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Minimum Wage Hikes and Technology Adoption: Evidence from U.S. Establishments

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Abstract

This article studies the effects of state minimum wage increase on information technology (IT) adoption at the establishment level in the United States. Our results show that treatment establishments on average allocate between $10,328 and $66,808 more per year to their IT budgets during the first 3 years after experiencing significant state minimum wage increases. Additional evidence shows that state minimum wage increases on average lead to an economically small decrease in employment. The estimated employment effect is larger for establishments that have more incentives to automate labor. Our results suggest that establishments adopt technology to countervail increased labor costs.

I. Introduction

Minimum wage remains a highly controversial policy and continues to spark heated debates among policymakers. Given the rapid rise of information technology (IT) and the decreased cost of IT capital (Eden and Gaggl (2019)), recent discussions on minimum wage start to pay attention to the possibility of firms adopting new technology as a response to increased labor costs. Anecdotal evidence suggests that firms in minimum wage-sensitive industries indeed accelerate the automation process in response to higher wage floors. For example, fast-food giant Wendy's began to roll out interactive kiosks in 2017 after experiencing a rise in labor costs in the previous year (see https://qz.com/923442/wendys-is-responding-to-the-rising-minimum-wage-by-replacing-humans-with-robots/). Additionally, the president and CEO of the Illinois Hotel and Lodging Association expressed concerns that businesses may be forced to automate given that the Illinois minimum wage will increase from $8.25 per hour to $15.00 by 2025 (see https://www.chicagotribune.com/business/ct-biz-automation-minimum-wage-illinois-20190214-story.html).

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The idea that firms could adopt new technology as a response to minimum wage increases is dated at least back to Stigler (1946), who notes that "The second and offsetting result, the increase of labor productivity, might come about in one of two ways: the laborers may work harder; or the entrepreneurs may use different production techniques…The introduction of new techniques by the entrepreneurs is the more common source of increased labor productivity…" Despite the fact that this idea has existed for several decades, empirical evidence in the United States is scattered and conflicting (Chen (2019), Cho (2021), and Gustafson and Kotter (2022)). Furthermore, none of these papers focus on investment in IT capital, which could play a key role in automating low-wage jobs. This article provides the first *direct* evidence on how state minimum wage increases affect IT investment decisions at the establishments of U.S. corporations.

To measure IT investment decisions at the establishment level, we use the establishment-level IT budget data from the Ci Technology database (CiTDB) owned by the Aberdeen Group, an intent-based marketing company. We follow the minimum wage literature and focus on establishments in the accommodation and food services (2-digit NAICS code is 72) and retail (2-digit NAICS code is 44 or 45) sectors, which are the two largest employers of workers who are paid within 110% of the binding federal or state minimum wages.

We face one major challenge when estimating the causal effects of state-level minimum wage increases on IT budget: the distribution of minimum wage policies is not random across states in the U.S. (Allegretto, Dube, Reich, and Zipperer (2017)). As a result, estimations based on the canonical two-way fixed effects panel regressions are difficult to be interpreted as causal. To overcome this challenge, we exploit 13 significant state minimum wage increases ($\geq$ \$0.25 per hour) and perform a difference-in-differences analysis. Specifically, for establishments located in a state that experiences a significant minimum wage increase (treatment establishments), we match them with establishments in neighboring states where there are no changes in minimum wage policies within an 8-year window around the minimum wage event. Furthermore, we follow Dube, Lester, and Reich (2010) and focus on establishments in contiguous counties across a state border where minimum wage policy discontinues (control establishments). Our empirical strategy compares the changes in IT budgets at treatment establishments relative to the ones at control establishments around minimum wage events.

Our findings are as follows: First, our main result shows that significant state minimum wage increases lead to higher IT budgets at treatment establishments. In particular, the estimate based on our preferred specification shows that the ratio of IT budget to the average pre-event revenue increases by 0.512 percentage points at treatment establishments relative to control establishments, representing a 22.4% increase relative to the sample mean in pre-event periods (2.289 percentage points). Our main results are robust to alternative specifications, dependent variables, and sets of minimum wage events. Depending on specifications, our estimates imply that treatment establishments on average allocate between \$10,328 and \$66,808 (in 2018 dollars) more per year to their IT budgets during the first 3 years after significant minimum wage increases.

Second, our evidence shows that two key identification assumptions of a difference-in-differences design, parallel trend assumption and stable unit treatment value assumption (SUTVA), are unlikely to be violated in the data. The event-study analysis shows that, prior to minimum wage events, IT budgets at both the treatment and control establishments evolve on parallel trends. After the events, the trajectories of IT budgets in these two groups start to diverge. During the first year in the post-event periods, the ratio of IT budget to the average pre-event revenue increases by 0.528 percentage points at treatment establishments relative to control establishments. The estimated effects persist during the second and third years. Furthermore, we do not find significant cross-border or within-firm spillovers that could bias our estimations, suggesting that SUTVA is unlikely to be violated.

Third, the effects of significant state minimum wage increases on IT budget are heterogeneous across industry, firm, or establishment characteristics. Our results show that the estimated effect is stronger for establishments in industries where employment shares of low-wage or routine occupations are larger. We also find that the estimated effect is concentrated in establishments whose parent firms have employment between 50 and 499. For establishments whose parent firms are small (employment $\leq$ 49) or large (employment $\geq$ 500), the estimated effects are much smaller. We next show that IT budget responses are stronger for establishments with lower IT capital, measured as the number of installed personal computers (PCs) per employee, in the year just prior to minimum wage events, suggesting that establishments with lower IT capital start to catch up with technology upgrading because of state minimum wage hikes. We further show that the estimated effect of significant state minimum wage increases on IT budget is larger for establishments with lower labor productivity (defined as revenue per employee) 1 year prior to minimum wage events.

Fourth, we examine the effects of significant state minimum wage increases on components of IT budget and IT capital. Our results show that all four identified components of IT budget (hardware, software, services-related, and communications-related budget) increase following significant state minimum wage increases. Our results also show that the increased IT budget following minimum wage events materializes into higher IT capital. Specifically, our estimations suggest that PCs per employee at treatment establishments increase by 0.051 after minimum wage events, representing an 8.6% increase relative to the average IT capital during pre-event periods. The results also imply that significant state minimum wage increases lead to purchases of two more PCs at treatment establishments relative to control establishments.

Finally, we examine the employment effect of significant state minimum wage increases. Our results show that employment in treatment establishments is on average 2.7% lower relative to the counterfactual after minimum wage events. To further examine how changes in establishment-level IT budget and employment are connected, we examine heterogeneous employment effects by industry-level employment share of routine occupations. Autor, Levy, and Murnane (2003) show that workers in routine occupation are more likely to be substituted by technology. As a result, establishments in industries with higher employment shares of routine occupations have more incentives to automate labor when state minimum wage increases. We find that the employment effect is indeed the largest for establishments in industries with the highest employment shares of routine occupations.

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We further examine the heterogeneous employment effects by establishment-level labor productivity 1 year prior to minimum wage events. We argue that increased wage floors are more likely to exceed the output value of workers in establishments with lower pre-event labor productivity, and therefore, these establishments have more incentives to automate labor because of this increased wedge between wages and labor productivity. We indeed find that the estimated employment effect is larger for establishments with lower pre-event labor productivity. Overall, our results strengthen the interpretation that state minimum wage increases could induce establishments to automate labor to countervail increased labor costs.

Our article is related to four strands of literature. First, it is related to the emerging literature on labor and finance, specifically how labor market friction affects corporate investment decisions (Bai, Fairhurst, and Serfling (2020), Ouimet, Simintzi, and Ye (2021)). Existing literature answers the question on how minimum wage regulations affect substitution between capital and labor by focusing on total investment in net fixed assets but the empirical evidence is mixed. By using firm-level data in the U.S., Gustafson and Kotter (2022) exploits federal minimum wage increases and find that firms in minimum wage-sensitive industries decrease capital investment compared to nonlabor-intensive firms across states that are bounded and unbounded by the federal minimum wage law. Cho (2021) reaches a similar conclusion by leveraging variations in state minimum wages. On the contrary, some studies document evidence that minimum wage increases lead firms or establishments to increase capital investment. For example, Chen (2019) reports evidence that U.S. manufacturers increase expenditures on machines as a response to minimum wage increases. There is also evidence from China and Hungary that is consistent with the results in Chen (2019) (Harasztosi and Lindner (2019), Hau, Huang, and Wang (2020), and Geng, Huang, Lin, and Liu (2022)). In contrast to these prior studies, we focus on investment in IT, which could play a key role in automating low-wage jobs. Our results shed new light on whether minimum wage hikes lead firms to adopt technology.

Second, our article contributes to the literature on minimum wage by providing direct evidence on how state minimum wage hikes affect technology adoption decisions at the establishments in minimum wage-sensitive industries. Our article is closely related to Lordan and Neumark (2018), Aaronson and Phelan (2019), and Aaronson and Phelan (2023). All three papers find that, among low-wage occupations, state minimum wage increases lead to a decrease (an increase) in the employment share of occupations that are more likely to be substituted by (complementary to) technology. All of these findings using labor market data suggest that firms may adopt new technology as a response to state minimum wage increases. However, direct evidence on this conjecture at the firm or establishment level is still scattered. Our article narrows this gap and documents an increase in IT budget and PC installations following significant state minimum wage increases.

Third, our article fits into the literature on the relation between labor scarcity and technological progress. Theoretically, Acemoglu (2010) characterizes the conditions under which labor scarcity would encourage technological adoption. On the empirical side, there exists evidence showing that labor scarcity and high wages induce technological progress, consistent with the *Habakkuk hypothesis* proposed by Habakkuk (1962). For example, Lewis (2011) shows that the skill mix of the workforce in metropolitan areas has significant effects on the adoption of automation machinery by U.S. manufacturing plants and Clemens, Lewis, and Postel (2018) find that employers adopt new production technologies if possible as a response to the Mexican bracero exclusion. Our article contributes to this literature by showing that higher wages induced by state minimum wage regulations encourage the adoption of IT in retail and accommodation and food services industries. The evidence suggests that adopting IT could countervail increased labor costs in these two minimum wage-sensitive sectors.

Finally, our article is related to the literature on the impacts of technology adoption on labor markets. One of the central questions in this literature is how technology adoption affects establishment-level or firm-level employment. The empirical evidence is mixed. Acemoglu, Lelarge, and Restrepo (2020), Domini, Grazzi, Moschella, and Treibich (2021), Aghion, Antonin, Bunel, and Jaravel (2022) use data from France and find that automation has a positive impact on employment at the firm level. Studies using data from Canada, the Netherlands, Germany, Spain, and Denmark reach similar conclusions (Dixon, Hong, and Wu (2021), Bessen, Goos, Salomons, and van den Berge (2020), Benmelech and Zator (2021), Humlum (2021), and Koch, Manuylov, and Smolka (2021)). But other studies find opposite results. For example, Bonfiglioli, Crinò, Fadinger, and Gancia (2020) use an instrumental variable strategy and find that firms that adopt more robots experience a larger reduction in employment in France. In the U.S., Acemoglu, Anderson, Beede, Buffington, Childress, Dinlersoz, Foster, Goldschlag, Haltiwanger, Kroff, Restrepo, and Zolas (2022a) find that use and adoption of technologies do not result in significant changes in employment level at the firm level. In our empirical setting, we find that establishments with higher incentives to automate labor increase IT budgets and lower total employment after experiencing significant increases in state minimum wage.

