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ABSTRACT

The Heterogeneous Effects of Large and Small Minimum Wage Changes: Evidence over the Short and Medium Run Using a Pre-analysis Plan*

This paper advances the use of pre-analysis plans in non-experimental research settings. In a study of recent minimum wage changes, we demonstrate how analyses of medium- and long-run impacts of policy interventions can be pre-specified as extensions to 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 low-skilled individuals by just over 2.5 percentage points. 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 medium-run effects exceed short-run effects and that the elasticity of employment with respect to the minimum wage is substantially more negative for large minimum wage increases than for small increases.

JEL Classification: J08, J23, J38

Keywords: minimum wages, employment, pre-commitment

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Pre-analysis plans have the potential to increase the transparency and reproducibility of empirical research (Christensen and Miguel, 2018; Burlig, 2018; Currie, Kleven, and Zwiers, 2020; Janzen and Michler, 2021). Outside of experiments, however, economists have rarely put pre-analysis plans into practice. Neumark's (2001) short-run analysis of the employment effects of minimum wage increases has long been the leading, rare example of a pre-analysis plan executed in a non-experimental setting.

This paper presents the completed results of a four-year, pre-committed analysis of the employment effects of recent minimum wage changes. We advance two potentially high-value uses of pre-analysis plans in non-experimental research settings. That is, we identify use cases that harness the central strengths of pre-analysis plans while mitigating their weaknesses. First, we demonstrate how analyses of relatively long-run impacts of policy interventions can be pre-specified as extensions to short-run analyses. Second, our pre-analysis plan includes theoretically motivated comparisons of the effects of large vs. small minimum wage increases. Our pre-analysis plan's focus on heterogeneity, with an emphasis on variations that connect to theoretical models of the minimum wage's effects, has scientific advantages on which we elaborate below.

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. This pause created a baseline (or "pre-period") for empirical purposes. It was followed by considerable divergence in states' minimum wage policies. A number of states legislated and began to enact 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 medium-run 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. (See their Figure A.4.)

The past decade's policy environment created an opportunity to pre-commit to using a transparent set of program evaluation methods to estimate the employment effects of minimum wage changes. 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). In these initial papers, we used 2011 to 2015 Current Population Survey (CPS) and American Community Survey (ACS) data to estimate the very short-run effects of recent minimum wage changes. We accompanied our initial analysis with a pre-commitment to analyze CPS and ACS data through 2019 using a common set of estimation frameworks (Clemens and Strain, 2017). Our analysis of 2011 to 2015 ACS data (Clemens and Strain, 2018b) incorporated minor refinements in response to referees. We carried these refinements forward through a series of annual project milestones, which incorporated 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.

Our results, as presented in Section V, are as follows. First, we estimate that, over the short and medium run, relatively large increases in minimum wages have reduced employment rates among individuals with low levels of experience and education by just over 2.5 percentage points. Second, our estimates of the effects of relatively small minimum wage increases are variable and centered on zero, as are our estimates of the effects of minimum wage increases linked to inflation-indexing provisions. Finally, our results provide evidence that the medium-run effects of large minimum wage changes are larger and more negative than their short-run effects.

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 growing agreement, however, regarding the validity of the estimation frameworks to which we pre-committed for analyzing precisely the set of minimum wage changes we analyze. Notably, Cengiz *et al.* (2019) argue that both event-based estimators and more basic, two-way fixed effects estimators produce unbiased estimates of the effects of minimum wage changes enacted between 1992 and 2016.[2] 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.

A second set of developments relate to the evolving state of best practice program evaluation methods. Specifically, a set of recent applied econometrics papers has highlighted threats to the interpretability of conventional difference-in-differences estimates in settings with staggered treatment rollouts and treatment effect heterogeneity (Baker, Larcker, and Wang, 2021; Borusyak, Jaravel, and Spiess, 2021; Callaway and Sant'Anna, 2020; Goodman-Bacon, 2021). We implement two proposed solutions to the issues these papers raise—namely a "stacked event study" estimator and the "imputation" estimator of Borusyak, Jaravel, and Spiess (2021). The evidence from both of these estimators, which accommodate key features of our empirical setting, supports the validity of our pre-committed frameworks. Estimates of the medium-run effects of relatively large minimum wage changes are more negative when we use these more recently developed estimators than when we use the

[2] 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.

estimators in our pre-analysis plan.

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 short- and medium-run effects. 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 heterogeneity to examine the predictions of economic models in an analysis of non-experimental data.

Our primary contribution to the minimum wage literature is to provide transparent evidence that large and small minimum wage changes have qualitatively different effects. We find that the employment effect of minimum wage increases is not described by a constant elasticity. That is, employment responds more elastically to large minimum wage increases than to small minimum wage increases.

While papers on the minimum wage's employment effects have filled volumes,[3] analyses of how elasticities vary with the size of the minimum wage increase are much less common. One recent example is Jardim *et al.*'s (2017) comparison of the initial and later increases enacted by the city of Seattle. A related, though distinct, example is Cengiz *et al.*'s (2019) analysis of heterogeneity with respect to the minimum wage's ratio relative to the median wage. (See their Figure 5.) These papers arrive at different conclusions regarding the relevance of the degree of the minimum wage's bite. Our

[3] Book-length assessments of the effects of minimum wages include Card and Krueger (1995), Neumark and Wascher (2008), and Belman and Wolfson (2014). Selected recent studies of the effects of minimum wages on employment include multiple papers on minimum wage increases in the United States across recent decades (Meer and West, 2016; Cengiz et al., 2019; Cengiz et al.. 2021; 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; Harastozi and Lindner (2019) on the effects of large minimum wage increases enacted in Hungary; Jardim et al. (2017, 2018) 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).

paper analyzes historically dramatic variations in the size of states' minimum wage changes, and it does so using a pre-analysis plan. Our pre-analysis plan's inclusion of this dimension of heterogeneity gives our analysis a distinctive scientific advantage.

Our finding that large minimum wage changes have substantial employment effects while small minimum wage changes have modest effects has important implications for forecasts of the effects of minimum wage proposals. While evidence on this issue is sparse, the idea that large minimum wage changes will tend to have more sharply negative employment effects than small minimum wage changes is motivated by a rich set of theoretical models of labor markets.[4] We present one such framework, which also accompanied our pre-analysis plan (Clemens and Strain, 2017), in Appendix B. We provide evidence that the elasticities relevant for contrasting the effects of large vs. small minimum wage increases differ more than recent analyses by the Congressional Budget Office (2019) assume.

Our contribution to the broader literature on empirical program evaluation is methodological. Pre-analysis plans are uncommon outside of experimental settings (Christensen and Miguel, 2018).[5] Neumark (2001) and Neumark and Yen (2020) provide notable examples of the use of pre-analysis

[4] A broad set of models of the labor market have the implication that elasticities of employment with respect to the minimum wage will be non-constant and may change signs as the minimum wage rises. In a textbook competitive market diagram, for example, the elasticity of employment with respect to the minimum wage is zero until it begins to bind, at which point the elasticity is negative. In a textbook monopsony diagram, the elasticity will be positive when the minimum wage begins to bind but will become negative once the minimum wage has passed the "efficient" wage level. Search-oriented models, like that of Engbom and Moser (2018), have a similar implication that the elasticity of employment with respect to the minimum wage can initially be positive but will eventually become negative. The "putty-clay" framework of Sorkin (2015) and of Aaronson et al. (2018) have a related implication for the long-run effects of permanently binding minimum wage increases; these authors highlight that observed elasticities will be larger when they incorporate long-run adjustments of firms' capital stocks and production technology choices. The "putty-clay" framework captures key intuitions that can be found in the Hicks-Marshall laws of derived demand (Marshall, 1890; Hicks, 1932). Clemens (2021) makes the complementary point that there are other margins, including fringe benefits, scheduling, effort requirements, and other aspects of working conditions, along which firms may be able to adjust before altering employment levels. This, too, will tend to produce modest elasticities of employment with respect to small minimum wage changes, while leaving open the possibility of large employment responses to large or otherwise strongly binding minimum wage changes.

[5] 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.

plans to analyze minimum wages. We depart from Neumark (2001) and Neumark and Yen (2020) by emphasizing the utility of pre-analysis plans for reducing "p-hacking" concerns when contrasting short- and medium-run effects or when comparing the effects of large and small minimum wage increases. As emphasized above, our heterogeneity analyses are designed to provide insight into the economic models and mechanisms that might best explain the minimum wage's effects.[6]

There are two reasons why theoretically motivated analyses of heterogeneity are a potentially important strength of pre-analysis plans in non-experimental settings. First, 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). Second, long-run analyses are exposed to an additional data mining concern; this concern reflects the countless ways a researcher can specify "controls" for factors that evolve gradually over time. Pre-committed analyses head off both of these data mining problems.

A separate point we illustrate is that pre-committed analyses of relatively long-run effects can be specified as extensions of estimation frameworks that have been validated for short-run analyses. This use case for pre-analysis plans could readily be encouraged by funding agencies that desire to encourage transparent and reproducible research on the long-run effects of economic policies. In our case, several now-published papers have used modest variants on our pre-committed research designs to estimate the short-run effects of the majority of the minimum wage changes we analyze (Cengiz *et al.*, 2019; Clemens, Kahn, and Meer, 2021; Gopalan *et al.*, 2021). These studies include the short-run analyses through which we initiated this project (Clemens and Strain, 2018b).

The relevance of our methodological innovation connects to the important role of heterogeneity

[6] Clemens, McNichols, and Sabia (2020) develop a similarly motivated pre-analysis plan for analyzing the long-run effects of the Affordable Care Act on insurance coverage.

analyses in program evaluation research. Heterogeneity analyses are often the primary analyses through which program evaluation research connects to economic theory. Comparisons of "high" and "low" concentration markets, for example, are central to efforts to understand the importance and implications of market power.[7] A second, widespread example involves analyses that assess the importance and implications of liquidity constraints; such analyses tend to compare the consumption and investment propensities of "high" vs. "low" liquidity households and firms.[8] In these and other settings, data mining concerns can threaten the validity of the use of subgroup analyses to test the relative importance of alternative economic theories (Christensen and Miguel, 2018; Humphreys, Sanchez de la Sierra, and Van der Windt, 2013). Pre-analysis plans have the potential to overcome this threat and, by extension, to solidify the scientific basis for many theory-testing exercises.

Our paper proceeds as follows. Section II provides further background regarding the minimum wage changes we analyze. Section III discusses the primary data sources we use. Section IV describes our pre-committed estimation frameworks, and Section V summarizes the results of these pre-committed analyses. Section VI presents additional analyses motivated by recent developments in the applied econometrics literature on best practice program evaluation methods. Section VII discusses the elasticities implied by the employment and wage impacts we estimate. Section VIII concludes.

Section II: 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

[7] Analyses including Okudaira, Takizawa, and Yamanouchi (2019) and Azar et al. (2019) consider this issue in the context of minimum wages. Cabral, Geruso, and Mahoney (2018) consider a distinctive setting involving the incidence of government payments to private insurance plans.
[8] One example of interest is Johnson, Parker, and Souleles' (2006) analysis of the role of liquidity constrains in shaping household responses to the 2001 tax rebate stimulus checks. A second is Zwick and Mahon's (2017) analyses of the differential effects of temporary tax changes on small firms relative to large firms and of changes that generate immediate tax benefits relative to future tax benefits. A third is Chetty's (2008) analysis of the effects of increases in the generosity of unemployment insurance benefits on the activities of households with varying amounts of liquid wealth.

efforts to increase minimum wages. Subsequently, as we have discussed in this project's initial papers, states diverged quite dramatically 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 medium-run effects of minimum wage changes, 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.

As discussed in our previous work, updates to states' minimum wage policies pose challenges to the development of pre-committed research designs. Most notably, several of the states that entered our analysis sample with inflation-indexing provisions have subsequently enacted minimum wage changes through new statutes. Our approach has thus been 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.[9] This full set of analyses is intended to maintain our analysis plan's transparency while also incorporating new opportunities to investigate the dynamic effects of this period's minimum wage changes.

Tables 1 and 2 present the full divisions of states associated with the policy groupings we use. 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. Hawaii shifts from the "small" change group to the "large" change group. Maine shifts from no change to the large change group. Alaska, New Jersey, Rhode Island, and South Dakota shift from the "large" change group to the "small" change group. Finally, Arizona, Colorado, Oregon, and Washington shift from the "indexer" group to the "small" change group. Figures 1 and 2 illustrate the dynamics of the changes in the average effective minimum wage rates across the groupings described in Tables 1 and 2.

We emphasize that both the "small" and "large" minimum wage changes in our analysis sample are substantial in comparison with historical minimum wage changes. In their analysis of 138 state-level minimum wage increases, for example, Cengiz et al (2019) report that the average increase was just over eight log points. (See their Figure A.4.) For the minimum wage changes we analyze, the comparably constructed medium-run increase is of roughly 25 log points within our "small" increase group and of roughly 35 log points across our "large" increase group. (See appendix Figure A1.) Note that these estimates describe increases enacted as of January 2019. In states like New York and California, which

[9] From January 2013 to January 2018, roughly half of the population in states with recent minimum wage legislation were in states that had enacted changes equal to or greater than $2.50. We thus use $2.50 as the more recent cutoff between states with "large" and "small" increases. Note that the bulk of the states shifting out of the indexing regime into the "new increase" regimes are categorized as "small" increasers. This reflects the fact that, although their total increases are now substantial, an increase of roughly $2 was forecastable for these states from January 2011 through January 2018 due to their inflation-indexing regimes. The net new increases enacted by these states are thus more modest than they initially appear.

have legislated pathways to a $15 minimum wage, the full increase to which firms are responding exceed 60 log points in total. Our analysis period thus provides an opportunity to estimate the effects of historical large and substantially varied increases in states' minimum wages and to do so using a pre-analysis plan.

Section III: Data Sources

Our primary data sources are the American Community Survey (ACS) and the Current Population Survey (CPS).[10] 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. As summarized in Clemens and Strain (2018b), Kromer and Howard (2010) provide detailed documentation of differences between the sampling procedures and employment questions posed in the ACS relative to the smaller and more commonly analyzed CPS.[11]

Tables 3 and 4 present summary statistics on the primary ACS and CPS samples we analyze, respectively. The first sample, described in Columns 1 and 2 of each table, 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. Comparing these baseline and later years in our sample, the summary statistics are consistent with the generally positive macroeconomic developments

[10] The remainder of this section quotes liberally from the text of this project's previous analyses.

[11] As summarized in our previous work, "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. Kromer and Howard (2010) document that improvements to the ACS's employment questions, first implemented in 2008, significantly improved the comparability of estimates generated using the two surveys."

that occurred over this time period. Employment rates rose for both groups, as did house prices and aggregate *per capita* incomes.

We supplement the ACS and CPS household survey data with data on macroeconomic covariates that may be relevant as control variables. 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. We proxy for variations in the recovery of the housing market using a quarterly statewide median house price index from the Federal Housing Finance Agency (FHFA). We proxy for aggregate economic performance using data on aggregate state income *per capita* from the Bureau of Economic Analysis (BEA). Finally, we proxy for variations in broader labor market developments using employment among skill groups not directly affected by the minimum wage.

Figure 3 presents time series on median house prices (Panel A) and aggregate income (Panel B) separately across the policy regimes we analyze. That is, it presents these series separately for states that enacted large minimum wage increases, small minimum wage increases, inflation-indexed minimum wage increases, and no minimum wage increases. The figure, which we discuss momentarily, thus presents two series that are relevant for gauging differences in the macroeconomic conditions facing the groups of states we analyze. Figures 4 (ACS) and 5 (CPS) present additional evidence on the evolution of employment among prime-age adults (ages 26–54) (Panel D) and among a group consisting of individuals ages 21–30 with high school degrees and individuals ages 30–64 with less than a completed high school degree (Panel C). The latter individuals thus have education and/or experience modestly beyond that obtained by most minimum wage workers. Additional tabulations of the data underlying Figures 3, 4, and 5 are in Tables 5, 6, 7, A3, A4, and A5.

The house price index reveals that the housing recovery following the Great Recession was quite strong in states that enacted relatively large minimum wage increases. Median house prices rose by roughly 49 percent in this group of states from the 2011–2013 base period through 2019 (Table 6). They rose by roughly 62 percent in states that indexed their minimum wage rates to inflation. Across states that did not increase their minimum wage rates, house prices rose roughly 36 percent, and in states that enacted small minimum wage increases, median house prices rose by an average of roughly 31 percent. The BEA's income data show that *per capita* incomes grew roughly $7,600 more in states that enacted relatively large minimum wage changes than in states that enacted no minimum wage changes. Underlying macroeconomic conditions thus appear to have improved to a greater degree in states that enacted large minimum wage changes than in other states. Similar differences prevail whether we allocate states based on minimum wage changes enacted through January 2015, as in our initial grouping, or through January 2018, as in our second grouping.

The employment series for prime-age individuals also suggest 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. (See Table 6.)

The remaining panels of Figures 4 and 5 display employment trends among the skill groups in our primary analysis samples. As summarized in Table 6, employment among individuals ages 16 to 25 with less than a completed high school education (labeled "Low-Skilled Employment" in the table), 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 measured in the ACS is –1.4 percentage points, while the difference measured in the CPS is –1.1 percentage points.

Employment changes among individuals in states with small minimum wage changes exhibit a substantial divergence when comparing ACS and CPS data. In the ACS data, employment among low-skilled individuals rose modestly less in these states relative to individuals in states that enacted no minimum wage changes. In the CPS data, by contrast, employment among low-skilled individuals rose nontrivially more in these states than in states that enacted no minimum wage changes.

Section IV: Framework for Estimating the Effects of Minimum Wage Changes

This section presents our regression frameworks for estimating the effects of recent minimum wage increases. The framework is the same as that described in the pre-commitment plan outlined in Clemens and Strain (2017, 2018b). The remaining text of this section is largely unchanged from our prior work.

Our analysis plan adopts a standard program evaluation approach in which we divide states into groups based on the minimum wage policy changes they have implemented over the time period we analyze. We then 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):

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

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. These groups consist of young adults (individuals ages 16 to 21) and individuals ages 16 to 25 with less than a completed high school education.

Like any standard difference-in-differences specification, equation (1) 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.

We use $Policy_{g(s)}$ to represent 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 all of the estimates we present, 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 a base period consisting of 2011, 2012, and 2013 through a post period consisting of 2019.

The coefficient $\beta_{g(s)}$ is an estimate of the causal effect of states' minimum wage policy changes on employment under standard, but nontrivial, assumptions. The key assumption is that employment among low-skilled individuals would, in the absence of the minimum wage changes we analyze, have evolved similarly across the various groups of states. We investigate threats to this assumption in multiple ways. First, we investigate the robustness of our estimates to changes in the variables used to control for variations in economic conditions. That is, we examine whether our estimates are robust 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 detailed in Section VI, we implement a "stacked event study" estimator (Cengiz *et al.*, 2019) and the "imputation" estimator of Borusyak, Jaravel, and Spiess (2021) to confirm that our estimates are not driven by concerns that can arise in difference-in-differences settings that feature staggered treatment rollouts and heterogeneous treatment effects.

Third, we estimate a triple-difference extension of equation (1). The triple-difference framework is described by equation (2). The notation for equation (2) adds the subscript *d(i)* for demographic groups, which distinguishes between the within-state control groups and the groups that are "targeted" by minimum wages. Equation (2) augments equation (1) with three sets of two-way fixed effects. These include 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:

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

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).

Section V: 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;[12] (2) analysis samples consisting of individuals ages 16 to 25 with less than a completed high school education (low-skilled workers)[13] or samples consisting of all individuals ages 16 to 21 (young workers);[14] (3) difference-in-differences specifications described by equation (1) or triple-difference specifications described by equation (2);[15] (4) a "post" period consisting of 2015, 2016, 2017, 2018, and 2019 or a "post" period consisting solely of 2019;[16] (5) the barrier between "large" and "small" changes based on changes enacted through January 2015 or based on changes enacted through January 2018;[17] and (6) including all states in the analysis or omitting states that shift policy categories between January 2015 and January 2018.[18] Rather than discuss results on an estimate-by-estimate basis, we use Table 8 to summarize the key patterns we observe across our pre-

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[12] For ACS estimates, see appendix Tables 6A, 7A, 8A, 9A, 10A, 11A, 12A, 13A 14A, and 15A. For CPS estimates, see appendix Tables 6B, 7B, 8B, 9B, 10B, 11B, 12B, 13B, 14B, and 15B.

[13] For estimates on individuals ages 16 to 25 with less than a completed high school education, see Columns 1 and 2 of appendix Tables 6A, 6B, 7A, 7B, 12A, 12B, 14A, and 14B and Panel A of appendix Tables 8A, 8B, 9A, 9B, 10A, 10B, 11A, 11B, 13A, 13B, 15A, and 15B.

[14] For estimates on all individuals ages 16 to 21, see Columns 3 and 4 of appendix Tables 6A, 6B, 7A, 7B, 12A, 12B, 14A, and 14B and Panel B of appendix Tables 6A, 6B, 7A, 7B, 8A, 8B, 9A, 9B, 11A, 11B, 15A, and 15B.

