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Effects of the Minimum Wage on Employment Dynamics

Upjohn Institute Working Paper 15-233

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ABSTRACT

The voluminous literature on minimum wages offers little consensus on the extent to which a wage floor impacts employment. We argue that the minimum wage will impact employment over time, through changes in growth rather than an immediate drop in relative employment levels. We conduct simulations showing that commonly-used specifications in this literature, especially those that include state-specific time trends, will not accurately capture these effects. Using three separate state panels of administrative employment data, we find that the minimum wage reduces job growth over a period of several years. These effects are most pronounced for younger workers and in industries with a higher proportion of low-wage workers.

JEL Classification Codes: J31, J38, J63

Key Words: Minimum wage, employment growth, administrative data, low-wage workers

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Abstract

The voluminous literature on minimum wages offers little consensus on the extent to which a wage floor impacts employment. We argue that the minimum wage will impact employment over time, through changes in growth rather than an immediate drop in relative employment levels. We conduct simulations showing that commonly-used specifications in this literature, especially those that include state-specific time trends, will not accurately capture these effects. Using three separate state panels of administrative employment data, we find that the minimum wage reduces job growth over a period of several years. These effects are most pronounced for younger workers and in industries with a higher proportion of low-wage workers.

*Author emails are jmeer@econmail.tamu.edu and westj@mit.edu. We are grateful for valuable comments from Kerwin Charles and two anonymous referees, as well as from David Autor, Jeffrey Brown, Jeffrey Clemens, Jesse Cunha, Jennifer Doleac, David Figlio, Craig Garthwaite, Daniel Hamermesh, Mark Hoekstra, Scott Imberman, Joanna Lahey, Michael Lovenheim, Steven Puller, Harvey S. Rosen, Jared Rubin, Juan Carlos Saurez Serrato, Ivan Werning, William Gui Woolston, and seminar participants at the Massachusetts Institute of Technology, the Naval Postgraduate School, Texas A&M University, and the Stata Texas Empirical Microeconomics workshop. We benefited greatly from discussions regarding data with Ronald Davis, Bethany DeSalvo, and Jonathan Fisher at the U.S. Census Bureau, and Jean Roth at NBER. Sarah Armstrong and Kirk Reese provided invaluable research assistance. This work was supported in part by the W.E. UpJohn Institute for Employment Research.

1 Introduction

The question of how a minimum wage affects employment remains one of the most widely studied – and most controversial – topics in labor economics, with a corresponding dispute in the political sphere. Neoclassical economic theories present a clear prediction: as the price of labor increases, employers will demand less labor. However, many recent studies testing this prediction have found very small to no effects of the minimum wage on the level of employment (e.g. Zavodny, 2000; Dube et al., 2010; Giuliano, 2013). One possible explanation for these findings is that demand for low-wage labor is fairly inelastic; another is that more complicated dynamics cloud identification of the effect of the minimum wage on employment.[1]

We argue that there is basis in theory for believing that the minimum wage may not reduce the level of employment in a discrete manner. We show that if this is indeed the case, then traditional approaches used in the literature are prone to misstating its true effects. We also demonstrate that a common practice in this literature – the inclusion of state-specific time trends as a control – will attenuate estimates of how the minimum wage affects the employment level. Specifically, we perform a simulation exercise which shows that if the true effect of the minimum wage is indeed in the growth rate of new employment, then even real causal effects on the level of employment can be attenuated to be statistically indistinguishable from zero.

To implement our analysis, we use a number of different empirical approaches to examine effects of the minimum wage on employment growth and levels; broadly, all of our approaches leverage a difference-in-differences identification strategy using state panels. We perform numerous robustness checks to test the validity of our identification strategy, which requires that the pre-existing time-paths of outcomes for states which increase their minimum wages do not differ relative to states that do not see an increase. We evaluate this possibility by adding leads of the minimum wage into our specifications; if increases in the minimum wage showed a negative effect on employment dynamics *before* their implementation, this would suggest that the results are being driven by unobserved trends. This is not the case. Indeed, for our results to be driven by confounders, one would have to believe that increases in the minimum wage were systematically correlated with unobserved shocks to that state in the same time period, but not other states in that region, and that these shocks are not reflected in measures of state-specific demographics or business cycles. Our results are

[1] Hirsch et al. [2011] and Schmitt [2013] focus on other channels of adjustment in response to increases in the minimum wage, such as wage compression, reductions in hours worked, and investments in training.

additionally robust to varying the specifications to account for finer spatial and temporal controls, the recent financial crisis, and inflation indexing of state minimum wages, as well as across different panel lengths and time periods.

We use three administrative data sets in our analysis: the Business Dynamics Statistics (BDS), the Quarterly Census of Employment and Wages (QCEW), and the Quarterly Workforce Indicators (QWI). These data sets vary in their strengths and weaknesses, discussed at length below, but together they encompass a long (1975-2012) panel of aggregate employment metrics for the population of employers in the United States. Our findings are consistent across all three data sets, indicating that employment declines significantly in response to increases in the minimum wage over the span of several years.

Finally, we find that the effect on job growth is concentrated in lower-wage industries, among younger workers, and among those with lower levels of education. Much of the existing literature focuses on these groups, though it is important to note that the minimum wage could affect other industries or elsewhere in the age and education distributions (e.g. Neumark et al., 2004).

If the minimum wage is to be evaluated alongside alternative policy instruments for increasing the standard of living of low-income households, a more conclusive understanding of its effects is necessary. The primary implication of our study is that the minimum wage does affect employment through a particular mechanism. This is important for normative analysis in theoretical models (e.g. Lee and Saez, 2012) and for policymakers weighing the tradeoffs between the increased wage for minimum wage earners and the potential reduction in hiring and employment. Moreover, we reconcile the tension between the expected theoretical effect of the minimum wage and the estimated null effect found by some researchers. We show that because minimum wages reduce employment levels through dynamic effects on employment growth, research designs incorporating state-specific time trends are prone to erroneously estimated null effects on employment. In contrast, the minimum wage significantly reduces job growth, at least in the context that we are able to analyze.

This article proceeds as follows: in Section 2 we provide a brief review of the literature on the employment effects of the minimum wage and build our case for examining employment dynamics. Section 3 presents our econometric models and demonstrates that existing approaches used in this literature obtain incorrect results if the true effect of the minimum wage is on the growth rate of employment. Section 4 describes the data used in our study and presents empirical results. We conclude in Section 5.

2 Theoretical and Empirical Framework

The economic literature on minimum wages is longstanding and vast. Neumark and Wascher [2008] provide an in-depth review of the field, which continues to be characterized by disagreement on how a minimum wage affects employment. The majority of recent studies, following Card and Krueger [1994], use difference-in-differences comparisons to evaluate the effect of these policies on employment levels. Recent papers generally focus on modifying the specification to improve the quality of the counterfactual comparisons, with disagreement on appropriate techniques and often-conflicting results (e.g. Allegretto et al., 2011 and Neumark et al., 2013). Importantly, these models test whether there is a discrete change in the level of employment before and after a state changes its minimum wage, relative to the counterfactual change as measured by other states' employment.

Yet there is basis in theory for believing that the minimum wage may not reduce the level of employment in a discrete manner. While the basic analysis of the effects of the minimum wage argues for rapid adjustments to a new equilibrium employment level (e.g. Stigler, 1946), transitions to a new employment equilibrium may not be smooth [Hamermesh, 1989] or may be relatively slow [Diamond, 1981; Acemoglu, 2001]. In this case, the effects of the policy may be more evident in net job creation.[2] In worker search-and-matching models (e.g. Van den Berg and Ridder, 1998; Acemoglu, 2001; Flinn, 2006, 2011), summarized concisely in Cahuc and Zylberberg [2004], the minimum wage has opposing effects on job creation. Although it reduces demand for labor by raising the marginal cost of employing a new worker, a higher minimum wage increases the gap between the expected returns to employment relative to unemployment, inducing additional search effort from unemployed workers. By increasing the pool of searching workers (and the intensity of their searching), the minimum wage improves the quality of matches between employers and employees, generating surplus. The theory thus has ambiguous predictions for the effect of a minimum wage on job creation. If workers' additional search effort sufficiently improves the worker-firm match quality, then job creation should not be adversely affected and may even increase. However, if the demand-side effect dominates, then increasing the minimum wage will cause declines in hiring.[3]

[2] Of course, any effect on growth does not exclude a discrete effect on the employment level. We separate these types of effects in the illustrations that follow to facilitate clearer exposition.

[3] With our reduced-form empirical analysis, we cannot distinguish the true mechanism driving the relationship between the minimum wage and employment. For instance, it is possible possible that the minimum wage would discretely affect employment, but that frictions in the labor market cause this effect to manifest over time. At a practical and policy-relevant level, these two situations are equivalent, and we are agnostic on the underlying mechanism which, as we discuss in Section 4.4, limits our ability to make sweeping statements about how the minimum wage truly impacts labor markets.

Sorkin [2013] builds a model that formalizes this potentially slow adjustment of labor demand, focusing on firms' difficulties in adjusting their capital-labor ratios, and applies it to minimum wage increases. He argues that "the ability to adjust labor demand is limited in the short run" and that this "provide[s] an explanation for the small employment effects found in the minimum wage literature." Fundamentally, this identification problem stems from the "sawtooth pattern" exhibited in states' real minimum wages. Sorkin argues that "difference-in-difference faces challenges in measuring the treatment effect of interest, which in this case is the effect of a permanent minimum wage increase, whenever there are dynamic responses to the treatment and the treatment itself is time-varying."

To be clear, if the true effect a minimum wage is to change the *slope* for employment growth, rather than the employment *level*, then the traditional approaches used in this literature – namely, difference-in-differences estimates of the effects of the minimum wage on employment levels – will yield incorrect inference.[4]

2.1 Staggered Treatments and Difference-in-Differences

We illustrate this potential shortcoming of the classic difference-in-differences approach in Figure 1. This toy example depicts employment in two hypothetical jurisdictions, which initially exhibit identical growth rates. At some time t_1, Jurisdiction A is treated; at some later time t_2, Jurisdiction B is treated with the same intensity. In Panel (a), treatment has a *discrete* and symmetric negative effect on the employment level, whereas in Panel (b), the treatment has a symmetric negative effect on employment growth, but does not discretely alter the employment level. Consider the standard difference-in-differences (DiD) identification of the employment effect:

$$\text{Employment}_{it} = \delta_B \cdot \text{I}\{\text{Jurisdiction} = \text{B}\} + \tau_t \cdot \text{I}\{\text{Time} = t\} + \beta \cdot \text{I}(\text{Treatment}_{it} = 1) + u_{it}$$

Because both jurisdictions are initially untreated and both are eventually treated, the only time period(s) in which the treatment effect β may be identified separately from the time fixed effects τ_t are those during which *only* Jurisdiction A is treated. During all other time periods, $\text{I}(\text{Treatment}_{it} = 1)$ takes the same value for both states. Thus, the DiD model compares the average difference in employment between the jurisdictions during the time

[4] Several recent studies are exceptions to the focus on employment levels. Dube et al. [2011] examine the relationship between the minimum wage and employee turnover for teenagers and restaurant workers using the 2001-2008 Quarterly Workforce Indicators (QWI). Brochu and Green [2013] assess firing, quit, and hiring rates in Canadian survey data. Both studies find a reduction in hiring rates but do not estimate the effect on net job growth.

period between t_1 and t_2 to that in the time periods prior to t_1 and following t_2.

This evaluation is obvious for the discrete employment effect in Panel (a). The difference between jurisdictions' employment is clearly smaller during the middle time period, compared to the outer time periods, and the DiD estimate is correctly some negative number. Moreover, the duration of each of the three time periods is irrelevant for obtaining the correct inference.

If instead the treatment effect is on growth as in Panel (b), then DiD is very sensitive to the relative duration of each (outer) time period. To highlight this sensitivity, consider first the extreme case in which there is a long pre-treatment timespan between times zero and t_1, but a very short timespan between t_2 and T, the end of the sample period. In this situation, the average difference in employment during the outer time periods is determined nearly entirely by the pre-treatment period, and the DiD estimate for the treatment effect will be negative. Contrast this with the other extreme: a very short timespan between times zero and t_1, but a long period following t_2, during which both jurisdictions are treated. In this situation, the average difference in employment during the outer time periods is determined nearly entirely by the later period, and the DiD estimate for the effect of the *same* treatment will be positive. And, if T is selected such that the two outer periods have equivalent duration (i.e. $t_1 - 0 = T - t_2$), then DiD yields a null treatment effect, visibly at odds with the plotted time paths of employment.

This toy example underscores the pitfalls in using a standard difference-in-differences model to identify treatment effects if there is staggered treatment intensity and the treatment affects the growth of the outcome variable. As a state-level policy, the minimum wage clearly exhibits this type of staggered treatment: Figure 2 (along with Appendix C) shows that the effective minimum wage changed in at least one state in 33 of the 37 years from 1976 through 2012 – more than 700 changes in total – including every year after 1984.[5] We investigate the implications of this concern more thoroughly using Monte Carlo simulation in Section 3. First, though, we discuss a separate but related concern.

[5]Inflation is an additional consideration when evaluating the minimum wage as a policy treatment. Historically, minimum wages have been set in nominal dollars, with their value eroding substantially over time (see Appendix C for details). This means that the actual *intensity* of treatment changes over time, even in the absence of any subsequent (own or counterfactual) explicit policy change. This situation would not be problematic if the minimum wage affected employment in an abrupt, discrete manner. But if the minimum wage predominantly affects job creation, then it may take years to observe a statistically significant difference in the level of employment. In Section 4.4, we revisit the implications of inflation for minimum wage policy in the context of our empirical findings.

2.2 Implications of Jurisdiction Time Trends as Controls

Many recent studies of the minimum wage include state- or county-specific time trends to control for heterogeneity in the underlying time-paths by which labor markets evolve within different areas that might be correlated with treatment intensity (e.g. Page et al., 2005; Addison et al., 2009; Allegretto et al., 2011). These models generally find little or no effect of the minimum wage on employment levels. However, if the policy change affects the growth rate of the response variable, rather than its level, then specifications including jurisdiction-specific trends will mechanically attenuate estimates of the policy's effect. The basic intuition is that including state-specific time trends as controls will adjust for two sources of variation. First, if there is any *pre-treatment* deviation in outcomes that is correlated with treatment – e.g. if states that exhibit stronger employment growth are also more likely to increase their minimum wage – then this confounding variation may be appropriately controlled for by including state-specific time trends. The potential cost of this added control is that if the actual treatment effect, the *post-treatment* employment variation, acts upon the trend itself, then inclusion of jurisdiction time trends will attenuate estimates of the treatment effect and often leads to estimating (statistical) null employment effects.[6]

A simple illustration of this is provided in Figures 3 and 4. Figure 3 depicts employment in two hypothetical jurisdictions which exhibit identical employment growth rates prior to period $t = 0$. After period $t = 0$, the employment growth rate in the Treated jurisdiction falls relative to the Control, but there is no discrete change in the level of employment. Figure 4 presents the difference in employment by time period for both levels and growth, with and without adjustment for jurisdiction time trends. The computed employment effect is large and negative when state trends are omitted (in Panel (a)), but shrinks nearly to zero with the inclusion of jurisdiction time trends (Panel (b)). This occurs despite identical pre-treatment employment trends. In contrast, inference about the effect on employment growth is the same regardless of whether the the data are detrended (Panels (c) and (d)), because the effect on growth is discrete.