II. Hypothesis Development

In this section, we develop hypotheses on the effects of state minimum wage increases on establishment-level technology adoption and employment decisions. In the discussions below, we differentiate between two types of labor. The first is labor in tasks that could be automated (e.g., workers who use tools to perform the routine cutting of meat). The second is labor in nonautomated tasks (e.g., workers who are in charge of helping customers with new technology).

When state minimum wage increases, an establishment has the incentive to automate low-wage workers that could be replaced by technology. As a result, the employment level of this type of labor would be lower and capital expenditures on technology would be higher relative to the counterfactual (the *displacement effect* in Acemoglu and Restrepo (2019)). However, the effect of state minimum wage increases on the employment of nonautomated labor is unclear. On the one hand, if the adoption of new technology increases the establishment's total factor productivity sufficiently large, then the establishment could demand more labor in nonautomated tasks (the *productivity effect* in Acemoglu and Restrepo (2019)). On the other hand, the establishment could scale down and lower the employment level of low-wage nonautomated labor relative to the counterfactual because of the

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increased labor costs (we term this the *scaling effect*). Given that labor in non-automated tasks and technology are more likely to be complements, then capital expenditures on technology could be lower following an increase in state minimum wage.

Therefore, it is not clear ex ante how state minimum wage increases would impact total capital expenditures on technology and total employment at the establishment level. The net effects would depend on the relative magnitudes of the displacement, productivity, and scaling effects. It is ultimately an empirical question, and we provide evidence in the following sections.

III. Selection of Industries and Data

A. Selection of Industries

In the empirical analysis, we focus on industry sectors that are sensitive to minimum wage changes. It is well established in the literature that the accommodation and food services sector (2-digit NAICS code is 72) and the retail sector (2-digit NAICS code is 44 or 45) employ a large fraction of minimum wage workers (Dube et al. (2010)). We confirm this fact using the Current Population Survey (CPS) Outgoing Rotation Group (ORG) data from 2018 IPUMS (Flood, King, Rodgers, Ruggles, and Warren (2018)).[1] To determine wages for hourly workers, we use the reported hourly wage. For workers who are not paid by the hour, we estimate their hourly wage by dividing the weekly earnings by the usual hours worked per week. For each month, we match the observations with the state-level minimum wage using the household's state of residence in CPS. We define a worker as a minimum wage worker if her hourly wage is less than or equal to 110% of the corresponding mandated wage floor. The statistics show that the accommodation and food services and retail sectors employ 30.6% and 18.6% of all minimum wage workers in 2018, respectively, making them the largest two employers of minimum wage workers in the United States.

B. Data

1. State Minimum Wage Data

We obtain the state-level minimum wage data from David Neumark's website (https://sites.socsci.uci.edu/~dneumark/datasets.html). The original data is at the state-month level and for each state-year observation, we define the annual minimum wage level to be the maximum of the monthly minimum wage in the year. Here we also provide a brief overview of the recent minimum wage changes between 2010 and 2018, the period during which the data on IT budget is mostly available. All the state minimum wage changes during this period are reported in Table B1, and those used in our sample are in bold font.

Between 2010 and 2012, there was a lull in minimum wage policymaking at both the federal and state level, as discussed in Clemens and Strain (2017). At the federal level, there was no further minimum wage increase since July 2009, when it

[1]We obtain the data from IPUMS, available at https://cps.ipums.org/cps/.

was increased to \$7.25. At the state level, there have been 20 minimum wage changes made by 12 states. However, only four states enacted statutory minimum wage increases, and the remaining eight states indexed their minimum wage increases to a measure of cost of living (Clemens and Strain (2017)). The average magnitude of these increases was also small, with \$0.25 in nominal term and \$0.064 in 2018 dollars, respectively.

However, we observe a different pattern of state minimum wage policies since 2013. There were 113 minimum wage increases made by 26 states and Washington, DC, and 18 states and DC enacted statutory minimum wage increases.[2] The average magnitude of these increases was also much larger, with \$0.50 in nominal terms and \$0.38 in 2018 dollars, respectively.

2. IT Investment Data

The data on corporate IT investment is from the Ci Technology (CiTDB) owned by the Aberdeen Group, an intent-based marketing firm. The Aberdeen Group surveys and interviews high-level IT staff at establishments across the United States to obtain information on IT adoption and investment. The interviews were conducted throughout a year. The data has been used by the sales and marketing teams in large IT firms (e.g., IBM and Dell) and have become a key source of information on IT usage and investment in U.S. firms. The data has also been used in numerous academic studies, including Brynjolfsson and Hitt (2003) and Forman, Goldfarb, and Greenstein (2012).

The data is at the establishment level. For each establishment, the database provides detailed information on IT adoption and investment, such as the stock of technologies (computers, servers, printers, etc.) and the budgets for new IT investment. It also provides firmographic information, including establishment name, location (latitude, longitude, ZIP code, county, and state), industry (4-digit 1987 SIC code and 6-digit NAICS code), estimated revenues and employment, and the linked parent firm identifier. Each establishment is assigned a unique ID in the database, and this ID is invariant to ownership change. Such a feature allows us to track each establishment over time.

The coverage of establishments in the database varies over time. The database includes around 140,000 establishments surveyed in 1996, and the number increases to 487,000 in 2009. Since 2010, CiTDB has expanded its coverage, and now more than 3 million establishments are covered in the database.

We use IT budget as our primary measure for IT investment in our analysis. CiTDB starts to provide information on budgets allocated for new IT investment since 2007, but such data has mainly been available since 2010. As a result, our sample period is between 2010 and 2018, the last year we have access to the data. We inflate revenue and IT budget to 2018 dollars using the Consumer Price Index research series (CPI-U-RS) from the Bureau of Labor Statistics ((BLS), https://www.bls.gov/cpi/research-series/home.htm). In addition to the total IT budget,

[2] Rhode Island enacted statutory minimum wage increases in 2015 and 2016 while switched back to index its minimum wages to CPI since 2017 (Act No. 2014-273). Vermont switched from indexing its minimum wages to CPI to statutory increases between 2015 and 2018 and switched back to inflation indexing since 2019 (Act 176 of 2014).

TABLE 1
Summary Statistics

Table 1 reports the summary statistics of variables used in the estimations. Panels A and B report the summary statistics of variables at the establishment level and state level, respectively. IT budget and revenue at the establishment level are expressed in 2018 dollars. Columns 1 and 2 in Panel C report the average characteristics of treatment and control establishments prior to minimum wage events, respectively. Column 3 of Panel C reports the *p*-values of the differences between columns 1 and 2. Variable definitions are available in Appendix A.

	No. of Obs.	Mean	Std. Dev.	P10	Median	P90
Panel A. Establishment-Level Characteristics						
IT_BUDGET/REVENUE (%)	64,532	2.936	3.732	0.490	2.053	5.337
IT_BUDGET ($000)	64,532	157.033	315.739	20.680	56.857	339.769
HARDWARE/REVENUE (%)	64,532	0.386	0.471	0.072	0.282	0.704
SOFTWARE/REVENUE (%)	64,532	0.626	0.806	0.103	0.382	1.497
SERVICES/REVENUE (%)	64,532	1.479	1.899	0.232	1.035	2.634
COMMUNICATIONS/REVENUE (%)	64,532	0.248	0.273	0.022	0.128	0.595
EMPLOYMENT	64,532	55.688	91.037	10.000	25.000	125.000
REVENUE ($ Million)	64,532	10.523	22.075	1.060	3.180	24.616
PC_PER_EMPLOYEE	64,504	0.723	0.500	0.278	0.600	1.286
Panel B. State-Level Characteristics						
GDP_PER_CAPITA_GROWTH_RATE	314	0.010	0.024	−0.009	0.012	0.026
HPI_GROWTH_RATE	314	1.866	2.905	−2.450	2.245	5.330

Panel C. Treatment Versus Control Establishments

	Treatment	Control	*p*-Value of 1–2
	1	2	3
IT_BUDGET/REVENUE (%)	2.289	2.408	0.448
IT_BUDGET ($ 000)	82.630	99.933	0.114
HARDWARE/REVENUE (%)	0.358	0.381	0.320
SOFTWARE/REVENUE (%)	0.391	0.411	0.515
SERVICES/REVENUE (%)	1.306	1.347	0.627
COMMUNICATIONS/REVENUE (%)	0.245	0.263	0.297
EMPLOYMENT	37.489	44.605	0.092
REVENUE ($ Million)	6.952	6.924	0.981
PC_PER_EMPLOYEE	0.594	0.598	0.645

CiTDB further identifies four components of the total IT budget: hardware, software, services-related, and communication-related.[3] Hardware includes PCs, servers, terminals, printers, and storage devices. IT services include systems integration, computer hardware support, and maintenance services. IT communications include routers, Wi-Fi transmitters, wide-area network (WAN) and local-area network (LAN) equipment, cable boxes, and other network equipment. We winsorize the establishment-level variables at 1% and 99% to mitigate the effects of outliers.

Panel A of Table 1 reports the summary statistics of the variables in the sample for estimations. In our sample, there are 64,532 establishment-year observations involving 5,852 unique establishments. One establishment could appear in multiple county pairs in a year. IT budget and revenue are expressed in 2018 dollars. The statistics show that, on average, the total budget for new IT investment is around $157,000 and accounts for around 2.9% of total revenue. Among all the subcomponents, the services-related budget accounts for the largest fraction (around 50%) of the total IT budget. Turning to other establishment-level characteristics, an average establishment in our sample has 56 employees and

[3]However, the sum of the four components is not equal to the total budget reported in CiTDB. The remaining part of the budget could be thought as other IT budget.

\$10.5 million in revenue. The average number of PCs per employee in our sample is 0.723.

3. State-Level Characteristics

The state-level characteristics are assembled from various sources. The real GDP per capita is from the Bureau of Economic Analysis (BEA) Regional Economic Accounts (https://www.bea.gov/data/economic-accounts/regional). The state-level annual housing price index (HPI) data is from the Federal Housing Finance Agency ((FHFA), https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx). Panel B of Table 1 reports the summary statistics for the variables at the state level.