[15] For difference-in-differences specifications, see appendix Tables 8A, 8B, 9A, 9B, 10A, 10B, 11A, 11B, 13A, 13B, 15A, and 15B. For triple-difference specifications, see Tables 6A, 6B, 7A, 7B, 12A, 12B, and 14A, 14B.

[16] For estimates in which the post period is 2015–2019, see appendix Tables 6A, 6B, 8A, 8B, 12A, 12B, 13A, and 13B. For estimates in which the post period is 2019 alone, see appendix Tables 7A, 7B, 9A, 9B, 10A, 10B, 11A, 11B, 14A, 14B, 15A, and 15B.

[17] For estimates using the division of states based on changes enacted as of January 2015, see appendix Tables 6, 7, 8, 9, 12, 13, 14, 15A, and 15B. For estimates using the division of states based on changes enacted as of January 2018, see appendix Tables 10, 11A, and 11B.

[18] For estimates including all states, see appendix Tables 6, 7, 8, 9, 10, and 11A–B. For estimates omitting states that shift policy categories between January 2015 and January 2018, see appendix Tables 12, 13, 14, and 15A–B.

committed specifications.

Our first finding is that large statutory minimum wage changes are, on average, associated with a differential employment decline of roughly 2.6 percentage points across the full set of specifications we estimate using both of our primary analysis samples. Across the full set of estimates, roughly four-fifths are statistically distinguishable from zero. Estimates are systematically more negative for the sample consisting of individuals ages 16 to 25 with less than a completed high school education than for the larger sample of all individuals ages 16 to 21. Estimates tend to have greater precision in our triple-difference specifications than in our difference-in-differences specifications.

Second, the results imply that the medium-run effects of large minimum wage changes are nontrivially larger than their short-run effects. This is most immediately apparent from the fact that our estimates for states with large statutory increases became systematically more negative with the addition of the 2017, 2018, and 2019 data to our analysis. In our analyses of data that extended through 2017 and 2016 (Clemens and Strain 2018a, 2019), the equivalent averages across coefficients were –2.1 and –1.0 percentage points, respectively.

Additionally, one can compare the estimates in appendix Tables 7A, 7B, 9A, 9B, 14A, 14B, 15A, and 15B with the overall distributions of point estimates. Appendix Tables 7A, 7B, 9A, 9B, 14A, 14B, 15A, and 15B are the tables in which states are categorized based on their earlier minimum wage changes (from January 2013 to January 2015) and in which 2014–2018 are excluded from the sample, such that we capture “medium-run” effects through 2019. The estimates in these tables average just over –2.7 percentage points. Equivalent estimates that include all data from 2015 to 2019 average –2.3 percentage points.

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 estimates for large statutory increases are slightly smaller, but the estimates are still negative and statistically distinguishable from zero in a sizable majority of specifications.

Fourth, estimates for small statutory minimum wage changes are highly variable for both young and low-skilled individuals. For states with small statutory minimum wage changes, the average estimate across our ACS specifications is –1.0 percentage point. Very few of these estimates are statistically distinguishable from zero. The average estimate across our CPS specifications is 1.7 percentage points. A modest number of CPS specifications yield positive and statistically significant point estimates for states with "small" minimum wage increases. Averaged across the ACS and CPS, the mean point estimate is 0.4 percentage points. The difference between our ACS and CPS results for states with small statutory increases is nontrivial and has been persistent across our annual updates. Taken together, the evidence implies that the smaller minimum wage changes in our sample have had no detectable impacts on employment.

Fifth, estimates of the effects of minimum wage increases linked to inflation-indexing provisions average 0.0 percentage points across our analyses of ACS and CPS data. For this group, the average estimate across our ACS specifications was 0.7 percentage points, while the average estimate across our CPS specifications was –0.5 percentage points. The average is thus quite close to zero, while the difference in signs when comparing the ACS and CPS is the opposite of what we observe in our analysis of "small" minimum wage increases. This leads us to conclude that differences we see across data sets are likely best interpreted as a result of sampling variations. Estimates using the ACS will tend to be more reliable than estimates using the CPS because the ACS has much larger samples and is subject to much lower survey non-response rates.

Section VI: Additional Analyses Outside of Our Precommitment Plan

In this section we present a set of analyses of ACS data that are outside of our pre-analysis plan, but that provide additional evidence on the validity and economic implications of our findings. We further investigate, for example, 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 (Baker, Larcker, and Wang, 2021; Borusyak, Jaravel, and Spiess, 2021; Callaway and Sant'Anna, 2020; 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 policy groupings:

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

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. It is not subject, for example, 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):

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

$$+ \varepsilon_{i,s,t}, \tag{4}$$

in which *p(s,t)* describes how many calendar years year *t* falls relative to the year immediately before a state implemented its first minimum wage change due to new legislation, and where $\boldsymbol{Time_{p(s,t)}}$ is a set of dummy variables associated with each value of *p(s,t)*. 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 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" (Sun and Abraham 2020), may fail to describe any treatment effects of genuine interest (Callaway and Sant'Anna, 2020), and may even carry the opposite sign of the underlying effects of interest (Goodman-Bacon, 2021).

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 (2021) 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:

$$Y_{i,s,g,c,t,p(s,t)} = \sum_{g\neq 0} \sum_{p(s,t)\neq 0} \beta_{g,p(s,t)} Policy_{g(s)} \times Time_{p(s,t)} + \alpha_{1s,c}\, State_{s,c}$$

$$+ \alpha_{2t}\, Time_t + X_{i,s,t}\, \gamma + \varepsilon_{i,s,t}. \tag{5}$$

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 (2021), 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 (2021) 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 arises 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 the negative weights problem by effectively eliminating staggered treatment timing.

Finally, we implement a design developed by Borusyak, Jaravel, and Spiess (2021), 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 fixed 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.[19] In the final step, 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 and distinguishing between short- and long-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 either the full prime-age population or the prime-age population with at least some college education.

Results from Supplemental Analyses Using Recently Proposed Estimation Frameworks

Figure 6 presents estimates of equation (3), which allows us to track the calendar time dynamics

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

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 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.[20] 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

[20] Roth (2021) 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.

as having “rich controls.”

Panels B and D of Figures 5 and 6 reveal that the inclusion of the richer set of covariates has 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 7 and 8 present the basic "event study" estimates described by equation (4). The medium-run estimates in Figure 7 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, the estimates labeled as applying to "small increasers and indexers" combine the states we initially categorized as having "small" minimum wage increases with the states that were initially categorized as "indexers," but which subsequently enacted new pieces of minimum wage legislation. In some panels of Figures 7 and 8, our estimates for the years preceding minimum wage increases have an ostensibly puzzling quirk. While there is no evidence of a secular, pre-policy-change trend, employment appears to be temporarily elevated during the year that falls two years prior to the enactment of a state's first minimum wage increase during our analysis period. Although the end-line estimates are quite similar to our previous estimates, the estimates of equation (4) might be said to look "less clean" than one might expect. 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 9 and 10. Interestingly, the pre-event estimates of equation (5) evolve quite smoothly and provide no evidence that would raise concerns regarding divergent trends over the years preceding the implementation of minimum wage increases.

Over the years following the enactment of minimum wage increases, estimated employment effects become increasingly negative with time. Across the full set of states that enacted minimum wage changes due to new legislation, we estimate employment declines quite close to zero as of the first year following the implementation of a state's first minimum wage change. 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 10, we present estimates of equation (5) in which we differentiate between large minimum wage increases and minimum wage increases that are either small or that were enacted by states that initially had inflation-indexing provisions but that subsequently enacted new minimum wage legislation. 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 or that initially had inflation-indexing provisions. 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 or that initially had inflation-indexing provisions. By contrast, the year 1 estimates for states with large minimum wage increases relative to states with small or inflation-indexed minimum wage increases differ modestly in economic terms and are not uniformly statistically distinguishable from one another.

Figures 11 and 12 present estimates comparable to those in Figures 9 and 10, but using the multistep “imputation” procedure proposed by Borusyak, Jaravel, and Spiess (2021) (BJS). 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 11 and 12 are largely indistinguishable from those in Figures 9 and 10. 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 or that initially had inflation-indexing provisions, 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, appendix Figures A2 through A5 present a set of falsification checks in which we investigate whether our specifications predict employment changes in groups that are unaffected by the minimum wage. The first group we consider is the full population of prime-age adults. The second group consists of prime-age adults with at least some college education. The figures reveal that employment among prime-age adults and prime-age adults with at least some college education moved almost perfectly in parallel when comparing our treatment and control groups. This is true 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 that have been 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).[21] Further, the analysis in this section has shown that the effects we estimate are robust to the adoption of specifications that resolve concerns that have been raised in recent applied econometrics research on difference-in-differences settings with staggered treatment rollouts and heterogeneous treatment effects.

Section VII: Wage Effects and Implied Elasticities

What do our estimates imply for our understanding of 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 sections with estimated changes in wages. We estimate the wage effects of recent minimum wage changes using the full set of difference-in-differences models we used to estimate employment effects. We summarize these estimates in Table 9. Alongside estimates of wage impacts, we present estimates of the relationship between our policy regimes and the minimum wage itself.

On average across our specifications, we estimate that large minimum wage changes involved minimum wage increases averaging \$2.90, that small minimum wage changes involved increases

[21] 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."

averaging \$1.90, and that states with inflation indexing regimes had increases averaging \$0.96.[22] 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 number for states with small minimum wage increases is \$0.92, while the corresponding number for the states with inflation-indexed minimum wage increases was \$0.47. For the sample of all individuals ages 16 to 21, the corresponding wage increases were of \$1.34, \$0.70, and \$0.33 in the states with large increases, small increases, and inflation-indexed increases, respectively.

Table 10 summarizes the key inputs for calculating 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.

We begin by presenting elasticities that average 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. We estimate an own-wage elasticity of –0.26 for individuals ages 16 to 25 with less than a completed high school education and of –0.23 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 –0.124 and –0.082. The latter estimates are close to the median estimate reported in Neumark and Shirley's (2021) recent meta-analysis. They are also within the range highlighted by the meta-analysis of Wolfson and Belman (2019).

We next compare elasticities across policy regimes. We find that the elasticities we estimate vary

[22] 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.

quite 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 –1.01 for individuals ages 16 to 25 with less than a completed high school education and of –0.41 for all individuals ages 16 to 21. For small minimum wage changes, we estimate an own-wage elasticity of 0.46 for individuals ages 16 to 25 with less than a completed high school education and of –0.032 for all individuals ages 16 to 21. Very few of the estimates underlying either of these two elasticities are individually statistically distinguishable from zero. For inflation-indexed minimum wage changes, we estimate an own-wage elasticity of 0.16 for individuals ages 16 to 25 with less than a completed high school education and of –0.17 for all individuals ages 16 to 21. For this final policy group, it is once again the case that very few of the estimates underlying either elasticity are statistically significant.

Elasticities of employment with respect to the minimum wage itself follow a quite similar pattern, though with a moderately different interpretation. 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.

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 that are either within the consensus range or that are more positive than the consensus range. As we have emphasized throughout, investigating this heterogeneity was a primary motivation for the pre-analysis plan to which we committed while analyzing data from 2011 to 2015.

Section VIII: 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 the employment effects of large and small minimum wage increases, as well as between their short- and medium-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 among individuals with low levels of experience and education. By contrast, our estimates of the effects of relatively small minimum wage increases are variable and 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.

We conduct additional analyses using empirical methods that include the recently developed "stacked event study" and "imputation" estimators. The evidence from these estimators supports the validity of our pre-committed analyses. Further, these estimators shed additional light on the dynamic effects of the minimum wage changes we analyze. Estimates from these frameworks show that the employment effects of relatively large minimum wage increases accumulate gradually over time. That is, medium-run employment effects are substantially larger than short-run employment effects. Because our analysis takes place during an economic expansion, it is unlikely to detect long-run effects driven by the adoption of relatively capital-intensive production technologies by new firms (Sorkin, 2015). Long-run effects will tend to emerge after a period of churn, as during the COVID-19 pandemic, and are thus a topic for future work.

The minimum wage increases we analyze relate quite closely to the $10/hour and $12/hour policy options that were recently analyzed by the Congressional Budget Office (CBO, 2019). As shown in Figures 1 and 2, the set of smaller minimum wage increases we analyze averaged just under $2 within our analysis samples, with January 2019 values averaging between $9 and $10. The set of larger minimum wage increases we analyze averaged just under $4. In this latter group, minimum wages averaged between $11 and $12 as of January 2019, with states including New York and California continuing on courses to minimum wages of $15. CBO also analyzed the effects of $15/hour, but such an increase would be outside of our sample. The minimum wage increases we analyze are thus much larger than the typical increases analyzed in previous research. Indeed, the average increase in our set of "large" increases is over four times as large, in percent terms, as the average size of the 138 increases analyzed by Cengiz *et al.* (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, 2019), and the conceptual framework that accompanied our pre-analysis plans (Clemens and Strain, 2017). For readers' convenience, the framework that accompanied our pre-analysis plan can be found in Appendix B. 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).

The dynamics of the employment effects we estimate connect to models in which firms face adjustment frictions. Standard adjustment frictions relate to firms' choices involving capital and technological infrastructure, which may be fixed over the short run, but which become choice variables over the long run (Sorkin, 2015; Aaronson *et al.*, 2018). Adjustments along these margins can be swift in the wake of an economic downturn because new firms must make fresh decisions over their reliance on labor, capital, and technology. Over the course of an economic expansion, however, as during our analysis samples, these adjustments may unfold quite gradually.

These standard theoretical considerations can help to make sense of a broad set of findings in the recent minimum wage literature. First, for example, analyses of historical variation in minimum wages in the United States tend to find quite modest employment effects (Cengiz *et al.*, 2019, 2021). 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.*, 2018). Models that incorporate both labor market search frictions and costs to firms' adjustments to their production technologies can quite readily make sense of the full set of findings described above.

Stepping outside of the minimum wage literature, we conclude by discussing the role of pre-analysis plans in the applied econometrics toolkit. We emphasize that pre-analysis plans can have high value for analyses of treatment effect heterogeneity in non-experimental settings. This is because analyses of treatment effect heterogeneity are prone to data mining (or "p-hacking") concerns beyond those that arise when estimating average treatment effects. Heading off these data mining concerns is precisely where pre-analysis plans excel. Pre-committed analyses may be particularly attractive for distinguishing between the short- and long-run effects of policy interventions. In this important use case, pre-committed analyses of long-run effects can be structured as extensions of frameworks that have been vetted in the context of short-run analyses. Indeed, analyses of this sort could quite readily be encouraged by research funding agencies. We view this proposed use of pre-analysis plans as having promise for improving the scientific basis for our understanding of the long-run effects of public policy. In addition to supporting analyses of long-run effects, pre-analysis plans can reduce the p-hacking concerns that limit the scientific value of heterogeneity analyses (e.g., contrasting the effects of small vs. large minimum wage changes) for testing the relevance of economic models and mechanisms.

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Figures and Tables

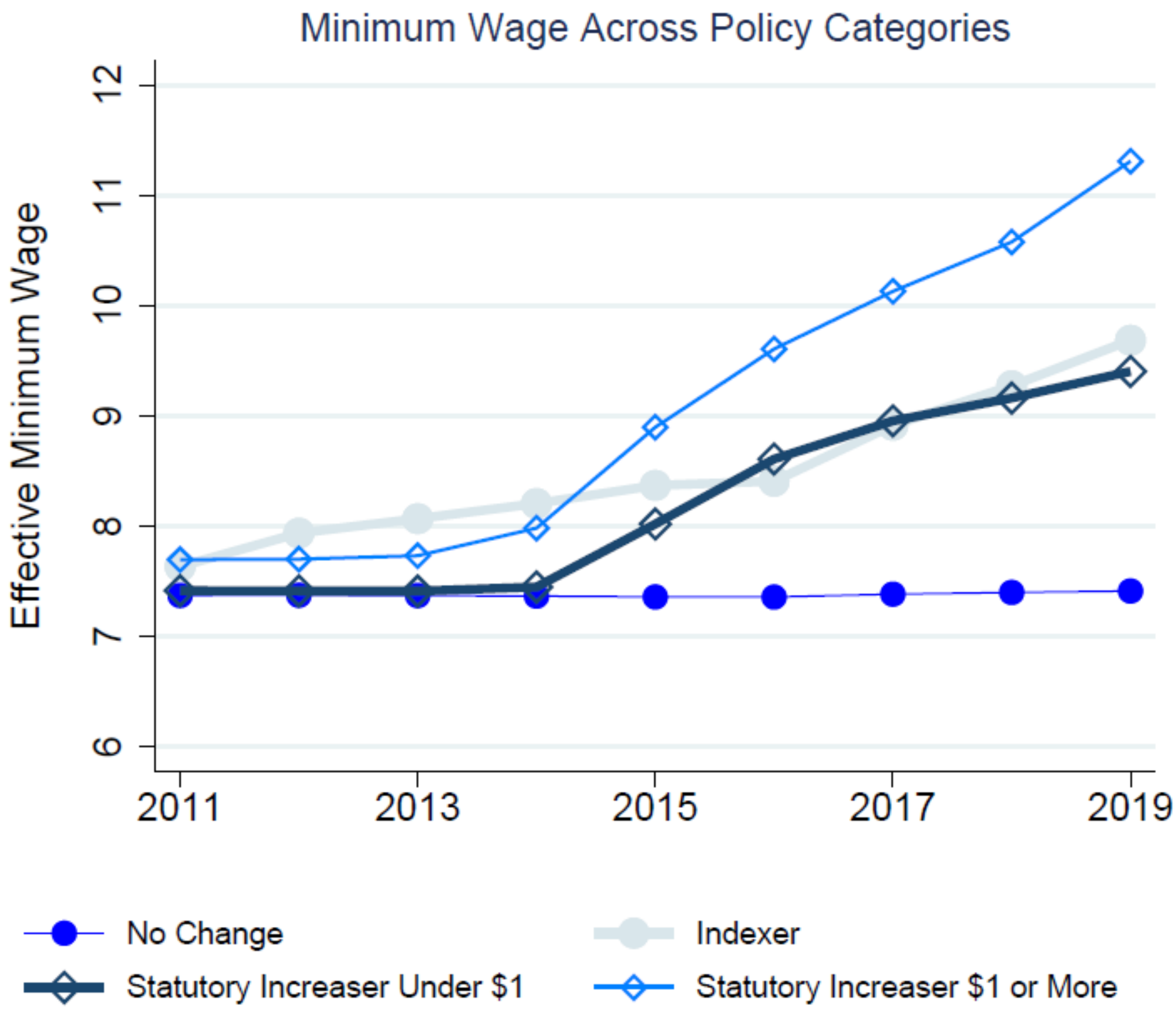

Figure 1. Average Minimum Wage Across Policy Categories: This figure plots the average annual effective minimum wage for states in each of our four policy categories from January 2011 to January 2019. States are defined as statutory increasers under $1 if the combined statutory increase in their minimum 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 US Department of Labor. Data on minimum wage policies come from the National Conference of State Legislatures. Averages are weighted by population.

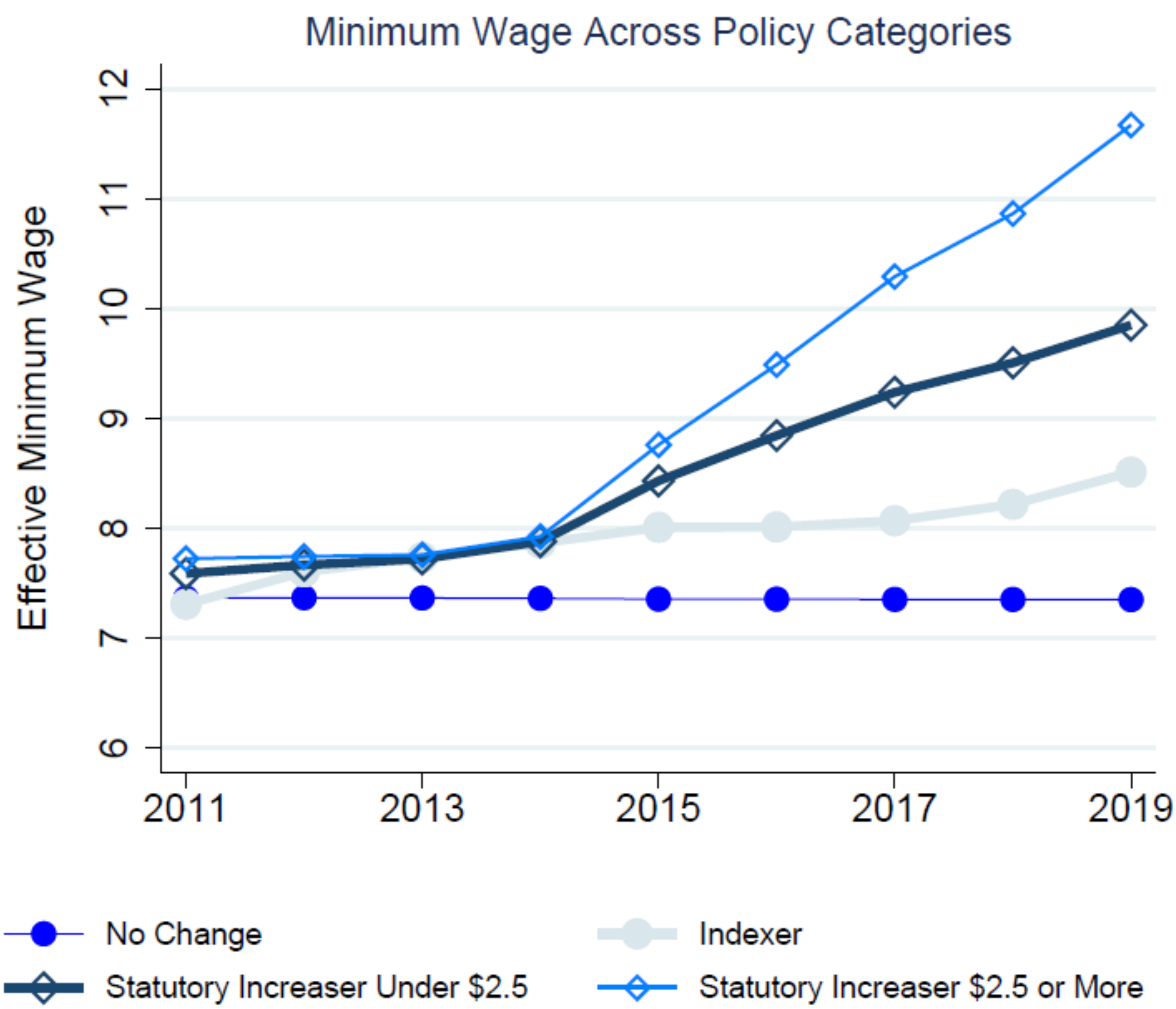

Figure 2. Average Minimum Wage Across Policy Categories: This figure plots the average annual effective minimum wage for states in each of our four policy categories from January 2011 to January 2019. States are defined as statutory increasers under $2.50 if the combined statutory increase in their minimum wage between January 2013 and January 2018 was under $2.50. States are defined as statutory increasers of $2.50 or more if the combined statutory increase in their minimum wage was $2.50 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 US Department of Labor. Data on minimum wage policies come from the National Conference of State Legislatures. Averages are weighted by population.