We are by no means the first to make this point. In examining the effects of changes in divorce laws, Wolfers [2006] makes a general observation that a "a major difficulty in difference-in-differences analyses involves separating out trends from the dynamic effects of a policy shock." Lee and Solon [2011] expound on this point in a discussion of Wolfers [2006], pointing out that "the sharpness of the identification strategy suffers" when jurisdiction-specific time trends are included and, "the shift in the dependent variable may vary with the

[6]We are grateful to Cheng Cheng and Mark Hoekstra, as well as Justin Wolfers for this insight.

length of time since the policy change." This problem has been discussed in other contexts, including bias in estimates of the effects of desegregation (Baum-Snow and Lutz, 2011) and marijuana decriminalization (Williams, 2014).

However, this approach remains common in the minimum wage literature and, indeed, for many other important policy questions in which researchers ask "a much more nuanced question than just whether the dependent variable series showed a constant discrete shift at the moment of policy adoption" (Lee and Solon, 2011). We hope that our examples and simulations will serve as a useful guide to researchers considering how to approach estimation of policies whose effects may differ over time and, especially, may be reflected in changes in the growth rate of the variable of interest. We delve further into the question of how best to estimate these effects in Section 3.

3 Econometric Specifications and Simulations

In Section 2, we provide theoretical support for the hypothesis that the minimum wage affects the growth rate of employment, even if it does not induce a discrete drop in the level of employment, and we illustrate several complications for attempts to empirically quantify the magnitude of such an employment effect. In this section, we present several econometric models as candidates to estimate this effect, comparing their strengths and shortcomings both analytically and using simulated data in a Monte Carlo framework. The goal of this section is not to argue for one "correct" model to estimate the relationship between the minimum wage and employment, but rather to underscore the tradeoff between the various assumptions that can be invoked in order to obtain causal inference about this treatment effect.

3.1 Candidate Specifications

Consider the following panel difference-in-differences model relating the minimum wage to employment:

$$\text{emp}_{it} = \alpha_i + \tau_t + \gamma_i \cdot t \ + \sum_{r=0}^{s} \beta_r \text{mw}_{it-r} + \psi \cdot \text{controls}_{it} + \epsilon_{it}$$

in which emp_{it} is the level of employment in state i at time t, α_i are jurisdiction fixed effects, τ_t are macroeconomic time period fixed effects, $\gamma_i \cdot t$ are jurisdiction-specific linear time trends, and ϵ_{it} is the idiosyncratic error term.

If the true treatment effect is fully discrete in levels, as in the scenario depicted in Panel (a) of Figure 1, then $\beta_r = 0 \ \ \forall r > 0$, as lags of the minimum wage do not separately affect the current employment level. The model reduces to:

$$\text{emp}_{it} = \alpha_i + \tau_t + \gamma_i \cdot t \ + \beta_0 \text{mw}_{it} + \psi \cdot \text{controls}_{it} + \epsilon_{it} \tag{1}$$

and the estimate $\hat{\beta}_0$ identifies the total causal impact of the minimum wage on employment. Specification 1 is the "classic" variant of the difference-in-differences specification, in levels, and has been used extensively in the literature.

In contrast, if the true treatment effect instead acts on the growth rate of employment, as in the scenario depicted in Panel (b) of Figure 1, then $\beta_r \neq 0$ for at least some lagged values of the minimum wage. The full set of lag terms are necessary, yielding a distributed lag model in levels:

$$\text{emp}_{it} = \alpha_i + \tau_t + \gamma_i \cdot t \ + \sum_{r=0}^{s} \beta_r \text{mw}_{it-r} + \psi \cdot \text{controls}_{it} + \epsilon_{it} \tag{2}$$

An alternate approach is to difference the model, yielding the distributed lag model in first-differences:

$$\Delta\text{emp}_{it} = \theta_t + \gamma_i \ + \sum_{r=0}^{s} \beta_r \Delta\text{mw}_{it-r} + \psi \cdot \Delta\text{controls}_{it} + \Delta\epsilon_{it} \tag{3}$$

Either Specification 2 or Specification 3 can be used to flexibly identify the dynamics of the effect of the minimum wage on employment, and summing the β_r identifies the overall effect on the employment level. Whether it is preferable to estimate distributed lag coefficients using a fixed effects versus a first-differenced model is not clear.[7] Nichols [2009] notes that a major consideration in this decision is the timing between the change in treatment and the observed effect, the theoretical relationship of which is not obvious in this context. Moreover, depending on the degree of serial correlation between ϵ_{it} and between $\Delta\epsilon_{it}$, either Specification 2 or 3 may be more efficient; as Wooldridge [2002] notes, the "truth is likely to lie somewhere in between." Our focus on importance of changes from year to year, as opposed to comparing differences in pre- and post-periods, suggests that the first-differenced approach is more appropriate in this case. Nevertheless, we leverage both variations of the

[7] An additional consideration is that the asymptotic properties of the fixed effects estimator rely on $N \to \infty$, and there are only 51 U.S. jurisdictions (states) included in the data sets we evaluate.

distributed lag model, testing them in the Monte Carlo simulation below and presenting both in the primary results tables.

Although distributed lag models such as these are relatively common in the program evaluation literature, both forms of the specification suffer from a common shortcoming when examining minimum wage effects. Specifically, the high frequency variation in treatment intensity makes it difficult to make credible causal inference about the employment effects of higher-order lags of the minimum wage, because the large number of changes and potential long-run confounders make a fully-specified model fragile. Put another way: in practice the number of included lags s must be fairly small in any distributed lag specification, in either levels or first-differences. Including only a short number of lag terms reduces the utility of using a distributed lag specification to estimate an effect on growth.

Given this restriction on the number of lag terms that can sensibly be included, a natural approach is to use a dynamic panel specification (e.g. Arellano and Bond, 1991). This allows us to estimate both the short- and long-run effects, at the cost of imposing a stricter assumption on the nature of this relationship. The specification then takes the form:

$$\text{emp}_{it} = \mu \cdot \text{emp}_{it-1} + \alpha_i + \tau_t + \gamma_i \cdot t \ + \sum_{r=0}^{s} \beta_r \text{mw}_{it-r} + \psi \cdot \text{controls}_{it} + \epsilon_{it}$$

which differs from the above models in that the lag of employment is included on the right hand side. This can be first-differenced to eliminate the α_i jurisdiction fixed effects:

$$\Delta\text{emp}_{it} = \mu \cdot \Delta\text{emp}_{it-1} + \theta_t + \gamma_i \ + \sum_{r=0}^{s} \beta_r \Delta\text{mw}_{it-r} + \psi \cdot \Delta\text{controls}_{it} + \Delta\epsilon_{it} \tag{4}$$

In this dynamic panel model, the short run marginal effect of the minimum wage on employment is β_0, and the effect after one year of a sustained change is captured by $\beta_1 + (1+\mu) * \beta_0$. Due to the properties of a geometric series, the long run effect on employment is determined by $(\beta_0 + \beta_1)/(1-\mu)$. Importantly, this long run effect (in fact, the specific time path of the effect) can be identified using only a single lag term for the minimum wage. Thus, a dynamic panel specification skirts much – although not all – of the concern about constantly changing treatment intensities.[8]

[8] In solving one identification problem, the dynamic panel approach introduces another, as the Δemp terms are autocorrelated. The standard practice, as in Arellano and Bond [1991], is to use deeper lags of employment as instruments for the lagged employment term. However, these may not be exogenous, depending on the degree of autocorrelation. As we discuss later, our results are robust to a number of approaches, including the use of deeper lags of the *minimum wage* rather than employment as instruments. We are grateful to an anonymous referee for suggesting this approach.

We have yet to discuss the role of the jurisdiction time trends, $\gamma_i \cdot t$, in comparing these specifications. Provided the true treatment effect is fully discrete in levels, then including jurisdiction time trends will not bias the estimated $\hat{\beta}_0$ in any of the above models (recall that for an effect that is fully discrete in levels, $\beta_r = 0 \ \ \forall r > 0$). Jurisdiction-specific time trends can be included as controls for any underlying variation in employment trends – which might be correlated with treatment intensity – without biasing the estimate for the β_0 parameter of interest. However, if the true treatment effect instead acts on the growth rate of employment, then including jurisdiction time trends will bias estimates in all of the above models. In this case, because the minimum wage actually *affects* the slope of the employment trend, including jurisdiction-specific time trends in the specification will directly bias estimates of the β_r parameters of interest.[9]

One possibility to avoid this bias would be to identify the jurisdiction-specific time trends using only pre-treatment time periods: that is, to estimate γ_i for each jurisdiction during the pre-treatment period only, and then extrapolate these trajectories throughout the entire study timeframe. This approach may work well for many studies in the program evaluation literature, in which treatments are usually discrete one-time changes. However, the validity of this approach requires that there actually is a sufficient pre-treatment period, a condition that demonstrably fails to hold in the case of the minimum wage in the United States. In this context, this first option is off the table.

A second option is to test for the presence of pre-treatment variation in employment trends directly by using a common "leading values" falsification test, and – provided this test is passed – simply exclude jurisdiction-specific time trends from the specification. Recall that the concern is that jurisdictions which disproportionately increase their minimum wage might have had comparatively negative employment trends even in the absence of differences in treatment. If the econometric test reveals that this is unlikely to be the case, then the model can be changed to force $\gamma_i = 0 \ \ \forall i$. The β_r terms will yield unbiased estimates of the distributed lag effects of the minimum wage provided that jurisdiction time trends are not of importance in the true model.

Testing for pre-treatment deviation in outcomes should alleviate concerns about the importance of controlling for heterogeneity in jurisdiction time trends. But, if it remains unpalatable to eliminate jurisdiction time trends entirely from the model (and provided the treatment effect is on growth), then the remaining option is to impose an additional strong

[9] Note that jurisdiction time trends would still bias estimates for a treatment effect on growth even if it were possible to fully saturate the model with post-treatment lags of the treatment variable. The fundamental issue stems from the treatment affecting the trend itself, as illustrated earlier in Figures 3 and 4.

restriction by setting $\beta_0 \equiv \beta_1 \equiv ... \equiv \beta_s$. This restriction requires that the minimum wage affect employment *growth* discretely and permanently – that there is not a dynamic relationship between the minimum wage and employment growth. This restriction is consistent with the relationship depicted in Figure 3, in which the minimum wage causes a break-in-trend for the employment level, rather than a discrete drop in the employment level. Provided that this assumption holds, then:

$$\sum_{r=0}^{s} \beta_r \Delta \text{mw}_{it-r} = \beta_0 \cdot (\Delta \text{mw}_{it} + \Delta \text{mw}_{it-1} + ... + \Delta \text{mw}_{it-s}) = \beta_0 \cdot \text{mw}_{it}$$

and Specification 3 is equivalent to:

$$\Delta \text{emp}_{it} = \theta_t + \gamma_i + \beta_0 \cdot \text{mw}_{it} + \psi \cdot \Delta \text{controls}_{it} + \Delta \epsilon_{it} \quad (5)$$

Specification 5, which we refer to as the "break-in-trend" model, is the only specification of these five that is robust to including jurisdiction time trends without biasing estimates of β_r for a treatment effect on growth. This distinction comes at the cost of a strong assumption about the nature of the dynamics of the treatment effect. In practice, it seems very unlikely that the minimum wage would *permanently* reduce the growth rate of employment – indeed, extrapolating such an effect far into the future would predict immense employment effects. For this reason, we primarily view Specification 5 as a trends-robust indication of *whether* the minimum wage affects the growth rate of employment, with the possibility of calculating back-of-the-envelope estimates of the magnitudes of proposed policy, given certain assumptions. We return to this issue in detail in Section 4.4.

We will present results from each of these five specifications – classic, distributed lags in levels, distributed lags in first-differences, dynamic panel, and break-in-trend – both with and without including jurisdiction time trends, for all three data sets, in Section 4. First, though, we use Monte Carlo repetitions of a fairly simple simulation to underscore how severely time trends bias estimates of an effect on growth across these specifications.

3.2 Monte Carlo Simulation

In this section, we conduct a Monte Carlo exercise with simulated data to compare the efficacy of the five models and to illustrate how severely including jurisdiction time trends biases estimates when the treatment effect is on growth.

Our data generating process starts with an annual panel of actual state minimum wages and employment (in the Business Dynamics Statistics data, discussed below in Section 4). Drawing without replacement from these data, we form two independent distributions of changes, one for real minimum wages and one for employment. We merge these distributions together to form a new panel containing 35 periods for 51 state entities, repeating this process within each Monte Carlo repetition.

Next, we impose a treatment effect relating the minimum wage to the growth rate of employment. To prevent the effect from being purely deterministic, we draw the treatment effect from a $Normal(-0.03, 0.015)$ distribution for each state-year observation. That is, each 10% increase in a state's real minimum wage causes, in expectation, a 0.3 percentage point reduction in employment growth. Because the effect is on the employment growth rate, the treatment effect in a state in one year persists throughout all future years, a pattern such as that illustrated earlier in Figure 3. While imposing this type of treatment effect is extreme – an increase in the real minimum wage will permanently reduce the growth rate of employment – it facilitates clarity in comparing the five models and highlighting the concern with jurisdiction time trends.

With these simulated data, we estimate the relationship between the minimum wage and employment using each of the five specifications, separately with and without including jurisdiction time trends. Table 1 reports the median coefficients from 10,000 Monte Carlo repetitions of these estimations.[10] Consider first Column (1) in Panel [A], which excludes jurisdiction time trends. The standard difference-in-differences model clearly identifies a negative average treatment effect, though this coefficient does not clarify whether the treatment discretely affects the level of the outcome or if it affects the growth rate. In contrast, when time trends are added in Panel [B], the coefficient in Column (1) is attenuated to, essentially, a zero estimated treatment effect. This occurs despite the fact that time trends cannot actually be helpful for these estimations, because the simulated data have random employment shocks that are by construction only correlated with minimum wages through the imposed treatment effect.

The dynamics of the treatment effect are more salient in the distributed lag specifications in Columns (2) and (3) of Panel [A]: it is clear that the treatment does not simply induce a one-time contemporaneous drop in the level of the outcome, but instead continues to negatively affect employment in future periods, i.e. an effect on growth. The pattern in Column (2) for the estimated dynamics when using distributed lags in levels shows that

[10] The full code used in this simulation, along with all other code and data included in this study, is available from the authors at http://econweb.tamu.edu/jmeer/Meer_West_MinimumWage_Code.zip.

there is no contemporaneous effect on the employment level and an increasing cumulative effect over time, with the final lag term capturing the remaining average treatment effect. We somewhat arbitrarily opted to include only three lag terms, but this basic pattern of a "zero" contemporaneous effect and a large final lag term holds regardless of the number of lag terms included in Specification 2, be it one or many. The important thing to note is that this approach does not yield accurate results, either, though it does highlight that there is a dynamic response following the simulated treatment. Panel [B], which includes time trends, shows an even larger deviation from the true effect, with a relatively large and positive contemporaneous coefficient.

Turning to Column (3), the distributed lags with first differences model accurately captures the constant treatment effect that was imposed on growth. Yet as with the previous two specifications, this model also exhibits attenuation of the estimates when jurisdiction time trends are included in Panel [B].

Column (4) shows the results of the dynamic panel simulation. The autoregressive term for the lag of employment is 0.852, with the contemporaneous minimum wage coefficient equaling -0.019 and the first lag equaling -0.041. This implies that a permanent, real increase in the minimum wage results in a short-run elasticity of -0.019 in the first year and -0.076 in the second year. The long-run effect, calculated as explained above, is -0.407. While this model does require stricter assumptions, the primary advantage is the ability to examine the short- and long-run elasticities; essentially, these results allow us to plot out the effect on the level of employment, showing an initial dip to a new employment level that subsequently runs parallel to that of the counterfactual.[11] Much like the previous specifications, including jurisdiction-specific trends in Panel [B] substantially biases the estimates: the short-run impact changes to a positive 0.014 in the first year and 0.019 in the second year – that is, not even the sign is correct – with a very small permanent effect of -0.016.

