

# Effects of the Minimum Wage on Employment Dynamics

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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 show 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. This finding is supported using several empirical specifications.

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## I. 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 (examples include 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.<sup>1</sup>

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 control variables – will attenuate estimates of how the minimum wage affects the employment level.

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 *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 primary results are 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.

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.

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 (for example, 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.

This article proceeds as follows: in Section II we provide a brief review of the literature on the employment effects of the minimum wage and build the case for examining employment dynamics. We discuss the data in Section III and present our specifications and results in Section IV. We conclude in Section V.

## **II. Theoretical and Econometric 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 (Allegretto et al. 2011; Neumark et al. 2014). 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 (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.<sup>2</sup> In worker search-and-matching models (such as Van den Berg and Ridder 1998;

Acemoglu 2001; Flinn 2006; Flinn 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.<sup>3</sup>

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." Baker et al. (1999), using Canadian data, argue that "employment adjusts to long-run, evolutionary changes in the minimum wage," necessitating a focus on dynamic effects; they further argue, similar to Sorkin's model, that "employment variation could result from the substitution of alternative factors of production with longer planning horizons," such as capital.

In a similar vein, Neumark and Wascher (1992) discuss the importance of including lagged effects of the minimum wage and find substantial impacts beyond a contemporaneous effect. To be clear, if the true effect a minimum wage is to change employment over time rather than to have an immediate effect on the employment level, then the traditional approaches used in this literature – namely, panel fixed effect estimates of the contemporaneous effects of the minimum wage on employment levels – will yield incorrect inference.<sup>4</sup>

### A. Staggered Treatments and Difference-in-Differences

We illustrate the potential shortcoming of this 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:

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

Because both jurisdictions are initially untreated and both are eventually treated, the only time period(s) in which the treatment effect  $\beta$  may be identified separately from the time fixed effects  $\tau_t$  are those during which *only* Jurisdiction A is treated. During all other time periods,  $I(\text{Treatment}_{it} = 1)$  takes the same value for both jurisdictions. Thus, the DiD model compares the average difference in employment between the jurisdictions during the time 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. As an example, 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 (that is,  $t_1 - 0 = T - t_2$ ), then DiD yields a zero treatment effect, visibly at odds with the plotted time paths of employment.

This toy example underscores the pitfalls in using a standard panel fixed effects model if treatments are staggered and the effects are on 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 Online Appendix 4<sup>5</sup>) 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.<sup>6</sup> We investigate the implications of this concern more thoroughly in our Section IV. First, though, we discuss a separate but related concern.

## B. 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 (for example, 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 – for instance, 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.<sup>7</sup>

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 treatment begins in period  $t = 0$ , the employment growth rate in the Treated jurisdiction falls relative to the Control, and this occurs over the following four periods before leveling off after time  $t = 4$ . Figure 4 presents the difference in employment by time period for both levels and residuals to jurisdiction time trends. The computed employment effect is large and negative when state trends are omitted in Panel (a), but shrinks essentially to zero with the inclusion of



jurisdiction time trends in Panel (b). This sharp attenuation occurs despite identical pre-treatment employment trends.

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 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 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 IV.

### **III. 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.<sup>8</sup> 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 Online Appendix 1. 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<sup>th</sup>). 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 1. Full descriptions of the QCEW and QWI, including their summary statistics, are located in Online Appendix 1.

### **A. State Minimum Wages**

We draw historical data on state minimum wages from state-level sources.<sup>9</sup> 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 12<sup>th</sup> 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.<sup>10</sup>

## **B. 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. 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 (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.<sup>11</sup> 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).

## **IV. Results**

In Section II, we provide theoretical support and motivation for the hypothesis that the minimum wage affects employment over time rather than through a single, immediate drop in its

level, and we illustrate several complications for attempts to quantify the magnitude of such an effect using standard approaches. In this section, we present several specifications which estimate this effect. As discussed above, we report results here using the Business Dynamics Statistics, with the same specifications using the QCEW and QWI in Online Appendix 1. There is little difference in the overall results across datasets, which is unsurprising given that all three examine the near-population of jobs in the United States. An illustrative example of the methodological nature of these various estimates is provided in Appendix 1.