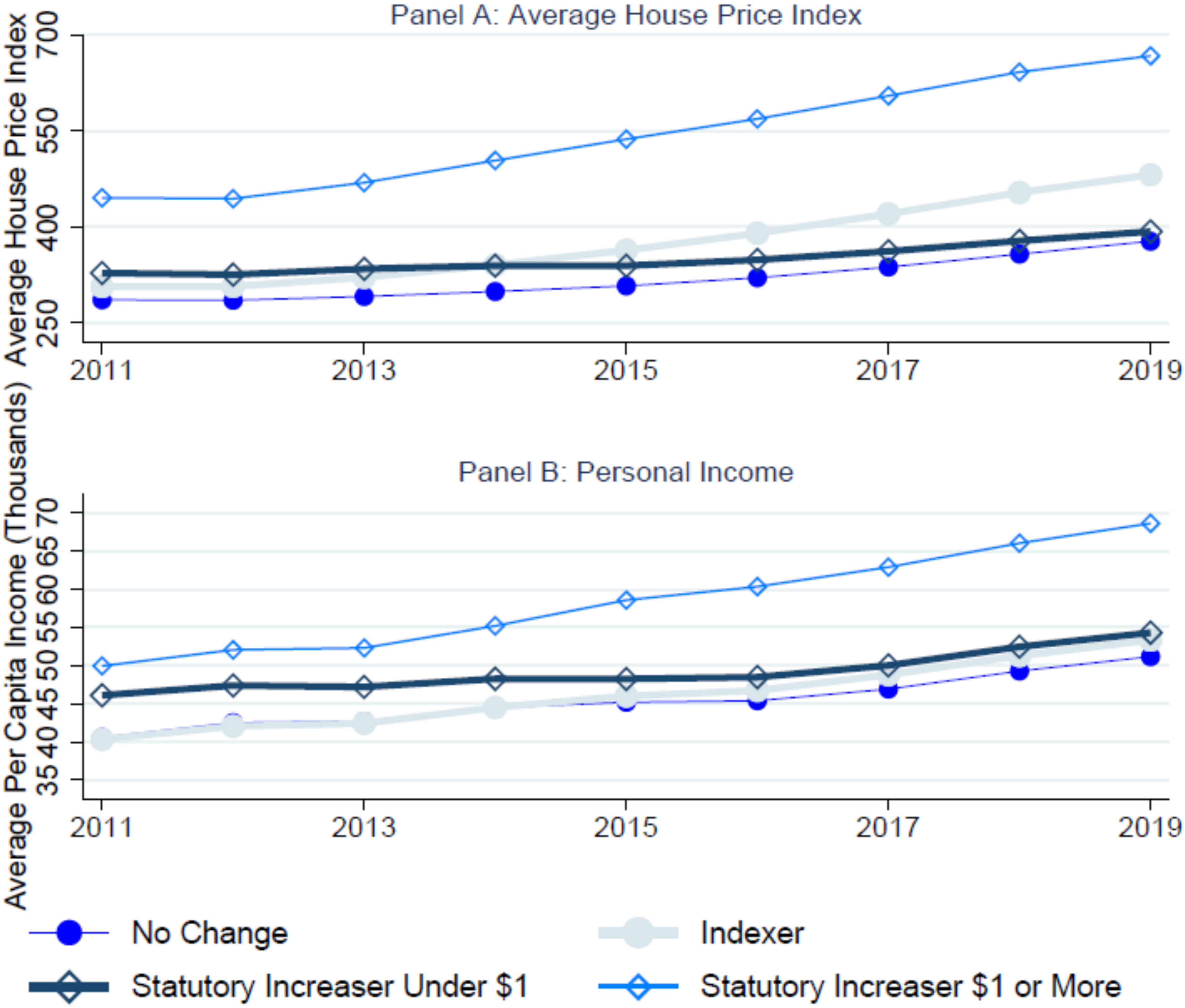

Figure 3. Macroeconomic Time Series Across Policy Categories: Panel A plots the average housing price index variable for each of our four policy categories from 2011 to 2019. Housing price index data come from the Federal Housing Finance Agency. Panel B plots average *per capita* income for each of our 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.

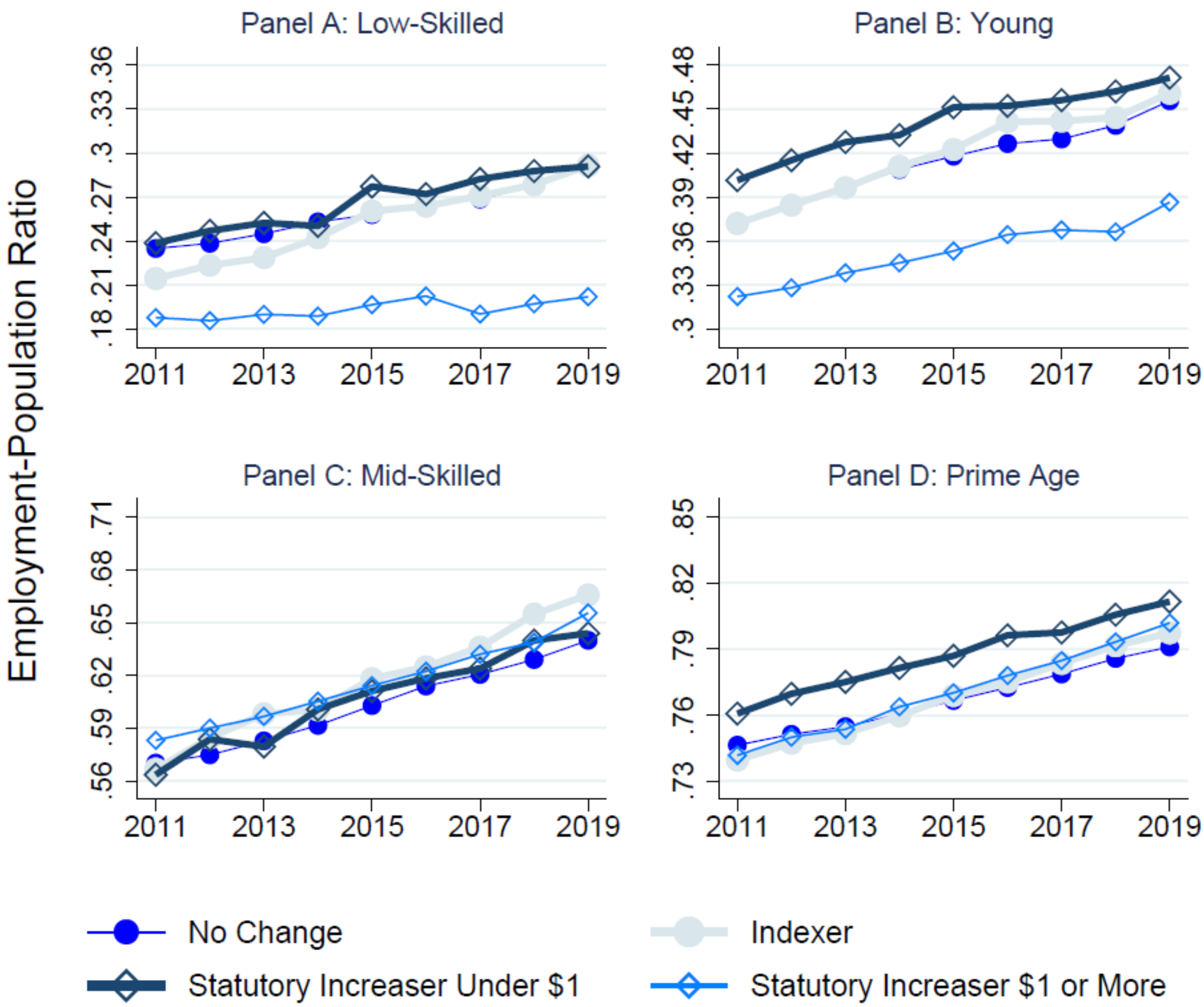

Figure 4. Employment Series in the ACS: This figure plots average annual employment rates for each of our four policy groups, broken out across four subsamples, from 2011 to 2019. Panel A plots employment rates for low-skilled individuals, defined as individuals ages 16 to 25 without a completed high school education. Panel B plots employment rates for young adults, defined as individuals ages 16 to 21. Panel C plots employment rates for mid-skill individuals, defined as individuals ages 22 to 30 with a high school degree and high school dropouts between the ages of 30 and 64. Panel D plots employment rates for prime-age individuals, defined as individuals between the ages of 26 and 54. Employment data come from the American Community Survey (ACS). 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. Averages are weighted by population.

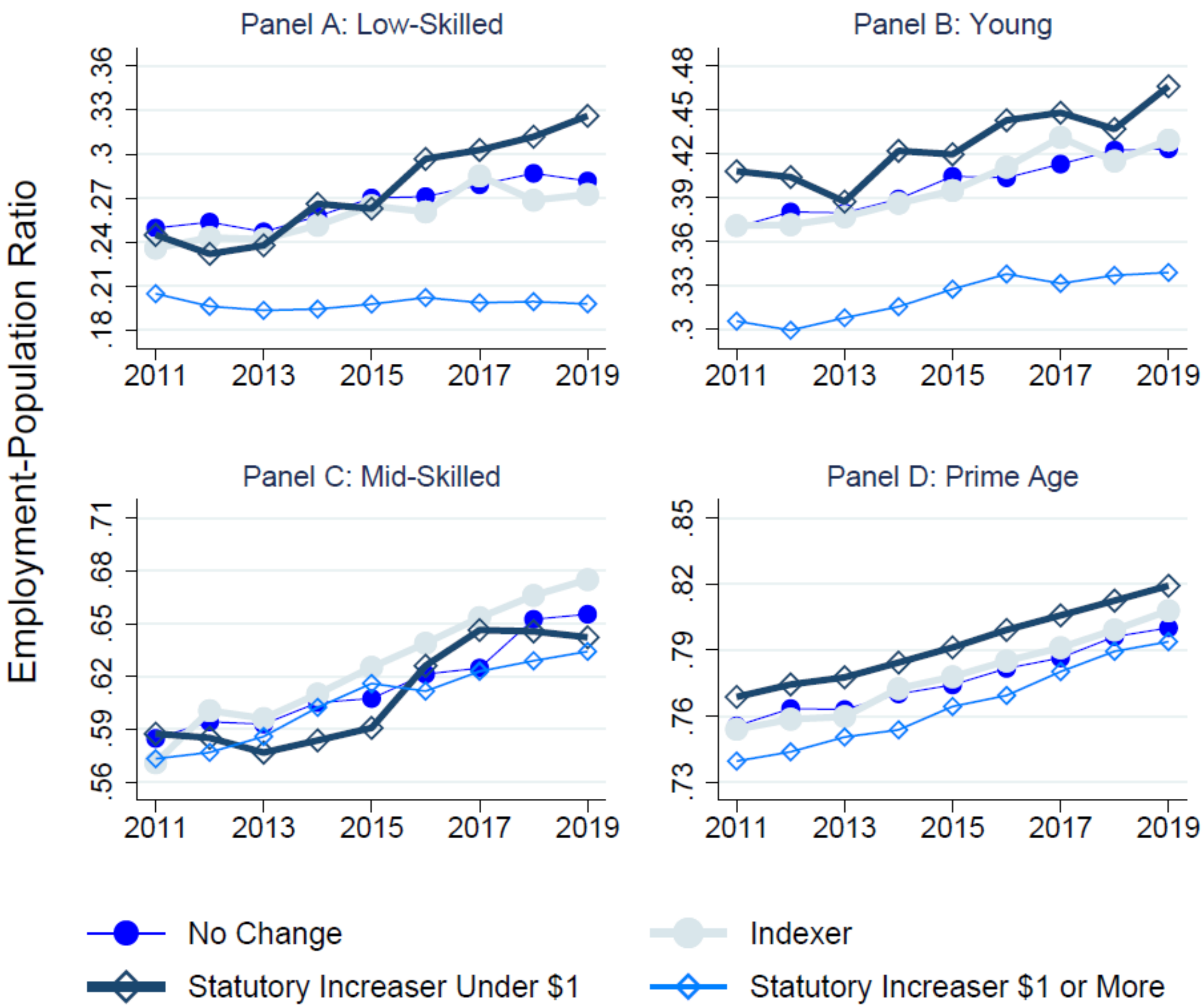

Figure 5. Employment Series in the CPS: This figure plots average annual employment rates for each of our four policy groups, broken out across four subsamples, from 2011 to 2019. Panel A plots employment rates for low-skilled individuals, defined as individuals ages 16 to 25 without a completed high school education. Panel B plots employment rates for young adults, defined as individuals ages 16 to 21. Panel C plots employment rates for mid-skill individuals, defined as individuals ages 22 to 30 with a high school degree and high school dropouts between the ages of 30 and 64. Panel D plots employment rates for prime-age individuals, defined as individuals between the ages of 26 and 54. Employment data come from the Current Population Survey (CPS). 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

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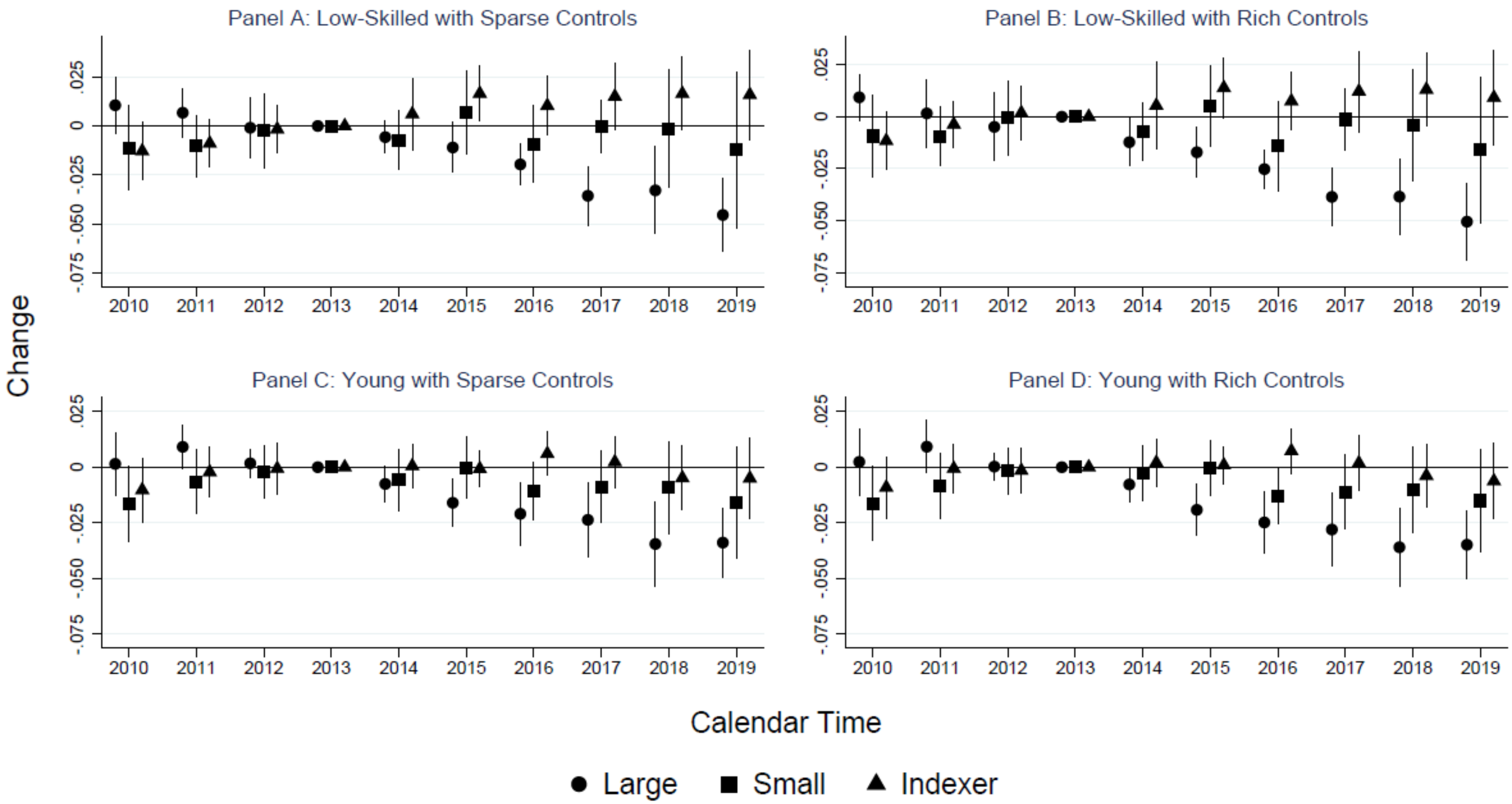

Figure 6. Event Studies of the Change in Employment Relative to 2013: This figure displays coefficients from event study regressions described by equation (3). All coefficients are estimates relative to a base year of 2013. States are divided into the large, small, and indexer groupings defined in Table 1. Panels A and B plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels C and D plot coefficients from regressions for young individuals defined as all individuals ages 16–21. The samples are from the ACS. 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.

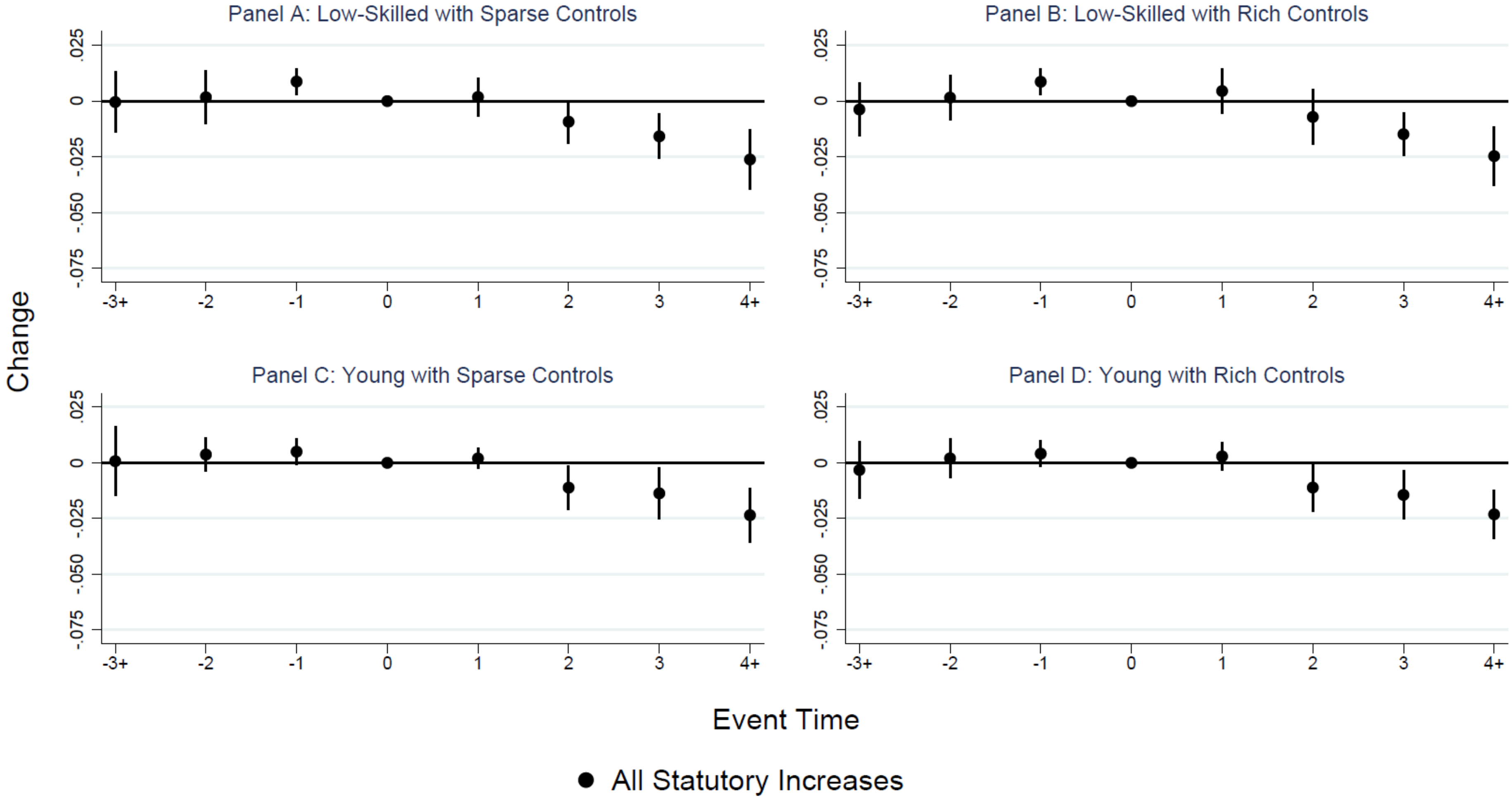

Figure 7. Event Studies of Changes in Employment Following Initial Statutory Minimum Wage Increases: This figure displays coefficients from event study regressions described by equation (4). Event Time is defined such that year "1" corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. Panels A and B plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels C and D plot coefficients from regressions for young individuals defined as all individuals ages 16–21. The samples are from the ACS. 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.