Finally, the Break-in-Trend specification in Column (5) identifies the nature of the "kink" in the employment time path. We stress again that the accuracy of the estimated *magnitude* of this coefficient depends on the validity of the strong identifying assumption about a permanent effect on growth (which happens to be true by construction in this simulation). The value of this specification is that – in only this model – the coefficient is not biased when jurisdiction time trends are included, as we showed analytically above and as is evidenced by comparing Panel [A] to Panel [B] of Column (5) in Table 1.

[11] Note that, in the case of the extreme data-generating process that we impose, this prediction is incorrect. We discuss the general difficulties of making inference about permanent changes in the minimum wage, especially without imposing model-based restrictions, in Section 4.4

Summarizing the findings of this Monte Carlo simulation, we have shown that – if the true treatment effect is on employment growth – including jurisdiction time trends can starkly bias estimates from a difference-in-differences specification, whether the model is the classic form, a distributed lag specification, or even a dynamic panel model. For exposition, we simulated the extreme case of a permanent treatment effect of the real minimum wage on employment growth. However, our findings generalize to drawing entire minimum-wage histories rather than individual-year changes; to allowing the minimum wage treatment intensity to be eroded due to inflation; to introducing underlying jurisdiction-specific trends that are correlated with whether the jurisdiction has a high or low minimum wage; and to treatment effects that attenuate over time. Most importantly, this simulation exercise contrasts the various specifications that we will estimate in the next section and illustrates the general pattern of results to be expected of an effect on employment growth.

4 Empirical Results

4.1 Data

We estimate employment effects using three data sets: the *Business Dynamics Statistics* (BDS) and the *Quarterly Workforce Indicators* (QWI), both from the Bureau of the Census, and the *Quarterly Census of Employment and Wages* (QCEW) from the Bureau of Labor Statistics. The QCEW and QWI report quarterly employment for each state, while the BDS is annual. All of these data are administrative in nature; the QCEW and QWI programs collect data from county unemployment insurance commissions, while the BDS reports on employment rosters furnished to the U.S. Internal Revenue Service. As such, each of the data sets we study accounts for virtually the entire population of non-farm employment.[12] For brevity and clarity of exposition, we report results from the BDS in the main body of the paper, with results from the full set of specifications using the QCEW and QWI in Appendix

[12] The employer-sourced administrative nature of these data is important for our research question. Population-level data provide for a cleaner assessment of the overall policy impact of minimum wages by avoiding sampling error. Moreover, as discussed in Section 2, a higher minimum wage may induce additional searching effort on the part of the currently unemployed. Mincer [1976] shows that this positive supply elasticity often leads to an increase in the number of *unemployed* that differs substantially from the change in employment. Because employment is the policy-relevant outcome, measuring job counts using employer-sourced data provides a better identification of any disemployment effects than do surveys of individuals, such as the Current Population Survey. Finally, employment data directly reported by firms to maintain legal compliance have been shown to be more accurate than responses to individual-level surveys such as the CPS [Abraham et al., 2009].

A. As we note below, there is little difference in the overall results across the three data sets, which is unsurprising given that all three examine the near-population of jobs in the United States.

The BDS covers all non-agriculture private employer businesses in the U.S. that report payroll or income taxes to the IRS. The heart of the BDS is the Census Bureau's internal Business Register, which is sourced from mandatory employer tax filings and augmented using the Economic Census and other data to compile annual linked establishment-level snapshots of employment statistics (on March 12^{th}). The Census Bureau releases the BDS as a state-year panel (all fifty states, plus the District of Columbia), currently covering 1977 to 2011. Summary statistics from the BDS are provided in Table 2. Full descriptions of the QCEW and QWI, including their summary statistics, are located in Appendix A.

4.1.1 State Minimum Wages

We draw historical data on state minimum wages from state-level sources.[13] For the QCEW and QWI, we use the minimum wage value as of the first of each quarter. For the BDS, we use the value as of the previous March 12th each year, directly corresponding to the panel years in the BDS data. Some states have used a multiple-track minimum wage system, with a menu of wages that differ within a year across firms of different sizes or industries; we therefore use the maximum of the federal minimum wage and the set of possible state minimum wages for the year. To the extent that there is firm-level heterogeneity in the applicable wage level, our definition allows the minimum wage term to serve as an upper bound for the minimum wage a firm would actually face. We transform minimum wages into constant 2011 dollars using the (monthly) CPI-U from the Bureau of Labor Statistics.[14]

4.1.2 Other Control Variables

Although our econometric specifications include an extensive set of time period controls, precision may be gained by accounting for additional state-specific time-varying covariates.

[13] Although historical state minimum wage data are available from sources such as the U.S Department of Labor (`http://www.dol.gov/whd/state/stateMinWageHis.htm`), these data suffer several limitations. For one, minimum wage values are only reported only as of January first each year, whereas the panel used in our study necessitates values as of other dates. Additionally these DOL data incompletely characterize changes to state minimum wages, especially during the early years of our panel. This DOL table is frequently used as the source of historical state minimum wage values for recent studies in this literature, and we caution future researchers to be careful not to inadvertently attribute minimum wage changes to years in which they did not occur.

[14] Because we use a national-level deflator, specifying the log minimum wage term as real or nominal does not affect our results. Time period fixed effects incorporate this added variation.

The Census Bureau's Population Distribution Branch provides annual state-level population counts, including estimates for intercensal values. Total state population represents a determinant of both demand for (indirectly by way of demand for goods and services) and supply of employees. Because states differ non-linearly in their population changes, controlling directly for population may be important. The range in population between states and across time is enormous, so we use the natural log of state population in our specifications. We additionally include the share of this population aged 15-59, which provides a rough weight for how population might affect demand for versus supply of labor. Demographic controls such as these are commonly used in this literature (e.g. Burkhauser et al., 2000; Dube et al., 2010). Following Orrenius and Zavodny [2008], we also include the natural log of real gross state product per capita.[15] After controlling for state population, this term can be thought of as a rough proxy for average employee productivity as well as a measure of state-level fluctuations in business cycles [Carlino and Voith, 1992, Orrenius and Zavodny, 2008].

4.2 Results

We begin with a very simple diagnostic check: if the true effect of the minimum wage is on growth, then specifications that are differenced over increasingly long time periods should yield larger coefficients for the effect of the minimum wage on employment. We take Equation 3 with a single minimum wage term, and increase the number of years over which we difference the equation. Indeed, this simple check shows evidence for effects on growth: the coefficient on the minimum wage term for a one-year difference is -0.020 (s.e. = 0.018); taking the difference from two years previously changes the coefficient to -0.039 (s.e. = 0.021); for three years, it is -0.050 (s.e. = 0.024); for four years, it is -0.051 (s.e. = 0.024). The coefficient is stable around this magnitude even when differencing by as much as eight years, and similar results are seen in the QCEW and QWI. While this diagnostic does not provide definitive proof that the effects of the minimum wage are on growth – after all, many other factors can change over such long periods – the absence of such a pattern could be taken as evidence against our hypothesis.

In Table 3, we present results for the five specifications from Section 3 to identify the effect of the minimum wage on employment using the Business Dynamics Statistics (as mentioned above, results for the QCEW and QWI are available in Appendix A). Of course,

[15] We compute the log of the real value of total GSP per capita using all industry codes, including government. Results are virtually unaffected by using ln(real *private sector* GSP/capita) instead, but we view total GSP as the more appropriate definition given that the population term reflects total state population.

estimations using the actual data do not generate coefficients that are as tidy as those using a prescribed data-generating process.[16] Nevertheless, the models in Section 3 that are shown to accurately capture effects on growth yield similar estimates in all three data sets, and, broadly, estimates across all specifications show similar patterns to their counterparts using the artificially-generated data.

We focus first on Panel [A], which excludes the jurisdiction-specific trend terms. The classic difference-in-differences model in Column (1), which corresponds to Specification 1 in Section 3, shows a significant disemployment effect of the minimum wage. Specifically, the estimate is that a permanent ten percent increase in the real minimum wage causes about a 1.7 percent decline in total employment. As with the results from the Monte Carlo simulation, the classic model cannot distinguish between an effect on growth and a discrete effect on the employment level. The dynamics of the treatment effect are more apparent in the distributed lag model in Column (2). It is clear that the effect is not encompassed in a one-time discrete drop in employment; rather, the minimum wage appears to have a fairly constant negative effect on the growth rate of employment over the period covered by the lags. The effects for each minimum wage coefficient are negative and, with the exception of the third lag, statistically significant. A permanent increase in the minimum wage, according to this model, would yield an employment elasticity of -0.29 (s.e. = 0.06). In Column (3), the distributed lag model in first differences, we see a fairly steady and negative impact of the minimum wage on employment, similar to the one found in Table 1; the third lag is positive and statistically insignificant, suggesting that the effects of a minimum wage change fade out after about three years, though this pattern could also result from the high-frequency variation in minimum wage changes. Importantly, much of the impact comes in the two years *after* the change, suggesting that short-term data immediately after an increase in the minimum wage is unlikely to show its true impact. Summing up these coefficients yields the effect of a permanent change: -0.074 (s.e. = 0.036).[17] Irrespective of the magnitudes, we view the results in these two columns as strong evidence that the effect of the minimum wage on employment is of a more dynamic nature than that supposed in the frictionless

[16] This reduced precision is partly a (lack of) Law of Large Numbers issue: the simulation had 10,000 repetitions of 1785 observations to obtain those coefficients, whereas these results have only the 1785 real-world observations, based on 51 jurisdictions . In addition, unlike in the simulation, real minimum wages are not randomly assigned: there is strong bunching of changes around certain years, for instance. Finally, the simulation prescribed a simple effect just on employment growth, whereas the minimum wage in practice could affect both the level and growth of employment.

[17] Additional lags do not make a qualitative difference to the sum of coefficients, and the coefficients on the first three minimum wage terms remain similar in magnitude and significance.

neoclassical framework. This is further evidenced by the dynamic panel specification in Column (4).[18] The contemporaneous elasticity of a minimum wage increase is -0.031 (s.e. = 0.017), with the lag term (-0.054, s.e. = 0.02) implying that the impact after one year at the same treatment intensity would be -0.10 (s.e. = 0.033) and after two years, -0.14 (s.e. = 0.49); the long-run impact of a permanent real increase in the minimum wage effect is -0.20 (s.e. = 0.088).

Contrast these results with those in Panel [B], in which jurisdiction-specific trend terms are included. Across the first four models, the coefficients are sharply attenuated and few remain statistically different from zero. Given the clear evidence in Panel [A] that the effect is not discrete on the employment level, this attenuation is exactly what we would expect based on the theoretical and econometric arguments made in Sections 2 and 3. Moreover, the pattern to this contrast between Panels [A] and [B] of Table 3 closely mirrors that shown in the Monte Carlo simulation in Table 1: including jurisdiction trends mechanically biases the estimated coefficients across all four models.

Finally, consider the Break-in-Trend model in Column (5). Note that to ensure identification is coming from within-jurisdiction changes in minimum wage, we include the initial minimum wage by jurisdiction as an additional control in Panel [A].[19] The strong assumptions underlying this specification require caution in drawing causal inference about the *magnitude* of the estimated employment effect.[20] That said, it is reassuring that this model yields an estimate in Panel [A] that is similar in magnitude to the per-period coefficients identified for

[18] The standard approach is to use deeper lags of the dependent variable as instruments [Arellano and Bond, 1991]. Concerns about endogeneity suggest using deeper lags of the minimum wage values themselves as instruments. We use Roodman's (2009) Stata module, which allows for flexible estimation of dynamic panel models, using this approach, though the coefficients on the minimum wage terms are stable across different sets of instruments. We are grateful to an anonymous referee for this suggestion.

[19] The essence of the difference-in-differences identification strategy is to identify the effect using temporal variation within jurisdiction, rather than between jurisdictions. Whereas Columns (1)-(4) either include a jurisdiction fixed effect or first-difference the minimum wage term, Specification (5) in Panel [A] does neither. In the absence of a jurisdiction fixed effect (which is added to Specification (5) in Panel [B]), including the initial minimum wage by jurisdiction controls for heterogeneity in the baseline differences in jurisdictions' minimum wages and ensures that identification comes from within-jurisdiction variation. This was not an issue in the simulation, for which initial minimum wage values were randomly assigned.

[20] This is not to say that the results do not hold implications for nominally-set minimum wages. One reasonable approach is to apply the average "erosion" rates of the minimum wage in the data (see Appendix C for historical minimum wage erosion rates, as well as the discussion in Section 4.4). Suppose that a state increases its nominal minimum wage by 10% relative to other states within its Census region. The average erosion rate in our panel predicts a remaining effective difference of 6.64% after one year. This relative difference shrinks to 3.87% by the next year, to 2.31% the year after, and to 0.84% after four years, before fully eroding. This suggests a cumulative that is 2.37 times the coefficient in the break-in-trend graph, implying a long-run employment elasticity for the type of minimum wage increases seen in the data of -0.064.

first-differences in the distributed lag model in Column (3). Perhaps more importantly, the estimated coefficient in Specification (5) changes little (and remains statistically equivalent) when jurisdiction trends are included in Panel [B]. We view this as further evidence that trends are not a confounding factor, but if anything, the slight increase in magnitude shows that estimates are biased *towards* zero when trends are omitted from these models.

4.3 Additional Specifications and Robustness Checks

In this section, we present a number of alternative specifications to assess the robustness of our empirical results. Most importantly, we perform the common leading-values falsification test for pre-treatment deviation in employment outcomes, thereby examining the validity of the key identifying assumption underlying the difference-in-difference methodology.[21] In addition, we show that our results are consistent for different time periods within our sample, and demonstrate invariance of our results to allowing for finer spatial and time controls, accounting for minimum wage inflation indexing, and dropping the years of the recent financial crisis. For these additional results, we present estimates using Specification 3, the distributed lag model in first-differences, and Specification 5, the Break-in-Trend model, as these two specifications most accurately identify the effect on growth in the Monte Carlo simulation.

Robustness checks using Specification 3 are in Table 4. Column (1) replicates the results from Column (3) of Table 3: Panel [A], for comparison. Columns (2)-(4) include either the first or second leading value of the minimum wage, or both. If increases in the minimum wage appear to have an effect on employment dynamics *before* their implementation – especially if contemporaneous changes lose their effect – then our results might be driven by unobserved trends. This is not the case: although some precision is lost, the contemporaneous and lagged minimum wage coefficients in Columns (2)-(4) remain close to those in Column (1), and the leading value terms are comparatively small and statistically insignificant. This strongly suggests that confounding trends leading to both lower job growth and higher minimum wages are not a factor. In Column (5), we allow the time effects to vary by Census Division, rather than Region; the coefficients remain similar to those in Column (1). Some precision is lost, though this to be expected – there are four Census Regions containing

[21] An additional approach to examine the potential endogeneity of minimum wage changes is to examine the results with different combinations of the time-varying covariates. Results from different combinations of time fixed effects (national versus Region versus Division) and other time-varying controls are stable in magnitude, sign, and significance, particularly across the specifications shown above to accurately reflect minimum wage effects, namely, distributed lags with first differences, dynamic panel, and break-in-trend.

nine Divisions, and the median Division includes only five states. In Column (6), we assess whether states that have shifted to indexing their minimum wage for inflation affect our results by dropping these observations. Results remain similarly unchanged. Finally, in Column (7), we evaluate the role of the 2008-2009 recession. Because we include time period fixed effects, the recent recession should not unduly affect our results. However, these two years of our panel additionally experienced several large and high frequency changes in real minimum wage levels, primarily resulting from the federal increases during these years (see Figure 2). As a check that these particular years are not overly influencing identification of the minimum wage term, we estimate specifications using only pre-2008 data. Again, the estimated effects are not meaningfully different from our main results, though the sum of the minimum wage terms is significant only at p = 0.13; this is somewhat unsurprising given that about fifteen percent of the observations are lost.