IV. Empirical Strategy

The key empirical challenge when estimating the causal impacts of state minimum wages on IT budget is that the distribution of state-level minimum wage policies is not random. Previous studies show that states with higher minimum wages are fundamentally different from states with lower minimum wages (Allegretto et al. (2017), Clemens and Wither (2019)). As a result, state-level minimum wage increases could be correlated with unobserved state-level shocks and estimates based on the canonical two-way fixed effects panel regressions are difficult to be interpreted as causal. To overcome this challenge, we follow Dube et al. (2010) and estimate the effects by exploiting minimum wage policy discontinuities at state borders and restricting our sample to establishments in border counties across a state border. Our empirical strategy compares changes in IT budgets at establishments in treatment border counties (treatment establishments) around minimum wage events, relative to the changes in IT budgets at establishments in border control counties (control establishments) within the chosen event window. There are two advantages of this empirical strategy. First, compared to a random county in a control state, a border control county is more similar to a treatment county and therefore serves as a better control (Dube et al. (2010)). Therefore, establishments in border control counties would be better controls for establishments in treatment counties. Econometrically, this implies that the trends of IT budgets in treatment and control establishments should be parallel before minimum wage increases and we confirm this in Section V.B. Second, in each minimum wage event, we can include county pair × year and establishment fixed effects. Including county pair × year would fully absorb any macroeconomic shock within each pair of treatment and control counties. Including establishment fixed effects would control for any time-invariant establishment characteristics. One concern with this empirical strategy is that the labor market within a pair of treatment and control counties is linked, and establishments in border control counties could be affected by minimum wage increases in treatment states (i.e., the SUTVA could be violated). In Section V.C, we perform several tests to mitigate such a concern. In the following sections, we discuss the details of empirical design as well as how we select state minimum wage increases in the sample.

We perform a difference-in-differences analysis to estimate the causal impacts of state minimum wage increases on IT budget. For a given state minimum wage increase, we choose the event window to be 4 years before and 3 years after the event (Cengiz, Dube, Lindner, and Zipperer (2019)). Given that IT budget data is mainly available between 2010 and 2018, we focus on the minimum wage changes occurring between 2011 and 2017. There were 109 minimum wage increases made by 28 states during these years. However, we do not use all; instead, we focus on estimating the effects of significant increases in the empirical analysis. Following Cengiz et al. (2019), we define a state minimum wage increase to be significant if the increase is at least 25 cents per hour. There were 65 significant state minimum wage increases during these years (minimum wage events). As discussed in Section V.D, our empirical results are robust if we define a minimum wage event to be significant if the increase is at least 50 cents or 75 cents per hour.

For each state that experiences a significant minimum wage increase (treatment state), we define a set of control states as those that are adjacent to the treatment state and do not make any minimum wage increase between up to 3 years before and up to 4 years after the event year. There are 42 minimum wage events in which treatment states can be matched with control states. There were 12 states (Arkansas, California, Delaware, Massachusetts, Maryland, Michigan, Minnesota, Nebraska, New York, Oregon, Vermont, and West Virginia) and Washington, DC that increase minimum wages larger than \$0.25 in multiple years. We use only the first significant minimum wage increases in the empirical analysis because the subsequent increases would be more expected by establishments located in those states. For the treatment state in each minimum wage event, we further require that there is no minimum wage change up to 4 years before the event. By imposing this criterion, we further drop six minimum wage events: Vermont in 2015, California and Oregon in 2016, and Arizona, Colorado, and Washington in 2017. Finally, our sample has 13 minimum wage events with 13 treatment states and 15 control states.

We then follow Dube et al. (2010) and further focus on establishments in border counties in treatment and control states to perform the empirical analysis. For each minimum wage event, we require establishments in each pair of contiguous counties to have at least one observation before and after the event and to remain in the same counties throughout the chosen event window.

Across all 13 minimum wage events, there are 128 counties in treatment states and 124 counties in control states. There are 231 county pairs in total. One county in a state could be paired with more than one county in the neighboring states along the same state border. Figure B1 reports the county pairs used in our analysis. There are 2,987 and 2,865 establishments in treatment and control states, respectively, across all the minimum wage events.

Panel C of Table 1 reports the comparison of average characteristics during years prior to minimum wage events between treatment and control establishments. Columns 1 and 2 report the average characteristics of treatment and control establishments prior to minimum wage events, respectively. In column 3, we report the *p*-values of the differences between columns 1 and 2. The results in column 3 show that only the difference in total employment is marginally significant at the 10% level, suggesting that treatment and control establishments are fairly similar prior to minimum wage events. Table 2 summarizes the minimum wage events used in our analysis. For each event, it reports the treatment state, the corresponding control

TABLE 2

State Minimum Wage Events in the Sample

Table 2 reports the 13 state minimum wage events used in the empirical analyses. For each event, we report the treatment state, the corresponding control states, the event year, the dollar (ΔMW) and percentage (% Change in MW) changes in minimum wage from the previous level.

Treatment State	Control States	Event Year	ΔMW	% Change in MW
AR	LA, MS, OK, TN, TX	2015	0.25	3.44
DC	VA	2014	1.25	15.15
DE	PA	2014	0.50	6.90
MA	NH	2015	1.00	12.50
MD	PA, VA	2015	1.00	13.79
ME	NH	2017	1.50	20.00
MI	IN, WI	2014	0.75	10.13
MN	IA, ND, WI	2014	0.75	10.34
NE	IA, KS, WY	2015	0.75	10.34
NJ	PA	2014	1.00	13.79
NY	PA	2014	0.75	10.34
SD	IA, ND, WY	2015	1.25	17.24
WV	KY, PA, VA	2015	0.75	10.34

states, the event year, and the dollar and percentage changes in the minimum wage from the previous level.

Main Specification

To estimate the impacts of state minimum wage increases on IT budget, we stack the establishment-level observations from the treatment and border control counties across the 13 events and estimate the following difference-in-differences specification as in Appendix D of Cengiz et al. (2019). By stacking all the events and aligning them by event time, this specification is equivalent to a setting in which all the events happen at the same time rather than being staggered over time. The main advantage is that it prevents negative weights on some events in a staggered event-study design (Sun and Abraham (2021)).

$$\text{(1)} \qquad \text{IT\_BUDGET}_{kicspt} = \beta \times \text{TREATED}_{ks} \times 1(t - t_k > 0) + \Gamma' X_{ist} + \mu_{ki} + \eta_{kpt} + \varepsilon_{kicspt},$$

where k, i, c, s, p, and t index for minimum wage event, establishment, county, state, county-pair, and year, respectively. TREATED_{ks} is a dummy variable equal to 1 if state s experiences a significant increase in minimum wage in event k, and 0 otherwise. t_k is the event year of event k. We also control for a vector of characteristics at the establishment and state level in X_{ist}. For establishment-level characteristics, we control for the establishment size, measured as the natural logarithm of revenue.[4] For state-level characteristics, we control for the growth rates of real GDP per capita and Housing Price Index (HPI). Such state-level controls are important because state-level economic conditions could be correlated with a state's decision to change minimum wages (Clemens and Wither (2019)). We include μ_{ki}, a set of event × establishment fixed effects, to control for any unobservable time-invariant establishment characteristics in an event. We also include η_{kpt}, a set of

[4]We do not control for establishment age in our main analysis due to poor data quality, but our results are robust if we do so. The results are reported in column 1 in Table 5.

event × county pair × year fixed effects, to absorb any common shock in a county-pair in a year for an event.

Our main measure for IT_BUDGET is the ratio of an establishment's IT budget to its average revenue in pre-event periods. This normalization ensures that our estimated effect is attributed to the variation in IT budget and alleviates the concern that establishment-level revenue maybe affected by significant state minimum wage increases. The coefficient β captures the changes in IT investment before and after minimum wage events between treatment and control establishments. We cluster the standard errors at both the state and state border levels (Dube et al. (2010)).

V. Main Results

A. Baseline Estimations

In this subsection, we report our baseline estimations. Columns 1–2 of Table 3 report the estimates of equation (1). In column 1, we only include event × establishment and event × county pair × year fixed effects without any establishment-level or state-level controls. The coefficient on TREATED × POST is 0.478 and statistically significant at the 5% level. In column 2, we control for establishment size, measured as the natural logarithm of revenue, as well as the growth rates of real GDP per capita (GDP_PER_CAPITA_GROWTH_RATE) and HPI (HPI_GROWTH_RATE) at the state level. The estimated coefficient on TREATED × POST becomes slightly larger and more statistically significant after including further controls.

The economic magnitude of our estimate is modest. The estimation in column 2 suggests that the ratio of IT budget to the average pre-event revenue at treatment establishments increases 0.512 percentage points after minimum wage events, compared to control establishments. Given that the sample mean of the dependent variable in treatment establishments during the pre-event periods is 2.289 percentage points, the estimated effect represents a 22.4% increase. Given that the sample mean of the average pre-event revenue across treatment establishments is \$6.952 million, the estimation in column 2 implies that treatment establishments on average allocate around \$35,594 (= 0.512/100 × 6.952 × 1,000,000) more *per year* to IT budgets relative to control establishments during the first 3 years after experiencing significant minimum wage increases. The average minimum wage increase in treatment states is \$0.86 per hour higher than that in control states. Our estimate then suggests that a \$1 per hour increase in state or national minimum wage would lead to a \$41,388 (= \$35,594/0.86) increase in IT budgets in treatment establishments. Our estimate could be used to evaluate the impacts of increasing the national minimum wage to 15 per hour on establishment-level technology adoption, but we would recommend doing so cautiously as an increase in the minimum wage to \$15 per hour is far outside of the observed minimum wage increases in our sample.

Our results suggest that retailers and restaurant owners may adopt technology when facing higher labor costs, but the impacts of technology on workers are unclear. Some anecdotal evidence suggests that technology may displace some

TABLE 3

Effects of Significant State Minimum Wage Increases on IT Budget

Table 3 reports the estimated effect of significant state minimum wage increases on IT budget at the establishment level. The dependent variable is the ratio of IT budget to the average revenue during years prior to minimum wage events. Columns 1 and 2 report the estimated average treatment effect based on equation (1). Column 3 reports the estimated dynamic treatment effect based on equation (2). For each minimum wage event, TREATED is a dummy variable equal to 1 if an establishment is located in the state experiencing a significant minimum wage increase, and equal to 0 if an establishment is located in an adjacent state that does not experience any minimum wage increase between up to 3 years before and up to 4 years after the event year. POST is a dummy variable equal to 1 for the years after the event year, and 0 otherwise. YEAR $-\tau$ is a dummy variable equal to 1 for the τth year before the event year, and equal to 0 otherwise. YEARτ is a dummy variable equal to 1 for the τth year after the event year, and equal to 0 otherwise. For the treatment state in each minimum wage event, we further require that there is no minimum wage change during up to 4 years before the event. The estimation sample includes establishments in bordered counties in treatment and control states across all 13 minimum wage events. log(REVENUE) is the natural logarithm of revenue at the establishment level. GDP_PER_CAPITA_GROWTH_RATE is the growth rate of real GDP per capital at the state level. HPI_GROWTH_RATE is the growth rate of annual housing price index (HPI) at the state level. Variable definitions are available in Appendix A. Standard errors in parentheses are robust and clustered at the state and state border levels. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	1	2	3
TREATED × POST	0.478**	0.512***	
	[0.205]	[0.143]	
TREATED × YEAR – 4			–0.003
			[0.187]
TREATED × YEAR – 3			0.009
			[0.157]
TREATED × YEAR – 2			0.118
			[0.094]
TREATED × YEAR0			0.008
			[0.150]
TREATED × YEAR1			0.528***
			[0.157]
TREATED × YEAR2			0.568***
			[0.200]
TREATED × YEAR3			0.549***
			[0.184]
log(REVENUE)		3.014***	3.014***
		[0.286]	[0.286]
GDP_PER_CAPITA_GROWTH_RATE		–2.841	–2.829
		[1.698]	[1.855]
HPI_GROWTH_RATE		–0.017	–0.010
		[0.024]	[0.030]
Event × establishment FE	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes
Adj. R^2	0.500	0.708	0.708
No. of obs.	64,532	64,532	64,532

low-wage workers. For example, Ryan Hillis, a vice president of Meltwich, a chain restaurant, mentioned to *The New York Times* that Meltwich would need fewer workers on a shift because of advanced kitchen equipment, software, and other technological advances. However, some anecdotal evidence suggests that technology could increase labor demand. For example, to solve the labor shortage problem caused by COVID-19, a franchisee of Checkers in the Atlanta area installed voice-recognition drive-thrus to keep her business booming. This owner did not plan to cut jobs and was in fact looking to hire more workers. She told *The New York Times* that technology is an assistant and allows employees to focus on customers (see https://www.nytimes.com/2021/07/03/business/economy/automation-workers-robots-pandemic.html). We will examine how changes in IT budget and employment at the establishment level are connected in Section VII.C.