### A. Standard Panel Fixed Effect Specifications

We begin with Equation 1, the “classic” variant of the panel fixed effects specification in levels that has been used extensively in the literature. In this specification,  $emp_{it}$  is the level of employment in jurisdiction  $i$  at time  $t$ ,  $\alpha_i$  are jurisdiction fixed effects,  $\tau_t$  are macroeconomic time period fixed effects,  $\gamma_i \cdot t$  are jurisdiction-specific linear time trends, and  $\epsilon_{it}$  is the idiosyncratic error term.

$$(1) \quad emp_{it} = \alpha_i + \tau_t + \gamma_i \cdot t + \beta_0 mw_{it} + \psi \cdot controls_{it} + \epsilon_{it}$$

If this model “correctly” fits the data – that is, if the effect of the minimum wage can be adequately captured via a single treatment coefficient within a panel fixed effects levels specification – then the coefficient  $\hat{\beta}_0$  identifies the total causal impact of the minimum wage on employment. In light of the considerations discussed in Section II, it is questionable how well this assumption holds. We provide results using this specification in order to benchmark our findings within the literature, to alleviate concerns regarding differing pretrends, to motivate that an effect could be on growth, and to underscore how dramatically the inclusion of jurisdiction time trends attenuates the classic estimates of the effect on employment.

In Table 2, we present results from estimating variations of this specification. Using

national time fixed effects with no other time-specific controls in Column (1), there is a large negative effect of the minimum wage, with an elasticity of about -0.19, though the estimate is imprecise. Including state-specific time-varying controls in Column (2) does not qualitatively change the point estimate, but precision is greatly improved: this estimate is -0.15 ( $p < 0.01$ ).<sup>12</sup> Column (3) switches to using Regional time fixed effects and the estimate is quite similar in magnitude (-0.17) and precision to that in Column (2).

If states that increased their minimum wages would have had worse employment outcomes even without these changes, then indicators for future minimum wages will show a negative effect on current employment. This would imply that the relationship between higher minimum wages and lower employment outcomes is spurious and simply reflects unobserved trends. In Columns (4) and (5), we add leading values of the minimum wage as a falsification exercise. The lead terms are statistically insignificant, while the contemporaneous effect is still large, negative, and statistically significant, albeit slightly reduced in magnitude to about -0.13. This outcome suggests that if pre-existing underlying trends are in fact different between states, they are not different by very much and are unlikely to be a key driver of the overall result. Taken as a group, the results from Columns (1) to (5) of Table 2 indicate that the minimum wage negatively affects employment and that underlying state-specific trends are not responsible for this disemployment effect. This classic specification is unable to distinguish between an effect that is immediate from one that occurs over a longer span of time, but it clearly indicates *some* negative relationship between a state's minimum wage and its employment level.

If the effect estimated in Columns (1) - (5) is indeed of a dynamic nature, as argued in Section II, occurring over several time periods rather than discretely at the time of the policy change, then one would expect the inclusion of jurisdiction time trends to substantially attenuate

the estimated treatment effect in this model. This is exactly the result when we add state-specific time trends in Column (6): in sharp contrast to the other columns, the estimated effect here is nearly zero. This sequence of results is very akin to that depicted in the illustrative example in Figure 4. Of course, the existence of this pattern does not, in itself, provide definitive evidence that the effect of the minimum wage occurs over several periods. It does, however, serve as suggestive evidence that the negative effect estimated in the classic model without jurisdiction time trends – both in this paper and elsewhere in the literature – is not simply an artifact of underlying heterogeneity in states' time-paths of employment that merely happen to be correlated with states' minimum wage policies.