Table 5 presents the additional results for the Break in Trend model, Specification 5. Column (1) reproduces the main estimates from Table 3. In Column (2), we include an indicator which equals one if the nominal minimum wage changes the following period. In Columns (3)-(4), we include the leading value of the log of the minimum wage either two or three periods in advance.[22] Columns (5)-(7) present, respectively, results using Division-by-year fixed effects, observations without inflation-indexed minimum wages, and pre-2008 data only. As with the distributed lags of first-differences model, these alternative results reflect those in the baseline specification, and the break-in-trend model consistently indicates a statistically significant and economically meaningful effect of the minimum wage on employment growth. Coefficients for the leading indicator or values of the minimum wage again support the validity of the difference-in-differences identifying assumption in this context.

We additionally evaluate the sensitivity of the results to the time period used. For difference-in-differences estimates, there is nearly always a concern that results could be particular to the time-span included in the study. Generally, it seems most appropriate to use all available periods within a data set unless given a compelling reason to do otherwise.

[22]Note that we do not include a one-period leading value nor include multiple leads simultaneously. This is because there is explicit collinearity between the current and the lag of the minimum wage term. For simplicity, suppose that the true data-generating process is $Y_t = \beta_1 \ln(MW_t) + \epsilon_t$. Ordinarily, including $\ln(MW_{t+1})$ would show no effect in this regression. However, since $Y_t = (emp_t - emp_{t-1})$ and $\ln(MW_{t+1})$ is related to Y_{t+1}, which includes emp_t, adding a single-period lead introduces substantial endogeneity. This is not an issue for leads of at least two periods difference from each other. If the pre-trend identification assumption is violated, it is difficult to believe that it would not be apparent two periods prior as well. Moreover, including a binary variable for whether there is a change in the following period (as opposed to the actual minimum wage value) yields little indication that there is some negative shock that is correlated with both increases in the minimum wage and reductions in job growth.

However, such an approach cannot guarantee that estimated effects are not particular to the time period used. We evaluate results obtained from estimating Specification 5 separately for all possible subsample spans of two or more consecutive years in the BDS (1977-2011), yielding 595 point estimates using the BDS. We also examine the QCEW, which has 704 such periods.[23] Appendix B includes histograms of these coefficients. Sorted by magnitude, the median coefficient is -0.0322 in the BDS, and the first point estimate with a positive value is at the 96th percentile. This exercise indicates that the result of a negative job growth effect of the minimum wage is not simply an artifact of the time spans of data used in this study.

We also examine the effects of the minimum wage by industry, age group, and education level in Section A.2.2.

4.4 Discussion

Our results show that the minimum wage negatively affects employment and that this occurs over a period of several years. The results from the distributed lag specification in first differences suggest that a 10% permanent increase in the real minimum wage reduces employment by about 0.7 percent after three years. In the dynamic panel model, we leverage additional assumptions to estimate an employment elasticity of about -0.17 after three years and -0.20 in the long run. Taken at face value, our most restrictive model, the break-in-trend specification, suggests that a 10% permanent increase in the real minimum wage reduces job growth by about 0.3 percentage points annually, or about 15 percent of the baseline level. This effect is not small, and extrapolated sufficiently into the future this implies a deleterious effect on employment of enormous magnitude, far surpassing that of any historical recession. The purpose of this section is to caveat our findings and to place the results into perspective for considering the short- and long-run impact of minimum wage policy.

First, we study employment effects in the context of policies in the United States over the past few decades, during which increases in minimum wages have been relatively small in magnitude, albeit frequent. Extrapolating the effect we estimate to the distant future or to much larger increases in the minimum wage requires strong assumptions and is a wildly out-of-sample prediction, one that we refrain from making. Essentially, there is no way – without model-based assumptions – to gain a full understanding of the dynamic responses to a large, real and permanent increase in the minimum wage, because no such change has

[23] For an initial year of 1977, the BDS has 34 possible spans of at least two years: 1977-1978, 1977-1979, 1977-1980, ..., 1977-2011. An initial year of 1978 has 33 possible such spans, etc. For the QCEW, we could instead consider spans of quarters, but this would not add much in the way of inference. Note that the QWI is too short and too unbalanced to benefit from this exercise.

ever occurred in the data. This issue is not specific to our study, but does hamper the ability of researchers in this field to make definitive statements on the effects of these policies.

Second, our specifications estimate the relationship between the *real* minimum wage and employment. Historically, most minimum wage changes have been set in nominal terms and not adjusted for inflation. As we show in Appendix C, inflation substantially erodes the "bite" of a wage floor over time; this is not because nominal minimum wages *affect* employment less significantly than do real minimum wages but rather because the intensity of the policy itself is mitigated. This is illustrated in our brief discussion in Footnote 20, in which we apply the "erosion" rate of the treatment intensity to our break-in-trend specification to evaluate the effect of a nominal minimum wage increase.

The upshot of this distinction is that nominally-denoted minimum wages should have a smaller employment impact than a wage floor that is indexed for inflation. To date, little is known empirically about how inflation indexing may alter the effects of a minimum wage on employment even as at least ten states now use regional CPI measures to index their minimum wages for inflation, a relatively recent practice [Allegretto et al., 2011]. Ongoing minimum wage proposals, such as the federal minimum wage increase proposed by President Obama in 2013, continue to include provisions indexing the wage floor for inflation. As such, this line of inquiry is likely to grow in importance.

5 Conclusion

We examine whether the minimum wage impacts employment through a discrete change in its level or if it is reflected over time through a change in the growth rate. Much of the previous literature on the topic has assumed that an increase in the minimum wage results in a relatively rapid adjustment in employment. Yet, there are theoretical reasons to believe that this change may be slower. Using both illustrative models and Monte Carlo simulations, we show that the empirical specifications used in the prior literature will systematically err if the true effects are on growth rates. Moreover, we show that the common practice in this literature of including jurisdiction-specific time trends will bias estimates towards zero in this case.

We show results from three administrative data sets that consistently indicate negative effects of the minimum wage on job growth. Our results are robust to a number of specifications, and we find that the minimum wage reduces employment over a longer period of time than has been previously examined in the literature. This phenomenon is particularly important given the evidence that minimum wage jobs often result in relatively rapid transitions to higher-paying jobs [Even and Macpherson, 2003].

This paper, of course, does not settle the debate of a contentious topic, but we do shed light on the mechanisms by which the minimum wage affects employment and provide directions for future research delving more deeply into the dynamics of this relationship.

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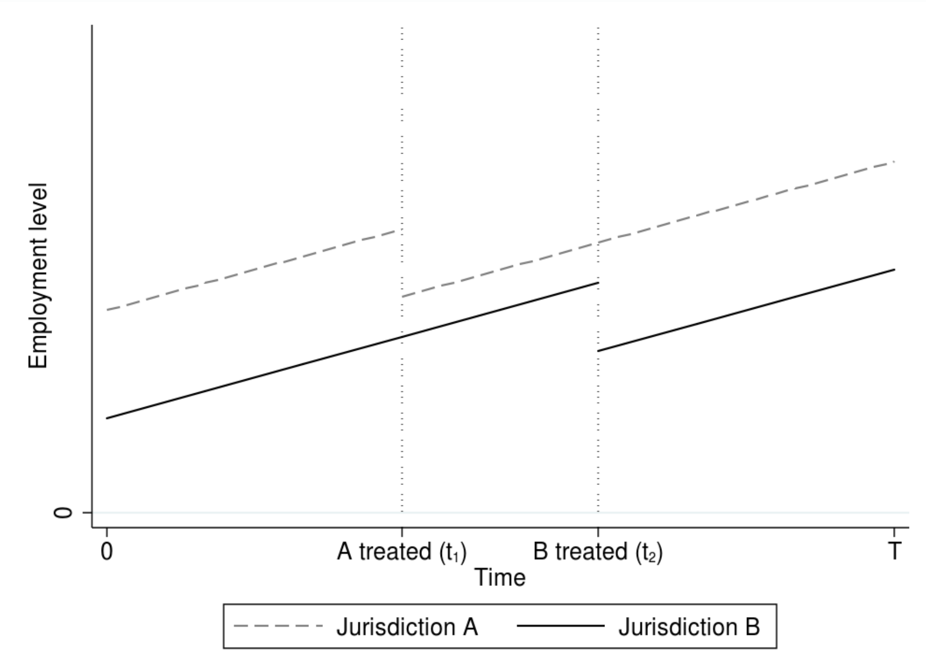
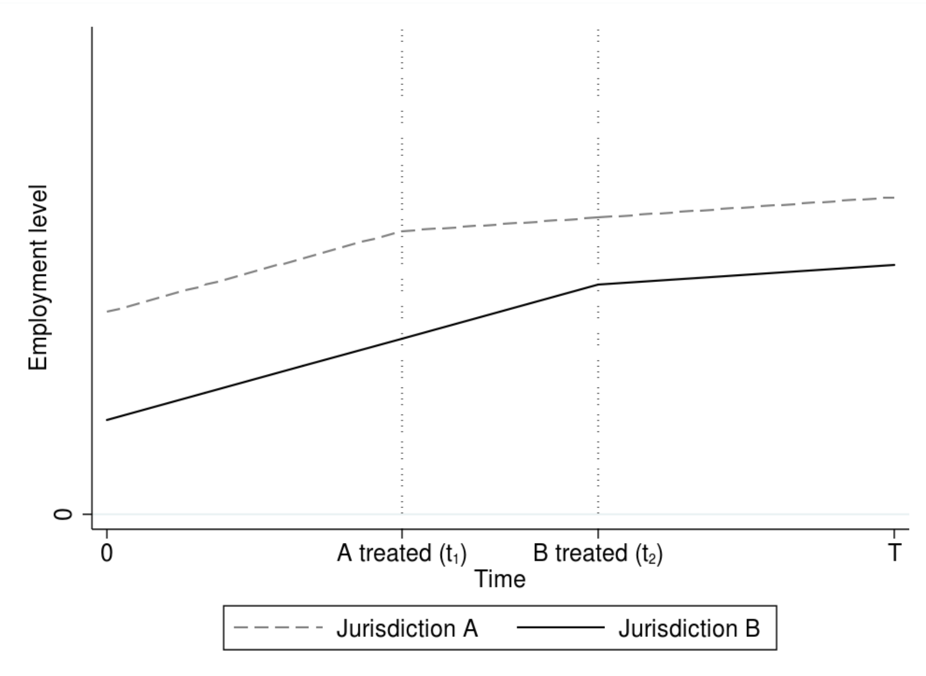


(a) Treatment effect discrete in levels



(b) Treatment effect discrete in growth

Figure 1: Illustration of two types of treatment effects with staggered treatments

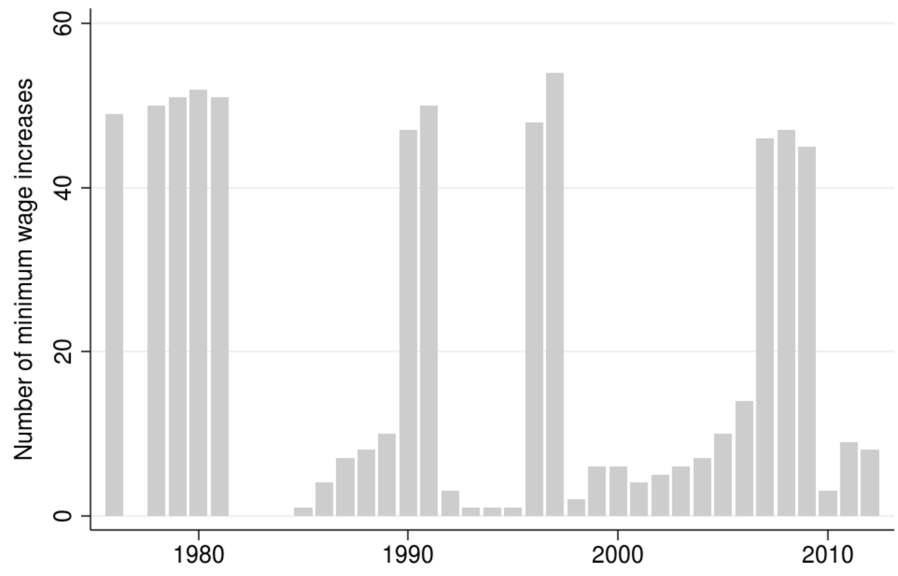


Figure 2: Frequency of increases to effective state nominal minimum wages (1976-2012)

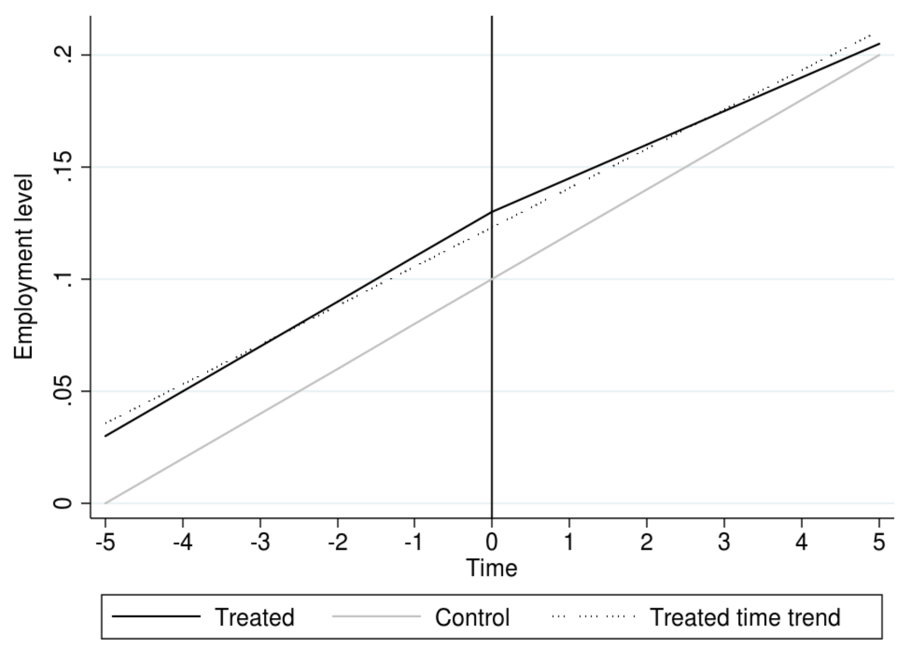
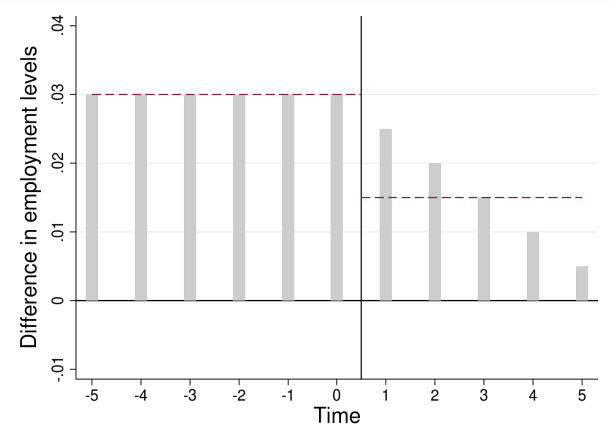
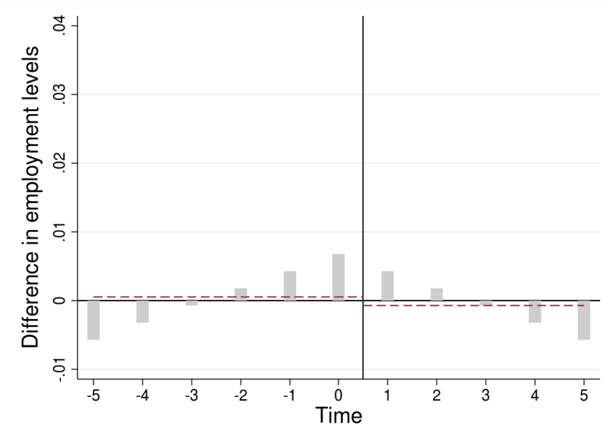