B. Parallel Trends Assumption

One key assumption of a difference-in-differences design is that IT budgets would have evolved on a parallel trend between treatment and control establishments without significant minimum wage increases, the so-called parallel trend assumption. In this subsection, we test whether this assumption is satisfied in the data empirically. Specifically, we estimate the following event-study model to track the dynamic effects:

$$\text{(2)} \qquad \text{IT\_BUDGET}_{kicspt} = \text{TREATED}_{ks} \times \sum_{n=-4 \text{ and } n \neq -1}^{3} \beta_n \times 1(t - t_k = n) + \Gamma' X_{ist} + \mu_{ki} + \eta_{kpt} + \varepsilon_{kicspt}.$$

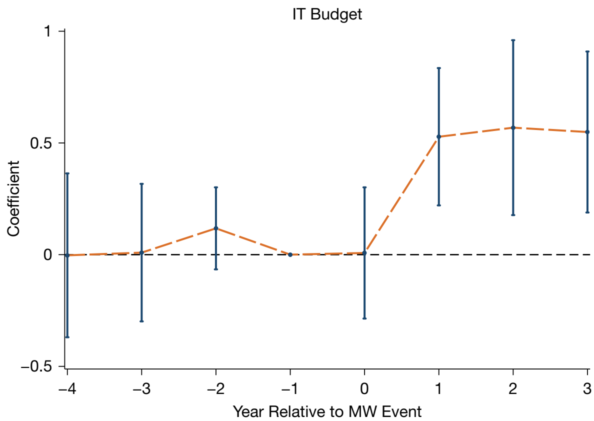
The coefficients of interest are β_n. The estimated coefficients capture the dynamics of the relative outcome between treatment and control establishments over time. The omitted category is $n = -1$, the year immediately before a minimum-wage event. β_n is interpreted as the average relative change in the IT investment between establishments in treatment and control border counties across all the minimum wage events during time n, relative to time -1. If IT investment measures at treatment and control establishments are on similar trends before events, then β_{-4}, β_{-3}, β_{-2}, and β_0 would be small in magnitude and statistically insignificant.

The results are reported in column 3 of Table 3 and Figure 1. Our estimations show that, in pre-event years, the estimated coefficients on β_{-4}, β_{-3}, and β_0 are close to zeros and statistically insignificant. The estimated coefficient on β_{-2} is relatively larger but the magnitude is much smaller compared to the estimated coefficients in the post-treatment period and remains statistically insignificant.

FIGURE 1

Dynamic Treatment Effects of Significant State Minimum Wage Increases on IT Budget

Figure 1 reports the estimated dynamic treatment effects of significant state minimum wage increases on establishment-level IT budget based on equation (2). The estimated coefficients represent the relative change in IT budget between treatment and control establishments 4 years before and 3 years after minimum wage events, compared to the year immediately before each event. The bar around each dot represents the 95% confidence interval.



In post-event years, the results suggest that the paths of IT budget between treatment and control establishments begin to diverge significantly starting from the first year after significant minimum wage events. Specifically, our estimations show that, relative to the year just prior to the event year, the ratio of IT budget to pre-event revenue increases by 0.528 percentage points during the first year in treatment establishments relative to control establishments. This estimated effect represents an increased \$36,707 (= 0.528/100 × 6.952 × 1,000,000) in total IT budget at treatment establishments. During the second and third years following significant minimum wage increases, the estimated effects are relatively stable. The estimated coefficients are 0.568 and 0.549, respectively. Overall, our estimates suggest that the parallel trend assumption is unlikely to be violated in the data.

In Figure B2, we also plot the trends of IT budgets for treatment and control establishments separately. For each period relative to the event year, we first calculate the average IT budgets for treatment and control establishments within each pair of border counties in a minimum wage event. We then calculate the average IT budgets for treatment and control establishments across all pairs of border counties in all minimum wage events. It does not seem that the results in Figure 1 are driven by decreases in IT budgets in control establishments after minimum wage events.

C. SUTVA

Another important identification assumption of a difference-in-differences design is the SUTVA, which requires that establishments in border control counties are not affected by minimum wage increases in treatment states. We perform two tests below to empirically assess whether SUTVA is violated in our setting.

1. Cross-Border Spillovers

One of the concerns with the border counties analysis is that positive or negative spillovers may exist between treatment and control border counties since the labor market within a pair of contiguous counties could be linked. This would affect our estimates of the effects of significant state minimum wage increases on IT budget. There could be two scenarios. On the one hand, the wages of establishments in the border control county may decrease after the treatment county experiences an increase state minimum wage. This scenario could arise in a model in which the labor market is competitive and disemployment happens in the treatment county. As a result, labor supply could be increased in the contiguous county, resulting in lower wages. In this case, establishments in the contiguous county would have lower incentives to invest in labor cost-saving technologies, and our estimated effects in Table 3 would be larger than the true effect; on the other hand, the wages of establishments in the border control county may increase after the treatment county experiences an increase in state minimum wage. This scenario could arise in a model in which employees search for jobs and employers in the border control county are forced to match the increased value of employees' outside options. If this case is true, then establishments in the border control county would have more incentives to upgrade technologies to save labor costs, and we would underestimate the true effect in Table 3.

To test for potential spillovers across state borders, we restrict the sample to establishments in the control states and examine whether establishments in border and interior counties respond differently to minimum wage increases in the treatment states as in Dube et al. (2010). An interior county is defined as a county that is not adjacent to any county in a different state. We drop establishments in counties that are adjacent to counties in states other than the treatment states from the sample. If we do not observe a differential response of IT budgets between establishments in border and interior counties in control states, then SUTVA is unlikely to be violated in the data. We cluster the standard errors at the state level. Specifically, we run the following regression:

$$\text{(3)} \quad \text{IT\_BUDGET}_{kicst} = \beta \times \text{BORDER}_{kc} \times 1(t - t_k > 0) + \Gamma' X_{kit} + \mu_{ki} + \eta_{kst} + \varepsilon_{kicst}.$$

As before, k, i, c, s, and t index for significant minimum wage event, establishment, county, state, and year, respectively. BORDER_{kc} is a dummy variable equal to 1 if county c lies on the state border that is adjacent to the treatment state in event k, and equal to 0 if county c is not adjacent to any county in a different state. η_{kst} is a set of event × state × year fixed effects, and including this set of fixed effects would absorb any shocks at the control state-year level in each event.

If the estimated β is negative and statistically significant, this would suggest that our baseline estimate may overestimate the true effect; however, if β is positive and statistically significant, the results would be consistent with the prediction from an efficiency wage model and our baseline estimate would underestimate the true effect.

We report the results in column 1 of Table 4. The estimated coefficient on BORDER × POST is -0.049 and statistically insignificant. This evidence suggests that, at least in our sample, significant spillovers across state borders are unlikely to exist.

2. Within-Firm Spillovers

Another concern with our analysis is that IT investment decisions at establishments within the same firm could be connected, and such within-firm spillovers may affect our estimated effects of significant state minimum wage increases on IT budget.

On the one hand, firms can reallocate capital resources from establishments in control states to establishments in treatment states as a response to minimum wage shocks. For example, Giroud and Mueller (2015) show that, for financially constrained firms, capital, and labor are reallocated within the same firms after one establishment experiences positive shocks to investment opportunities. If this case is true, then our estimations may overestimate the true effect as within-firm capital reallocation would mechanically drive part of the results.

On the other hand, it is also possible that IT budgets increase at both treatment and control establishments after significant minimum wage events. For example, Giroud and Mueller (2019) show that local economic shocks can be propagated through firms' internal networks of establishments. In the context of state minimum wage policies, Silva (2021) shows that wage increases in one establishment propagate to other establishments within the same firm, even if the states in which the

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TABLE 4

Cross-Border and Within-Firm Spillovers

Table 4 reports the estimated cross-border and within-firm spillovers. In column 1, we restrict the sample to establishments in the control states and examine whether establishments in border and interior counties respond differently to significant minimum wage increases in the treatment states. For each minimum wage event, BORDER is a dummy variable equal to 1 if a county lies on the state border that is adjacent to the treatment state, and equal to 0 if the county is not adjacent to any county in a different state. In column 2, we restrict the sample to establishments in border control counties. For each minimum wage event, OTHER is a dummy variable equal to 1 if a control establishment belongs to a firm that also has operations in the treatment state, and 0 otherwise. In column 3, we restrict the sample to stand-alone establishments. For each minimum wage event, TREATED is a dummy variable equal to 1 if an establishment is located in the state experiencing a significant minimum wage increase, and equal to 0 if an establishment is located in an adjacent state that does not experience any minimum wage increase between up to 3 years before and up to 4 years after the event year. POST is a dummy variable equal to 1 for the years after the event year, and 0 otherwise. The estimation sample in column 3 includes establishments in bordered counties in treatment and control states across all 13 minimum wage events. We control for the natural logarithm of revenue at the establishment level as well as growth rates of real GDP per capital and annual housing price index (HPI) at the state level in all regressions. Variable definitions are available in Appendix A. Standard errors in parentheses are robust and clustered at the state level in columns 1 and 2 and clustered at both state and state border levels in column 3. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Establishments in Control States	Establishments in Border Control Counties	Stand-Alone Establishments
	1	2	3
BORDER × POST	−0.049 [0.066]		
OTHER × POST		0.035 [0.438]	
TREATED × POST			0.453*** [0.108]
Controls	Yes	Yes	Yes
Event × establishment FE	Yes	Yes	Yes
Event × state × year FE	Yes	Yes	
Event × county pair × year FE			Yes
Adj. R^2	0.671	0.663	0.815
No. of obs.	158,198	14,625	23,535

other establishments operate do not raise minimum wages. As a result, control establishments could also increase IT budgets as a response to minimum wage increases in treatment states. Furthermore, IT investment decisions could be centralized at the firm level as in McDonald's and Wendy's. For these firms, their treatment and control establishments would receive increased budgets for upgrading technologies if one state in which the firm has operations increases its minimum wage. Regardless of the particular reason, if control establishments increase IT budgets after significant minimum wage events, then our estimation would underestimate the true effect due to within-firm spillovers.