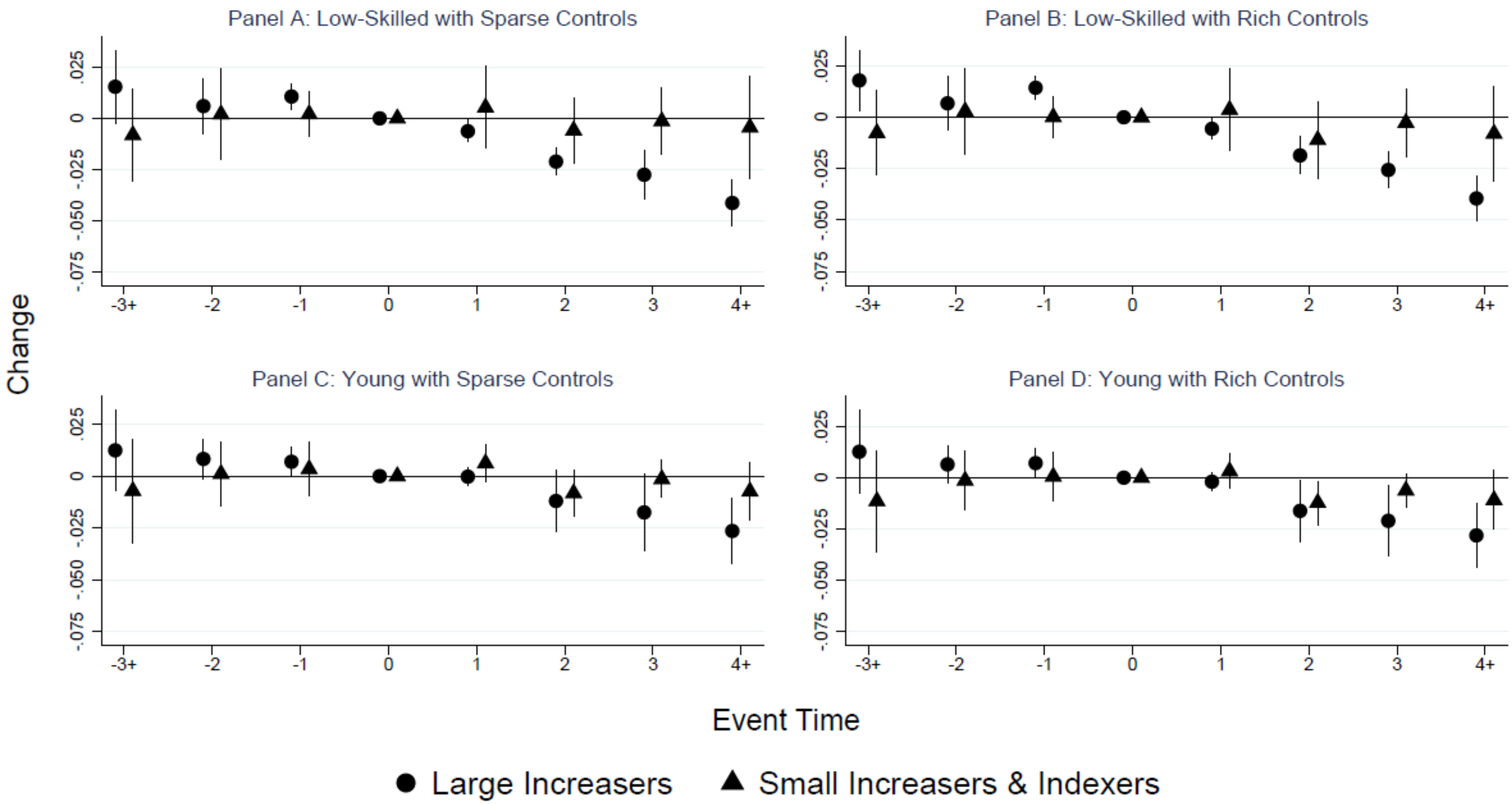

Figure 8. Event Studies of Changes in Employment Following Initial Statutory Minimum Wage Increases Broken Out by Policy Groups: This figure displays coefficients from event study regressions described by equation (4). Event Time is defined such that year "1" corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. We compare estimates for large vs. small increases as defined in the main text. Panels A and B plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels C and D plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. 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.

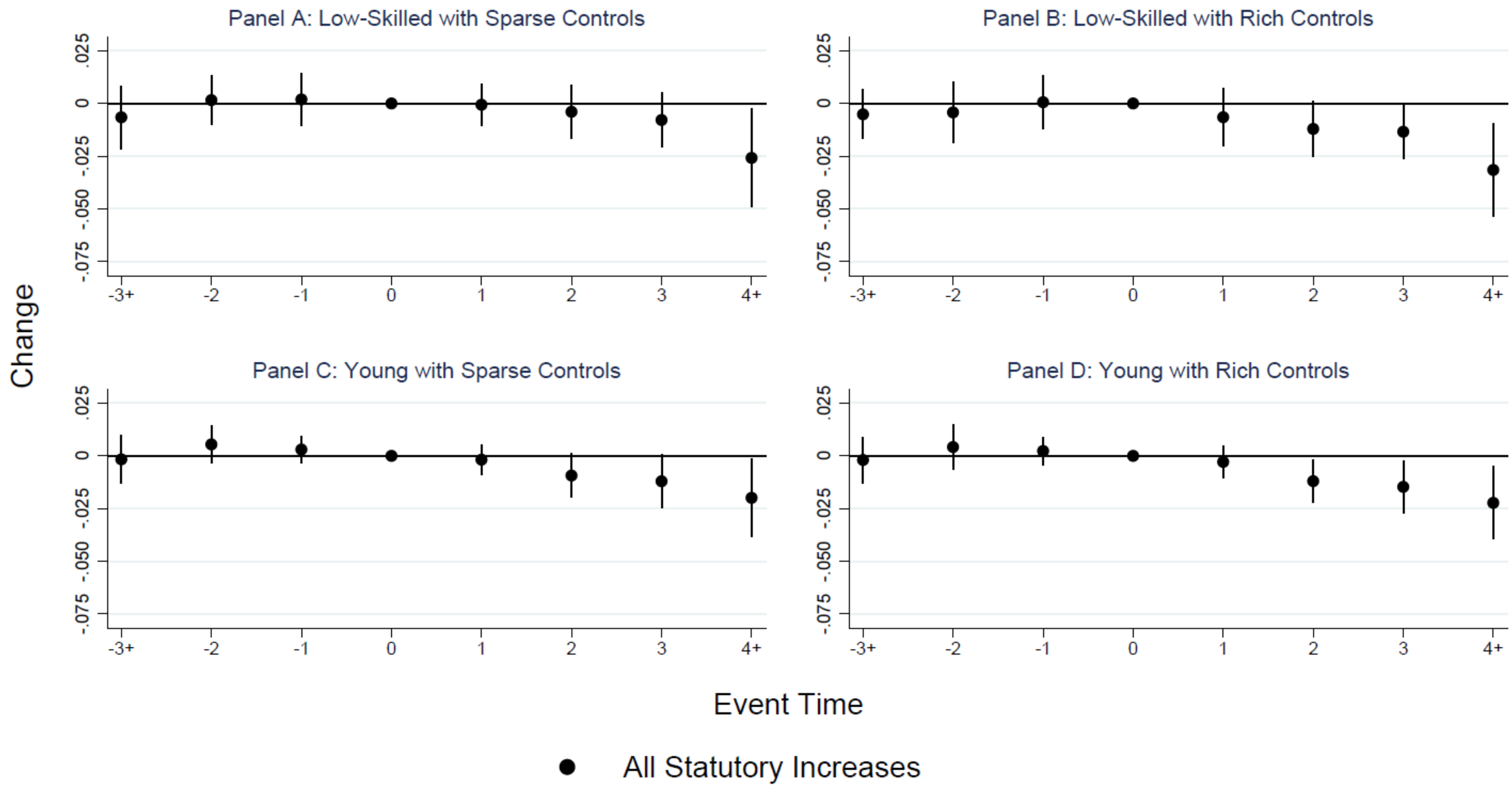

Figure 9. Stacked Event Studies of Changes in Employment Following Initial Statutory Minimum Wage Increases: This figure displays coefficients from the "stacked event study" estimator described by equation (5). Event Time is defined such that year "1" corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. Panels A and B plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels C and D plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. 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.

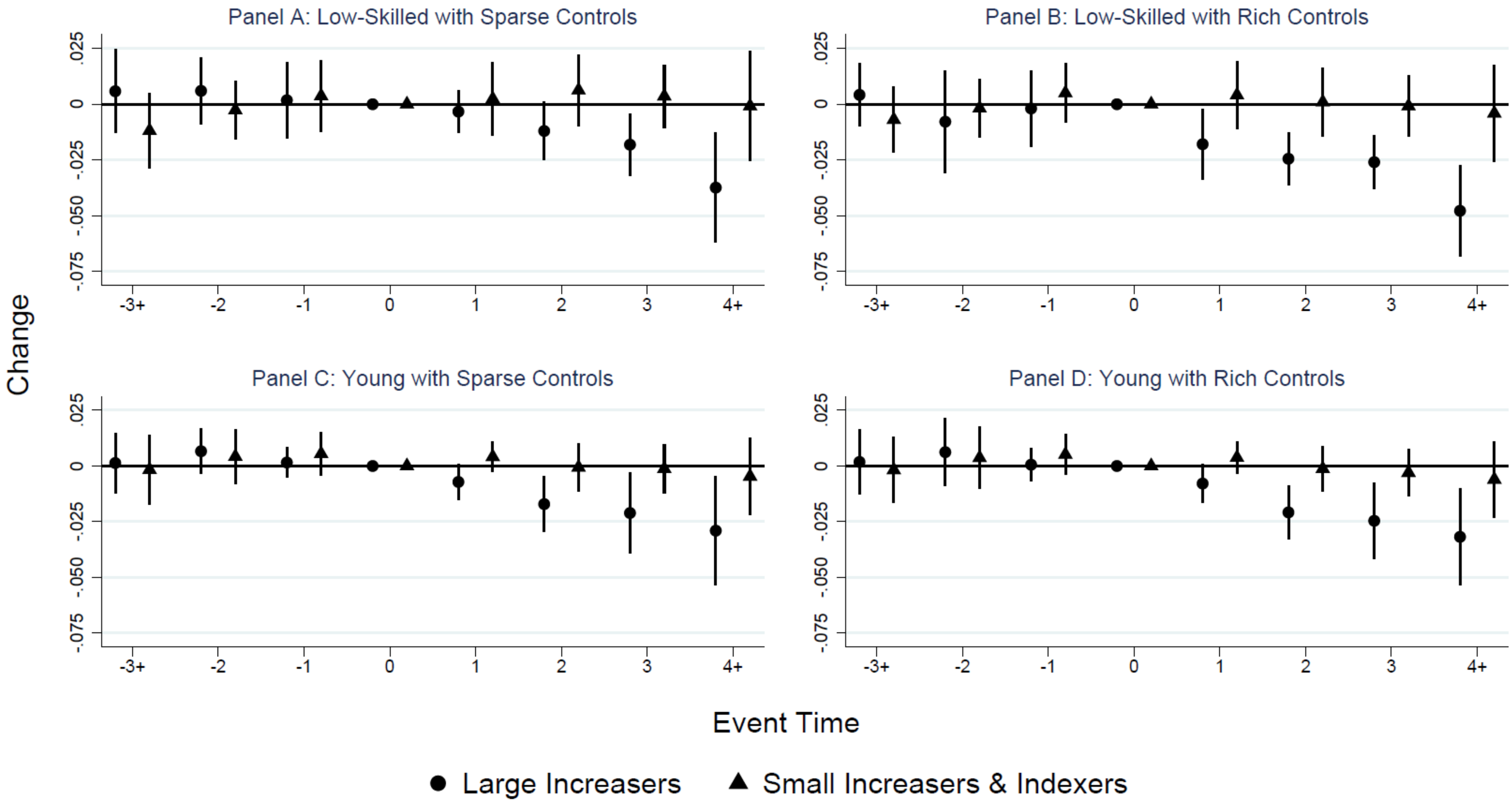

Figure 10. Stacked Event Studies of Changes in Employment Following Large and Small Statutory Minimum Wage Increases: This figure displays coefficients from the "stacked event study" estimator described by equation (5). Event Time is defined such that year "1" corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. We compare estimates for large vs. small increases as defined in the main text. Panels A and B plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels C and D plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. 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.

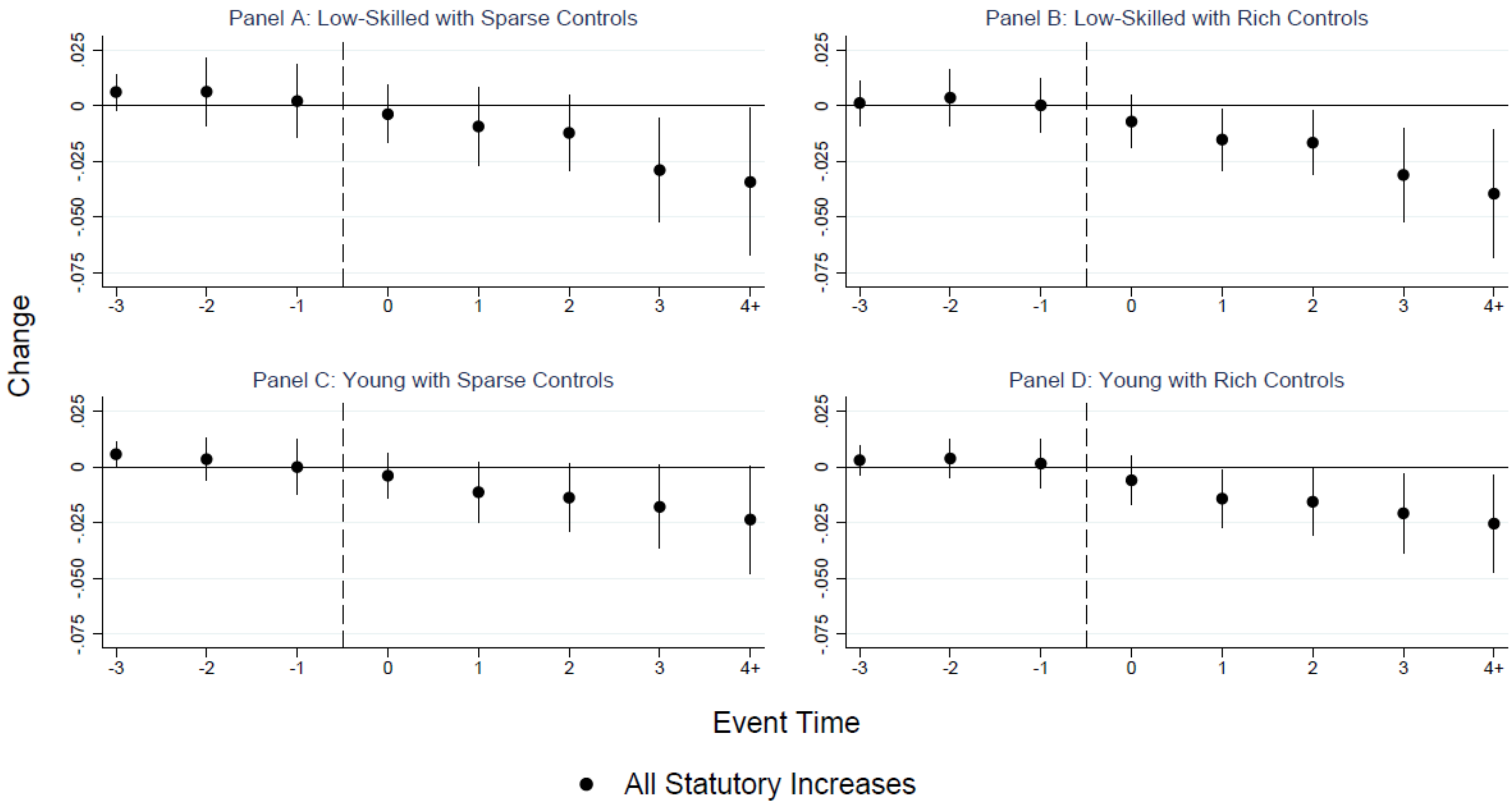

Figure 11. Event Studies of Changes in Employment Following Statutory Minimum Wage Increases Using the BJS Imputation Estimator: This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel and Spiess (2021) (BJS). For the BJS estimator, we code the first treatment year as the year in which a state's first statutory minimum wage increase took effect. Note that this appears graphically as "year 0" in the BJS figures, but corresponds with year 1 in the stacked event study figures. Panels A and B plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels C and D plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. 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 base 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.

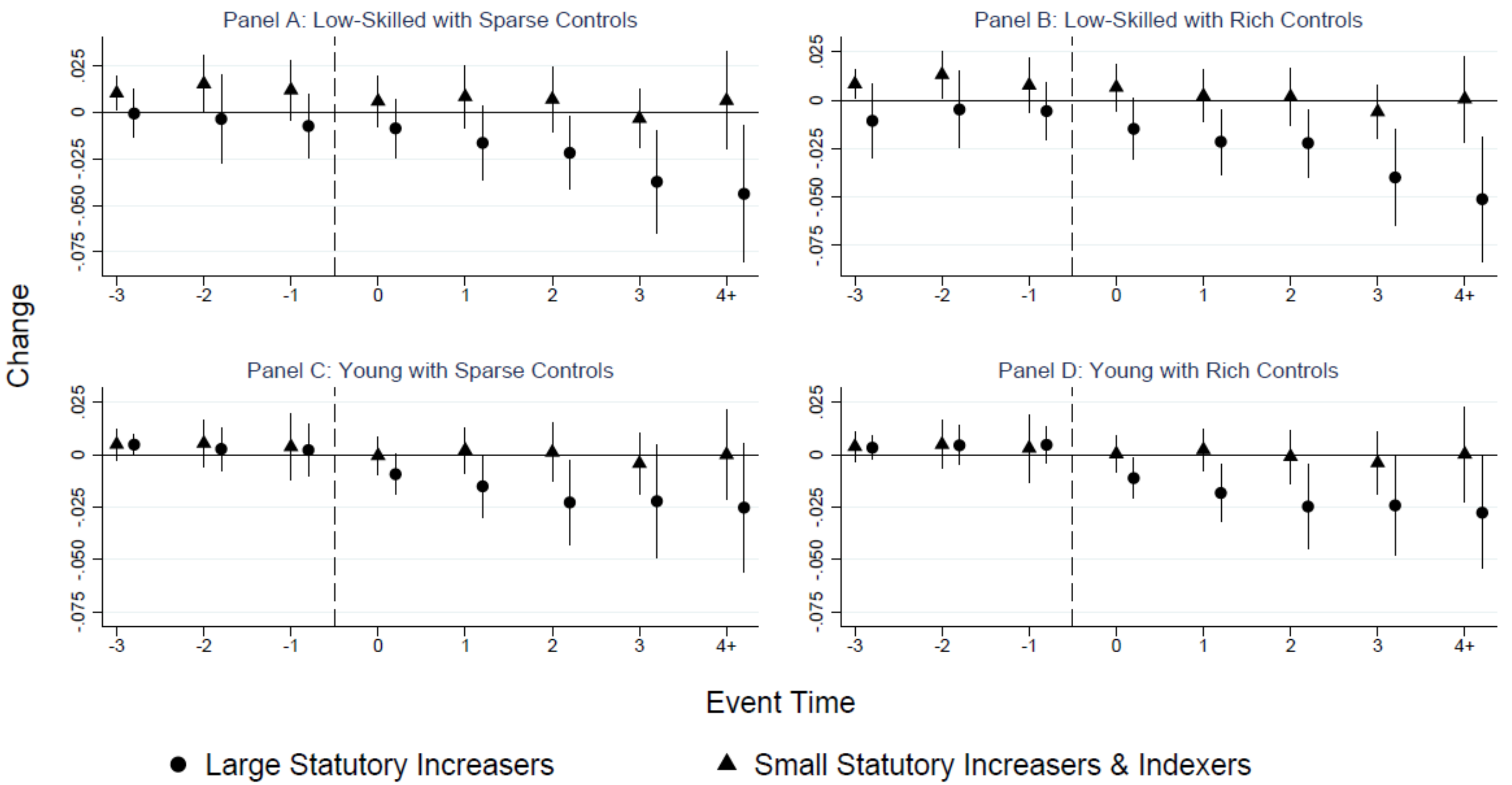

Figure 12. Event Studies of Changes in Employment Following Large and Small Statutory Minimum Wage Increases Using the BJS Imputation Estimator: This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel and Spiess (2021) (BJS). For the BJS estimator, we code the first treatment year as the year in which a state's first statutory minimum wage increase took effect. Note that this appears graphically as "year 0" in the BJS figures but corresponds with year 1 in the stacked event study figures. We compare estimates for large vs. small increases as defined in the main text. Panels A and B plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels C and D plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. 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 base 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. List of States with Statutory Minimum Wage Increases and Inflation-Indexed Increases Using Changes from 2013 to 2015 and $1 Cutoff

Statutory Increasers of $1 or More	Statutory Increasers Under $1
Alaska	Arkansas
California	Connecticut
District of Columbia	Delaware
Massachusetts	Hawaii
New Jersey	Maryland
New York	Michigan
Rhode Island	Minnesota
South Dakota	Nebraska
	West Virginia
Indexers	
Arizona	
Colorado	
Florida	
Missouri	
Montana	
Ohio	
Oregon	
Vermont	
Washington	

Note: Data on minimum wage indexing provisions come from the National Council of State Legislatures. The states labeled as "Indexers" link annual updates to their effective minimum wage rates to a measure of inflation. Data on minimum wage changes come from the U.S. Department of Labor. States are counted as statutory increasers of under $1 if the combined statutory increase in the minimum wage from January 1, 2013 through January 1, 2015 was under $1. States are counted as statutory increasers of $1 or more if the combined statutory increase in the minimum wage was $1 or more.