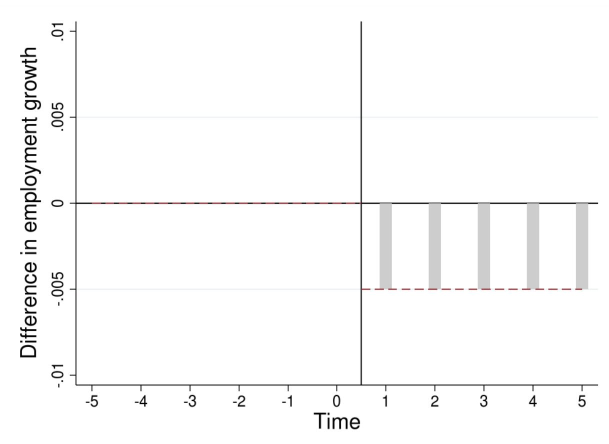
Figure 3: Simple example of disemployment effect in growth rate



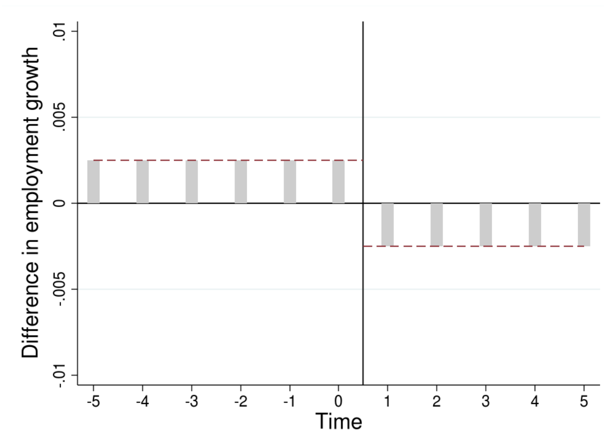
(a) Levels: without trends



(b) Levels: residual to trends



(c) Growth: without trends



(d) Growth: residual to trends

Figure 4: Example difference-in-differences without versus with jurisdiction time trends

Table 1: Estimates from Monte Carlo simulation exercise

	Simulated True Effect	Classic DiD	Distributed lag		Dynamic Panel	Break-in-Trend
			Levels	FD		
	(0)	(1)	(2)	(3)	(4)	(5)
[A] Without trends						
Log-MW	-0.0300	-0.2750	-0.0005	-0.0299	-0.0190	-0.0300
1st lag of log-MW	-0.0300		-0.0242	-0.0301	-0.0412	
2nd lag of log-MW	-0.0300		-0.0229	-0.0302		
3rd lag of log-MW	-0.0300		-0.2855	-0.0302		
1st lag of employment					0.852	
[B] Jurisdiction trends						
Log-MW	-0.0300	-0.0153	0.0310	-0.0164	0.0135	-0.0299
1st lag of log-MW	-0.0300		-0.0158	-0.0156	-0.0201	
2nd lag of log-MW	-0.0300		-0.0143	-0.0146		
3rd lag of log-MW	-0.0300		-0.0605	-0.0137		
1st lag of employment					0.593	

Notes: The data generating process simulates a true effect of *Normal*(-0.03, 0.015) relating the minimum wage to the first difference of employment. Columns (1) - (5), which correspond to Specifications (1) - (5) as discussed in Section 3, report the median coefficients from 10,000 Monte Carlo repetitions for each model, separately for specifications with and without jurisdiction-specific time trends.

Table 2: Summary statistics for state characteristics and employment

Business Dynamics Statistics (Annual, 1977 - 2011)			
	Mean	Std. Dev.	Median
State minimum wage ($)	4.40	1.360	4.25
State minimum wage ($real)	7.09	0.916	6.89
Jobs (thousands)	1888.0	2103.8	1224.9
Job growth (thousands)	27.2	85.59	15.4
Job growth (log)	0.017	0.0348	0.019
Job creation (thousands)	314.8	370.4	206.5
Job destruction (thousands)	282.0	337.3	180.1
State annual characteristics			
Population (thousands)	5160.6	5725.6	3513.4
Share aged 15-59	0.62	0.0196	0.62
GSP/capita ($real)	41591.6	16309.7	38447.1
Observations	1785		

Notes: We define each state's minimum wage annually as of March 12 in the BDS, using the maximum of the federal minimum wage and the state's minimum wage each period, drawn from state-level sources. Employment statistics are computed for the aggregate population of non-agricultural employees in each state. Job growth is the annual change in each state's employment level. All real dollar amounts are indexed to $2011 using the CPI-Urban.

Table 3: Estimated effect of the minimum wage on employment (Business Dynamics Statistics)

	Classic DiD (1)	Distributed lag: Levels (2)	Distributed lag: FD (3)	Dynamic Panel (4)	Break-in-Trend (5)
[A] Without trends					
Log-MW	-0.1693***	-0.0825***	-0.0204	-0.0309*	-0.0243***
	(0.0383)	(0.0233)	(0.0162)	(0.0171)	(0.0078)
1st lag of log-MW		-0.0524***	-0.0321**	-0.0543***	
		(0.0184)	(0.0139)	(0.0204)	
2nd lag of log-MW		-0.0503***	-0.0304**		
		(0.0131)	(0.0128)		
3rd lag of log-MW		-0.0552	0.0093		
		(0.0410)	(0.0147)		
1st lag of employment				0.5772***	
				(0.0960)	
[B] Jurisdiction trends					
Log-MW	-0.0125	0.0021	-0.0174	-0.0099	-0.0271**
	(0.0160)	(0.0159)	(0.0159)	(0.0159)	(0.0125)
1st lag of log-MW		-0.0166	-0.0278**	-0.0242	
		(0.0153)	(0.0136)	(0.0149)	
2nd lag of log-MW		-0.0161	-0.0258**		
		(0.0150)	(0.0127)		
3rd lag of log-MW		0.0384	0.0169		
		(0.0314)	(0.0136)		
1st lag of employment				0.3160***	
				(0.0925)	
Observations	1785	1632	1581	1683	1734

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Robust standard errors are clustered by state and reported in parentheses. Columns (1)-(5) correspond to Specifications (1) - (5) in Section 3; coefficients for each model are reported separately with and without jurisdiction-specific time trends. All specifications include Census Region by year fixed effects and state-level annual controls for log-population, the share aged 15-59, and log real gross state product per capita. Controls are first-differenced for columns (3)-(5), as in the corresponding specifications. Column (5) of Panel [A] also includes a control for the initial minimum wage in each state to ensure that the identifying variation is within rather than between states. See the text for discussion.

Table 4: Robustness checks for the effect of the minimum wage in the distributed lag first-differences model

	Baseline	Leading values tests			Division	Inflation	Pre-2008
	results	$t+1$	$t+2$	Both	time FE	indexing	only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log-MW	-0.0204	-0.0206	-0.0176	-0.0178	-0.0188	-0.0226	-0.0219
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.018)	(0.018)
1st lag of log-MW	-0.0321**	-0.0336**	-0.0267	-0.0283	-0.0310*	-0.0268	0.0020
	(0.014)	(0.014)	(0.017)	(0.017)	(0.018)	(0.017)	(0.023)
2nd lag of log-MW	-0.0304**	-0.0317**	-0.0337**	-0.0353**	-0.0244	-0.0343**	-0.0467***
	(0.013)	(0.013)	(0.014)	(0.014)	(0.015)	(0.016)	(0.017)
3rd lag of log-MW	0.0093	0.0084	0.0119	0.0109	0.0107	0.0074	0.0162
	(0.015)	(0.015)	(0.023)	(0.023)	(0.019)	(0.019)	(0.023)
Sum MW effects	**-0.0736****	**-0.0776****	**-0.0660**	**-0.0705**	**-0.0634**	**-0.0764****	**-0.0504**
	(0.036)	**(0.038)**	**(0.046)**	**(0.049)**	**(0.051)**	**(0.034)**	**(0.033)**
1st lead of log-MW		-0.0075		-0.0073			
		(0.008)		(0.010)			
2nd lead of log-MW			0.0060	0.0057			
			(0.012)	(0.012)			
Observations	1581	1581	1530	1530	1581	1536	1377

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Column (1) replicates Specification (3) without trends from Table 3. Separately: Columns (2) - (4) include, respectively, the leading value of the log minimum wage at time t+1 or t+2, or both. Column (5) uses Division-by-time fixed effects, rather than Region-by-time. Column (6) drops the observations with an inflation-indexed state minimum wage, and Column (7) uses only pre-2008 data.

Table 5: Robustness checks for the effect of the minimum wage in the break-in-trend model

	Baseline results	Leading values tests			Division time FE	Inflation indexing	Pre-2008 only
		$t+1$	$t+2$	Both			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log-MW	-0.0243***	-0.0240***	-0.0221**	-0.0259**	-0.0321**	-0.0286***	-0.0261***
	(0.008)	(0.008)	(0.011)	(0.011)	(0.012)	(0.008)	(0.008)
I(ΔMW_{t+1})		-0.0019					
		(0.002)					
2nd lead of log-MW			-0.0043				
			(0.007)				
3rd lead of log-MW				0.0018			
				(0.007)			
Observations	1734	1734	1683	1632	1734	1689	1530

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Column (1) replicates Specification (5) without trends from Table 3. Separately: Column (2) adds an indicator equal to one if the nominal minimum wage increases in the following period. Columns (3) - (4) include, respectively, the leading value of the log minimum wage at time t+2 or t+3. Column (5) uses Division-by-time fixed effects, rather than Region-by-time. Column (6) drops the observations with an inflation-indexed state minimum wage, and Column (7) uses only pre-2008 data.

A Results using additional data sets

In this appendix we provide empirical results similar to those in the main text, but using data from the Quarterly Census of Employment and Wages and the Quarterly Workforce Indicators, rather than from the Business Dynamics Statistics. The results are consonant with those in Section 4.2. Note that these data are quarterly rather than annual. As such, additional lags are included in the distributed lag models to cover the same span as the annual specifications from the BDS, and the break-in-trend models are the effects on quarterly rather than annual growth.

A.1 Quarterly Census of Employment and Wages (QCEW)

The *Quarterly Census of Employment and Wages* (QCEW), housed at the Bureau of Labor Statistics, is a program which originated in the 1930s to tabulate employment and wages of establishments which report to the Unemployment Insurance (UI) programs of the United States. Per the BLS, employment covered by these UI programs today represents about 99.7% of all wage and salary civilian employment in the country (including public sector employment). The BLS currently reports QCEW data by state for each quarter during 1975-2012, a span slightly longer than that of the BDS.[24] The data are disaggregated by NAICS industry codes for 1990-2012.

A.1.1 Results

In Tables A.2 and A.3, we compare results for the five specifications discussed in Section 3, both with and without state-specific time trends. As with the Business Dynamics Statistics, we find that the classic difference-in-differences specification yields a negative and statistically significant elasticity of -0.14 that is reduced to 0.001 when trends are included. Similarly, the sum of the coefficients in a distributed lag model with fixed effects is reduced from a statistically significant -0.25 to a small and insignificant -0.033. Similar to the BDS, distributed lag models estimated with first differences produce approximately the same result. The estimated elasticity is nearly identical to that found in the BDS at about -0.075 (s.e. = 0.034). We include four minimum wage terms in the dynamic panel model and produce a predicted permanent effect of -0.11 (s.e. = 0.043); as expected, the effects are smaller in magnitude when trends are included.[25] Finally, the break-in-trend model yields similar coefficients with and without time trends. Since the QCEW is a quarterly data set, the coefficient must be scaled for comparison to the BDS. Doing so produces an estimated effect on the growth rate of -0.034, quite similar to that found in Table 3.

[24] Employment levels – and therefore also quarterly job growth rates – are not available in the QCEW for Alaska and the District of Columbia for any quarters during 1978-1980. Employment data is not missing for any other states or periods.

[25] These results are qualitatively and quantitatively similar when additional lags of the minimum wage are included.

We conduct the same set of robustness checks on the QCEW results as we did on the BDS in Tables 4 and 5. These can be found in Tables A.6 and A.7. The leading values tend to be small and *positive*, suggesting that confounding trends leading to both lower job growth and higher minimum wages are not at work.

Taken together, the results from the Quarterly Census of Employment and Wages are in line with those that use the Business Dynamics Statistics and provide further evidence that the effects of the minimum wage are on growth.

A.2 Quarterly Workforce Indicators

The *Quarterly Workforce Indicators* are data provided as part of the Longitudinal Employer-Household Dynamics (LEHD) program by the Bureau of the Census. Similar to the QCEW, these data originate from county employment insurance filings.[26] Compared to the QCEW or BDS, an advantage of the QWI is that these data offer finer measures of employees demographics such as age.

Yet, for our research design, a major shortcoming of the QWI is the substantially shorter – and highly unbalanced – length of the panel. At its onset in 1990, only four states opted into the QWI program, and additional states gradually joined each year (except 1992) through 2004. From 2004 on, the QWI includes forty-nine states (Massachusetts and Washington, D.C. are never included). Thus, the starting date for QWI participation varies considerably across states, and many are relatively recent. This is a particular concern for the distributed lag models, as including sixteen minimum wage terms reduces the sample size by over twenty percent.

Because the QWI is a highly unbalanced state panel, we make several other minor adjustments to certain specifications. In particular, for results in the QWI using the differenced Specifications 3 and 5 with included jurisdiction time trends – i.e. those in the "with trends" panel – we include both a jurisdiction fixed effect and a jurisdiction-by-year variable for each jurisdiction. Unlike in a balanced panel such as the BDS or QCEW, in the QWI these terms are not perfectly collinear. In fact, both terms are necessary in order to appropriately control for jurisdiction time trends. Recall that the motivation for including jurisdiction time trend terms is to control for any underlying heterogeneity in the time paths for jurisdictions' employment. But, in an unbalanced panel the jurisdiction time trend terms will be sensitive to the *representation* of jurisdictions across time. This means that jurisdictions will differ in their time trend terms simply because of *when* the jurisdictions are represented in the panel, irrespective of any actual economic differences between jurisdictions. Intuitively, this issue is resolved by including both a jurisdiction fixed effect (which accounts for differing representation) and also a jurisdiction-by-quarter term in the differenced specifications for the QWI. In addition, when using the initial minimum wage as a control in the Break-in-Trend model (without trends), we use the 1990 value for each state (1990 being the earliest year for any jurisdiction in the QWI, though, in practice, this choice of year does not seem to matter).