To test whether and how within-firm spillovers affect our estimations, we restrict the sample to establishments in border control counties. In particular, we examine whether IT budgets at establishments whose parent firms have operations in treatment states are differentially affected by significant minimum wage increases in treatment states, relative to establishments whose parent firms do not have operations in treatment states. We cluster the standard errors at the state level. Specifically, we run the following regression:

$$\text{(4)} \qquad \text{IT\_BUDGET}_{kist} = \beta \times \text{OTHER}_{ki} \times 1(t - t_k > 0) + \Gamma' X_{kit} + \mu_{ki} + \eta_{kst} + \varepsilon_{kist}.$$

OTHER_{ki} is a dummy variable equal to 1 if establishment i belongs to a firm that also has operations in the treatment state in event k, and equal to 0 otherwise. We report the results in column 2 of Table 4. The estimated coefficient on

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OTHER × POST is 0.035 and statistically insignificant, suggesting that within-firm spillovers do not significantly bias our estimations.

Another way to mitigate the concern that within-firm spillovers may bias our estimations is to restrict the sample to stand-alone establishments and reestimate equation (1). The estimated results are reported in column 3 of Table 4. The estimated coefficient on TREATED × POST is 0.453, which is comparable to the baseline estimate of 0.512 in Table 3. The average ratio of IT budget to pre-treatment revenue for treatment standalone establishments is 2.413 prior to minimum wage events, and our estimate represents an 18.8% relative to the sample mean after minimum wage events. This estimate further suggests that within-firm spillovers do not play an important role in biasing the estimated effects. Overall, the evidence in Table 4 suggests that SUTVA is unlikely to be violated in the data.

D. Robustness and Falsification Tests

In this subsection, we report robustness checks for the main results in Table 3 and perform one falsification test to further validate our estimates.

1. Control for Establishment Age

In column 1 of Table 5, we further control for establishment age. CiTDB starts to report the established year of each establishment since 2005. In the full database, there are 723,271 unique establishments, and 244,399 (34%) have nonmissing data on the established year. In the empirical analysis, we control for the natural

TABLE 5

Robustness and Falsification Tests

Table 5 reports robustness checks for the main results in Table 3 in columns 1–7 and column 8 reports results for a falsification test. In column 1, we further control for establishment age. log(1 + ESTABLISHMENT_AGE) is the natural logarithm of one plus establishment age. If the age of an establishment is missing in a year, then we replace log(1 + ESTABLISHMENT_AGE) as −1. ESTABLISHMENT_AGE_MISSING is a dummy variable equal to 1 if the age of an establishment is missing in a year, and equal to 0 otherwise. In column 2, we include event × parent firm × year fixed effects in the regression. In column 3, the dependent variable is the natural logarithm of one plus IT budget. In column 4, the dependent variable is the ratio of IT budget to the average employment during years prior to minimum wage events. In columns 5 and 6, we define a minimum wage increase at the state level to be significant if the increase is at least 50 and 75 cents per hour, respectively. Column 7 reports the results using a continuous treatment measure. Column 8 reports the results for a falsification test. We estimate equation (1) but use data for the following five sectors in which the fractions of minimum wage workers are low: Mining, quarrying, and oil and gas extraction (2-digit NAICS code 21), Utilities (2-digit NAICS code 22), Professional and technical services (2-digit NAICS code 54), Finance and insurance (2-digit NAICS code 52), and Construction (2-digit NAICS code 23). We further include event × NAICS sector × year fixed effects in column 8. We control for the natural logarithm of revenue at the establishment level as well as growth rates of real GDP per capital and annual housing price index (HPI) at the state level in all regressions. Variable definitions are available in Appendix A. Standard errors in parentheses are robust and clustered at the state and state border levels. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	1	2	3	4	5	6	7	8
TREATED × POST	0.503***	0.961***	0.125***	588.856***	0.524***	0.598***		0.016
	[0.141]	[0.258]	[0.039]	[210.989]	[0.151]	[0.133]		[0.228]
log(1 + ESTABLISHMENT_AGE)	0.027							
	[0.122]							
ESTABLISHMENT_AGE_MISSING	−0.476							
	[0.499]							
ΔMW × POST							0.547**	
							[0.141]	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event × establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event × parent firm × year FE		Yes						
Event × NAICS sector × year FE								Yes
Adj. R^2	0.709	0.960	0.859	0.672	0.701	0.707	0.763	
No. of obs.	64,532	39,094	64,532	64,532	58,100	44,136	64,532	583,689

logarithm of one plus establishment age. For the missing values, we replace them with −1 and include a dummy variable indicating whether the age of an establishment is missing in a year in the regression. Our main results are robust to controlling for establishment age, and the estimated coefficient on TREATED × POST hardly changes. The estimated coefficient on log(1+ESTABLISHMENT_AGE) shows that older establishments tend to have higher IT budgets, but the estimated coefficient is not statistically significant.

2. Control for Event × Firm × Year Fixed Effects

In column 2, we further control for event × parent firm × year fixed effects in equation (1). This specification absorbs all the effects of parent firm-level characteristics on establishment-level IT budget in a year. We drop single-unit establishments and establishments that cannot be linked to parent firms from the sample. We find that the estimated effect becomes larger. The estimated coefficient on TREATED × POST is 0.961 and statistically significant at the 1% level. This implies that treatment establishments increase IT budget by $66,808 (= 0.961/100 × 6.952 × 1,000,000) after minimum wage events relative to control establishments.

3. Alternative Dependent Variables

In columns 3 and 4, we further check the robustness of our main results using alternative dependent variables. The dependent variable in column 3 is the natural logarithm of one plus IT budget, and the estimate shows that a significant minimum wage increase on average leads to a 12.5% increase in IT budget in a year. Given that the average IT budget in treatment establishments during the pre-treatment period is $82,630, the estimate based on this specification suggests that treatment establishments increase IT budget by $10,328 (= 82,630 × 12.5%) *per year* during the first 3 years after minimum wage events relative to control establishments.

In column 4, the dependent variable is the ratio of IT budget to the average pre-event employment. The result shows that treatment establishments on average allocate $589 more per employee for new IT investment in a year after experiencing significant minimum wage increases relative to control establishments. The sample mean of average pre-event employment across treatment establishments is 37 and our estimate suggests that treatment establishments increase IT budget by $21,793 (= 589 × 37) *per year* during the first 3 years after minimum wage events relative to control establishments.

4. Alternative Definitions of Minimum Wage Events

In our baseline estimations, we define a state-level minimum wage increase to be significant if the increase is at least 25 cents per hour. In columns 5 and 6 of Table 5, we use alternative thresholds to define a minimum wage event. Specifically, a state-level minimum wage increase is defined to be significant if the increase is at least 50 and 75 cents per hour in columns 5 and 6, respectively. Our results remain robust, and the estimated effects are larger than our baseline estimate.

5. Continuous Treatment Measure

Instead of using the discrete treatment dummy variable in equation (1), we here use a continuous treatment measure. Specifically, we run the following regression:

$$\text{(5)} \qquad \text{IT\_BUDGET}_{kicspt} = \beta \times \Delta\text{MW}_{ks} \times 1(t - t_k > 0) + \Gamma' X_{ist} + \mu_{ki} + \eta_{kpt} + \varepsilon_{kicspt}.$$

For each significant minimum wage event k, ΔMW_{ks} is the change in the state-level minimum wage in the event year for the treatment state and is equal to 0 for a matched control state. We report the results in column 7. The estimated coefficient on $\Delta\text{MW}_{ks} \times 1(t - t_k > 0)$ is 0.547 and is statistically significant at the 1% level. Among all the 13 minimum wage events, the average increase in minimum wage is \$0.88 and our estimated effect suggests that this magnitude of minimum wage increase would lead to a \$33,464 (= 0.88 × 0.547/100 × 6.952 × 1,000,000) increase in IT budget per year.

6. Falsification Test

In column 8, we perform a falsification test to further validate our main results. We only include establishments in sectors in which the fractions of minimum wage workers are low. A worker is defined to be a minimum wage worker if he or she earns within 110% of the minimum wage at the state or federal level. Specifically, we utilize the CPS data from 2010 to 2018 to calculate the average fraction of minimum wage workers in a NAICS sector and use the bottom five sectors in terms of the average fraction of minimum wage workers in the test.

These five sectors are as follows, with the 2-digit NAICS code and the average fraction of minimum wage workers in each sector in the parentheses: Mining, quarrying, and oil and gas extraction (2-digit NAICS code 21, 0.75%), utilities (2-digit NAICS code 22, 0.95%), professional and technical services (2-digit NAICS code 54, 1.22%), finance and insurance (2-digit NAICS code 52, 1.30%), and construction (2-digit NAICS code 23, 2.00%).

We then estimate equation (1) and report the results in column 8 of Table 6. In this specification, we further include event × NAICS sector × year fixed effects in the regression. The estimated coefficient on TREATED × POST is economically

TABLE 6

Heterogeneous Effects by Minimum Wage Events

Table 6 reports the estimated effect of significant minimum wage increases on IT budget for each of the 13 minimum wage events based on equation (1). *p*-values in the last column are based on standard errors that are robust and clustered at the county level.

Treatment State	Event Year	Coefficient	*p*-Value
AR	2015	−0.181	0.645
DC	2014	0.552	0.017
DE	2014	−0.908	0.036
MA	2015	0.037	0.561
MD	2015	0.725	0.000
ME	2017	−0.075	0.049
MI	2014	1.031	0.000
MN	2014	0.857	0.000
NE	2015	0.875	0.002
NJ	2014	0.554	0.030
NY	2014	1.349	0.000
SD	2015	2.015	0.010
WV	2015	−0.529	0.263

small (0.016) and statistically insignificant. Reassuringly, the results show that, relative to establishments in border control counties, significant state minimum wage increases do not have large impacts on IT budgets at treatment establishments in these five sectors.

VI. Heterogeneous Responses

A. Event-by-Event Estimations

In this subsection, we report heterogeneous effects of significant minimum wage increases across the 13 minimum-wage events. Specifically, we estimate equation (1) for each minimum wage event separately. We cluster the standard errors at the county level and report the results in Table 6.

The results show that the effects on IT budgets are heterogeneous across minimum wage events. Among all 13 events, the estimated effects are positive in nine events, eight of which are at least statistically significant at the 5% level. The estimated effects are negative in the remaining four minimum wage events: Arkansas in 2015, Delaware in 2014, Maine in 2017, and West Virginia in 2015. The estimated coefficients on TREATED × POST are statistically significant at the 5% level for the minimum wage events in Delaware in 2014 (p-value = 0.036) and Maine in 2017 (p-value = 0.049). For the minimum wage events in Arkansas and West Virginia in 2015, the p-values of the estimated coefficients on TREATED × POST are 0.645 and 0.263, respectively.