## B. Long-Differences Specifications

We turn next to long-differences specifications to better examine whether there is a dynamic effect of the minimum wage. If the true effect of the minimum wage is on growth and reflected over time, then specifications that are differenced over increasingly longer time periods should yield larger coefficients for the effect of the minimum wage on employment, with the magnitude converging towards a longer run effect.<sup>13</sup> Specifically, we estimate specifications of the following form, with increasing duration of time span  $r$ :

$$(2) \quad \Delta_r emp_{it} = \tau_t + \gamma_i \cdot t + \beta_0 \Delta_r mw_{it} + \psi \cdot \Delta_r controls_{it} + \Delta_r \epsilon_{it}$$

If the minimum wage has a dynamic effect on employment, then the impact of the minimum wage may be small over short durations, but it will increase in magnitude and significance as the time span is increased. Ultimately, the estimated elasticity should level off in magnitude once the full effect has taken place. Equation 2 shows such a long-differenced equation, which we estimate (without jurisdiction time trends) in Panel A of Table 3, and the results indeed follow this pattern. Beginning with a gap of a single year in the first column, there is a relatively

small and statistically insignificant effect of -0.02. But as we move across columns to include longer differences, the effect increases in magnitude and becomes statistically significant. The coefficient stabilizes around -0.05 after three years and remains at that level even after differencing across eight years. These estimates provide more direct and compelling evidence of dynamic effects.

We add jurisdiction time trends to Equation 2 in Panel B of Table 3. As with the classic model, these point estimates are also substantially attenuated relative to those without trends. Moreover, as the duration of the time difference  $r$  increases across columns in Table 3, the point estimates in Panel B become monotonically larger relative to those in Panel A. If the negative effect of the minimum wage estimated in Panel A were merely an artifact of underlying differences in time trends, then the coefficients in Panel B should remain similar to each other (and near zero) as the duration increases. The increasing degree of attenuation as the number of periods increases is indicative of the time trends soaking up variation that is in actuality due to the treatment effect.<sup>14</sup>

### C. Distributed Lag Specifications

Finally, to provide yet a more direct and flexible view of the nature of the dynamic relationship between the minimum wage and employment, we estimate a distributed lag specification as in Equation 3, in which we not only take first differences to account for unobserved heterogeneity across states, but also include several lags of the minimum wage.<sup>15</sup>

$$(3) \quad \Delta emp_{it} = \theta_t + \gamma_i + \sum_{r=0}^s \beta_r \Delta mw_{it-r} + \psi \cdot \Delta controls_{it} + \Delta \epsilon_{it}$$

In Column (1) of Table 4, we show results from estimating Equation 3 with a contemporaneous term and three lags, with this lag structure selected based on the pattern of results seen in Table 3.<sup>16</sup> The contemporaneous term is negative, but statistically insignificant, but

the first two lags of the minimum wage are each statistically significant and similar in magnitude, around -0.03; the third lag is small and statistically insignificant, as suggested by the leveling-off of the long-differenced estimates after three years. By summing up these terms, we see that the total elasticity of employment with respect to the minimum wage is about -0.07, statistically significant, and similar in magnitude to that derived in the long-differenced estimates. In Columns (2) and (3), we add leads of the minimum wage to check for violations of the parallel trends assumption. The leading terms are statistically insignificant and trivial in size, both individually and summed together. The magnitude of the total minimum wage elasticity is unaffected, though some precision is lost with two leads. Even including in three leads does not greatly affect matters, with the main effect reduced slightly to -0.064 (s.e. = 0.043;  $p = 0.14$ ), and the sum of the three leading terms equal to 0.011 (s.e. = 0.023;  $p = 0.64$ ). Once again, this is strong evidence of a dynamic treatment effect of the minimum wage on employment, with little to indicate that differences in underlying growth trends are a relevant factor.

The remaining columns of Table 4 provide some further robustness tests. In Column (4), 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 nine Divisions, and the median Division includes only five states. In Column (5), 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 (6), 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 years of our panel experienced relatively 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. The estimated effects are slightly smaller, though not meaningfully different from our main results. However, 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.

One possible shortcoming of the distributed lag model is that high frequency variation in treatment intensity makes it difficult to inform 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 must be fairly small in these specifications. While our results are robust to the inclusion of more lag terms, and the long-differenced specifications suggest that the effects are fairly stable after several years, a natural additional approach is to use a dynamic panel specification (Arellano and Bond 1991). This allows us to estimate both the short- and long-run effects, albeit at the cost of imposing a stricter assumption on the nature of this relationship. We discuss these results in Online Appendix 3; while certain specifications are somewhat fragile, the conclusions are similar to those yielded by this section.