Table 2. List of States with Statutory Minimum Wage Increases and Inflation-Indexed Increases Using Changes from 2013 to 2018 and $2.50 Cutoff

Statutory Increasers of $2.50 or More	Statutory Increasers Under $2.50
Arizona	Alaska
California	Arkansas
District of Columbia	Colorado
Hawaii	Connecticut
Maine	Delaware
Massachusetts	Maryland
New York	Michigan
	Minnesota
	Nebraska
Indexers	New Jersey
Florida	Oregon
Missouri	Rhode Island
Montana	South Dakota
Ohio	Vermont
	Washington
	West Virginia

Note: Data on minimum wage indexing provisions come from the National Council of State Legislatures. The states labeled as "Indexers" link annual updates to their effective minimum wage rates to a measure of inflation. Data on minimum wage changes come from the U.S. Department of Labor. States are counted as statutory increasers of under $2.50 if the combined statutory increase in the minimum wage from January 1, 2013 through January 1, 2018 was under $2.50. States are counted as statutory increasers of $2.50 or more if the combined statutory increase in the minimum wage was $2.50 or more.

Table 3. 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	0.257	0.374	0.422
	(0.417)	(0.437)	(0.484)	(0.494)
Age	17.90	17.63	18.58	18.54
	(2.444)	(2.253)	(1.704)	(1.703)
Black	0.166	0.155	0.153	0.147
	(0.372)	(0.362)	(0.360)	(0.354)
High School Degree	0	0	0.343	0.358
	(0)	(0)	(0.475)	(0.479)
Some College Education	0	0	0.247	0.242
	(0)	(0)	(0.431)	(0.428)
House Price Index	325.9	413.3	330.4	419.8
	(99.86)	(133.1)	(101.6)	(135.9)
Income *per capita* ($1,000s)	43.81	51.82	44.04	52.24
	(6.270)	(8.524)	(6.364)	(8.665)
Effective Minimum Wage ($)	7.531	8.398	7.536	8.450
	(0.422)	(1.341)	(0.424)	(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 4. 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	0.261	0.360	0.398
	(0.424)	(0.439)	(0.480)	(0.490)
Age	17.97	17.73	18.50	18.47
	(2.423)	(2.243)	(1.730)	(1.734)
Black	0.164	0.156	0.155	0.150
	(0.370)	(0.363)	(0.362)	(0.357)
High School Degree	0	0	0.223	0.234
	(0)	(0)	(0.416)	(0.424)
Some College Education	0	0	0.299	0.290
	(0)	(0)	(0.458)	(0.454)
House Price Index	327.8	413.9	331.8	419.9
	(100.8)	(132.5)	(102.5)	(135.0)
Income *per capita* ($1,000s)	43.91	51.88	44.15	52.30
	(6.338)	(8.513)	(6.420)	(8.597)
Effective Minimum Wage ($)	7.535	8.416	7.541	8.461
	(0.423)	(1.344)	(0.426)	(1.366)
Observations	197,386	287,097	365,354	546,414

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 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 5. Unadjusted Differences Across Policy Regimes Using ACS Data and $1 Cutoff

	(1) 2011–2013	(2) 2015–2019	(3) Change	(4) Change Relative to Non-Increasers
Young Adult Employment				
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
Low-Skilled Employment				
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
Prime-Age Employment				
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
Mid-Skill Employment				
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
House Price Index				
Non-Increasers	274.0	336.0	62.0	
Indexers	290.6	410.0	119.4	57.4
Increase < $1	302.4	362.3	59.9	–2.1
Increase >= $1	455.0	609.1	154.1	92.1
Income *per capita* ($1,000s)				
Non-Increasers	40.99	47.61	6.62	
Indexers	40.87	49.00	8.13	1.51
Increase < $1	44.79	52.54	7.75	1.13
Increase >= $1	50.52	62.52	12.00	5.38

Note: 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 individuals 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-skill individuals are those ages 22 to 30 with a high school degree or high school dropouts between the ages of 30 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. 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 main text. 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 6. Unadjusted Differences Across Policy Regimes Using ACS Data and $1 Cutoff

	(1)	(2)	(3)	(4)
	2011-2013	2019	Change	Change Relative to Non-Increasers
Young Adult Employment				
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
Low-Skilled Employment				
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
Prime-Age Employment				
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
Mid-Skill Employment				
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
House Price Index				
Non-Increasers	274.0	373.8	99.8	
Indexers	290.6	469.7	179.1	79.3
Increase < $1	302.4	394.7	92.3	-7.5
Increase >= $1	455.0	677.4	222.4	122.6
Income *per capita* ($1000s)				
Non-Increasers	40.99	51.26	10.27	
Indexers	40.87	53.05	12.18	1.91
Increase < $1	44.79	56.50	11.71	1.44
Increase >= $1	50.52	68.42	17.9	7.63

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 skill 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-skill individuals are those ages 22 to 30 years old with a high school degree, or high school dropouts between the ages of 30 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. 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 7. Unadjusted Differences Across Policy Regimes Using ACS Data and \$2.50 Cutoff

	(1)	(2)	(3)	(4)
	2011–2013	2019	Change	Change Relative to Non-Increasers
Young Adult Employment				
Non-Increasers	0.384	0.455	0.071	
Indexers	0.383	0.457	0.074	0.003
Increase < \$2.50	0.402	0.463	0.061	–0.010
Increase >= \$2.50	0.330	0.392	0.062	–0.009
Low-Skilled Employment				
Non-Increasers	0.239	0.292	0.053	
Indexers	0.221	0.290	0.069	0.016
Increase < \$2.50	0.240	0.290	0.050	–0.003
Increase >= \$2.50	0.185	0.204	0.019	–0.034
Prime-Age Employment				
Non-Increasers	0.751	0.791	0.040	
Indexers	0.743	0.795	0.052	0.012
Increase < \$2.50	0.767	0.813	0.046	0.006
Increase >= \$2.50	0.743	0.797	0.054	0.014
Mid-Skill Employment				
Non-Increasers	0.576	0.640	0.064	
Indexers	0.566	0.650	0.084	0.020
Increase < \$2.50	0.593	0.666	0.073	0.009
Increase >= \$2.50	0.588	0.654	0.066	0.002
House Price Index				
Non-Increasers	272.8	372.7	99.9	
Indexers	266.2	411.1	144.9	45.0
Increase < \$2.50	342.0	479.0	137.0	37.1
Increase >= \$2.50	440.3	669.4	229.1	129.2
Income *per capita* (\$1,000s)				
Non-Increasers	40.99	51.26	10.27	
Indexers	40.34	51.14	10.8	0.53
Increase < \$2.50	46.34	59.64	13.3	3.03
Increase >= \$2.50	48.74	66.03	17.29	7.02

Note: This table reports employment rates for each of our four policy groups (non-increasers, indexers, increase < \$2.50, and increase >= \$2.50) 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 individuals 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-skill individuals are those ages 22 to 30 with a high school degree or high school dropouts between the ages of 30 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. 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 main text. 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 8. Summary of Employment Regression Results

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	All	All	All	All	ACS	ACS	ACS	CPS	CPS	CPS
Policy Group	All Changers	Large	Small	Indexer	Large	Small	Indexer	Large	Small	Indexer
Original Categories										
Post Period 2015–2019	–0.0038	–0.0277	0.0117	0.0046	–0.0262	–0.0035	0.0117	–0.0293	0.0269	–0.0024
Post Period 2019	–0.0080	–0.0419	0.0171	0.0009	–0.0463	–0.0116	0.0084	–0.0371	0.0458	–0.0066
Original Categories No Switchers										
Post Period 2015–2019	–0.0049	–0.0282	0.0104	0.0031	–0.0273	–0.0060	0.0109	–0.0292	0.0269	–0.0047
Post Period 2019	–0.0085	–0.0422	0.0172	–0.0006	–0.0463	–0.0115	0.0094	–0.0375	0.0459	–0.0106
Updated Categories										
Post Period 2019	–0.0094	–0.0315	0.0044	–0.0012	–0.0435	–0.0091	0.0085	–0.0194	0.0180	–0.0109
Overall Averages	–0.0066	–0.0340	0.0124	0.0020	–0.0377	–0.0079	0.0097	–0.0303	0.0327	–0.0058

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	All	All	All	All	ACS	ACS	ACS	CPS	CPS	CPS
Policy Group	All Changers	Large	Small	Indexer	Large	Small	Indexer	Large	Small	Indexer
Original Categories										
Post Period 2015–2019	–0.0059	–0.0182	0.0001	0.0005	–0.0217	–0.0049	0.0027	–0.0147	0.0051	–0.0017
Post Period 2019	–0.0090	–0.0235	0.0006	–0.0040	–0.0249	–0.0150	–0.0008	–0.0221	0.0163	–0.0071
Original Categories No Switchers										
Post Period 2015–2019	–0.0064	–0.0177	0.0013	–0.0027	–0.0205	–0.0023	0.0025	–0.0150	0.0050	–0.0079
Post Period 2019	–0.0104	–0.0238	0.0007	–0.0081	–0.0250	–0.0150	0.0012	–0.0227	0.0164	–0.0174
Updated Categories										
Post Period 2019	–0.0076	–0.0115	–0.0066	–0.0049	–0.0215	–0.0139	–0.0034	–0.0015	0.0008	–0.0064
Overall Averages	–0.0075	–0.0190	–0.0010	–0.0023	–0.0229	–0.0108	0.0001	–0.0150	0.0087	–0.0048

Note: This table presents averages across estimates from the regression analyses in our pre-analysis plan. The underlying estimates can be found in our appendix tables and 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 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 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 Table 1, which is the grouping from our original pre-analysis plan. The grouping of states we describe as "Updated" corresponds with the grouping in Table 2, 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.) Panel A summarizes analyses of individuals ages 25 and younger with less than a completed high school education, and Panel B summarizes analyses of all individuals ages 16 to 21.

Table 9. Summary of Wage Regression Results (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Elasticity	Wage	Wage	Wage	Wage	MW	MW	MW	MW
Policy Group	All Changers	Large	Small	Indexer	All Changers	Large	Small	Indexer
Original Categories								
Post Period 2015–2019	0.8314	1.2411	0.7598	0.4932	1.4252	2.0574	1.5059	0.7124
Post Period 2019	1.1076	1.8335	0.9577	0.5316	2.2255	3.3498	2.1191	1.2077
Original Categories No Switchers								
Post Period 2015–2019	0.8093	1.2698	0.7703	0.3878	1.3823	2.0911	1.5139	0.5420
Post Period 2019	1.0257	1.9056	0.9717	0.1999	2.1221	3.4198	2.1337	0.8128
New Categories								
Post Period 2019	1.1684	2.0541	1.1693	0.2818	2.3021	3.7441	2.2409	0.9212
Overall Averages	1.0093	1.6407	0.9208	0.4663	1.9235	2.9117	1.8982	0.9605
Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Elasticity	Wage	Wage	Wage	Wage	MW	MW	MW	MW
Policy Group	All Changers	Large	Small	Indexer	All Changers	Large	Small	Indexer
Original Categories								
Post Period 2015–2019	0.5858	0.9313	0.5180	0.3081	1.4176	2.0563	1.5146	0.6819
Post Period 2019	0.9013	1.5355	0.7775	0.3909	2.2316	3.3569	2.1423	1.1955
Original Categories No Switchers								
Post Period 2015–2019	0.5662	0.9541	0.5228	0.2216	1.3744	2.0881	1.5233	0.5117
Post Period 2019	0.8404	1.5894	0.7886	0.1431	2.1335	3.4297	2.1588	0.8119
New Categories								
Post Period 2019	0.9653	1.7630	0.8948	0.2383	2.3078	3.7504	2.2512	0.9218
Overall Averages	0.7879	1.3393	0.6971	0.3272	1.9212	2.9154	1.9130	0.9353

Note: This table presents averages across estimates of equation (1). They are thus estimates of the change in either the wage or in the applicable minimum wage 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 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 wages, all analyses use data from the Outgoing Rotation Groups of the Current Population Survey. The grouping of states we describe as "Original" corresponds with the grouping in Table 1, which is the grouping from our original pre-analysis plan. The grouping of states we describe as "Updated" corresponds with the grouping in Table 2, 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.) Panel A summarizes analyses of employed individuals ages 25 and younger with less than a completed high school education, and Panel B summarizes analyses of employed individuals ages 16 to 21.

Table 10. Summary of Wage Regression Elasticities (D-in-D Estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Skill Group	Low-Skill	Low-Skill	Low-Skill	Low-Skill	Young	Young	Young	Young
Policy Group	All Changers	Large	Small	Indexer	All Changers	Large	Small	Indexer
Panel A. Employment								
Overall Average Effects	–0.007	–0.034	0.012	0.002	–0.007	–0.019	–0.001	–0.002
Mean in 2011–2013 Baseline	0.211	0.188	0.246	0.222	0.365	0.330	0.415	0.384
Change from Baseline (%)	–3.111	–18.105	5.033	0.884	–2.043	–5.752	–0.250	–0.612
Panel B. Hourly Wages								
Overall Average Effects	1.009	1.641	0.921	0.466	0.788	1.339	0.697	0.327
Mean in 2011–2013 Baseline	8.511	9.192	8.448	8.549	8.794	9.535	8.963	8.978
Change from Baseline (%)	11.858	17.849	10.900	5.454	8.960	14.046	7.778	3.645
Panel C. Minimum Wages								
Overall Average Effects	1.923	2.912	1.898	0.961	1.921	2.915	1.913	0.935
Mean in 2011–2013 Baseline	7.690	7.721	7.407	7.804	7.686	7.713	7.411	7.810
Change from Baseline (%)	25.013	37.712	25.627	12.308	24.997	37.798	25.813	11.976
Panel D. Elasticities								
Own Wage	–0.262	–1.014	0.462	0.162	–0.228	–0.409	–0.032	–0.168
Minimum Wage	–0.124	–0.480	0.196	0.072	–0.082	–0.152	–0.010	–0.051

Note: This table reports average employment and wage effects for each minimum wage policy group and skill group. 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 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. Young adults are defined as individuals ages 16 to 21. Low-skilled individuals are those ages 16 to 25 without a completed high school education. Average effects for employment (Panel A), hourly wages (Panel B), and minimum wages are taken from Tables 8 and 9. The own-wage elasticity is the estimated percent change in employment divided by the percent change in average hourly wages, and the minimum wage elasticity is the estimated percent change in employment divided by the percent change in the minimum wage.

Appendix A: Additional Tables and Figures

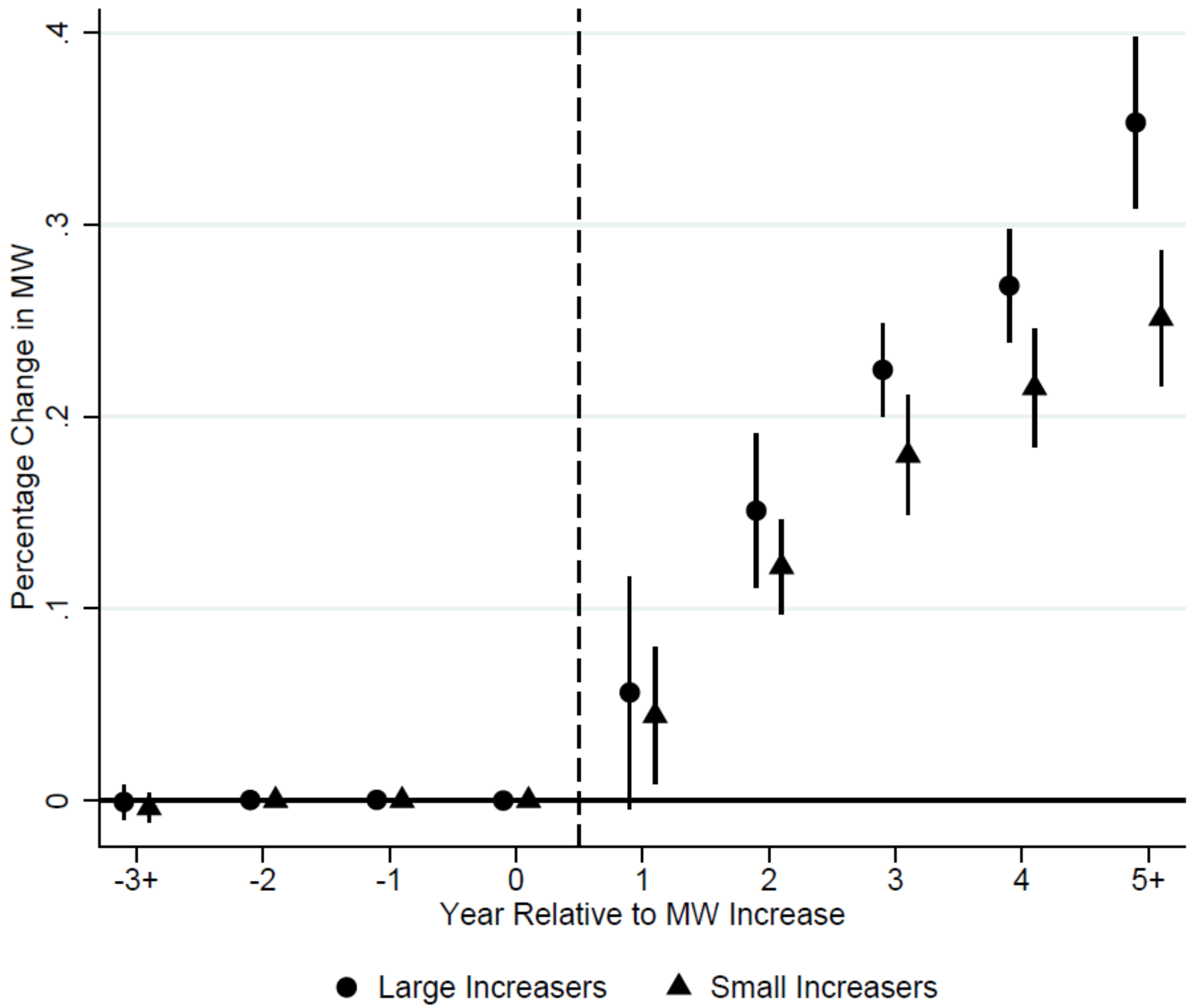

Figure A1. Changes in State Effective Minimum Wage Following Initial Statutory Minimum Wage Increases: This figure displays coefficients from the "stacked event study" estimator described by equation (5). The dependent variable is the log of the minimum wage. Event Time is defined such that year "1" 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 increases as defined in Table 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.