For the minimum wage events in Arkansas and West Virginia in 2015, the fractions of small treatment establishments with average pre-event employment less than or equal to 10 are the highest (25.3%) among all 13 events. Small establishments could be financially constrained and could choose to scale down after experiencing an increase in labor costs. This could explain why the estimated effects on IT budget are negative in these two events. For the minimum wage event in Delaware in 2017, the average pre-event local product market competition, measured as the sales Herfindahl–Hirschman index (HHI) at the county×4-digit NAICS level, is the lowest among all 13 events. We use Data Axle (formerly known as InfoGroup) to measure sales HHI at the local product market level. When the product market is more competitive, then establishment-level profit margin would be lower and these establishments would not have enough resources to upgrade technology. As a result, treatment establishments in Delaware could choose to scale down as a response. For the event in Maine in 2017, it has the largest minimum wage increase in both level and percentage change among all 13 events. Furthermore, among all 13 events, the average pre-event fraction of treatment establishments in the accommodation and food services sector among establishments in minimum wage-sensitive industries is the lowest (29.1%) in Maine. All of these factors could potentially contribute to the negative estimated effect on IT budget in this event.

Overall, the results in Table 6 show that our baseline results in Table 3 are not driven by some particular state minimum wage events and add confidence in the external validity of the baseline estimates.

TABLE 7

Heterogeneous Effects by Industry Sectors

Table 7 reports the estimated effect of significant state minimum wage increases on IT budget separately for establishments in the accommodation and food services sector (2-digit NAICS code is 72) and the retail sector (2-digit NAICS code is 44 or 45). The dependent variable is the ratio of IT budget to the average revenue during years prior to minimum wage events. In each column, the dependent variable is scaled by the sample mean of treatment establishment prior to minimum wage events so that the estimated coefficients are comparable across two columns. For each minimum wage event, TREATED is a dummy variable equal to 1 if an establishment is located in the state experiencing a significant minimum wage increase, and equal to 0 if an establishment is located in an adjacent state that does not experience any minimum wage increase between up to 3 years before and up to 4 years after the event year. POST is a dummy variable equal to 1 for the years after the event year, and 0 otherwise. The estimation sample includes establishments in bordered counties in treatment and control states across all 13 minimum wage events. We control for the natural logarithm of revenue at the establishment level as well as growth rates of real GDP per capital and annual housing price index (HPI) at the state level in all regressions. Variable definitions are available in Appendix A. Standard errors in parentheses are robust and clustered at the state and state border levels. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Accommodation and Food Services	Retail
	1	2
TREATED × POST	0.287***	0.111
	[0.047]	[0.101]
Controls	Yes	Yes
Event × establishment FE	Yes	Yes
Event × county pair × year FE	Yes	Yes
Adj. R^2	0.750	0.710
No. of obs.	31,711	32,741

B. Accommodation and Food Services Sector Versus Retail Sector

In this subsection, we estimate the effects of significant minimum wage increases on IT budgets based on equation (1) separately for establishments in the accommodation and food services sector (2-digit NAICS code is 72) and the retail sector (2-digit NAICS code is 44 or 45). We report the results in Table 7. In each sector, we scale the dependent variable by the sample mean of treatment establishment prior to significant minimum wage events so that the estimated coefficients are comparable across these two sectors.[5]

The estimated effect is stronger for establishments in the accommodation and food services sector. The estimated coefficient on TREATED × POST is 0.287 and statistically significant at the 1% level. This suggests that state minimum wage increases lead to a 28.7% increase in IT budget relative to the sample mean of IT budget-to-revenue ratio of treatment establishments in this sector during years prior to minimum wage events. For establishments in the retail sector, the estimated coefficient on TREATED × POST is 0.111, suggesting that state minimum wage increases lead to an 11.1% increase relative to the sample mean of IT budget-to-revenue ratio of treatment establishments in this sector during years prior to minimum wage events. The estimate is marginally significant with a p-value equal to 0.101.

C. Industry-Level Employment Share of Low-Wage Occupations

We next examine how establishments' responses vary with the share of low-wage occupations employment at the 4-digit NAICS industry. To define

[5]Within the estimation window of each significant minimum wage increase, establishments may change sectors and we drop establishments in which the assigned sectors change over time.

low-wage occupations, we use the Occupational Employment Statistics (OES) from the Bureau of Labor Statistics (BLS). Specifically, for each year, we sort the 10th percentile of the hourly wage distribution at the 6-digit Standard Occupational Classification code (SOC) level into deciles. An occupation is defined as low-wage if its 10th percentile of the hourly wage distribution is in the lowest decile. We then use the 4-digit NAICS × 6-digit SOC matrices from the OES to calculate the fraction of low-wage occupations at the 4-digit NAICS industry level in each year.

To obtain a consistent definition of a 4-digit NAICS code, we convert the 2007 and 2012 version 4-digit NAICS code to the 2017 version. We calculate the average fraction of low-wage occupations employment for each 4-digit NAICS code between 2010 and 2018. Although we focus on food & accommodation services and retail sectors, there still are variations in the use of low-wage workers at the 4-digit NAICS level. For example, the fraction of low-wage workers in Electronic Shopping and Mail-Order Houses (NAICS code = 4,541) is 16.7%, whereas the fraction in Restaurants and Other Eating Places (NAICS code = 7,225) is 89.1%.

We sort industries into terciles based on the calculated fraction and then estimate equation (1) for establishments in industries in each tercile. We scale the dependent variable by the sample mean in each subsample so that the estimated coefficients are comparable across subsamples. The results are reported in Panel A of Table 8.[6]

Columns 1–3 report the results for establishments in industries employing low, medium, and high fractions of low-wage occupations employment, respectively. We observe that the estimated coefficient on TREATED × POST is larger when the fraction of low-wage occupations employment in an industry is higher. In column 1, the estimated coefficient on TREATED × POST is 0.001 and statistically insignificant. For establishments that are most likely to employ workers with low-wage occupations, the estimated coefficient is 0.266 in column 3. The difference between the estimated coefficients in columns 1 and 3 is statistically significant at the 1% level. The results are consistent with our expectation that state minimum wage increases have more bites for establishments in industries with larger fractions of low-wage occupations employment.

D. Industry-Level Employment Share of Routine Employment

Workers in routine occupations are more likely to be substituted by technologies (Autor et al. (2003). As a result, after experiencing significant minimum wage increases, establishments would have more incentives to adopt technology if more jobs could be automated. Indeed, Lordan and Neumark (2018) and Aaronson and Phelan (2023) use labor market data and show that both employment level and share of routine occupations decline after state minimum wage increases. In this subsection, we provide more direct evidence and examine how the routineness of occupations interacts with the impact of minimum wage hikes on IT investment.

[6]Within the estimation window of each significant minimum wage increase, establishments may change industries and we drop establishments in which the assigned terciles of low-wage employment shares change over time.

TABLE 8

Heterogeneous Effects by Industry, Firm, and Establishment Characteristics

Table 8 reports the heterogeneous effects of significant state minimum wage increases on IT budget by industry, firm, and establishment characteristics. Panel A reports the estimated effects for establishments in industries with low, medium, and high average employment shares of low-wage occupations between 2010 and 2018. Panel B reports the estimated effects for establishments in industries with low, medium, and high average employment shares of routine occupations between 2010 and 2018. Panel C reports the estimated effects for establishments in small, medium, and large firms, respectively. A firm is defined to be small, medium, and large if its employment in the year just prior to a minimum wage event is less than 50, between 50 and 499, and larger than or equal to 500, respectively. Panel D reports estimated effects for establishments with low, medium, or high personal computers (PCs) per employee in the year just prior to a minimum wage event. Panel E reports estimated effects for establishments with low, medium, or high labor productivity, defined as revenue per employee, in the year just prior to a minimum wage event. In each column of a panel, the dependent variable is scaled by the sample mean in the subsample so that the estimated coefficients are comparable across subsamples. For each minimum wage event, TREATED is a dummy variable equal to 1 if an establishment is located in the state experiencing a significant minimum wage increase, and equal to 0 if an establishment is located in an adjacent state that does not experience any minimum wage increase between up to 3 years before and up to 4 years after the event year. POST is a dummy variable equal to 1 for the years after the event year, and 0 otherwise. The estimation sample includes establishments in bordered counties in treatment and control states across all 13 minimum wage events. We control for the natural logarithm of revenue at the establishment level as well as growth rates of real GDP per capital and annual housing price index (HPI) at the state level in all regressions. Variable definitions are available in Appendix A. Standard errors in parentheses are robust and clustered at the state and state border levels. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Low	Medium	High
	1	2	3
Panel A. Employment Share of Low-Wage Occupations			
TREATED × POST	0.001	0.107	0.266***
	[0.060]	[0.081]	[0.048]
Controls	Yes	Yes	Yes
Event × establishment FE	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes
Adj. R^2	0.685	0.766	0.717
No. of obs.	17,799	9,074	36,392
Panel B. Employment Share of Routine Occupations			
TREATED × POST	0.045	0.092*	0.244***
	[0.106]	[0.046]	[0.052]
Controls	Yes	Yes	Yes
Event × establishment FE	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes
Adj. R^2	0.713	0.761	0.715
No. of obs.	6,599	19,261	37,785
Panel C. Parent Firm Size			
	Small	Medium	Large
	1	2	3
TREATED × POST	−0.013	0.236***	0.022
	[0.057]	[0.072]	[0.080]
Controls	Yes	Yes	Yes
Event × establishment FE	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes
Adj. R^2	0.781	0.718	0.737
No. of obs.	12,294	27,783	15,499
Panel D. IT Capital			
	Low	Medium	High
	1	2	3
TREATED × POST	0.215***	0.168*	0.028
	[0.058]	[0.091]	[0.074]
Establishment and state controls	Yes	Yes	Yes
Event × establishment FE	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes
Adj. R^2	0.732	0.687	0.756
No. of obs.	22,669	21,045	19,001

(continued on next page)

TABLE 8 (continued)

Heterogeneous Effects by Industry, Firm, and Establishment Characteristics

Panel E. Labor Productivity

	Low	Medium	High
	1	2	3
TREATED × POST	0.114**	0.123*	0.054
	[0.048]	[0.064]	[0.095]
Establishment and state controls	Yes	Yes	Yes
Event × establishment FE	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes
Adj. R^2	0.755	0.771	0.634
No. of obs.	22,211	21,158	19,346

To measure the routineness of an occupation, we use data from O*NET and calculate six composite indices for each occupation following Acemoglu and Autor (2011).[7] These six composite indices represent the extent to which an occupation is routine cognitive, nonroutine cognitive analytical, nonroutine cognitive interpersonal, routine manual, nonroutine manual physical, and nonroutine manual interpersonal. We standardize each composite index so that the minimum value is 0 and the standard deviation is 1. We calculate the routine task share of each occupation as the sum of the standardized routine cognitive and routine manual task values divided by the sum of all six standardized values.

In each year, we sort the routine task share at the 6-digit SOC level into deciles. An occupation is defined as routine if its routine task share is in the top decile in a year. We again use the 4-digit NAICS × 6-digit SOC matrices from the OES to calculate the fraction of routine occupations at the 4-digit NAICS industry level in each year and then calculate the average fraction of routine occupations employment for each 4-digit NAICS code between 2010 and 2018. Examples of industries with the highest employment shares of routine occupations include Vending Machine Operators (NAICS code = 4542) and Restaurants and Other Eating Places (NAICS code = 7225) while Shoe Stores (NAICS code = 4482) and Clothing Stores (NAICS code = 4481) have the lowest employment shares of routine occupations among all industries within food and accommodation services and retail sectors.