## **V. Conclusion**

We examine whether the minimum wage impacts employment through a discrete change in its level or if it is reflected over time. Much of the previous literature on the topic has assumed that an increase in the minimum wage would result in a relatively rapid adjustment in employment. Many have viewed a lack of such a finding as indicating that the minimum wage has minimal effects on employment, yet there are theoretical reasons to believe that this change may be slower. Using illustrative models, we show that the empirical specifications used in the prior literature will

systematically err if the true effects are dynamic. 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 the literature has focused on in recent years. 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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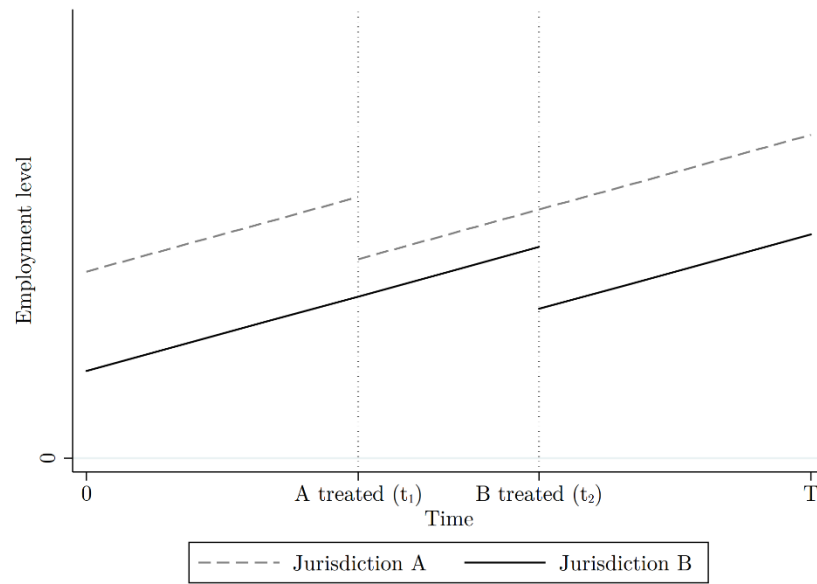
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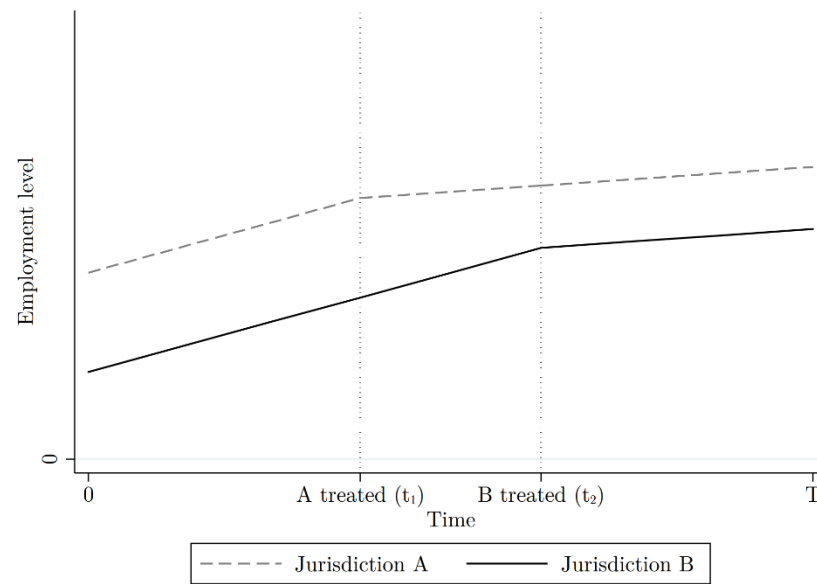
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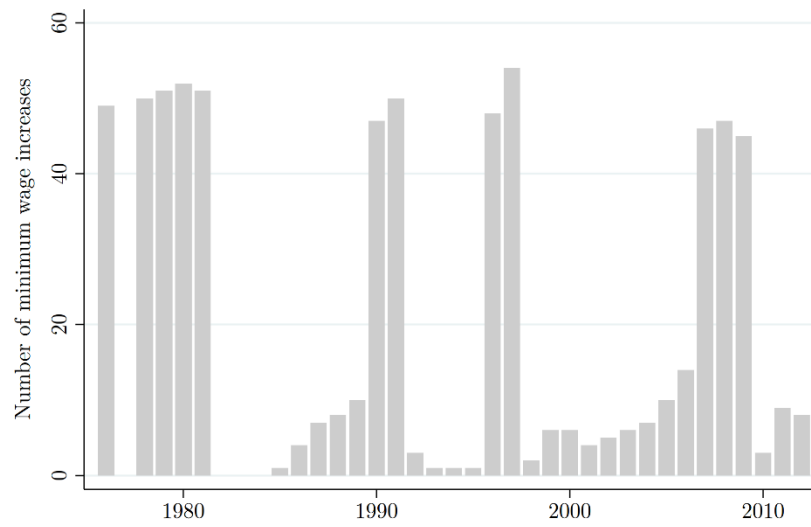
## A. Treatment Effect Discrete in Levels