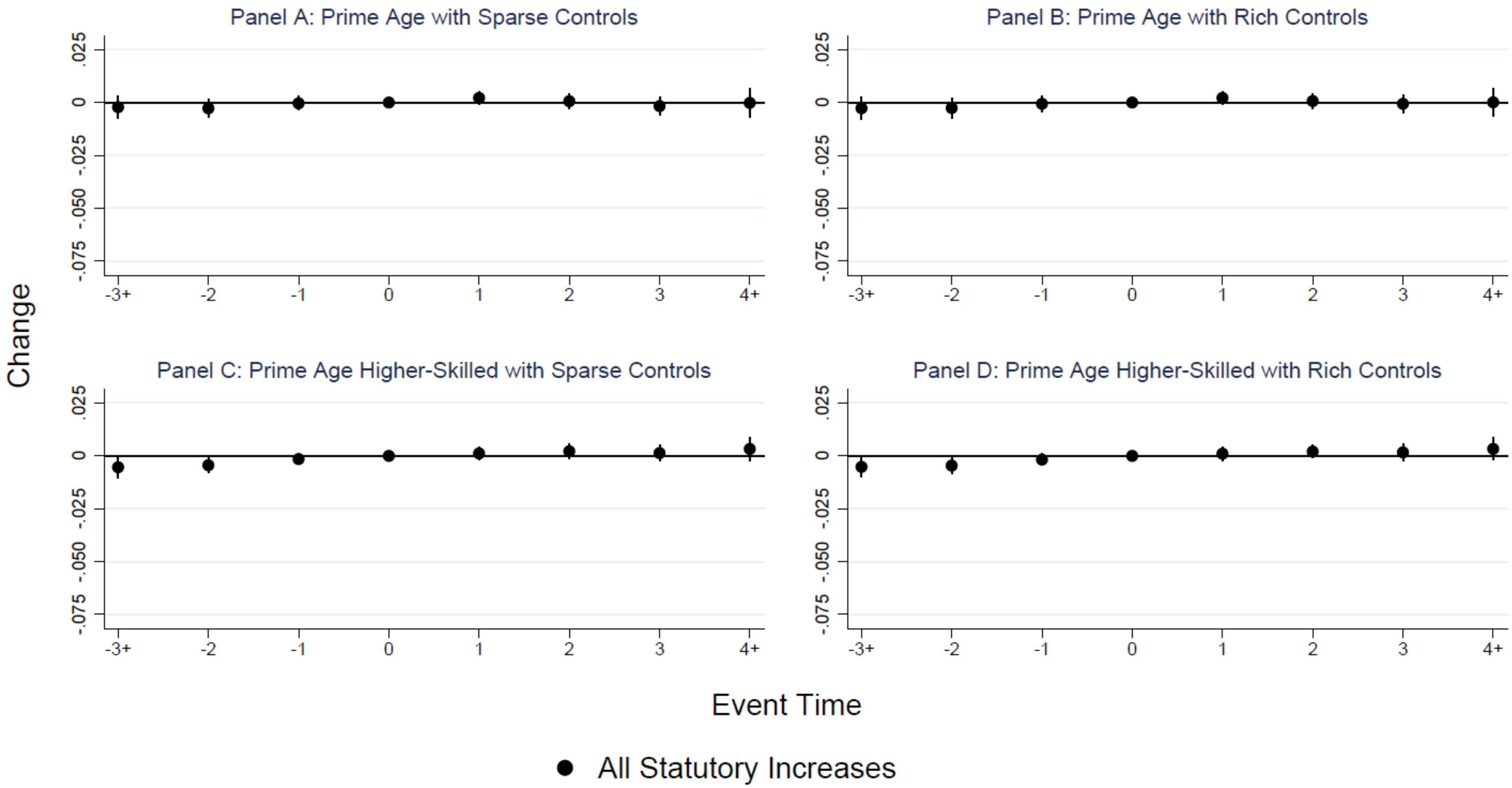

Figure A2. Stacked Event Studies of Changes in Prime-Age Employment Following Initial Statutory Minimum Wage Increases: This figure displays coefficients from the "stacked event study" estimator described by equation (5). Event Time is defined such that year "1" corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. Panels A and B plot coefficients for prime-age individuals defined as individuals ages 26–54. Panels C and D plot coefficients for prime-age, higher-skilled individuals defined as all individuals ages 26–54 who have completed at least one year of college. The samples are from the ACS. 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 base 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.

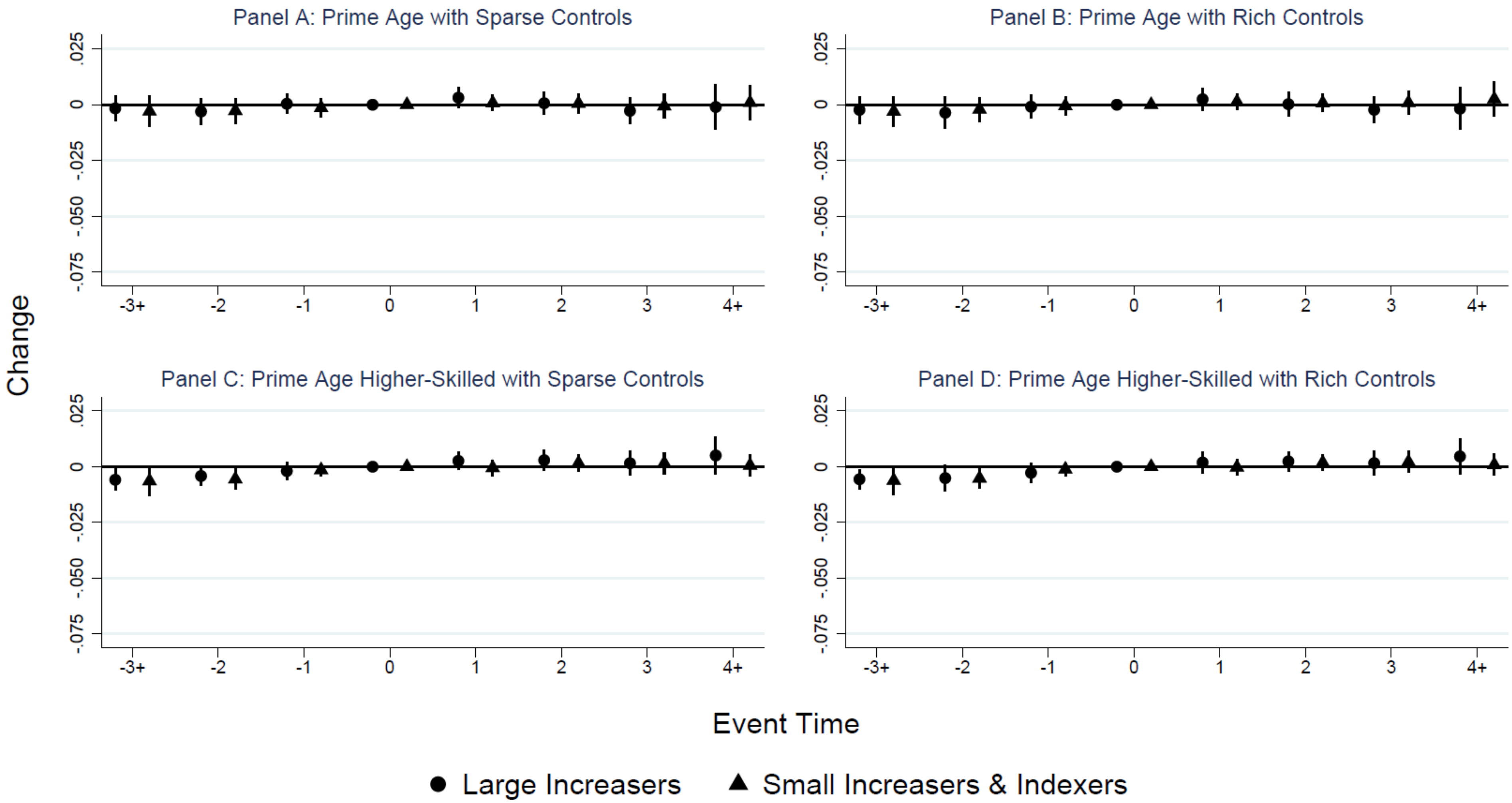

Figure A3. Stacked Event Studies of Changes in Prime-Age Employment Following Initial Statutory Minimum Wage Increases: This figure displays coefficients from the "stacked event study" estimator described by equation (5). Event Time is defined such that year "1" corresponds with the year during which a given state enacted its first minimum wage change due to legislation passed during our sample period. We compare estimates for large vs. small increases as defined in the main text. Panels A and B plot coefficients for prime-age individuals defined as individuals ages 26–54. Panels C and D plot coefficients for prime-age, higher-skilled individuals defined as all individuals ages 26–54 who have completed at least one year of college. The samples are from the ACS. 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 base 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.

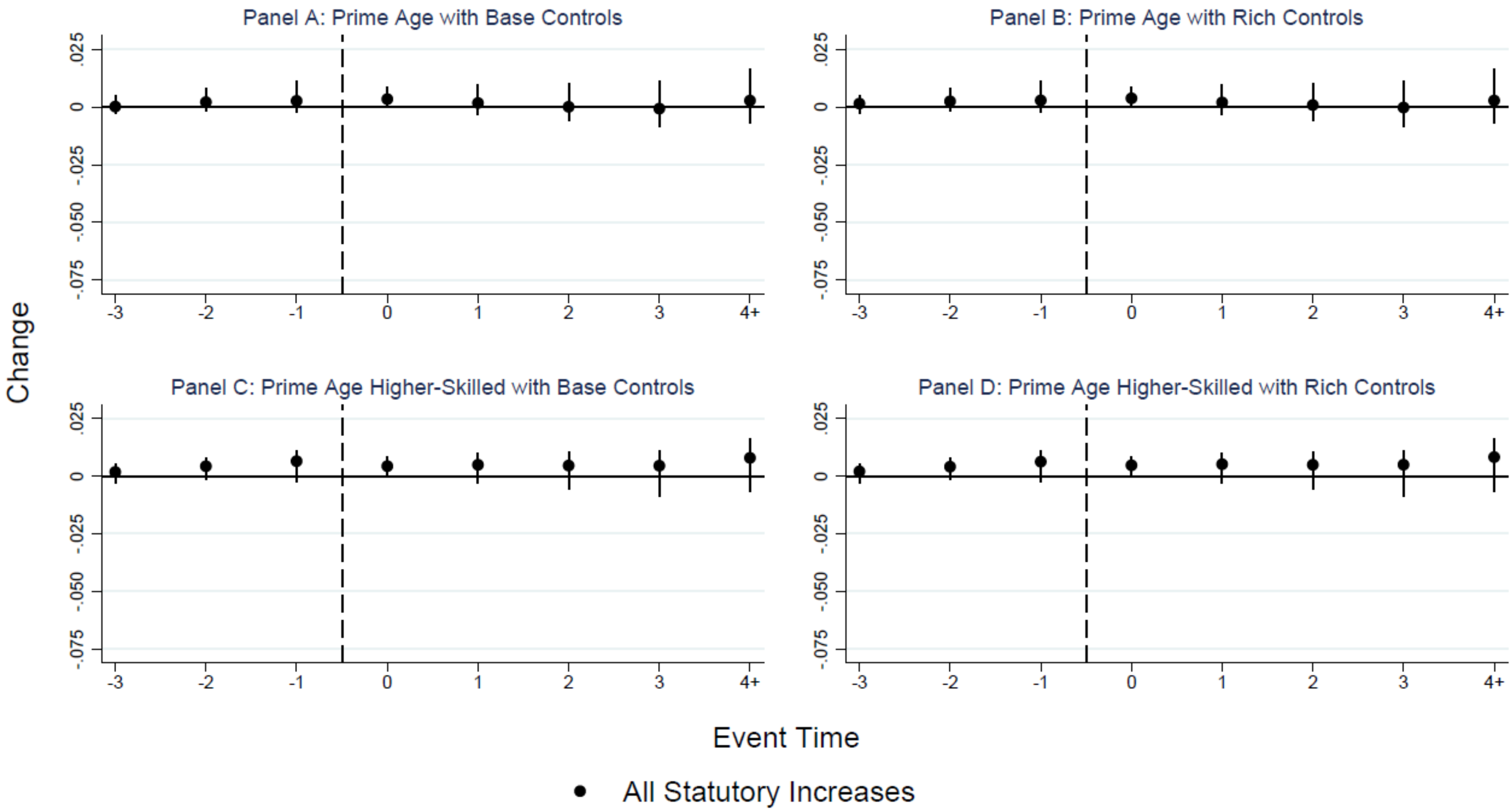

Figure A4. Event Studies of Changes in Prime-Age Employment Following Statutory Minimum Wage Increases Using the BJS Imputation Estimator: This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2021) (BJS). For the BJS estimator, we code first treatment year as the year in which a state's first statutory minimum wage increase took effect. Note that this appears graphically as "year 0" in the BJS figures, but corresponds with year 1 in the stacked event study figures. Panels A and B plot coefficients for prime-age individuals defined as individuals ages 26–54. Panels C and D plot coefficients for prime-age, higher-skilled individuals defined as all individuals ages 26–54 who have completed at least one year of college. The samples are from the ACS. 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 base 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.

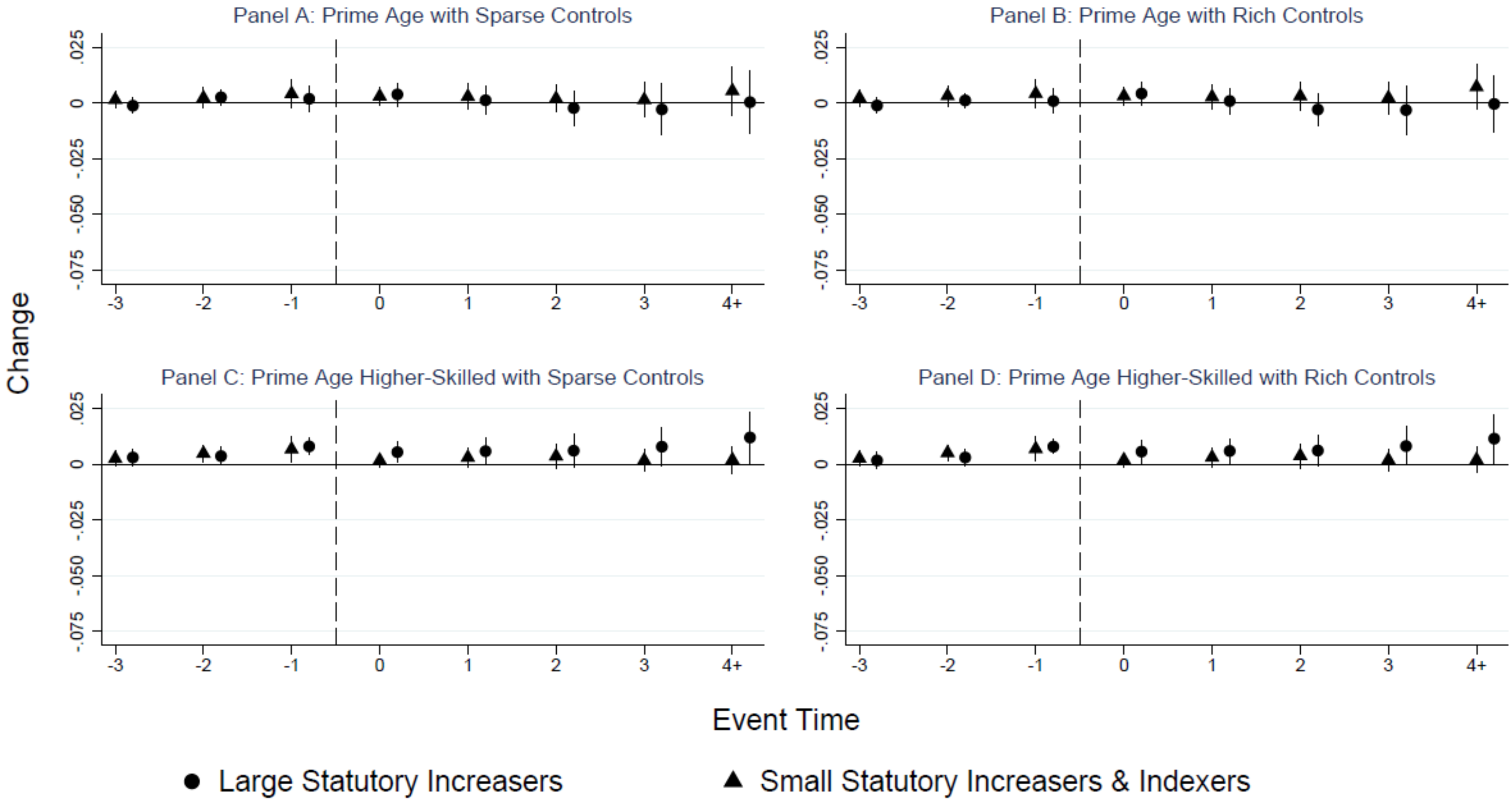

Figure A5. Event Studies of Changes in Prime-Age Employment Following Large and Small Statutory Minimum Wage Increases Using the BJS Imputation Estimator: This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2021) (BJS). For the BJS estimator, we code first treatment year as the year in which a state's first statutory minimum wage increase took effect. Note that this appears graphically as "year 0" in the BJS figures, but corresponds with year 1 in the stacked event study figures. We compare estimates for large vs. small increases as defined in the main text. Panels A and B plot coefficients for prime-age individuals defined as individuals ages 26–54. Panels C and D plot coefficients for prime-age, higher-skilled individuals defined as all individuals ages 26–54 who have completed at least one year of college. The samples are from the ACS. 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 base 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 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	0.273	0.374	0.442
	(0.417)	(0.445)	(0.484)	(0.497)
Age	17.90	17.53	18.58	18.54
	(2.444)	(2.155)	(1.704)	(1.696)
Black	0.166	0.148	0.153	0.146
	(0.372)	(0.355)	(0.360)	(0.353)
High School Degree	0	0	0.343	0.368
	(0)	(0)	(0.475)	(0.482)
Some College Education	0	0	0.247	0.240
	(0)	(0)	(0.431)	(0.427)
House Price Index	325.9	460.9	330.4	466.9
	(99.86)	(143.2)	(101.6)	(146.1)
Income *per capita* ($1,000s)	43.81	56.10	44.04	56.45
	(6.270)	(8.965)	(6.364)	(9.118)
Effective Minimum Wage	7.531	8.899	7.536	8.960
	(0.422)	(1.812)	(0.424)	(1.837)
Observations	346,135	98,302	774,438	243,315

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 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 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	0.266	0.360	0.410
	(0.424)	(0.442)	(0.480)	(0.492)
Age	17.97	17.62	18.50	18.47
	(2.423)	(2.118)	(1.730)	(1.729)
Black	0.164	0.153	0.155	0.149
	(0.370)	(0.360)	(0.362)	(0.356)
High School Degree	0	0	0.223	0.239
	(0)	(0)	(0.416)	(0.427)
Some College Education	0	0	0.299	0.291
	(0)	(0)	(0.458)	(0.454)
House Price Index	327.8	460.9	331.8	465.6
	(100.8)	(143.2)	(102.5)	(144.8)
Income *per capita* ($1,000s)	43.91	56.14	44.15	56.40
	(6.338)	(8.962)	(6.420)	(9.044)
Effective Minimum Wage	7.535	8.919	7.541	8.971
	(0.423)	(1.810)	(0.426)	(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 A3. Unadjusted Differences Across Policy Regimes Using CPS Data and $1 Cutoff

	(1) 2011–2013	(2) 2015–2019	(3) Change	(4) Change Relative to Non-Increasers
Young Adult Employment				
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
Low-Skilled Employment				
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
Prime-Age Employment				
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
Mid-Skill Employment				
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
House Price Index				
Non-Increasers	273.3	335.8	62.5	
Indexers	288.3	411.2	122.9	60.4
Increase < $1	301.3	361.2	59.9	–2.6
Increase >= $1	454.1	610.5	156.4	93.9
Income *per capita* ($1,000s)				
Non-Increasers	41.02	47.51	6.49	
Indexers	40.74	49.03	8.29	1.8
Increase < $1	44.68	52.76	8.08	1.59
Increase >= $1	50.48	62.54	12.06	5.57

Note: 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-skill individuals are those ages 22 to 30 with a high school degree or high school dropouts between the ages of 30 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. 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 main text. 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 A4. Unadjusted Differences Across Policy Regimes Using CPS Data and $1 Cutoff

	(1) 2011–2013	(2) 2019	(3) Change	(4) Change Relative to Non-Increasers
Young Adult Employment				
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
Low-Skilled Employment				
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
Prime-Age Employment				
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
Mid-Skill Employment				
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
House Price Index				
Non-Increasers	273.3	373.7	100.4	
Indexers	288.3	472.9	184.6	84.2
Increase < $1	301.3	391.9	90.6	–9.8
Increase >= $1	454.1	678.0	223.9	123.5
Income *per capita* ($1,000s)				
Non-Increasers	41.02	51.19	10.17	
Indexers	40.74	53.19	12.45	2.28
Increase < $1	44.68	56.74	12.06	1.89
Increase >= $1	50.48	68.34	17.86	7.69

Note: 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-skill individuals are those ages 22 to 30 with a high school degree or high school dropouts between the ages of 30 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. 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 main text. 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 CPS Data and $2.50 Cutoff