[26] In fact, the QWI and QCEW originate identically from the same county unemployment insurance records. Thus, differences in the data stem from either the periods during which each state or county is included, or differing imputation methods employed by BLS versus Census [Abowd and Vilhuber, 2013].

A.2.1 Results

We conduct a similar exercise to that in Section A.1.1 with the QWI, presenting results in Tables A.4 and A.5. We again note that a negative and statistically significant elasticity from the classic difference-in-differences specification is reduced to zero when trends are included; while the sum of coefficients in the distributed lag model in levels is not statistically significant, it too is attenuated when trends are included. But as with the QCEW above, the distributed lag model in first differences produces a negative and statistically significant long-run coefficient of -0.10 with and without trends. The dynamic panel model yields a predicted permanent effect of -0.053 (s.e. = 0.025); while none of the individual coefficients are statistically significant, the overall effect is significant at p = 0.037. Finally, the break-in-trend model yields a statistically significant coefficient of -0.0076, much like that from both the QCEW and, when scaled to an annual effect of -0.030, the BDS.

As with the QCEW, the results from the QWI perform well in the robustness checks in Tables A.6 and A.7. As expected, some precision is lost in the leading-values tests, but the magnitudes are consistent across the various specifications.

The results from the Quarterly Workforce Indicators, especially in conjunction with those from the QCEW above, underscore that our findings are not driven by the use of the Business Dynamics Statistics.

A.2.2 Industry, Age, and Education

To this point, we have presented results for virtually the entire workforce, including workers of all ages in all industries. In this section, we disaggregate the effect on job growth rates by industry, age group, and educational attainment (for adults 25 years and older). The BDS does not report separate employment outcomes by state and industry, but these are disaggregated in the QCEW and QWI. The QWI additionally reports outcomes by age group and education level. In Table A.8, we estimate the effects of the minimum wage in different industries (two-digit NAICS code), focusing on the break-in-trend model (with jurisdiction time trends) for brevity.[27] Much of the literature focuses on one or several industries that are conjectured to be more responsive to changes in the minimum wage. Echoing points made in Clemens and Wither [2014], we choose to show all industries as it is not necessarily clear which particular *industry codes* ought not to be sensitive to the minimum wage. That said, industries that tend to have a higher concentration of low-wage jobs show more deleterious effects on job growth from higher minimum wages, and the results appear consistent between the QCEW and QWI.[28] Of the 40 coefficients we report in the two data sets, none are positive and statistically significant.

Table A.9 shows the effects of higher minimum wages by age bin reported in the QWI using the break-in-trend model. As one might expect, the effects are by far the strongest on

[27] See http://www.naics.com/search.htm for a full list of the component industries of each category.

[28] It may seem anomalous that professional services would be negatively affected, but firms in this category span a broad array, from lawyers' offices to direct mail advertising. The large negative effect on the offices of holding companies ("management") is perhaps stranger; note, though, that the effect is only present in the QCEW and that this category has among the fewest firms of any industry.

those aged 14 to 18, twice the size of the effect on those age 19 to 21 and over three times the size of the effect on those age 22 to 24. By age 35, the effects are very small and insignificant; they rise again but remain statistically insignificant for those over age 65. Similarly, Table A.10 shows results by education level for adults aged 25 and over. Since the effects of the minimum wage are heavily concentrated among younger workers, the overall effect of the minimum wage is smaller for this group. Yet the effects are strongest for those who do not have a high school diploma, with significant but smaller effects seen on high school diploma and GED recipients. The effects diminish to an insignificant -0.0015 for those with a college degree or more. These results, for both age and education, are in line with the expectation that the minimum wage will reduce job prospects for those with less skill and experience.

A.3 Tables

Table A.1: Summary statistics for state characteristics and employment outcomes in three administrative data sets

	BDS Annual, 1977 - 2011			QCEW Quarterly, 1975 - 2012			QWI Quarterly, varies - 2012*		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
State minimum wage ($)	4.40	1.360	4.25	4.53	1.535	4.25	5.86	1.094	5.15
State minimum wage ($real)	7.09	0.916	6.89	7.28	0.975	7.05	6.89	0.729	6.85
Employment variables:									
Jobs (thousands)	1888.0	2103.8	1224.9	2167.9	2402.9	1441.7	2621.4	2794.2	1763.7
Job growth (thousands)	27.2	85.59	15.4	8.77	65.04	4.28	4.35	74.03	6.15
Job growth (log)	0.017	0.0348	0.019	0.0051	0.0256	0.0049	0.0019	0.0241	0.0061
Job creation (thousands)	314.8	370.4	206.5				573.9	633.3	384.7
Job destruction (thousands)	282.0	337.3	180.1				548.5	613.3	367.3
State annual covariates:									
Population (thousands)	5160.6	5725.6	3513.4	5138.0	5704.7	3502.0	6136.5	6784.5	4343.4
Share aged 15-59	0.62	0.0196	0.62	0.62	0.0199	0.62	0.62	0.0145	0.62
GSP/capita ($real)	41,592	16,310	38,447	41,302	16,334	38,148	45,345	8384	43,969
Observations	1785			7752			3029		

Notes: We define each state's minimum wage annually as of March 12 in the BDS, and as of the first date for each quarter in the QCEW and QWI. We use the maximum of the federal minimum wage and the state's minimum wage each period, drawn from state-level sources. Employment statistics are computed for the aggregate population of non-agricultural employees in each state for each of the three listed data sets. Job growth is the change in each state's employment level from one time period to the next. We use job growth and employment outcomes annually for the BDS and quarterly for the QCEW and QWI. The QCEW does not report gross job creation or destruction. All real dollar amounts are indexed to $2011 using the CPI-Urban. The QWI is a highly unbalanced panel, beginning with only four states in 1990 and gradually expanding until forty-nine states had joined by 2004. We include all available state-quarters of the QWI.

Table A.2: Estimated effect of the minimum wage on employment (QCEW): [**A**] **Without trends**

	Classic	Distributed lag		Dynamic	Break-in-
	DiD	Levels	FD	Panel	Trend
	(1)	(2)	(3)	(4)	(5)
Log-MW	-0.1391***	-0.0270	-0.0070	-0.0075	-0.0096***
	(0.0473)	(0.0247)	(0.0085)	(0.0100)	(0.0019)
1st lag of log-MW		-0.0110	-0.0069	-0.0025	
		(0.0085)	(0.0079)	(0.0107)	
2nd lag of log-MW		-0.0082	-0.0018	0.0031	
		(0.0080)	(0.0071)	(0.0108)	
3rd lag of log-MW		-0.0024	0.0044	-0.0227***	
		(0.0121)	(0.0091)	(0.0070)	
4th lag of log-MW		-0.0124*	-0.0097*		
		(0.0071)	(0.0053)		
5th lag of log-MW		-0.0195**	-0.0169**		
		(0.0074)	(0.0072)		
6th lag of log-MW		-0.0096	-0.0026		
		(0.0070)	(0.0059)		
7th lag of log-MW		-0.0108	-0.0054		
		(0.0078)	(0.0071)		
8th lag of log-MW		-0.0103	-0.0063		
		(0.0063)	(0.0054)		
9th lag of log-MW		-0.0186**	-0.0100		
		(0.0070)	(0.0065)		
10th lag of log-MW		-0.0026	0.0000		
		(0.0058)	(0.0051)		
11th lag of log-MW		0.0158	0.0015		
		(0.0097)	(0.0061)		
12th lag of log-MW		-0.0253***	-0.0119*		
		(0.0091)	(0.0062)		
13th lag of log-MW		-0.0220**	-0.0062		
		(0.0100)	(0.0078)		
14th lag of log-MW		-0.0145	-0.0050		
		(0.0087)	(0.0070)		
15th lag of log-MW		-0.0731***	0.0089		
		(0.0267)	(0.0074)		
Sum MW effects		-0.2515***	-0.0749**		
		(0.0656)	(0.0344)		
1st lag of log-emp.				0.7383***	
				(0.0608)	
Observations	7728	6969	6918	7520	7522

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Robust standard errors are clustered by state and reported in parentheses. Columns/specifications correspond to those for the BDS in Table 3. See Table 3 notes and the text for additional discussion.

Table A.3: Estimated effect of the minimum wage on employment (QCEW): **[B] With trends**

	Classic	Distributed lag		Dynamic	Break-in-
	DiD	Levels	FD	Panel	Trend
	(1)	(2)	(3)	(4)	(5)
Log-MW	0.0010	0.0081	-0.0069	0.0010	-0.0084***
	(0.0171)	(0.0153)	(0.0085)	(0.0123)	(0.0031)
1st lag of log-MW		-0.0047	-0.0069	-0.0037	
		(0.0092)	(0.0080)	(0.0098)	
2nd lag of log-MW		-0.0021	-0.0019	0.0024	
		(0.0078)	(0.0071)	(0.0090)	
3rd lag of log-MW		0.0071	0.0044	-0.0147*	
		(0.0097)	(0.0091)	(0.0085)	
4th lag of log-MW		-0.0109	-0.0096*		
		(0.0067)	(0.0053)		
5th lag of log-MW		-0.0137	-0.0168**		
		(0.0083)	(0.0073)		
6th lag of log-MW		0.0006	-0.0023		
		(0.0064)	(0.0059)		
7th lag of log-MW		-0.0073	-0.0051		
		(0.0068)	(0.0071)		
8th lag of log-MW		-0.0019	-0.0059		
		(0.0063)	(0.0054)		
9th lag of log-MW		-0.0139*	-0.0097		
		(0.0074)	(0.0066)		
10th lag of log-MW		0.0007	0.0002		
		(0.0055)	(0.0051)		
11th lag of log-MW		0.0016	0.0017		
		(0.0076)	(0.0060)		
12th lag of log-MW		-0.0040	-0.0116*		
		(0.0086)	(0.0063)		
13th lag of log-MW		-0.0052	-0.0058		
		(0.0083)	(0.0078)		
14th lag of log-MW		0.0000	-0.0044		
		(0.0078)	(0.0069)		
15th lag of log-MW		0.0126	0.0095		
		(0.0122)	(0.0073)		
Sum MW effects		-0.0328	-0.0711*		
		(0.0398)	(0.0357)		
1st lag of log-emp.				0.4190***	
				(0.0682)	
Observations	7728	6969	6918	7520	7522

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Robust standard errors are clustered by state and reported in parentheses. Columns/specifications correspond to those for the BDS in Table 3. See Table 3 notes and the text for additional discussion.

Table A.4: Estimated effect of the minimum wage on employment (QWI): [A] **Without trends**

	Classic	Distributed lag		Dynamic	Break-in-
	DiD	Levels	FD	Panel	Trend
	(1)	(2)	(3)	(4)	(5)
Log-MW	-0.0447*	-0.0018	-0.0007	0.0055	-0.0076***
	(0.0231)	(0.0205)	(0.0141)	(0.0144)	(0.0024)
1st lag of log-MW		-0.0128	-0.0065	-0.0117	
		(0.0110)	(0.0103)	(0.0178)	
2nd lag of log-MW		-0.0103	-0.0196	-0.0074	
		(0.0134)	(0.0121)	(0.0120)	
3rd lag of log-MW		0.0130	0.0072	-0.0078	
		(0.0124)	(0.0122)	(0.0158)	
4th lag of log-MW		0.0080	-0.0009		
		(0.0107)	(0.0083)		
5th lag of log-MW		-0.0090	-0.0184*		
		(0.0110)	(0.0098)		
6th lag of log-MW		-0.0072	-0.0097		
		(0.0112)	(0.0108)		
7th lag of log-MW		-0.0060	-0.0069		
		(0.0138)	(0.0131)		
8th lag of log-MW		0.0044	-0.0010		
		(0.0126)	(0.0103)		
9th lag of log-MW		-0.0237**	-0.0162		
		(0.0107)	(0.0097)		
10th lag of log-MW		-0.0056	-0.0014		
		(0.0095)	(0.0078)		
11th lag of log-MW		0.0062	0.0028		
		(0.0104)	(0.0086)		
12th lag of log-MW		0.0053	-0.0065		
		(0.0112)	(0.0098)		
13th lag of log-MW		-0.0085	-0.0097		
		(0.0113)	(0.0111)		
14th lag of log-MW		-0.0128	-0.0106		
		(0.0100)	(0.0090)		
15th lag of log-MW		-0.0014	-0.0043		
		(0.0192)	(0.0120)		
Sum MW effects		-0.0623	-0.1024**		
		(0.0458)	(0.0391)		
1st lag of log-emp.				0.5927***	
				(0.0720)	
Observations	3029	2294	2245	2833	2833

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Robust standard errors are clustered by state and reported in parentheses. Columns/specifications correspond to those for the BDS in Table 3. See Table 3 notes and the text for additional discussion.

Table A.5: Estimated effect of the minimum wage on employment (QWI): **[B] With trends**

	Classic DiD (1)	Distributed lag: Levels (2)	Distributed lag: FD (3)	Dynamic Panel (4)	Break-in-Trend (5)
Log-MW	-0.0071	0.0132	-0.0018	0.0124	-0.0099**
	(0.0147)	(0.0183)	(0.0137)	(0.0145)	(0.0047)
1st lag of log-MW		-0.0121	-0.0081	-0.0122	
		(0.0112)	(0.0109)	(0.0158)	
2nd lag of log-MW		-0.0129	-0.0211	-0.0091	
		(0.0125)	(0.0128)	(0.0116)	
3rd lag of log-MW		0.0158	0.0054	-0.0030	
		(0.0127)	(0.0119)	(0.0170)	
4th lag of log-MW		0.0075	-0.0024		
		(0.0099)	(0.0080)		
5th lag of log-MW		-0.0133	-0.0185*		
		(0.0095)	(0.0096)		
6th lag of log-MW		-0.0052	-0.0098		
		(0.0112)	(0.0101)		
7th lag of log-MW		-0.0038	-0.0071		
		(0.0141)	(0.0143)		
8th lag of log-MW		0.0063	-0.0010		
		(0.0098)	(0.0106)		
9th lag of log-MW		-0.0179*	-0.0165		
		(0.0096)	(0.0101)		
10th lag of log-MW		-0.0011	-0.0014		
		(0.0086)	(0.0080)		
11th lag of log-MW		0.0085	0.0029		
		(0.0096)	(0.0091)		
12th lag of log-MW		0.0034	-0.0062		
		(0.0106)	(0.0098)		
13th lag of log-MW		-0.0107	-0.0096		
		(0.0111)	(0.0115)		
14th lag of log-MW		-0.0076	-0.0101		
		(0.0099)	(0.0096)		
15th lag of log-MW		0.0089	-0.0027		
		(0.0144)	(0.0127)		
Sum MW effects		-0.0210	-0.1080**		
		(0.0399)	(0.0438)		
1st lag of log-emp.				0.3940***	
				(0.0581)	
Observations	3029	2294	2245	2833	2833

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Robust standard errors are clustered by state and reported in parentheses. Columns/specifications correspond to those for the BDS in Table 3. See Table 3 notes and the text for additional discussion.