## B. Treatment Effect Discrete in Growth

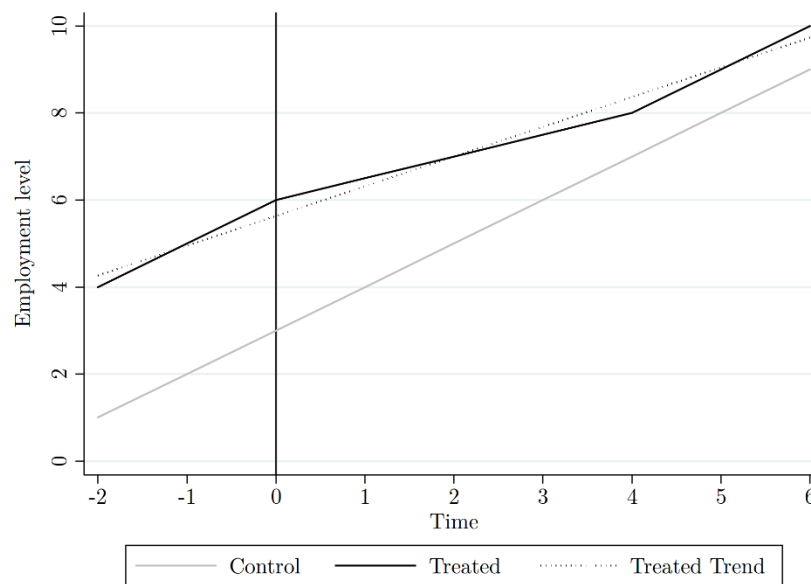
**Figure 1**

*Illustration of Two Types of Treatment Effects with Staggered Treatments*



**Figure 2**

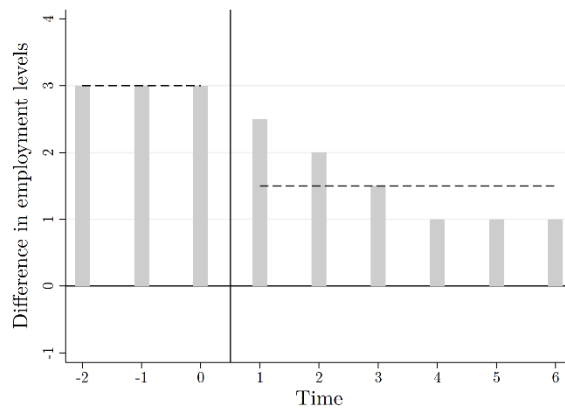
*Frequency of Increases to Effective State Nominal Minimum Wages (1976-2012)*



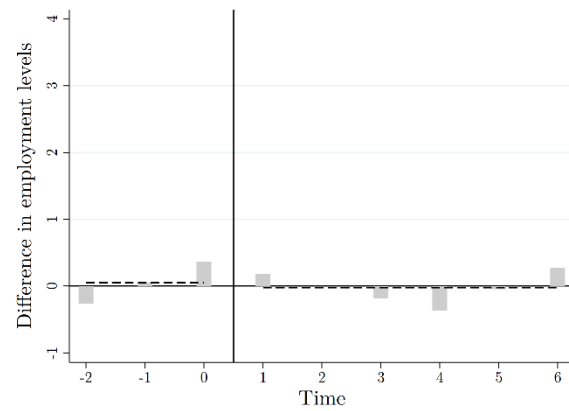
**Figure 3**

*Simple Example of Disemployment Effect in Growth Rate*





A. Levels: without Trends



B. Levels: Residual to Trends

**Figure 4**

*Example Difference-in-Differences without versus with Jurisdiction Time Trends*

Table 1

## 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
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 2

Classic State-Panel Fixed Effect Estimates for the Effect of the Minimum Wage on Log-Employment (BDS)