We sort industries into terciles based on the calculated routine occupations employment share and then estimate equation (1) for establishments in industries in each tercile. We scale the dependent variable by the sample mean in each subsample so that the estimated coefficients are comparable across subsamples. The results are reported in Panel B of Table 8.[8]

Columns 1–3 report estimated effects for establishments in industries with low, medium, and high employment shares of routine occupations, respectively. We observe that the estimated coefficient on TREATED × POST is larger when the

[7]We use version 25.0 of O*NET data released in Aug. 2020. The code to calculate the six composite indices is available at https://economics.mit.edu/people/faculty/david-h-autor/data-archive.

[8]Within the estimation window of each significant minimum wage increase, establishments may change industries and we drop establishments in which the assigned terciles of routine occupation employment shares change over time.

fraction of routine occupations employment in an industry is higher. In column 1, the estimated coefficient on TREATED × POST is 0.045 and statistically insignificant. Among establishments that are most likely to employ workers with routine occupations, the estimated coefficient is 0.244 in column 3. The difference between the estimated coefficients in columns 1 and 3 is statistically significant at the 10% level.

E. Pre-Event Parent Firm Size

We next examine the heterogeneity in IT budget response by the size of parent firm in pre-event periods. Since 2011, CiTDB starts to report the estimated number of employees in each establishment's parent firm. For each minimum wage event, we split the sample based on the parent firm employment 1 year prior to the event. Specifically, we define a parent firm to be large if the firm-level employment is greater than or equal to 500 and to be small if the firm-level employment is less than or equal to 49. All other firms are classified as medium-sized firms (employment between 50 and 499).[9] We then estimate equation (1) for each subsample, and again, we scale the dependent variable by the sample mean in each subsample.

The results are reported in Panel C of Table 8. Our estimations show that the effect of significant state minimum wage increases on IT budget is largest among medium-sized firms. The estimated effects for establishments in small and large firms are much smaller and statistically insignificant.[10]

The small impacts of significant state minimum wage increases on small and large firms could be due to different reasons. Small firms could be financially constrained and hence do not have enough resources to upgrade technology; however, large firms do not necessarily need to adopt new technology to absorb minimum wage shocks. Large firms usually operate in multiple sectors or geographic areas and have access to internal capital markets. As a result, large firms could reallocate resources among different establishments within the same firm and could better weather cash flow shocks without relying on investing in labor cost-saving technologies. This result for large firms is consistent with the evidence in Ashenfelter and Jurajda (2022) in which the authors do not find an association between the adoption of labor-saving technology and minimum wage hikes based on data from McDonald's restaurants.

F. Pre-Event IT Capital

We next examine the heterogeneity in the IT budget response by IT capital. Following Brynjolfsson and Hitt (2003), we use the number of installed PCs per employee to measure IT capital. For each significant minimum wage event, we sort establishments into terciles based on the IT capital in the year just prior to the event. We then estimate equation (1) for establishments in each tercile and scale the

[9]The choice of the size cutoffs is motivated by Haltiwanger, Jarmin, and Miranda (2013), but we further aggregate firm size classes. Otherwise, the number of observations in each size class would be small.

[10]There are 8,956 observations missing compared to other panels of this table. The reason is that there are 1,900 establishments that do not have the assigned ranks of parent firm sizes because they do not have data 1 year prior to minimum wage events.

dependent variable by the sample mean in each subsample so that the estimated coefficients are comparable across subsamples.

In the bottom tercile, the average IT capital is 0.47 PC per employee while, among establishments in the top tercile, the average IT capital is 1.02 PC per employee. We report the results in Panel D of Table 8. Our estimations show that the effect of state minimum wage increases on IT budget is larger for establishments with lower IT capital in pre-event periods, and the estimated effect monotonically decreases with the pre-event IT capital. For establishments with the lowest IT capital prior to the minimum wage event, the estimated coefficient on TREATED × POST in column 1 suggests that state minimum wage increases lead to a 21.5% increase in IT budget relative to the sample mean, and the estimated coefficient is statistically significant at the 1% level. Turning to the establishments with the highest IT capital prior to the event, the results in column 3 show that the estimated coefficient on TREATED × POST is only 0.028 and statistically insignificant. Moreover, the difference between the estimated coefficients in columns 1 and 3 is statistically significant at the 5% level. Our results suggest that establishments with lower IT capital start to catch up with technology upgrading because of state minimum wage hikes.

G. Pre-Event Labor Productivity

We finally examine the heterogeneity in the IT budget response by labor productivity. We define labor productivity as revenue per employee. For each significant minimum wage increase, we sort establishments into terciles based on labor productivity in the year just prior to the event. We then estimate equation (1) for establishments in each tercile and scale the dependent variable by the sample mean in each subsample so that the estimated coefficients are comparable across subsamples.

There is a large dispersion in the pre-event labor productivity across the three terciles. For establishments in the bottom tercile, the average labor productivity is 0.06; however, for establishments in the top tercile, the average labor productivity increases to 0.45. We report the results in Panel E of Table 8.

For establishments with low and medium pre-event labor productivity, the estimated coefficients on TREATED × POST in columns 1 and 2 suggest that significant state minimum wage increases lead to an 11.4% and a 12.3% increase in IT budget relative to the sample mean, respectively, and the estimated coefficients are at least statistically significant at the 10% level. For establishments with high pre-event labor productivity, the estimated coefficient on TREATED × POST is 0.054 and is not statistically significant.

Our interpretation of the results is that significant state minimum wage increases raise labor costs for low-wage workers, and the increased wage floors are more likely to exceed the output value of workers in establishments with lower pre-event labor productivity. As a result, these establishments are more likely to replace labor with technology because of the increased wedge between wages and labor productivity. These results are consistent with our expectation that establishments have more incentives to automate labor when increased minimum wages impose higher costs.

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VII. Components of IT Budget, IT Capital, and Employment

A. Components of IT Budget

CiTDB decomposes the total IT budget into four major components: hardware, software, services-related, and communication-related.[11] Hardware includes PCs, servers, terminals, printers, and storage devices. IT services include systems integration, computer hardware support, and maintenance services. IT communications include routers, Wi-Fi transmitters, wide-area network (WAN) and local-area network (LAN) equipment, cable boxes, and other network equipment.

In this subsection, we utilize this more detailed data to estimate how the increases in total IT budget is allocated among different components. The results are reported in columns 1–4 in Table 9. The results show that a significant increase in state minimum wage has economically and statistically significant effects on all four components. Specifically, the estimations show that hardware and software IT budget increase by \$4,380 and \$5,492 per year, respectively, following a significant increase in state minimum wage. The estimated effect on communication-related budget is the smallest, and a significant state minimum wage increase would lead to a \$3,406 increase in communication-related budget per year. Finally, the estimated effect on services-related IT budget is the largest, and it is consistent with the summary statistics in Table 1. The results show that treatment establishments increase \$19,813 in services-related IT budget per year relative to control establishments. IT services include systems integration, computer hardware support, and maintenance services and it can be interpreted as part of the budget for purchasing

TABLE 9

Effects of Significant State Minimum Wage Increases on Components of IT Budget and PCs per Employee

Table 9 reports the estimated effects of significant state minimum wage increases on components of IT budget and personal computers (PCs) per employee. The dependent variables in columns 1–4 are hardware, software, services-related, and communication-related budget scaled by the average revenue during years prior to minimum wage events. The dependent variable in column 5 is the ratio of the number of installed PCs to employment. For each minimum wage event, TREATED is a dummy variable equal to 1 if an establishment is located in the state experiencing a significant minimum wage increase, and equal to 0 if an establishment is located in an adjacent state that does not experience any minimum wage increase between up to 3 years before and up to 4 years after the event year. POST is a dummy variable equal to 1 for the years after the event year, and 0 otherwise. The estimation sample includes establishments in bordered counties in treatment and control states across all 13 minimum wage events. We control for the natural logarithm of revenue at the establishment level as well as growth rates of real GDP per capital and annual housing price index (HPI) at the state level in all regressions. Variable definitions are available in Appendix A. Standard errors in parentheses are robust and clustered at the state and state border levels. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Hardware	Software	Services-Related	Communications-Related	PCs per Employee
	1	2	3	4	5
TREATED × POST	0.063***	0.079***	0.285***	0.049***	0.051***
	[0.016]	[0.025]	[0.083]	[0.014]	[0.018]
Controls	Yes	Yes	Yes	Yes	Yes
Event × establishment FE	Yes	Yes	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.699	0.738	0.670	0.756	0.730
No. of obs.	64,532	64,532	64,532	64,532	64,504

[11]The sum of the four subcomponent budgets is not equal to the total budget reported in CiTDB. The remaining part of the budget could be thought as other IT budget.

certain technologies. For example, if the owner of a fast-food restaurant decides to install voice recognition drive-thrus, then the owner needs to pay for service fees in addition to the costs of purchasing hardware and software.

For establishments in accommodation and food services or retail sectors, a typical upgrade in technology reflects the installation of interactive kiosks. Hence, it would be helpful to compare our estimated effect of significant state minimum wage increases on IT budget to the price of an interactive kiosk. Based on an article in QSR Magazine, the typical cost of a kiosk would be $5,000, but this cost does not include costs for software and services (https://www.qsrmagazine.com/finance/are-kiosks-too-expensive-restaurants). Our estimates suggest that the hardware budget at treatment establishments would increase by $4,380 per year following a significant minimum wage increase and a simple back-of-envelop calculation suggests that, relative to control establishments, an average treatment establishment in our sample would be affordable to purchase around three kiosks in the following 3 years after a significant minimum wage event.

B. Effects on IT Capital

So far, we document that establishments in states experiencing significant minimum wage increases allocate more resources to IT budgets compared to establishments in border control counties. In this subsection, we examine whether the increased IT budgets at treatment establishments materialize into higher IT capital stock. We again use the number of installed PCs per employee to measure IT capital. We focus on the measure of PCs per employee for two reasons. First, CiTDB does not provide information on technologies that are usually adopted by food services and accommodation or retail sectors, such as the number of installed self-serving kiosks in restaurants or the number of installed booking management systems in hotels. Second, computerization is a commonly used proxy for technology change (Autor et al. (2003), Autor and Dorn (2013)).

We use equation (1) to estimate the impact of significant state minimum wage increases on IT capital. The results are reported in column 5 of Table 9. We cluster standard errors at both the state and state border levels. In column 1, the estimated coefficient on TREATED × POST is 0.051, and statistically significant at the 1% level. In terms of economic magnitude, the estimated effect represents an 8.6% increase relative to the average IT capital in treatment establishments during pre-event periods (0.594). Given that the sample mean of pre-event employment at treatment establishments is 37, our estimation also implies that, relative to control establishments, treatment establishments purchase almost two more PCs as a response to significant increases in the state minimum wage.

C. Employment

Finally, we examine the employment effects of state minimum wage hikes. We first report the estimated average treatment effect of significant minimum wage increases on total employment at the establishment level and then examine how IT budget and employment are connected.