Table A.6: Robustness checks for the distributed lag first-differences model (QCEW and QWI)

	Baseline results	Leading values tests			Division time FE	Inflation indexing	Pre-2008 only
		$t+1$	$t+2$	Both			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
[A] QCEW							
Sum MW effects	-0.0749**	-0.0700*	-0.0750**	-0.0659*	-0.0551	-0.0559*	-0.0700*
	(0.0344)	(0.0368)	(0.0358)	(0.0367)	(0.0342)	(0.0331)	(0.0355)
1st lead of log-MW		0.0143		0.0152			
		(0.010)		(0.010)			
2nd lead of log-MW			0.0069	0.0085			
			(0.006)	(0.006)			
Observations	6918	6867	6816	6816	6918	6698	5898
[B] QWI							
Sum MW effects	-0.1024**	-0.0956**	-0.1106***	-0.1010**	-0.0765**	-0.0695*	-0.1180**
	(0.0391)	(0.0371)	(0.0382)	(0.0393)	(0.0361)	(0.0354)	(0.0490)
1st lead of log-MW		0.0160		0.0148			
		(0.013)		(0.012)			
2nd lead of log-MW			-0.0074	-0.0060			
			(0.011)	(0.011)			
Observations	2245	2196	2147	2147	2245	2026	1366

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Column (1) replicates Specification (3) without trends from Tables A.2 and A.4. Separately: Columns (2) - (4) include, respectively, the leading value of the log minimum wage at time t+1 or t+2, or both. Column (5) uses Division-by-time fixed effects, rather than Region-by-time. Column (6) drops the observations with an inflation-indexed state minimum wage, and Column (7) uses only pre-2008 data.

Table A.7: Robustness checks for the break-in-trend model (QCEW and QWI)

	Baseline results	Leading values tests			Division time FE	Inflation indexing	Pre-2008 only
		Indicator	$t+2$	$t+3$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
[A] QCEW							
Log-MW	-0.0096***	-0.0097***	-0.0211***	-0.0137***	-0.0107***	-0.0097***	-0.0086***
	(0.002)	(0.002)	(0.007)	(0.004)	(0.003)	(0.002)	(0.002)
I(ΔMW_{t+1})		0.0022					
		(0.002)					
2nd lead of log-MW			0.0123*				
			(0.007)				
3rd lead of log-MW				0.0047			
				(0.003)			
Observations	7522	7471	7420	7369	7522	7302	6502
[B] QWI							
Log-MW	-0.0076***	-0.0061**	-0.0101	-0.0068	-0.0078**	-0.0064***	-0.0059***
	(0.002)	(0.002)	(0.008)	(0.006)	(0.004)	(0.002)	(0.002)
I(ΔMW_{t+1})		0.0044*					
		(0.003)					
2nd lead of log-MW			0.0029				
			(0.007)				
3rd lead of log-MW				-0.0018			
				(0.005)			
Observations	2833	2784	2735	2686	2833	2613	1953

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Column (1) replicates Specification (5) without trends from Tables A.3 and A.5. Separately: Column (2) adds an indicator equal to one if the nominal minimum wage increases in the following period. Columns (3) - (4) include, respectively, the leading value of the log minimum wage at time t+2 or t+3. Column (5) uses Division-by-time fixed effects, rather than Region-by-time. Column (6) drops the observations with an inflation-indexed state minimum wage, and Column (7) uses only pre-2008 data only.

Table A.8: Effect of the minimum wage by industry (Break-in-Trend model)

		QCEW		**QWI**	
		Coef.	Std. Err.	Coef.	Std. Err.
Industry	All: full QCEW (1975-2012)	-0.0084***	(0.0031)		
(NAICS)	All: NAICS available (1990-)	-0.0085**	(0.0038)	-0.0099**	(0.0047)
	11: Agriculture and wildlife	-0.0024	(0.0179)	0.0092	(0.0254)
	21: Mining	-0.0392	(0.0257)	0.0028	(0.0211)
	22: Utilities	-0.0051	(0.0167)	-0.0094	(0.0154)
	23: Construction	-0.0237*	(0.0132)	-0.0314*	(0.0175)
	31-33: Manufacturing	0.0021	(0.0105)	0.0011	(0.0096)
	42: Wholesale trade	-0.0039	(0.0042)	-0.0108	(0.0067)
	44-45: Retail trade	-0.0061	(0.0042)	-0.0126**	(0.0054)
	48-49: Transportation and warehouse	-0.0213*	(0.0120)	-0.0037	(0.0058)
	51: Information service	-0.0065	(0.0143)	0.0024	(0.0095)
	52: Finance and insurance	-0.0033	(0.0048)	-0.0052	(0.0085)
	53: Real estate	-0.0073	(0.0050)	-0.0060	(0.0070)
	54: Professional service	-0.0157***	(0.0049)	-0.0324***	(0.0106)
	55: Management	-0.0566**	(0.0243)	-0.0359	(0.0586)
	56: Administrative support	-0.0197**	(0.0085)	-0.0203*	(0.0108)
	61: Education related	0.0345	(0.0238)	0.0173	(0.0151)
	62: Health care	0.0004	(0.0025)	-0.0007	(0.0045)
	71: Arts and entertainment	-0.0350*	(0.0178)	-0.0314	(0.0188)
	72: Accommodation and food	-0.0140**	(0.0069)	-0.0215**	(0.0088)
	81: Other service	-0.0453	(0.0299)	-0.0080	(0.0074)
	92: Public administration	-0.0083	(0.0078)	-0.0128	(0.0115)
	Observations	4437		2833	

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Each coefficient represents a separate regression of the first difference in employment for that industry on the natural log of a state's real minimum wage, using Specification 5 with trends from Table A.3. Robust standard errors are clustered by state and reported in parentheses. The QCEW provides consistent (NAICS) industry codes beginning in 1990.

Table A.9: Effect of the minimum wage by age group (Break-in-Trend model)

	QWI	
	Coef.	Std. Err.
All: 14-99	-0.0099**	(0.0047)
14-18	-0.0459***	(0.0163)
19-21	-0.0223*	(0.0124)
22-24	-0.0156**	(0.0068)
25-34	-0.0102**	(0.0048)
35-44	-0.0031	(0.0046)
45-54	-0.0028	(0.0039)
55-64	-0.0039	(0.0035)
65-99	-0.0084	(0.0060)
Observations	4437	

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Each coefficient represents a separate regression of the first difference in employment for that age group on the natural log of a state's real minimum wage, using Specification 5 with trends from Table A.5. Robust standard errors are clustered by state and reported in parentheses.

Table A.10: Effect of the minimum wage by education level (Break-in-Trend model)

	QWI	
	Coef.	Std. Err.
All education levels	-0.0053	(0.0041)
Less than high school	-0.0100*	(0.0058)
High school or equivalent	-0.0073*	(0.0042)
Some college or associates	-0.0056	(0.0038)
Bachelors or higher degree	-0.0015	(0.0043)
Observations	4437	

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Notes: Each coefficient represents a separate regression of the first difference in employment for that education level (for adults aged 25 or older) on the natural log of a state's real minimum wage, using Specification 5 with trends from Table A.5. Robust standard errors are clustered by state and reported in parentheses.

B Robustness of Estimates to Time Period Included

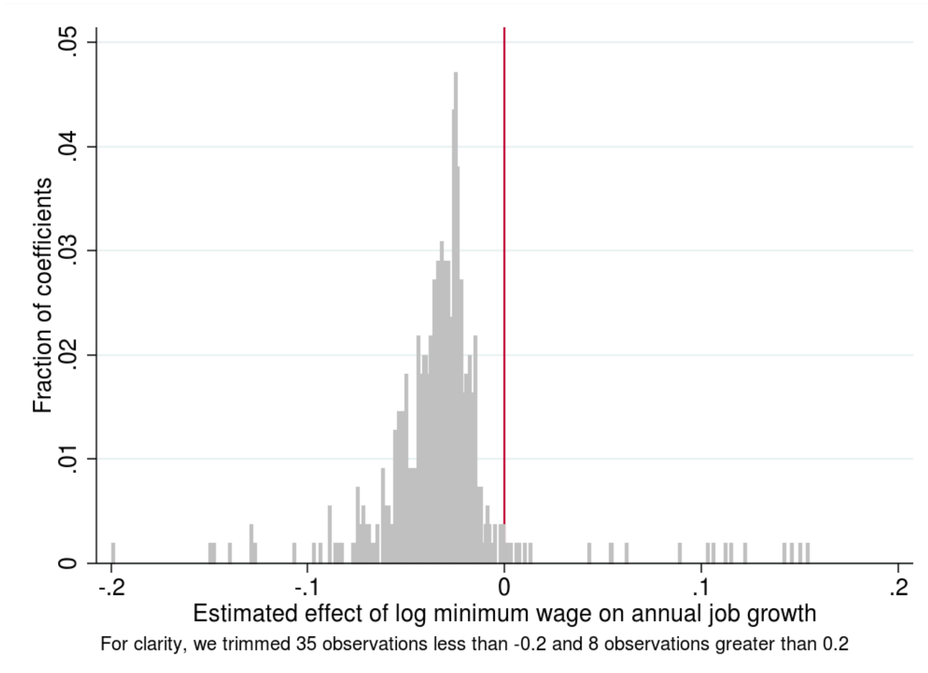


Figure B.1: Distribution of point estimates from subsamples of time-spans in the BDS

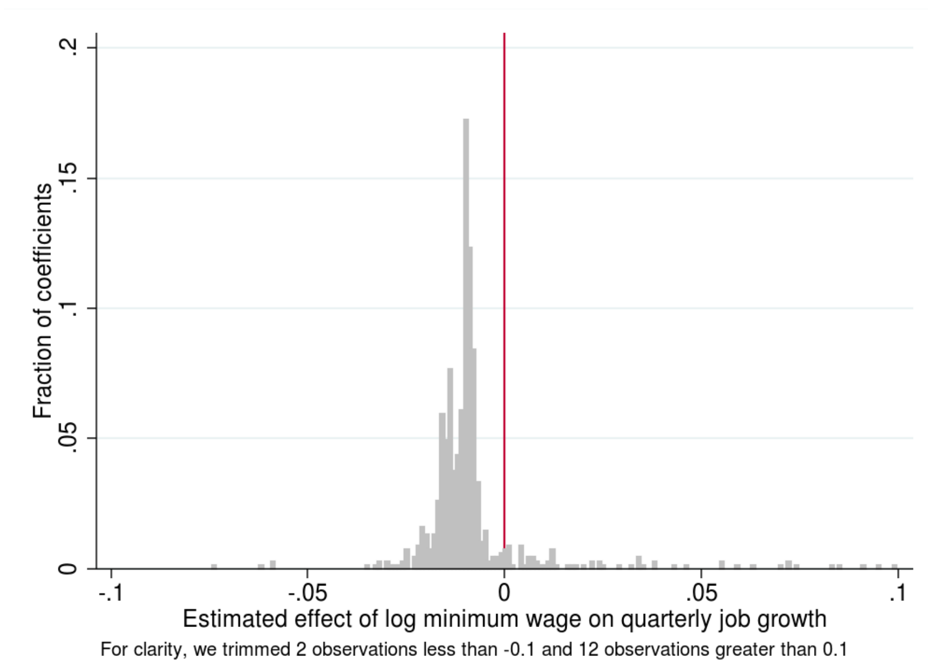


Figure B.2: Distribution of point estimates from subsamples of time-spans in the QCEW

C Historical Minimum Wage Increases and Erosion

Historically, minimum wages have been set in nominal dollars and not adjusted for inflation, so any nominal wage differential between two jurisdictions will become economically less meaningful over time. This appendix section presents some figures depicting the frequency and magnitude of minimum wage changes – and their subsequent erosion due to inflation. Looking first only within-state, Figure C.6 shows that the mean real state minimum wage increase during 1976-2012 was 55 cents (the median was also 55 cents). By the time the same state next increased its real minimum wage, which took 54 months on average, the previous increase in minimum wage had eroded – via inflation – to an average cumulative real *decrease* of 11 cents (median -12 cents, see Figure C.7). In fact, Figure C.8 shows that the 62 percent of state-year real minimum wage increases that were eventually fully eroded by inflation did so in, on average, twenty-two months, and the median time elapsed was only sixteen months. Turning instead to comparisons within Census Region, the mean *relative* real increase in state minimum wage was 25 cents (median 13 cents, Figure C.9). By the time of the next within-state increase, the prior increase had eroded – both via inflation and from other regional neighbors changing their minimum wages – to an average decrease of 1 cents (median +2 cents, Figure C.10). For those 47 percent of state-year increases which fully eroded relative to regional states, this took only 17 months on average (median 12 months, Figure C.11). This exercise demonstrates that there is a relatively short duration of time during which a state difference-in-differences estimation can identify the effects of the minimum wage on employment levels.

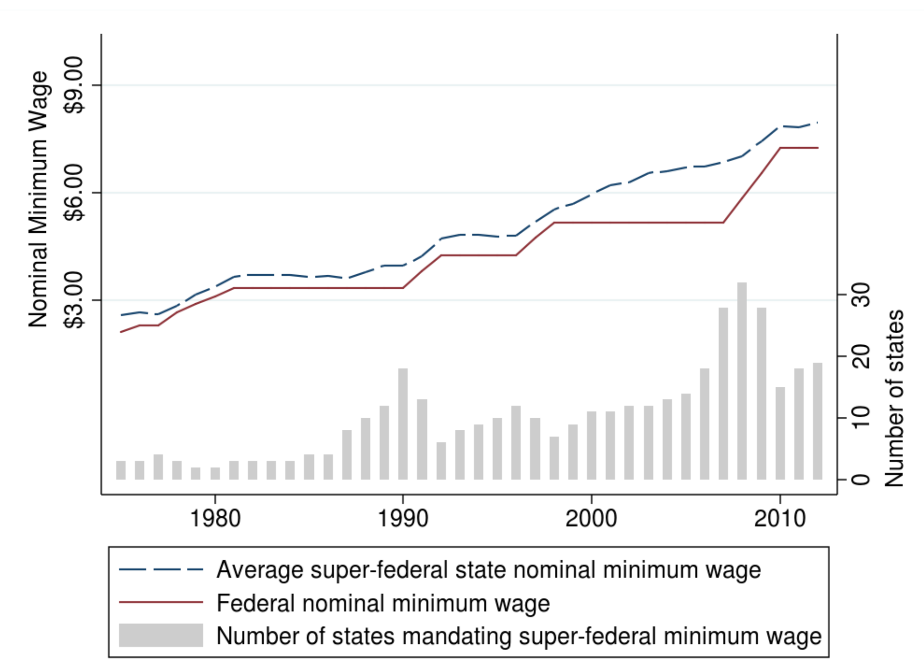


Figure C.3: Comparison of federal to state nominal minimum wages (January, 1975-2012)

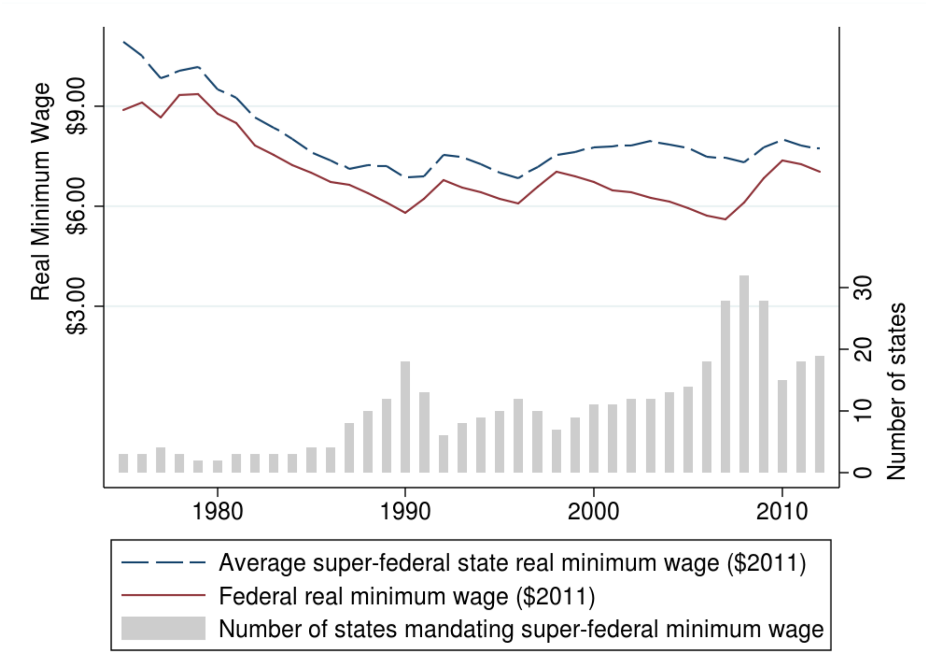


Figure C.4: Comparison of federal to state real minimum wages (January, 1975-2012))