	(1)	(2)	(3)	(4)	(5)	(6)
Log-MW	-0.1859 (0.1178)	-0.1515*** (0.0444)	-0.1693*** (0.0383)	-0.1376*** (0.0295)	-0.1230*** (0.0295)	-0.0125 (0.0160)
1st lead of log-MW				-0.0304 (0.0283)	-0.0065 (0.0167)	
2nd lead of log-MW					-0.0359 (0.0387)	
Observations	1785	1785	1785	1734	1683	1785
Time FE	National	National	Regional	Regional	Regional	Regional
Time-varying controls	No	Yes	Yes	Yes	Yes	Yes
Jurisdiction time trends	No	No	No	No	No	Yes

\*  $p < 0.1$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$  Notes: Robust standard errors are clustered by state and reported in

parentheses. All columns include state fixed effects. Where included, state-level annual time-varying controls are log-population, the share aged 15-59, and log real gross state product per capita.

Table 3

Long Difference Estimates for the Effect of the Minimum Wage on Log-Employment (BDS)

Number of years:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A: without trends</b>								
Long difference in log-MW	-0.0202	-0.0387*	-0.0496**	-0.0512**	-0.0463*	-0.0492**	-0.0470**	-0.0475**
	(0.0175)	(0.0213)	(0.0235)	(0.0239)	(0.0236)	(0.0213)	(0.0201)	(0.0222)
<b>Panel B: with trends</b>								
Long difference in log-MW	-0.0196	-0.0364*	-0.0444*	-0.0388	-0.0223	-0.0137	-0.0007	0.0085
	(0.0176)	(0.0212)	(0.0234)	(0.0246)	(0.0254)	(0.0242)	(0.0211)	(0.0201)
Observations	1734	1683	1632	1581	1530	1479	1428	1377

\*  $p < 0.1$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$  Notes: Robust standard errors are clustered by state and reported in parentheses. The column numbers correspond to the number of periods over which the long difference is taken. All columns include state fixed effects, year-by-Region time fixed effects, and state-specific time-varying controls: log-population, the share aged 15-59, and log real gross state product per capita.

Table 4

Distributed Lag First-Differences Estimates for the Effect of the Minimum Wage on Log-Employment (BDS)

	Baseline	Leading values		Division FE	Non-indexed	Pre-2008
	(1)	(2)	(3)	(4)	(5)	(6)
Log-MW	-0.0204 (0.0162)	-0.0206 (0.0163)	-0.0178 (0.0161)	-0.0188 (0.0163)	-0.0226 (0.0183)	-0.0219 (0.0178)
1st lag of log-MW	-0.0321** (0.0139)	-0.0336** (0.0140)	-0.0283 (0.0171)	-0.0310* (0.0175)	-0.0268 (0.0166)	0.0020 (0.0231)
2nd lag of log-MW	-0.0304** (0.0128)	-0.0317** (0.0132)	-0.0353** (0.0141)	-0.0244 (0.0152)	-0.0343** (0.0158)	-0.0467*** (0.0171)
3rd lag of log-MW	0.0093 (0.0147)	0.0084 (0.0149)	0.0109 (0.0232)	0.0107 (0.0193)	0.0074 (0.0188)	0.0162 (0.0227)
1st lead of log-MW		-0.0075 (0.0085)	-0.0073 (0.0095)			
2nd lead of log-MW			0.0057 (0.0124)			
Sum MW effects	-0.0736** (0.036)	-0.0776** (0.038)	-0.0705 (0.049)	-0.0634 (0.051)	-0.0764** (0.034)	-0.0504 (0.033)
Observations	1581	1581	1530	1581	1536	1377

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\*  $p < 0.1$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$  Notes: Robust standard errors are clustered by state and reported in parentheses. All columns include state fixed effects, year-by-Region time fixed effects, and state-specific time-varying controls: log-population, the share aged 15-59, and log real gross state product per capita. Columns (2) - (3) include, respectively, the leading value of the log minimum wage at time  $t+1$  or both  $t+1$  and  $t+2$ . Column (4) uses Division-by-time fixed effects, rather than Region-by-time. Column (5) drops the observations with an inflation-indexed state minimum wage, and Column (6) uses only pre-2008 data.

