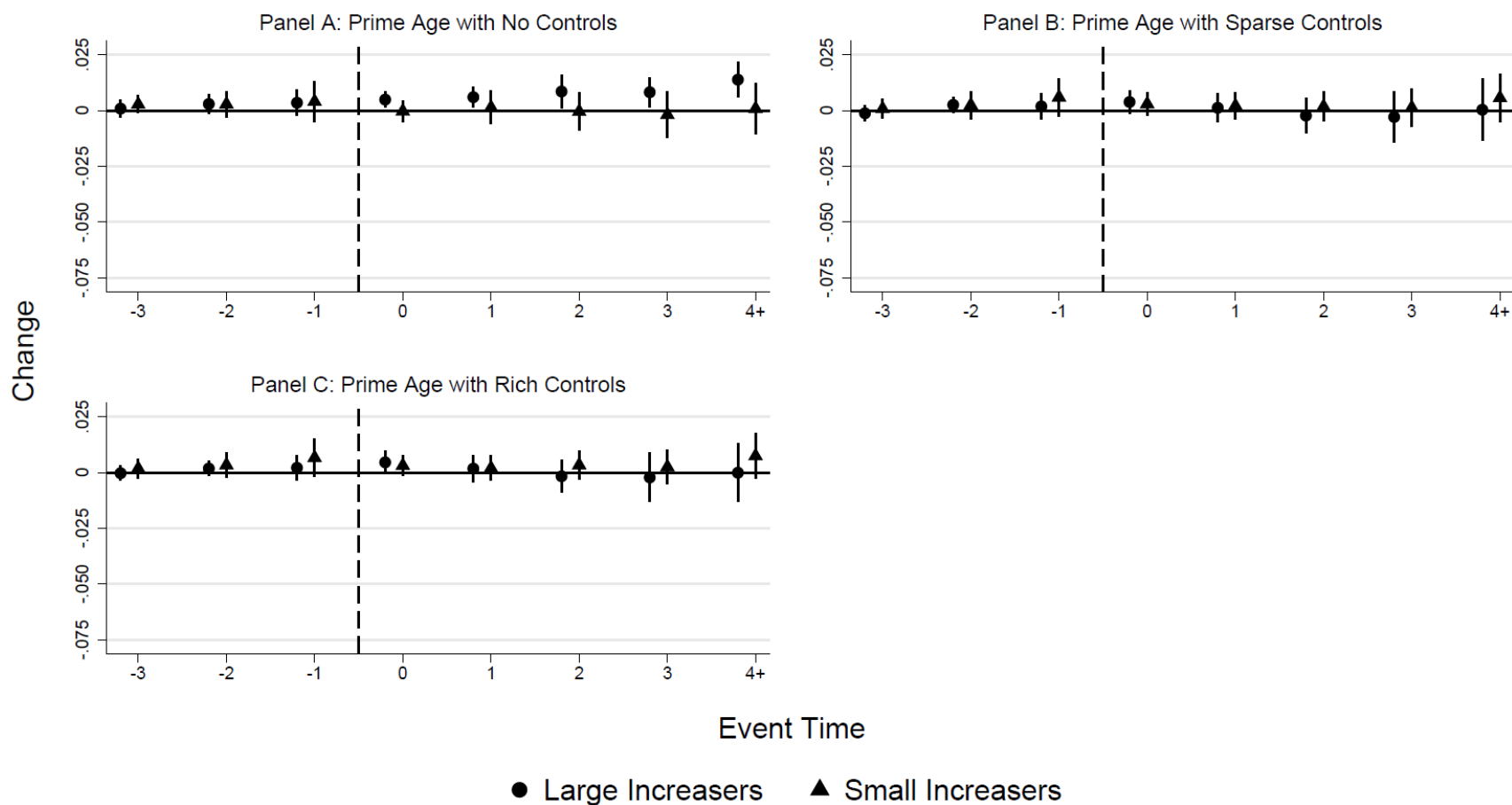


**Figure B9. Stacked Event Studies of Changes in Prime Age Employment Following Large and Small Statutory Minimum Wage Increases:** This figure displays coefficients from the “stacked event study” estimator described by equation (5). Event Time is defined such that year “1” corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. We compare estimates for large vs. small increasers as defined in the main text. Panels A, B, and C plot coefficients for prime age individuals defined as individuals ages 26–54. The samples are from the ACS. Regressions with “no controls” include state and year fixed effects and no time-varying covariates. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual average *per capita* income and the annual average state house price index used in our main regressions. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of log *per capita* income and the house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.





**Figure B10. Event Studies of Changes in Prime Age Employment Following Statutory Minimum Wage Increases Using the BJS Imputation Estimator:** This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2024) (BJS). For the BJS estimator, we code first treatment year as the year in which a state’s first statutory minimum wage increase took effect. Panels A, B, and C plot coefficients for prime age individuals defined as individuals ages 26–54. The samples are from the ACS. Regressions with “no controls” include only state and year fixed effects and no time-varying covariates. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual average *per capita* income and the annual average state house price index used in our main regressions. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of log *per capita* income and the house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.



**Figure B11. Event Studies of Changes in Prime Age Employment Following Large and Small Statutory Minimum Wage Increases Using the BJS Imputation Estimator:** This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2024) (BJS). For the BJS estimator, we code first treatment year as the year in which a state’s first statutory minimum wage increase took effect. We compare estimates for large vs. small increasers as defined in the main text. Panels A, B, and C plot coefficients for prime age individuals defined as individuals ages 26–54. The samples are from the ACS. Regressions with “no controls” include only state and year fixed effects and no time-varying covariates. Regressions with “sparse controls” include state and year fixed effects, as well as the log of annual average *per capita* income and the annual average state house price index used in our main regressions. Regressions with “rich controls” include all controls in the sparse controls regressions plus the three-year lag of log *per capita* income and the house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.

**Table B1. List of States with Statutory Minimum Wage Changes and the Year of First Associated Increase 2013-2019**

<u>State</u>	<u>Year of First Statutory Increase</u>
Alaska	2015
Arizona	2017
Arkansas	2015
California	2014
Colorado	2017
Connecticut	2014
Delaware	2014
District of Columbia	2014
Hawaii	2015
Maine	2017
Maryland	2015
Massachusetts	2015
Michigan	2014
Minnesota	2014
Missouri	2019
Nebraska	2015
New Jersey	2014
New York	2014
Oregon	2016
Rhode Island	2013
South Dakota	2015
Vermont	2015
Washington	2017
West Virginia	2015

Note: Data on minimum wage changes comes from the U.S. Department of Labor. States are counted as statutory increaser states if the minimum wage rate in force in that state increased between January 1, 2013 and January 1, 2019 as the result of a new piece of legislation passed between 2013 and 2018. The year of first statutory increase is the year in which the first minimum wage increase mandated by a new piece of legislation goes into effect. A slight tweak on this assignment rule involves the state of New York. New York passed legislation in March 2013 to increase its minimum wage on December 31, 2013. We assign the year of first statutory increase to 2014, reflecting that 2014 was the first year during which the increase was in effect.