	(1)	(2)	(3)	(4)
	2011-2013	2019	Change	Change Relative to Non-increasers
Young Adult Employment				
Non-Increasers	0.376	0.422	0.046	
Indexers	0.379	0.421	0.042	-0.004
Increase < $2.50	0.375	0.427	0.052	0.006
Increase >= $2.50	0.314	0.368	0.054	0.008
Low-Skill Employment				
Non-Increasers	0.250	0.281	0.031	
Indexers	0.243	0.263	0.020	-0.011
Increase < $2.50	0.230	0.284	0.054	0.023
Increase >= $2.50	0.206	0.224	0.018	-0.013
Prime-Age Employment				
Non-Increasers	0.761	0.800	0.039	
Indexers	0.755	0.804	0.049	0.010
Increase < $2.50	0.766	0.819	0.053	0.014
Increase >= $2.50	0.747	0.793	0.046	0.007
Mid-Skill Employment				
Non-Increasers	0.591	0.655	0.064	
Indexers	0.584	0.649	0.065	0.000
Increase < $2.50	0.597	0.670	0.073	0.008
Increase >= $2.50	0.575	0.639	0.064	-0.001
House Price Index				
Non-Increasers	272.1	372.6	100.5	
Indexers	265.1	412.6	147.5	47.0
Increase < $2.50	348.0	485.9	137.9	37.4
Increase >= $2.50	431.2	658.0	226.8	126.3
Income Per Capita ($1000s)				
Non-Increasers	41.03	51.19	10.16	
Indexers	40.32	51.15	10.83	0.7
Increase < $2.5	46.07	59.58	13.51	3.4
Increase >= $2.5	48.68	65.96	17.28	7.1

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-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-skill individuals are those ages 22 to 30 years old with a high school degree, or high school dropouts between the ages of 30 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. 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 A6A. Relationship Between Minimum Wage Increases and Employment Using ACS Data and $1 Cutoff with 2015–2019 as the Post Period (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0335***	–0.0316***	–0.0207***	–0.0234***
	(0.0077)	(0.0057)	(0.0061)	(0.0058)
Treated x Small Statutory Increaser x Post	–0.0010	–0.0066	–0.0087	–0.0101
	(0.0103)	(0.0089)	(0.0068)	(0.0071)
Treated x Indexer x Post	0.0088	0.0044	–0.0004	–0.0001
	(0.0092)	(0.0085)	(0.0052)	(0.0051)
Age and Education Controls	No	Yes	No	Yes
Observations	9,981,410	9,981,410	11,126,306	11,126,306
Adjusted R-Squared	0.1169	0.1613	0.1023	0.1615

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the ACS. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6B. Relationship Between Minimum Wage Increases and Employment Using CPS Data and $1 Cutoff with 2015–2019 as the Post Period (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0340***	–0.0288***	–0.0135	–0.0154*
	(0.0110)	(0.0078)	(0.0091)	(0.0079)
Treated x Small Statutory Increaser x Post	0.0275**	0.0166*	0.0003	0.0042
	(0.0125)	(0.0093)	(0.0072)	(0.0073)
Treated x Indexer x Post	–0.0021	–0.0091	0.0008	0.0043
	(0.0087)	(0.0062)	(0.0072)	(0.0074)
Age and Education Controls	No	Yes	No	Yes
Observations	5,121,663	5,121,663	5,548,948	5,548,948
Adjusted R-Squared	0.1301	0.1667	0.1158	0.1662

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the CPS. The dependent variable is whether the respondent was employed in the previous week. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals ages 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7A. Relationship Between Minimum Wage Increases and Employment Using ACS Data and $1 Cutoff with 2019 as the Post Period (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0537***	–0.0483***	–0.0281***	–0.0286***
	(0.0090)	(0.0069)	(0.0051)	(0.0047)
Treated x Small Statutory Increaser x Post	–0.0120	–0.0197	–0.0178*	–0.0174*
	(0.0168)	(0.0139)	(0.0102)	(0.0098)
Treated x Indexer x Post	0.0054	–0.0008	–0.0054	–0.0074
	(0.0130)	(0.0122)	(0.0073)	(0.0073)
Age and Education Controls	No	Yes	No	Yes
Observations	5,024,609	5,024,609	5,597,925	5,597,925
Adjusted R-Squared	0.1169	0.1620	0.1037	0.1627

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the ACS. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals ages 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7B. Relationship Between Minimum Wage Increases and Employment Using CPS Data and $1 Cutoff with 2019 as the Post Period (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0415**	–0.0370***	–0.0218*	–0.0200*
	(0.0175)	(0.0137)	(0.0121)	(0.0107)
Treated x Small Statutory Increaser x Post	0.0465*	0.0310*	0.0112	0.0117
	(0.0237)	(0.0184)	(0.0079)	(0.0091)
Treated x Indexer x Post	–0.0070	–0.0133	0.0010	–0.0039
	(0.0105)	(0.0095)	(0.0116)	(0.0126)
Age and Education Controls	No	Yes	No	Yes
Observations	2,612,524	2,612,524	2,830,119	2,830,119
Adjusted R-Squared	0.1298	0.1675	0.1164	0.1674

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the CPS. The dependent variable is whether or not the respondent was employed in the previous week. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals ages 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8A. Relationship Between Minimum Wage Increases and Employment Using ACS Data and $1 Cutoff with 2015–2019 as the Post Period (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0240***	–0.0404***	–0.0205*	–0.0218***	–0.0224***	–0.0275***
	(0.0080)	(0.0070)	(0.0105)	(0.0069)	(0.0070)	(0.0077)
Small Statutory Increaser x Post	0.0023	–0.0001	0.0023	0.0019	–0.0018	–0.0054
	(0.0155)	(0.0124)	(0.0157)	(0.0126)	(0.0140)	(0.0095)
Indexer x Post	0.0183*	0.0108	0.0206*	0.0133	0.0141	0.0096
	(0.0096)	(0.0092)	(0.0103)	(0.0092)	(0.0087)	(0.0065)
Ln(Income *per capita*)		0.2712**				0.3249***
		(0.1016)				(0.0800)
Housing Price Index Divided by 1,000			–0.0423			–0.1523***
			(0.0573)			(0.0500)
State Mid-Skill Emp-to-Pop Ratio				0.5020***		0.3852***
				(0.0902)		(0.0838)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	865,509	865,509	865,509	865,509	865,509	865,509
Adjusted R-Squared	0.0175	0.0176	0.0175	0.0176	0.0969	0.0972

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	–0.0114	–0.0318***	–0.0217**	–0.0098	–0.0135	–0.0302***
	(0.0090)	(0.0051)	(0.0085)	(0.0086)	(0.0088)	(0.0057)
Statutory Increaser Small x Post	–0.0057	–0.0087	–0.0056	–0.0060	–0.0062	–0.0091
	(0.0125)	(0.0086)	(0.0118)	(0.0103)	(0.0123)	(0.0075)
Indexer x Post	0.0088	–0.0004	0.0023	0.0054	0.0091	–0.0010
	(0.0062)	(0.0050)	(0.0082)	(0.0056)	(0.0062)	(0.0051)
Ln(Income *per capita*)		0.3396***				0.3115***
		(0.0490)				(0.0661)
Housing Price Index Divided by 1,000			0.1203***			–0.0112
			(0.0361)			(0.0443)
State Mid-Skill Emp-to-Pop Ratio				0.3636***		0.2286***
				(0.0850)		(0.0592)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	2,010,405	2,010,405	2,010,405	2,010,405	2,010,405	2,010,405
Adjusted R-Squared	0.0154	0.0156	0.0155	0.0155	0.1481	0.1483

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the ACS. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A8B. Relationship Between Minimum Wage Increases and Employment Using CPS Data and $1 Cutoff with 2015–2019 as the Post Period (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0261**	–0.0419***	–0.0249**	–0.0263***	–0.0217***	–0.0316***
	(0.0101)	(0.0103)	(0.0110)	(0.0094)	(0.0075)	(0.0084)
Small Statutory Increaser x Post	0.0326**	0.0303**	0.0326**	0.0321**	0.0235**	0.0201**
	(0.0155)	(0.0127)	(0.0155)	(0.0147)	(0.0109)	(0.0086)
Indexer x Post	0.0056	–0.0017	0.0064	0.0043	–0.0001	–0.0044
	(0.0083)	(0.0071)	(0.0095)	(0.0080)	(0.0068)	(0.0064)
Ln(Income *per capita*)		0.2631**				0.3025***
		(0.1185)				(0.0903)
Housing Price Index Divided by 1,000			-0.0145			–0.1000
			(0.0678)			(0.0621)
State Mid-Skill Emp-to-Pop Ratio				0.1122***		0.1081***
				(0.0279)		(0.0258)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	484,483	484,483	484,483	484,483	484,483	484,483
Adjusted R-Squared	0.0226	0.0227	0.0226	0.0226	0.1077	0.1078

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	–0.0058	–0.0239**	–0.0176**	–0.0059	–0.0082	–0.0255***
	(0.0105)	(0.0091)	(0.0087)	(0.0105)	(0.0097)	(0.0082)
Statutory Increaser Small x Post	0.0053	0.0026	0.0054	0.0049	0.0105	0.0077
	(0.0102)	(0.0075)	(0.0093)	(0.0095)	(0.0094)	(0.0062)
Indexer x Post	0.0086	0.0003	0.0010	0.0075	0.0118	0.0026
	(0.0074)	(0.0070)	(0.0078)	(0.0070)	(0.0080)	(0.0073)
Ln(Income *per capita*)		0.3030***				0.2669***
		(0.0611)				(0.0741)
Housing Price Index Divided by 1,000			0.1400***			0.0149
			(0.0444)			(0.0580)
State Mid-Skill Emp-to-Pop Ratio				0.0942***		0.0890***
				(0.0277)		(0.0234)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	911,768	911,768	911,768	911,768	911,768	911,768
Adjusted R-Squared	0.0216	0.0217	0.0216	0.0216	0.1504	0.1505

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the CPS. The dependent variable is whether or not the respondent was employed in the previous week. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A9A. Relationship Between Minimum Wage Increases and Employment Using ACS Data and $1 Cutoff with 2019 as the Post Period (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0399***	–0.0684***	–0.0416***	–0.0368***	–0.0353***	–0.0457***
	(0.0095)	(0.0105)	(0.0135)	(0.0077)	(0.0084)	(0.0089)
Small Statutory Increaser x Post	–0.0086	–0.0117	–0.0085	–0.0052	–0.0137	–0.0150
	(0.0230)	(0.0184)	(0.0229)	(0.0166)	(0.0199)	(0.0124)
Indexer x Post	0.0169	0.0044	0.0157	0.0051	0.0113	0.0004
	(0.0128)	(0.0130)	(0.0161)	(0.0105)	(0.0118)	(0.0090)
Ln(Income *per capita*)		0.3777***				0.3768***
		(0.1109)				(0.0995)
Housing Price Index Divided by 1,000			0.0148			–0.1389**
			(0.0701)			(0.0616)
State Mid-Skill Emp-to-Pop Ratio				0.6670***		0.5147***
				(0.1338)		(0.1043)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	444,437	444,437	444,437	444,437	444,437	444,437
Adjusted R-Squared	0.0161	0.0159	0.0161	0.1000	0.1003	0.1010

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	–0.0146*	–0.0398***	–0.0280**	–0.0124	–0.0148*	–0.0318***
	(0.0081)	(0.0057)	(0.0106)	(0.0079)	(0.0078)	(0.0067)
Statutory Increaser Small x Post	–0.0149	–0.0173	–0.0142	–0.0122	–0.0133	–0.0133
	(0.0161)	(0.0121)	(0.0151)	(0.0114)	(0.0154)	(0.0090)
Indexer x Post	0.0057	–0.0051	–0.0032	–0.0024	0.0043	–0.0095
	(0.0080)	(0.0077)	(0.0121)	(0.0060)	(0.0078)	(0.0068)
Ln(Income *per capita*)		0.3368***				0.2588***
		(0.0573)				(0.0652)
Housing Price Index Divided by 1,000			0.1172**			–0.0064
			(0.0507)			(0.0454)
State Mid-Skill Emp-to-Pop Ratio				0.4808***		0.3556***
				(0.1240)		(0.0915)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	1,017,753	1,017,753	1,017,753	1,017,753	1,017,753	1,017,753
Adjusted R-Squared	0.0156	0.0157	0.0157	0.0157	0.1467	0.1468

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the ACS. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A9B. Relationship Between Minimum Wage Increases and Employment Using CPS Data and $1 Cutoff with 2019 as the Post Period (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0312**	–0.0512***	–0.0329*	–0.0306**	–0.0278**	–0.0424***
	(0.0154)	(0.0150)	(0.0179)	(0.0152)	(0.0119)	(0.0134)
Small Statutory Increaser x Post	0.0533*	0.0510**	0.0533*	0.0537*	0.0394*	0.0372*
	(0.0282)	(0.0248)	(0.0281)	(0.0281)	(0.0219)	(0.0191)
Indexer x Post	0.0046	–0.0044	0.0034	0.0040	–0.0011	–0.0071
	(0.0121)	(0.0106)	(0.0145)	(0.0115)	(0.0116)	(0.0106)
Ln(Income *per capita*)		0.2679**				0.2802**
		(0.1274)				(0.1356)
Housing Price Index Divided by 1,000			0.0149			–0.0509
			(0.0860)			(0.0915)
State Mid-Skill Emp-to-Pop Ratio				0.0633		0.0673
				(0.0448)		(0.0416)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	248,795	248,795	248,795	248,795	248,795	248,795
Adjusted R-Squared	0.021	0.021	0.021	0.021	0.112	0.113

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	–0.0119	–0.0329**	–0.0316**	–0.0111	–0.0106	–0.0325**
	(0.0119)	(0.0136)	(0.0125)	(0.0119)	(0.0114)	(0.0126)
Statutory Increaser Small x Post	0.0173	0.0152*	0.0182*	0.0178*	0.0192	0.0183*
	(0.0107)	(0.0086)	(0.0101)	(0.0104)	(0.0125)	(0.0098)
Indexer x Post	0.0124	0.0032	–0.0010	0.0115	0.0075	–0.0051
	(0.0128)	(0.0129)	(0.0145)	(0.0122)	(0.0139)	(0.0139)
Ln(Income *per capita*)		0.2807***				0.2100**
		(0.0927)				(0.1026)
Housing Price Index Divided by 1,000			0.1743***			0.0621
			(0.0650)			(0.0769)
State Mid-Skill Emp-to-Pop Ratio				0.0933**		0.0983**
				(0.0410)		(0.0415)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	466,390	466,390	466,390	466,390	466,390	466,390
Adjusted R-Squared	0.0218	0.0218	0.0218	0.0218	0.1512	0.1513

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the CPS. The dependent variable is whether or not the respondent was employed in the previous week. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10A. Relationship Between Minimum Wage Increases and Employment Among Low-Skilled Groups Using ACS Data and $2.50 Cutoff with 2019 as the Post Period (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0495***	–0.0452***	–0.0244***	–0.0256***
	(0.0109)	(0.0081)	(0.0051)	(0.0045)
Treated x Small Statutory Increaser x Post	–0.0078	–0.0143	–0.0149*	–0.0145*
	(0.0137)	(0.0120)	(0.0082)	(0.0083)
Treated x Indexer x Post	0.0046	–0.0013	–0.0086	–0.0110
	(0.0174)	(0.0163)	(0.0095)	(0.0090)
Age and Education Controls	No	Yes	No	Yes
Observations	5,024,609	5,024,609	5,597,925	5,597,925
Adjusted R-Squared	0.1169	0.1620	0.1037	0.1627

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $2.50 and states that increased their minimum wage by $2.50 or more between January 1, 2013 and January 1, 2018. The sample is from the ACS. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals ages 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A10B. Relationship Between Minimum Wage Increases and Employment Using CPS Data and $2.50 Cutoff with 2019 as the Post Period (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0303	–0.0239	–0.0056	–0.0048
	(0.0204)	(0.0163)	(0.0088)	(0.0086)
Treated x Small Statutory Increaser x Post	0.0152	0.0030	–0.0057	–0.0038
	(0.0229)	(0.0195)	(0.0154)	(0.0145)
Treated x Indexer x Post	–0.0162	–0.0245***	–0.0098	–0.0191
	(0.0103)	(0.0081)	(0.0134)	(0.0119)
Age and Education Controls	No	Yes	No	Yes
Observations	2,612,524	2,612,524	2,830,119	2,830,119
Adjusted R-Squared	0.1297	0.1675	0.1164	0.1674

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $2.50 and states that increased their minimum wage by $2.50 or more between January 1, 2013 and January 1, 2018. The sample is from the CPS. The dependent variable is whether or not the respondent was employed in the previous week. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals ages 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A11A. Relationship Between Minimum Wage Increases and Employment Using ACS Data and $2.50 Cutoff and 2019 as the Post Period (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0349***	–0.0676***	–0.0435***	–0.0327***	–0.0308***	–0.0441***
	(0.0110)	(0.0139)	(0.0160)	(0.0083)	(0.0093)	(0.0110)
Small Statutory Increaser x Post	–0.0024	–0.0149	–0.0049	–0.0043	–0.0081	–0.0164
	(0.0162)	(0.0130)	(0.0167)	(0.0123)	(0.0145)	(0.0107)
Indexer x Post	0.0174	0.0126	0.0145	0.0049	0.0124	0.0030
	(0.0161)	(0.0167)	(0.0193)	(0.0136)	(0.0148)	(0.0108)
Ln(Income *per capita*)		0.4152***				0.3509***
		(0.1131)				(0.1038)
Housing Price Index Divided by 1,000			0.0680			–0.1005
			(0.0800)			(0.0691)
State Mid-Skill Emp-to-Pop Ratio				0.7134***		0.5480***
				(0.1205)		(0.1093)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	444,437	444,437	444,437	444,437	444,437	444,437
Adjusted R-Squared	0.0158	0.016	0.0158	0.0161	0.0999	0.1003

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	–0.0100	–0.0373***	–0.0270**	–0.0084	–0.0107	–0.0286***
	(0.0074)	(0.0071)	(0.0110)	(0.0068)	(0.0070)	(0.0077)
Statutory Increaser Small x Post	–0.0100	–0.0199**	–0.0147	–0.0110	–0.0092	–0.0168**
	(0.0112)	(0.0083)	(0.0112)	(0.0079)	(0.0110)	(0.0073)
Indexer x Post	0.0039	0.0001	–0.0016	–0.0048	0.0021	–0.0069
	(0.0098)	(0.0095)	(0.0135)	(0.0070)	(0.0092)	(0.0076)
Ln(Income *per capita*)		0.3490***				0.2422***
		(0.0580)				(0.0672)
Housing Price Index Divided by 1,000			0.1328***			0.0013
			(0.0485)			(0.0462)
State Mid-Skill Emp-to-Pop Ratio				0.5130***		0.3727***
				(0.1216)		(0.1009)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	1,017,753	1,017,753	1,017,753	1,017,753	1,017,753	1,017,753
Adjusted R-Squared	0.0156	0.0157	0.0156	0.0157	0.1466	0.1468

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $2.50 and states that increased their minimum wage by $2.50 or more between January 1, 2013 and January 1, 2018. The sample is from the ACS. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A11B. Relationship Between Minimum Wage Increases and Employment Using CPS Data and $2.50 Cutoff with 2019 as the Post Period (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0193	–0.0277	–0.0082	–0.0195	–0.0139	–0.0127
	(0.0191)	(0.0179)	(0.0180)	(0.0183)	(0.0153)	(0.0142)
Small Statutory Increaser x Post	0.0249	0.0216	0.0282	0.0248	0.0133	0.0126
	(0.0239)	(0.0231)	(0.0251)	(0.0237)	(0.0205)	(0.0202)
Indexer x Post	–0.0064	–0.0076	–0.0027	–0.0056	–0.0138	–0.0100
	(0.0108)	(0.0102)	(0.0128)	(0.0107)	(0.0096)	(0.0103)
Ln(Income *per capita*)		0.1076				0.1678
		(0.1419)				(0.1500)
Housing Price Index Divided by 1,000			–0.0874			–0.1138
			(0.0878)			(0.1035)
State Mid-Skill Emp-to-Pop Ratio				0.0694		0.0796*
				(0.0441)		(0.0419)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	248,795	248,795	248,795	248,795	248,795	248,795
Adjusted R-Squared	0.0204	0.0204	0.0204	0.0204	0.1109	0.1109

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	0.0050	–0.0054	–0.0059	0.0047	0.0051	–0.0049
	(0.0104)	(0.0163)	(0.0151)	(0.0095)	(0.0103)	(0.0151)
Statutory Increaser Small x Post	0.0036	–0.0003	0.0005	0.0035	0.0059	0.0023
	(0.0142)	(0.0143)	(0.0147)	(0.0138)	(0.0145)	(0.0142)
Indexer x Post	–0.0001	–0.0015	–0.0036	0.0010	–0.0088	–0.0093
	(0.0127)	(0.0128)	(0.0138)	(0.0134)	(0.0119)	(0.0136)
Ln(Income *per capita*)		0.1331				0.1055
		(0.1051)				(0.1234)
Housing Price Index Divided by 1,000			0.0856			0.0114
			(0.0666)			(0.0828)
State Mid-Skill Emp-to-Pop Ratio				0.0983**		0.1090**
				(0.0418)		(0.0420)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	466,390	466,390	466,390	466,390	466,390	466,390
Adjusted R-Squared	0.0217	0.0217	0.0217	0.0217	0.1511	0.1512