## Appendix 1

### Estimating Growth Effects for Staggered Treatments

This section uses a simple toy example to illustrate the nature of – and compare results from – several candidate approaches to estimating the dynamic effect of a treatment variable on the outcome of interest. In particular, it contrasts the “classic” fixed effects model with a differences model (both first-differences and longer differences) and with a distributed lag model in first-differences. Appendix Figure 1.1 depicts the raw (simulated) data. There are three hypothetical jurisdictions: (1) a “control” jurisdiction, whose employment level remains at 3 units for the entire duration of the available data, (2) a jurisdiction which is treated with the binary treatment following period 2, and (3) a jurisdiction which is treated with the same-magnitude binary treatment following period 4. Appendix Figure 1.1 shows the employment levels for the three hypothetical jurisdictions in each of 8 time periods.

Appendix Table 1.1 presents the point estimates from six possible specifications (in rows) to estimate the treatment effect of four possible sets of jurisdictions (in columns). The columns consider the estimates that would be obtained from estimating treatment effects from comparisons of jurisdictions, respectively: 1 to 2, 1 to 3, 2 to 3, or 1 to 2 to 3. All eight time periods are included in each of these regressions.

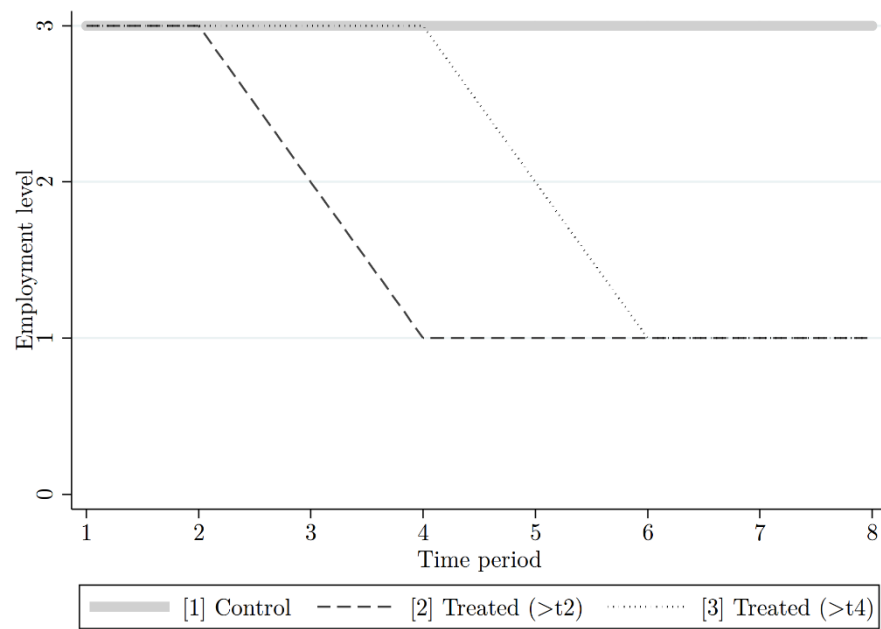
Starting with the classic panel fixed effects difference-in-differences specification in Column (1), it is clear that this approach compares the mean difference (zero) pre-treatment, to the mean difference (-1.83) post-treatment, between jurisdictions 2 and 1. This visibly understates the long-run treatment effect here of -2, as it averages in the period 3 difference of only -1.5; this is a basic artifact of the standard difference-in-differences model, rather than an anomaly. However, moving across the columns, which compare various combinations of possible jurisdictions in this

setting, one can easily see how the point estimate is sensitive to the set of included jurisdictions, varying by as much as 27 percent (-1.33 vs. -1.83) in this simple example in which there is a very clear actual treatment effect.