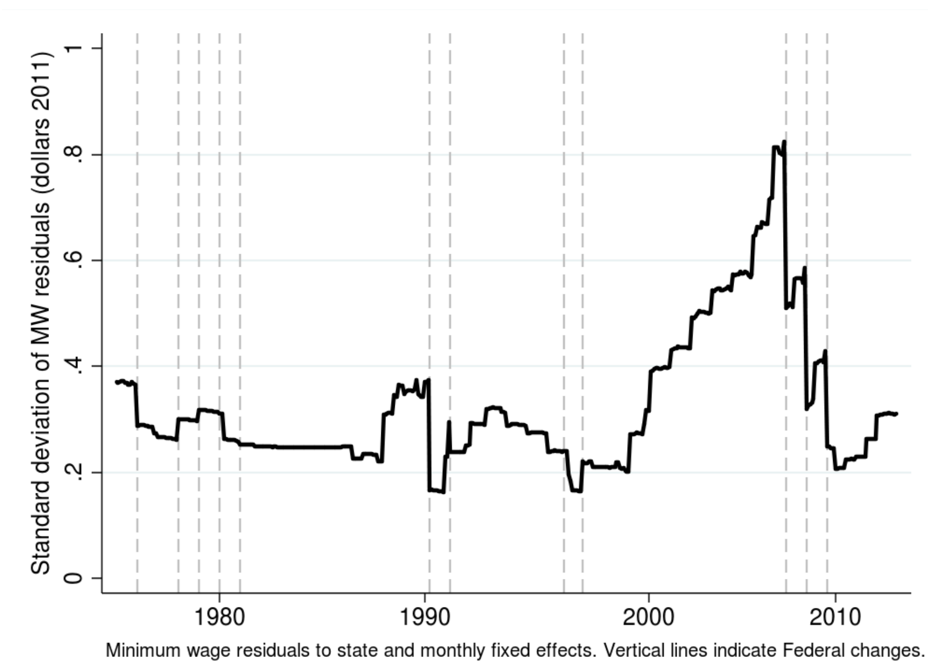


Figure C.5: Standard deviation of residual state real minimum wages (1975-2012)

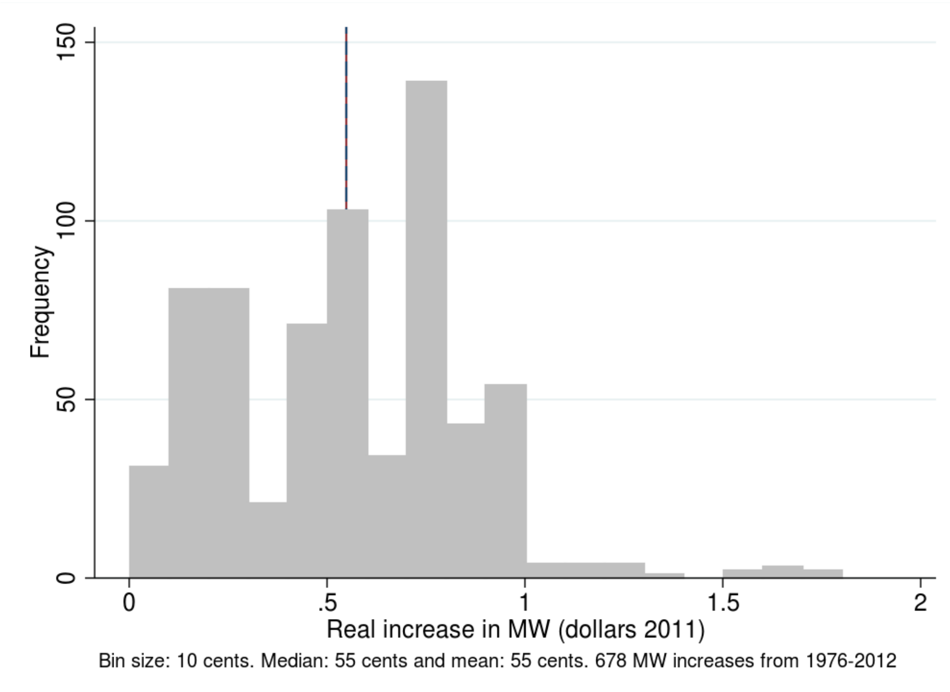


Figure C.6: Distribution of real minimum wage increases

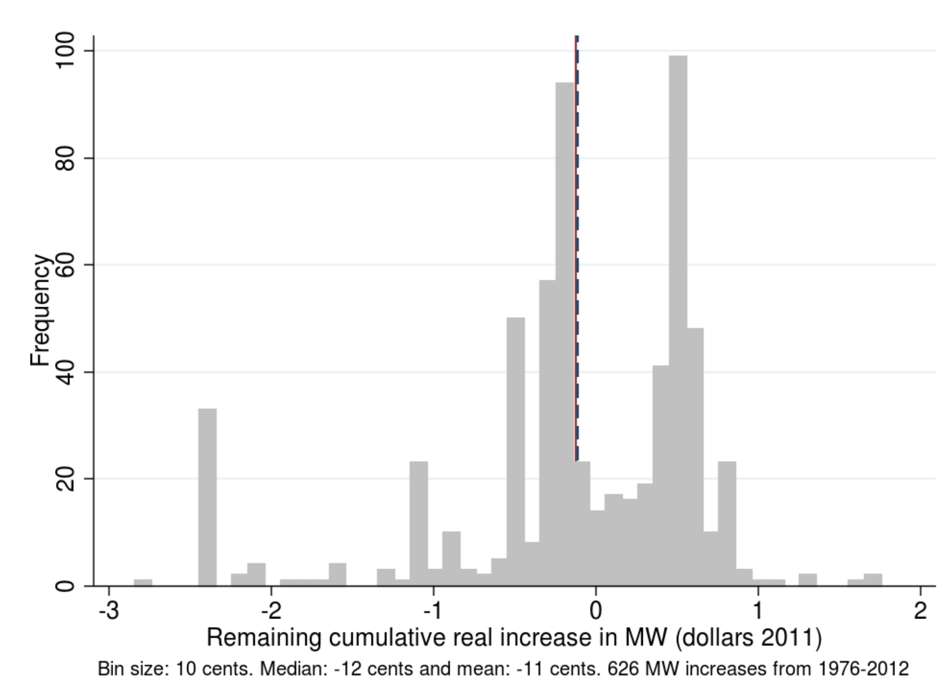


Figure C.7: Cumulative difference in real minimum wage prior to a new increase

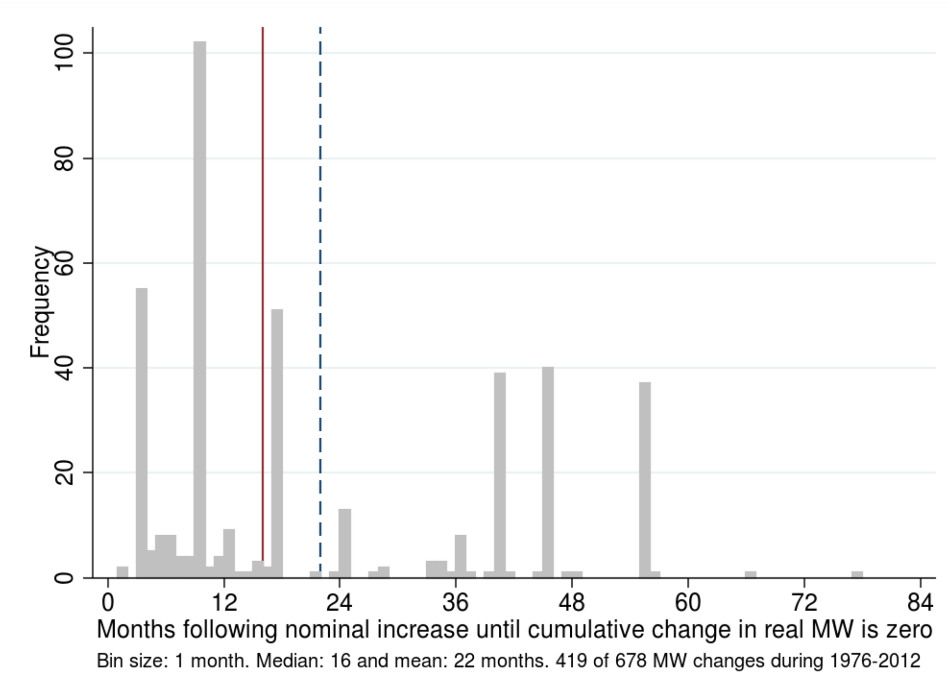


Figure C.8: Erosion of real increases in minimum wage

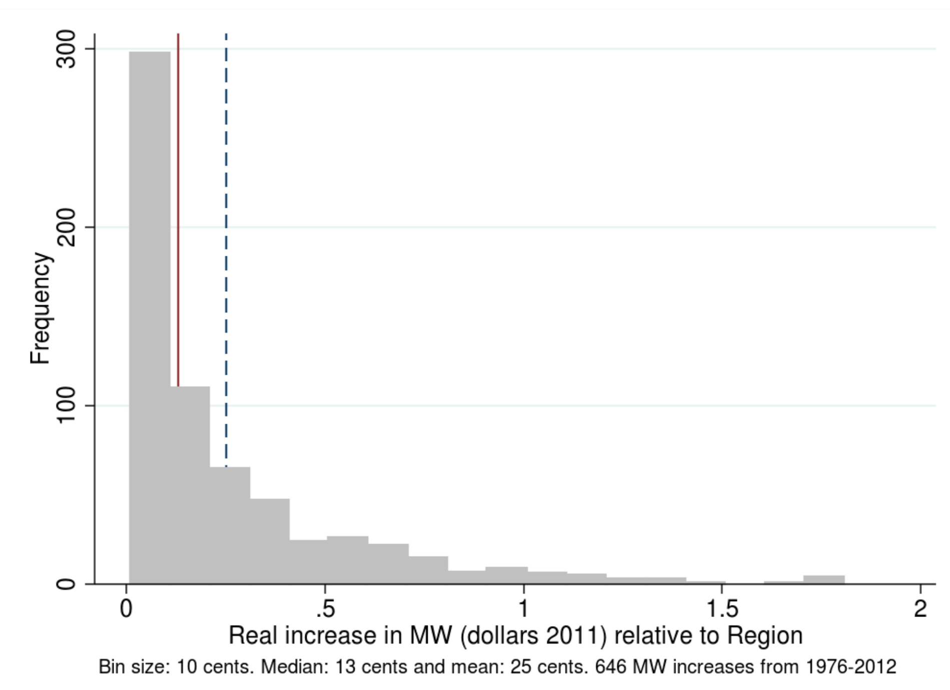


Figure C.9: Distribution of relative minimum wage increases

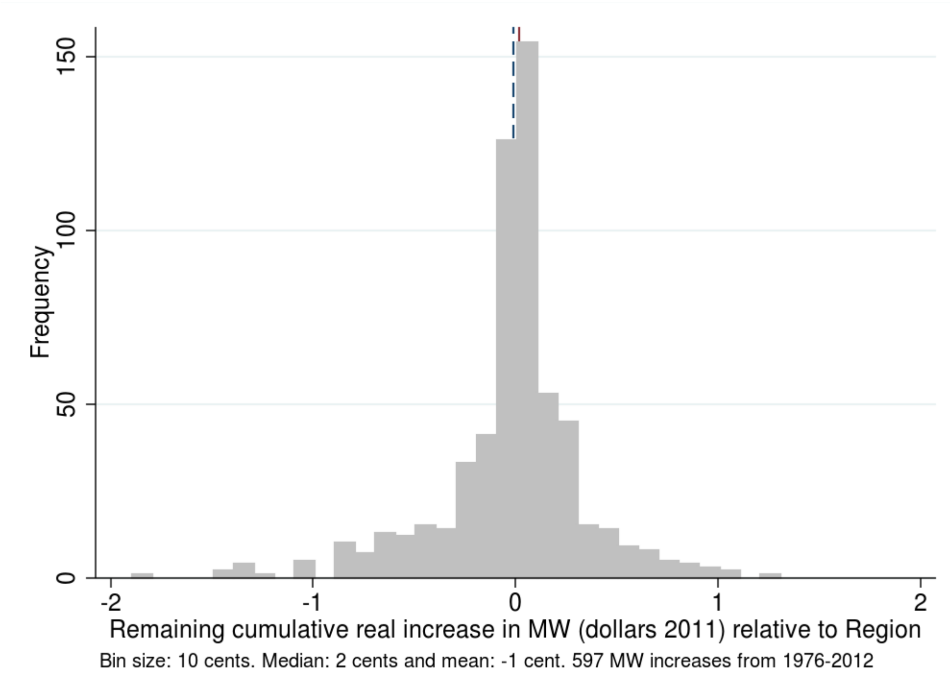


Figure C.10: Cumulative difference in relative minimum wage prior to a new increase

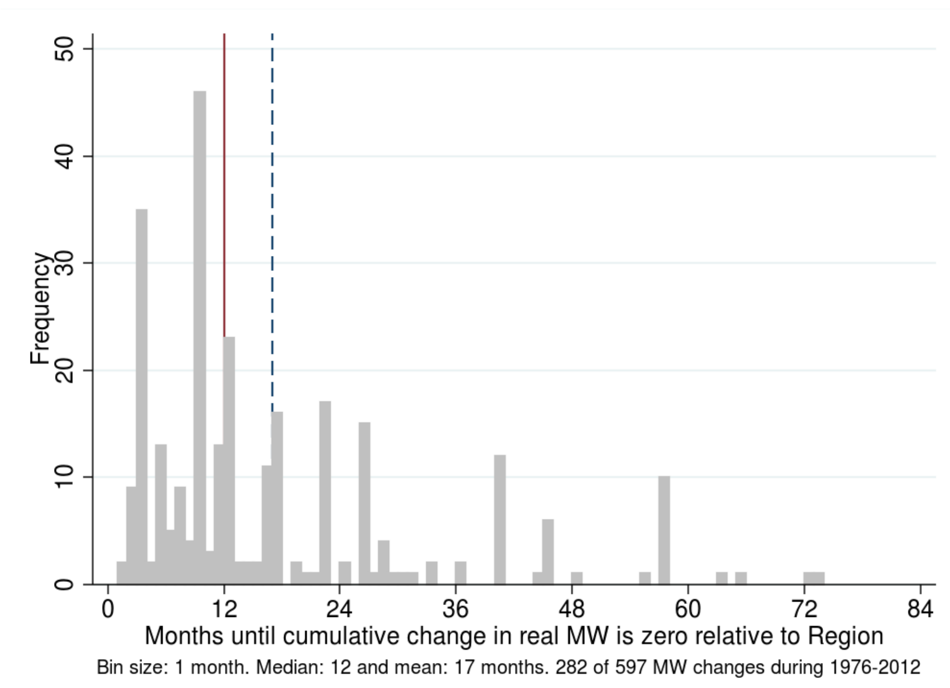


Figure C.11: Erosion of relative increases in minimum wage