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $2.50 and states that increased their minimum wage by $2.50 or more between January 1, 2013 and January 1, 2018. The sample is from the CPS. The dependent variable is whether or not the respondent was employed in the previous week. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A12A. Relationship Between Minimum Wage Increases and Employment Using ACS Data, $1 Cutoff, 2015–2019 as the Post Period, and Excluding States That Change Policy Groups (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0332***	–0.0314***	–0.0207***	–0.0234***
	(0.0077)	(0.0057)	(0.0061)	(0.0058)
Treated x Small Statutory Increaser x Post	–0.0007	–0.0064	–0.0088	–0.0101
	(0.0103)	(0.0089)	(0.0068)	(0.0071)
Treated x Indexer x Post	0.0060	0.0012	–0.0030	–0.0020
	(0.0132)	(0.0122)	(0.0062)	(0.0061)
Age and Education Controls	No	Yes	No	Yes
Observations	8,985,663	8,985,663	10,022,319	10,022,319
Adjusted R-Squared	0.1180	0.1626	0.1041	0.1632

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. Data come from the ACS. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals ages 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A12B. Relationship Between Minimum Wage Increases and Employment Using CPS Data, $1 Cutoff, 2015–2019 as the Post Period, and Excluding States That Change Policy Groups (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0341***	–0.0290***	–0.0137	–0.0155*
	(0.0111)	(0.0078)	(0.0091)	(0.0079)
Treated x Small Statutory Increaser x Post	0.0275**	0.0165*	0.0001	0.0041
	(0.0125)	(0.0094)	(0.0072)	(0.0073)
Treated x Indexer x Post	–0.0071	–0.0140**	–0.0115*	–0.0105*
	(0.0105)	(0.0064)	(0.0068)	(0.0054)
Age and Education Controls	No	Yes	No	Yes
Observations	4,582,215	4,582,215	4,967,062	4,967,062
Adjusted R-Squared	0.1304	0.1673	0.117	0.1672

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the CPS. The dependent variable is whether or not the respondent was employed in the previous week. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals ages 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A13A. Relationship Between Minimum Wage Increases and Employment Using ACS Data, $1 Cutoff, 2015–2019 as the Post Period, and Excluding States That Change Policy Groups (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0237***	–0.0418***	–0.0178	–0.0212***	–0.0222***	–0.0272***
	(0.0081)	(0.0072)	(0.0113)	(0.0068)	(0.0070)	(0.0082)
Small Statutory Increaser x Post	0.0027	–0.0001	0.0026	0.0022	–0.0016	–0.0053
	(0.0155)	(0.0121)	(0.0159)	(0.0124)	(0.0140)	(0.0095)
Indexer x Post	0.0167	0.0125	0.0194	0.0100	0.0121	0.0090
	(0.0131)	(0.0135)	(0.0122)	(0.0135)	(0.0118)	(0.0087)
Ln(Income *per capita*)		0.3004**				0.3387***
		(0.1170)				(0.0866)
Housing Price Index Divided by 1,000			–0.0693			–0.1618***
			(0.0636)			(0.0521)
State Mid-Skill Emp-to-Pop Ratio				0.5495***		0.3998***
				(0.0962)		(0.0884)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	707,035	707,035	707,035	707,035	707,035	707,035
Adjusted R-Squared	0.0180	0.0181	0.0180	0.0182	0.0979	0.0982

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	–0.0113	–0.0329***	–0.0215**	–0.0095	–0.0134	–0.0309***
	(0.0090)	(0.0053)	(0.0090)	(0.0086)	(0.0088)	(0.0059)
Statutory Increaser Small x Post	–0.0056	–0.0088	–0.0055	–0.0059	–0.0061	–0.0091
	(0.0125)	(0.0084)	(0.0118)	(0.0101)	(0.0124)	(0.0073)
Indexer x Post	0.0075	0.0027	0.0028	0.0027	0.0083	0.0012
	(0.0068)	(0.0068)	(0.0104)	(0.0066)	(0.0066)	(0.0068)
Ln(Income *per capita*)		0.3585***				0.3204***
		(0.0553)				(0.0703)
Housing Price Index Divided by 1,000			0.1180***			–0.0067
			(0.0426)			(0.0460)
State Mid-Skill Emp-to-Pop Ratio				0.4000***		0.2506***
				(0.0951)		(0.0660)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	1,816,771	1,816,771	1,816,771	1,816,771	1,816,771	1,816,771
Adjusted R-Squared	0.0157	0.0158	0.0157	0.0158	0.1473	0.1475

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the ACS. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A13B. Relationship Between Minimum Wage Increases and Employment Using CPS Data, $1 Cutoff, 2015–2019 as the Post Period, and Excluding States That Change Policy Groups (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0261**	–0.0425***	–0.0220*	–0.0263***	–0.0218***	–0.0315***
	(0.0101)	(0.0106)	(0.0114)	(0.0094)	(0.0076)	(0.0089)
Small Statutory Increaser x Post	0.0327**	0.0302**	0.0327**	0.0321**	0.0234**	0.0199**
	(0.0155)	(0.0126)	(0.0157)	(0.0146)	(0.0110)	(0.0086)
Indexer x Post	–0.0007	–0.0044	0.0010	–0.0010	–0.0056	–0.0060
	(0.0091)	(0.0076)	(0.0106)	(0.0086)	(0.0071)	(0.0078)
Ln(Income *per capita*)		0.2731*				0.3187***
		(0.1361)				(0.0960)
Housing Price Index Divided by 1,000			–0.0474			–0.1147
			(0.0752)			(0.0688)
State Mid-Skill Emp-to-Pop Ratio				0.1233***		0.1099***
				(0.0295)		(0.0282)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	434,719	434,719	434,719	434,719	434,719	434,719
Adjusted R-Squared	0.0239	0.0240	0.0239	0.0240	0.1096	0.1098

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	–0.0059	–0.0250**	–0.0182*	–0.0060	–0.0082	–0.0275***
	(0.0105)	(0.0094)	(0.0092)	(0.0105)	(0.0098)	(0.0083)
Statutory Increaser Small x Post	0.0053	0.0024	0.0053	0.0048	0.0104	0.0075
	(0.0102)	(0.0074)	(0.0092)	(0.0096)	(0.0094)	(0.0061)
Indexer x Post	–0.0050	–0.0093**	–0.0105**	–0.0053	–0.0031	–0.0081*
	(0.0049)	(0.0044)	(0.0050)	(0.0047)	(0.0060)	(0.0041)
Ln(Income *per capita*)		0.3210***				0.2785***
		(0.0674)				(0.0808)
Housing Price Index Divided by 1,000			0.1457***			0.0290
			(0.0491)			(0.0561)
State Mid-Skill Emp-to-Pop Ratio				0.0944***		0.0828***
				(0.0264)		(0.0212)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	819,566	819,566	819,566	819,566	819,566	819,566
Adjusted R-Squared	0.0223	0.0224	0.0223	0.0223	0.1497	0.1498

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the CPS. The dependent variable is whether or not the respondent was employed in the previous week. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A14A. Relationship Between Minimum Wage Increases and Employment Using ACS Data, $1 Cutoff, 2019 as the Post Period, and Excluding States That Change Policy Groups (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0533***	–0.0481***	–0.0280***	–0.0286***
	(0.0090)	(0.0069)	(0.0051)	(0.0048)
Treated x Small Statutory Increaser x Post	–0.0116	–0.0195	–0.0177*	–0.0173*
	(0.0169)	(0.0139)	(0.0102)	(0.0098)
Treated x Indexer x Post	0.0046	–0.0018	–0.0048	–0.0075
	(0.0201)	(0.0189)	(0.0105)	(0.0101)
Age and Education Controls	No	Yes	No	Yes
Observations	4,526,063	4,526,063	5,045,483	5,045,483
Adjusted R-Squared	0.1177	0.1631	0.1053	0.1642

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. Data come from the ACS. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals ages 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A14B. Relationship Between Minimum Wage Increases and Employment Using CPS Data and $1 Cutoff with 2019 as the Post Period and Excluding States That Change Policy Groups (D-in-D-in-D Estimates)

	(1)	(2)	(3)	(4)
	Ages 16 to 25 w/ Less Than High School		Ages 16 to 21	
Treated x Large Statutory Increaser x Post	–0.0413**	–0.0369**	–0.0216*	–0.0198*
	(0.0176)	(0.0137)	(0.0121)	(0.0107)
Treated x Small Statutory Increaser x Post	0.0467*	0.0313*	0.0115	0.0120
	(0.0237)	(0.0185)	(0.0080)	(0.0091)
Treated x Indexer x Post	–0.0154	–0.0220***	–0.0185	–0.0279***
	(0.0109)	(0.0080)	(0.0117)	(0.0081)
Age and Education Controls	No	Yes	No	Yes
Observations	2,319,562	2,319,562	2,513,271	2,513,271
Adjusted R-Squared	0.1303	0.1682	0.1174	0.1683

Note: This table reports triple-difference estimates of equation (2), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the CPS. The dependent variable is whether or not the respondent was employed in the previous week. The treated group consists of individuals ages 25 and younger without a completed high school education in Columns 1 and 2 and individuals ages 16 to 21 in Columns 3 and 4. The control group consists of prime-age individuals ages 26 to 54. Variable definitions and sources are discussed in the main text. Age and education controls consist of a dummy variable for each education group and age (included in Columns 2 and 4 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A15A. Relationship Between Minimum Wage Increases and Employment Using ACS Data, $1 Cutoff, 2019 as the Post Period, and Excluding States That Change Policy Groups (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0394***	–0.0717***	–0.0391***	–0.0361***	–0.0350***	–0.0478***
	(0.0095)	(0.0108)	(0.0142)	(0.0076)	(0.0085)	(0.0090)
Small Statutory Increaser x Post	–0.0082	–0.0116	–0.0082	–0.0045	–0.0135	–0.0147
	(0.0230)	(0.0179)	(0.0231)	(0.0162)	(0.0199)	(0.0120)
Indexer x Post	0.0193	0.0135	0.0195	0.0029	0.0140	0.0034
	(0.0183)	(0.0195)	(0.0199)	(0.0155)	(0.0168)	(0.0121)
Ln(Income *per capita*)		0.4261***				0.3921***
		(0.1253)				(0.1073)
Housing Price Index Divided by 1,000			–0.0028			–0.1280**
			(0.0768)			(0.0626)
State Mid-Skill Emp-to-Pop Ratio				0.7317***		0.5422***
				(0.1421)		(0.1092)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	400,919	400,919	400,919	400,919	400,919	400,919
Adjusted R-Squared	0.0163	0.0165	0.0163	0.0166	0.1011	0.1015

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	–0.0144*	–0.0422***	–0.0267**	–0.0122	–0.0146*	–0.0334***
	(0.0081)	(0.0058)	(0.0112)	(0.0079)	(0.0078)	(0.0066)
Statutory Increaser Small x Post	–0.0147	–0.0174	–0.0141	–0.0121	–0.0132	–0.0136
	(0.0161)	(0.0117)	(0.0152)	(0.0115)	(0.0155)	(0.0089)
Indexer x Post	0.0096	0.0048	0.0039	–0.0009	0.0074	–0.0030
	(0.0092)	(0.0099)	(0.0141)	(0.0075)	(0.0085)	(0.0080)
Ln(Income *per capita*)		0.3715***				0.2922***
		(0.0622)				(0.0696)
Housing Price Index Divided by 1,000			0.1071*			–0.0147
			(0.0535)			(0.0446)
State Mid-Skill Emp-to-Pop Ratio				0.4734***		0.3369***
				(0.1378)		(0.0958)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	920,339	920,339	920,339	920,339	920,339	920,339
Adjusted R-Squared	0.016	0.0161	0.016	0.0161	0.1458	0.1459

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the ACS. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A15B. Relationship Between Minimum Wage Increases and Employment Using CPS Data, $1 Cutoff, 2019 as the Post Period, and Excluding States That Change Policy Groups (D-in-D Estimates)

Panel A. Low-Skilled Workers	(1)	(2)	(3)	(4)	(5)	(6)
Large Statutory Increaser x Post	–0.0309*	–0.0522***	–0.0289	–0.0303*	–0.0276**	–0.0421***
	(0.0155)	(0.0149)	(0.0186)	(0.0152)	(0.0120)	(0.0138)
Small Statutory Increaser x Post	0.0536*	0.0511**	0.0535*	0.0540*	0.0396*	0.0372*
	(0.0282)	(0.0246)	(0.0284)	(0.0281)	(0.0220)	(0.0190)
Indexer x Post	–0.0058	–0.0095	–0.0048	–0.0056	–0.0108	–0.0112
	(0.0111)	(0.0096)	(0.0131)	(0.0109)	(0.0093)	(0.0090)
Ln(Income *per capita*)		0.2836*				0.3011**
		(0.1493)				(0.1448)
Housing Price Index Divided by 1,000			–0.0181			–0.0660
			(0.0988)			(0.1013)
State Mid-Skill Emp-to-Pop Ratio				0.0681		0.0654
				(0.0530)		(0.0479)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	222,285	222,285	222,285	222,285	222,285	222,285
Adjusted R-Squared	0.0218	0.0219	0.0218	0.0218	0.1129	0.1130

Panel B. Young Workers	(1)	(2)	(3)	(4)	(5)	(6)
Statutory Increaser Large x Post	–0.0115	–0.0352**	–0.0356***	–0.0108	–0.0103	–0.0364***
	(0.0120)	(0.0143)	(0.0121)	(0.0120)	(0.0114)	(0.0121)
Statutory Increaser Small x Post	0.0177	0.0152*	0.0187*	0.0182*	0.0195	0.0186*
	(0.0107)	(0.0084)	(0.0101)	(0.0105)	(0.0125)	(0.0097)
Indexer x Post	–0.0090	–0.0132	–0.0203**	–0.0089	–0.0170*	–0.0247***
	(0.0099)	(0.0090)	(0.0080)	(0.0100)	(0.0086)	(0.0074)
Ln(Income *per capita*)		0.3160***				0.2153*
		(0.0990)				(0.1185)
Housing Price Index Divided by 1,000			0.2121***			0.0943
			(0.0609)			(0.0764)
State Mid-Skill Emp-to-Pop Ratio				0.0879**		0.0921**
				(0.0406)		(0.0388)
Age and Education Controls	No	No	No	No	Yes	Yes
Observations	415,994	415,994	415,994	415,994	415,994	415,994
Adjusted R-Squared	0.0223	0.0224	0.0224	0.0223	0.1508	0.1509

Note: This table reports difference-in-differences estimates of equation (1), for which the policy indicator variables distinguish between states in which the minimum wage was increased by less than $1 and states that increased their minimum wage by $1 or more between January 1, 2013 and January 1, 2015. The sample is from the CPS. The dependent variable is whether or not the respondent was employed in the previous week. 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. Variable definitions and sources are discussed in the main text. All specifications include year and state fixed effects. Age and education controls consist of a dummy variable for each education group and age (included in Columns 5 and 6 as indicated in the table). Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix B: A Framework for Characterizing the Minimum Wage's Effects

This section presents a straightforward framework for characterizing the minimum wage's effects on employment and on the wage distribution. The remaining text of this appendix is lightly adapted from section I of the working paper in which we initially presented our short-run analyses and laid out pre-analysis plan (Clemens and Strain, 2017). The framework follows the dichotomy of Bound and Johnson (1992) in describing wage rates as arising from a combination of competitive market forces and bargaining institutions. Individual *i*'s productivity, the product of the quantity and market price of his or her output, is a_i per hour. Firms maximize profits by employing all individuals they can hire at wage rates less than or equal to a_i. Absent binding minimum wage regulation, firms offer individual *i* a wage of $\theta_i a_i$. If workers are paid precisely their marginal product, $\theta_i = 1$. When $\theta_i < 1$, workers are paid less than their marginal product. If $\theta_i < 1$, there is room for a minimum wage to transfer resources from firms to workers without reducing employment. The bargaining parameter θ_i, which is central to this possibility, may vary in magnitude due to a variety of labor market frictions.

The statutory minimum wage, w_{min}, may constrain firms' wage offers. So long as $a_i \geq w_{min}$, so that the value of the individual's expected output equals or exceeds the statutory minimum wage, a profit maximizing firm will offer employment at w_{min} when $\theta_i a_i < w_{min}$. When $a_i < w_{min}$, on the other hand, the firm will not offer the individual employment. The market and institutional forces captured by θ_i and a_i thus enable the framework to describe the minimum wage's primary intended and unintended effects.

To summarize: Let w_i be individual *i*'s observed wage. If a firm's unconstrained wage offer is not bound by the minimum wage, individual *i* will be employed at a wage of $w_i = \theta_i a_i$. If $\theta_i a_i < w_{min}$ and $a_i \geq w_{min}$, the firm will employ the individual at the minimum wage ($w_i = w_{min}$), but would have offered less if not constrained. Finally, the individual is out of work and has an observed wage of $w_i = 0$ if $a_i <$

w_{min}.

In the policy debate, minimum wage advocates typically intend for minimum wage increases to transfer income to workers out of their employers' profits. In this framework, that transfer can be large when the bargaining position of workers (θ_i) is relatively weak. Minimum wage opponents typically worry that increases will erode the labor market opportunities of low-skilled workers. In this framework, that employment effect can be large when the market value of many workers' output (a_i) is low relative to the level of the minimum wage. The overall impact of the minimum wage, then, depends on its level relative to the productivity of prospective workers and on the bargaining power those workers possess when negotiating wage rates with prospective employers.

We now take a somewhat deeper analytic look at the minimum wage's employment effects. Within this framework, the minimum wage's effect on the overall rate of employment depends on where the minimum wage falls within the productivity distribution. At time *t*, let a_i be distributed according to the probability density function $f_t(\cdot)$. The employment loss linked to a minimum wage of w_{min} is then

$$\int_0^{w_{min}} f_t\,(a_i)d(a_i) \qquad (B1)$$

Equation (B1) shows straightforwardly that the employment loss linked to the minimum wage will be large when the productivity of many individuals falls below it. This follows directly from the assumption that profit maximizing firms will only employ workers whose output has greater market value than the cost of employing them.[23] Both increases in the statutory minimum wage and decreases in the value of the goods and services a prospective worker would produce can increase the share of the

[23] In the simple framework developed here, the worker's wage is the only cost of employment to the firm. A more general model would highlight that the relevant employment costs include the costs of benefits, training, and regulatory compliance. A still richer model would emphasize uncertainties regarding workers' abilities and the dynamics of human capital development. In a multi-year contract, for example, it need not be the case that $a_i \geq w_i$, at all points in time or even over the life of any one worker's contract. Profit maximization requires only that the *expected value* of a worker's output exceeds expected employment costs over the course of the worker's contract.

workforce that lacks employment for this reason.

Equation (B1) is informative regarding two further, related questions of interest. First, how much employment loss should we expect to result from increasing the minimum wage? Equation (B1) makes clear that the employment loss due to a change in the minimum wage depends on the density of the productivity distribution between the minimum wage's old and new levels. Similarly, the wage gains associated with a minimum wage increase depend on the distribution of $\theta_i a_i$ between the minimum wage's old and new levels. The density of these distributions may, of course, vary significantly across settings. Minimum wage changes that move through thin portions of the productivity distribution will tend to have small employment effects while changes that move through thick portions can have large effects.

Additional factors may either dampen or augment the effect one would infer from the baseline productivity distribution. Perhaps most importantly, a minimum wage increase may alter the productivity distribution itself. This may occur through changes in both "real" and "nominal" productivity. Nominal productivity will rise with the minimum wage when minimum wage increases are passed onto consumers in the form of higher prices. With regards to equation (B1), this is germane because the relevant notion of productivity is "revenue product," meaning the quantity of output the worker produces multiplied by its market price. The minimum wage may also alter real productivity if it affects production arrangements, worker effort, or the skill composition of the workforce itself, among other factors.

Second, how will the employment effects of a given minimum wage evolve over time? This depends on how the productivity distribution evolves over time. Observing once more that a_i is nominal productivity, the relevant distribution can be shifted by either inflation or real productivity growth. The employment effect of a given minimum wage will thus be more sustained when inflation and real productivity growth are slow than when they are rapid. Relatedly, either deflation or a negative labor demand shock will increase the employment loss linked to a given minimum wage. In the face of such shocks, the minimum wage acts as a source of rigidity that mediates the transmission of the shock into some ensuing combination of wage and employment changes.

A final point worth emphasizing is that the prices and/or price indices relevant to the minimum wage's effects may differ from the general price level. The relevant prices are the market prices of the outputs low-skilled workers produce. These prices can be affected by factors other than general inflation. They can be affected, for example, by the evolution of the technologies through which goods in low-skill-intensive industries are produced. The introduction of a technology that substitutes for low-skilled labor at lower cost, for example, can reduce the prices of the goods low-skilled individuals produce. The introduction of such a technology would thus result in a downward shift of the relevant productivity distribution. This, in turn, would increase the employment loss associated with a given minimum wage. A similar analysis can be applied to expansions of trade with countries that are lower cost producers of the goods low-skilled workers produce in the United States.