Moving down the rows to the differenced specifications, it is clear that the first-differenced specification correctly estimates the -1.0 treatment effect of the initial reduction in employment from the change in treatment, regardless of the combination of jurisdictions in this illustration. The longer differences (2, 3, 4) are, however, much more sensitive to the combination of included jurisdictions. Finally, the distributed lag model correctly sums the -2.0 treatment effect, irrespective of the included jurisdictions.

The overall point of this appendix is not to argue for any *true* model. Rather, it is meant to illustrate how sensitive results in a difference-in-differences context may be to the nature of the treatment effect and to the form of the estimator.





**Appendix Figure 1.1**

*Illustration of Staggered Treatments with Growth Effects*

Appendix Table 1.1

## Treatment Coefficients for Estimates Using Various Methods

Model	Jurisdictions included			
	1,2	1,3	2,3	1,2,3
Fixed effects (classic)	-1.83	-1.75	-1.33	-1.65
First difference (FD)	-1.00	-1.00	-1.00	-1.00
Long difference (2 period)	-1.50	-1.50	-1.25	-1.38
Long difference (3 period)	-2.00	-1.67	-1.67	-1.75
Long difference (4 period)	-2.00	-1.75	-1.50	-1.75
Distributed lag FD (2 period)	-2.00	-2.00	-2.00	-2.00

Notes: Columns include sets of two or three jurisdictions, with identities corresponding to the [1], [2], and [3] in Appendix Figure 1.1. Values are point estimates from the models indicated in rows, and the linear combination of point estimates for the distributed lag specification.

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<sup>1</sup> Hirsch et al. (2015) 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.

<sup>2</sup> 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.

<sup>3</sup> 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 that the minimum wage would discretely affect employment, but that frictions in

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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 limits our ability to make sweeping statements about the specific channel(s) through which the minimum wage impacts labor markets.

<sup>4</sup> 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 a balanced panel subset (2001-2008) of the 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 overall dynamic effect on employment levels.

<sup>5</sup> All Online Appendices are available at <http://jhr.uwpress.org/>.

<sup>6</sup> 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 Online Appendix 4 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 IV, we revisit the implications of inflation for minimum wage policy in the context of our empirical findings.

<sup>7</sup> We are grateful to Cheng Cheng and Mark Hoekstra, as well as Justin Wolfers for this insight.

<sup>8</sup> The employer-sourced administrative nature of these data is important for our research

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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 II, 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. 2013).

<sup>9</sup> 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. All data and code used in this study are available from the authors online or by request.

<sup>10</sup> 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.

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<sup>11</sup> 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  $\ln(\text{real private sector GSP/capita})$  instead, but we view total GSP as the more appropriate definition given that the population term reflects total state population.

<sup>12</sup> One approach to examine the potential endogeneity of minimum wage changes is to examine the results with different combinations of the time-varying covariates. Including (all with national time fixed effects) only log-population as a covariate yields an estimate of -0.153 (se=0.055); log-population and the share aged 15-59 yields -0.125 (0.043); with just log-GSP/capita the estimate is -0.210 (0.118).

<sup>13</sup> Nichols (2009) and our Appendix 1 provide a graphical representation and commentary on the econometric strengths and weaknesses of the various specifications we present in this section.

<sup>14</sup> The nature of this monotonic attenuation pattern is straightforward. Consider the toy example in Figure 3 above. Long-difference estimates for this illustrative treatment effect are -0.5, -0.75, -1.25, and -1.75 for 1-4 time period differences, respectively, when time trends are not included. Including time trends yields corresponding long-difference estimates of -0.366, -0.426, -0.388, and -0.602. Thus, the estimates in this illustrative example are attenuated by 0.134, 0.324, 0.862, and 1.148 – monotonically increasing despite the clear absence of any difference in pre-treatment underlying time trends. The pattern shown via this basic illustration can be replicated more generally using simple Monte Carlo simulation.

<sup>15</sup> An alternative approach would be to estimate distributed lag coefficients in levels, rather than first differences, but this approach may suffer from several shortcomings, as

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highlighted in Neumark and Wascher (1992) and Baker et al. (1999). Estimates from distributed lags in levels using these same data are if anything *more* negative (and at least as statistically significant) as those using distributed lags in first-differences, as presented in this section.

<sup>16</sup> 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.