Panel A of Table 10 and Figure 2 report the estimated average and dynamic treatment effects of significant state minimum wage increases on employment. The

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TABLE 10

Effects of Significant State Minimum Wage Increases on Total Employment

Table 10 reports the estimated employment effects of significant state minimum wage increases. Panel A reports the estimated effects on total employment at the establishment level. The dependent variable is the natural logarithm of total employment. Column 1 reports the estimated average and dynamic treatment effects of significant state minimum wage increases on employment level. Panel B reports the estimated effects for establishments in industries with low, medium, and high average employment shares of routine occupations between 2010 and 2018. Panel C reports estimated effects for establishments with low, medium, or high labor productivity, defined as revenue per employee, in the year just prior to a minimum wage event. For each minimum wage event, TREATED is a dummy variable equal to 1 if an establishment is located in the state experiencing a significant minimum wage increase, and equal to 0 if an establishment is located in an adjacent state that does not experience any minimum wage increase between up to 3 years before and up to 4 years after the event year. POST is a dummy variable equal to 1 for the years after the event year, and 0 otherwise. We control for the natural logarithm of revenue at the establishment level as well as growth rates of real GDP per capital and annual housing price index (HPI) at the state level in all regressions. Variable definitions are available in Appendix A. Standard errors in parentheses are robust and clustered at the state and state border levels. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Panel A. Average and Dynamic Treatment Effects

	1	2
TREATED × POST	−0.027**	
	[0.012]	
TREATED × YEAR − 4		0.000
		[0.018]
TREATED × YEAR − 3		0.008
		[0.012]
TREATED × YEAR − 2		0.002
		[0.009]
TREATED × YEAR0		−0.005
		[0.004]
TREATED × YEAR1		−0.029***
		[0.010]
TREATED × YEAR2		−0.026
		[0.018]
TREATED × YEAR3		−0.023
		[0.022]
Controls	Yes	Yes
Event × establishment FE	Yes	Yes
Event × county pair × year FE	Yes	Yes
Adj. R^2	0.919	0.919
No. of obs.	64,532	64,532

	Low	Medium	High
	1	2	3
Panel B. By Industry-Level Share of Routine Occupations			
TREATED × POST	−0.029	0.005	−0.047**
	[0.063]	[0.033]	[0.019]
Event × establishment FE	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes
Adj. R^2	0.915	0.926	0.916
No. of obs.	6,599	19,261	37,785
Panel C. By Labor Productivity			
TREATED × POST	−0.047*	−0.058***	0.053
	[0.028]	[0.019]	[0.035]
Event × establishment FE	Yes	Yes	Yes
Event × county pair × year FE	Yes	Yes	Yes
Adj. R^2	0.903	0.928	0.920
No. of obs.	22,211	21,158	19,346

dependent variable is the natural logarithm of total employment. The results in column 1 show that total employment in treatment establishments is on average 2.7% lower relative to the counterfactual following significant state minimum wage increases. Column 2 reports the estimated dynamic treatment effects. The results

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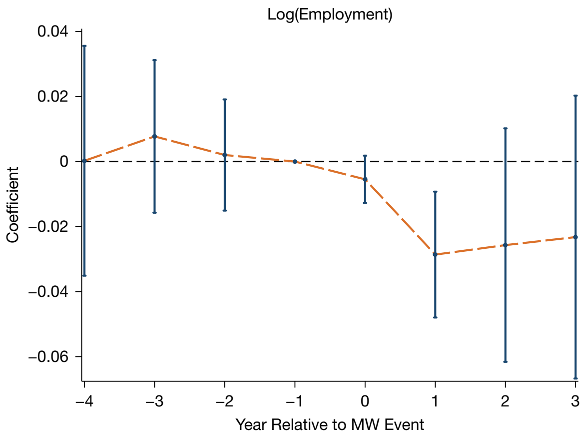
FIGURE 2

Dynamic Treatment Effects of Significant State Minimum Wage Increases on Employment Level

Figure 2 reports the estimated dynamic treatment effects of significant state minimum wage increases on establishment-level total employment based on equation (2). The estimated coefficients represent the relative change in the natural logarithm of total employment between treatment and control establishments 4 years before and 3 years after minimum wage events, compared to the year immediately before each event. The bar around each dot represents the 95% confidence interval.



show that, during the pre-treatment period, the estimated coefficients are close to zeros and not statistically significant, suggesting that the parallel trend assumption of a difference-in-differences design is unlikely to be violated in the data. The trajectories of employment at treatment and control establishments only start to diverge after a significant state minimum wage increase. Compared to the year just before each event, the total employment level in treatment establishments decrease by 2.9% relative to control establishments during the first year in the post-treatment period. The estimated coefficient is statistically significant at the 1% level. The magnitudes of the estimated effects stay stable during the second and third years in the post-treatment period. Specifically, the results suggest that significant state minimum wage increases lead to a 2.6% and a 2.3% decrease in employment at treatment establishments, respectively. However, the estimations are less precise, and the estimated effects are not statistically significant at the conventional level.

We also plot the trends of the natural logarithm of total employment for treatment and control establishments separately in Figure B3. For each period relative to the event year, we first calculate the average natural logarithm of total employment for treatment and control establishments within each pair of border counties in a minimum wage event. After that, we then calculate the average natural logarithm of total employment for treatment and control establishments across all pairs of border counties in all minimum wage events. Figure B3 shows that the trends of employment are parallel across treatment and control establishments prior to minimum wage events, and total employment decreases more in treatment establishments relative to control establishments after minimum wage events.

How are IT budget and total employment at the establishment level connected? The answer to this question is not clear ex ante. On the one hand, technology may displace workers in occupations that perform routine tasks (Autor et al. (2003)). On the other hand, adopting technology could increase workers' productivity and establishments' revenue, which in turn increases labor demand. The empirical evidence at the establishment or firm level is mixed in the literature. For example, Aghion, Antonin, Bunel, and Jaravel (2022) uses data from France and finds that automation has a positive impact on employment and the result holds at plant, firm, and industry level. Acemoglu et al. (2020) and Domini, Grazzi, Moschella, and Treibich (2021) also use firm-level French data and reach a similar conclusion. Studies using data from Canada (Dixon et al. (2021)), The Netherlands (Bessen et al. (2020)), Germany (Benmelech and Zator (2021)), and Spain (Koch et al. (2021)) also find that robot adoption is associated with higher firm-level employment. Humlum (2021) uses data from Demark and estimates a dynamic model to evaluate the labor market impacts of robot adoption. He finds that robot adoption leads firms to lay off production workers and hire more tech workers. Contrary to the aforementioned studies, Bonfiglioli, Crinò, Fadinger, and Gancia (2020) uses an instrumental variable strategy and finds that firms that adopt more robots experience a larger reduction in employment in France. In the USA, Acemoglu et al. (2022a) uses data from a new module introduced in the 2019 Annual Business Survey (ABS) conducted by the US Census Bureau in partnership with the National Center for Science and Engineering Statistics (NCSES) and finds that use and adoption of technologies do not result in significant changes in employment at the firm level. Acemoglu, Autor, Hazell, and Restrepo (2022b) use job posting data and find that establishments where tasks of jobs are compatible with artificial intelligence (AI) capabilities reduce hirings in overall and non-AI positions. But AI exposure at the industry or occupation level does not affect employment or wage growth.

To strengthen the interpretation that establishments could upgrade technology to automate labor after experiencing higher state minimum wages, we examine heterogeneous employment effects by industry and establishment characteristics that could affect establishments' incentives to do so. We focus on two characteristics.

First, we examine the heterogeneous employment effect by industry-level employment share of routine occupations. Autor, Levy, and Murnane (2003) argue and provide evidence showing that workers in routine occupations are more likely to be substituted by technology. As a result, when facing higher state minimum wages, establishments in industries with higher employment shares of routine occupations would have more incentives to replace labor with technology to save labor costs.

Results in Panel B of Table 8 show that treatment establishments in industries with the highest employment shares of routine occupations increase IT budget the most after minimum wage events. The average employment share of routine occupations in industries falling into the top tercile is 15.86%, and it is much higher than the ones in industries falling into the medium and bottom terciles (which are 4.97% and 2.40%, respectively). As a result, treatment establishments in industries with the highest employment shares of routine occupations have more incentives to replace labor with technology. We therefore expect to observe a lower employment level in these establishments relative to the counterfactual after minimum wage events.

We estimate the same specification as in Panel B of Table 8, but use the natural logarithm of establishment-level total employment as the dependent variable. The results are reported in Panel B of Table 10. The results are consistent with our expectation and show that the employment level of treatment establishments in industries with the highest employment shares of routine occupations is 4.7% lower relative to control establishments after experiencing significant state minimum wage increases. We do not find significant employment effects for treatment establishments in industries with low and medium employment shares of routine occupations.

Second, we examine the heterogeneous employment effect by establishment-level labor productivity 1 year prior to minimum wage events. We argue that increased wage floors are more likely to exceed the output value of workers in establishments with lower pre-event labor productivity, and therefore, these establishments have more incentives to automate labor because of this increased wedge between wages and labor productivity. Results in Panel E of Table 8 show that increases in IT budgets after minimum wage events concentrate in establishments with low and medium pre-event labor productivity. If these establishments replace labor with technology, then we expect to observe lower employment levels in these establishments relative to the counterfactual as a response to increased labor costs.

We estimate the same specification as in Panel E of Table 8, but use the natural logarithm of establishment-level total employment as the dependent variable. The results are reported in Panel C of Table 10. The results are consistent with our expectation. The estimations show that employment levels of treatment establishments with low and medium pre-event labor productivity are 4.7% and 5.8% lower, respectively, relative to control establishments after state minimum wage increases. Overall, the evidence in Panels B and C of Table 10 strengthens the interpretation that state minimum wage increases could induce establishments to automate labor to countervail increased labor costs.

We acknowledge the limitations of the results in Table 10. In Panel A, our estimated average treatment effect of significant minimum wage increases on establishment-level total employment is small. Given that the sample mean of pre-event employment in treatment establishments is 37, the estimated average treatment effect implies that treatment establishments' workforce on average reduces by one after minimum wage events. The small effect could be due to changes in workforce composition after minimum wage events. For example, establishments could displace employees with routine occupations but hire employees who could help customers interact with the newly adopted technologies (Autor et al. (2003), Acemoglu and Restrepo (2019)). Our results in Panels B and C provide indirect evidence on labor automation. It is unfortunate that we do not have data on employment by occupation at the establishment level in CiTDB, and such data limitation prevents us from further examining the labor composition hypothesis and providing more direct evidence on labor automation.

VIII. Conclusion

This article examines the effects of state minimum wage increases on IT budget at the establishment level. We explore 13 significant state minimum wage increases and use a difference-in-differences design to estimate the effects. Depending on the specification, our estimates show that establishments in states experiencing significant minimum wage increases on average allocate between $10,328 and $66,808 more per year to IT budget during the first 3 years after experiencing significant state minimum wage increases.

We also find that establishments' responses to minimum wage increases are heterogeneous. Specifically, our results show that the estimated effect is stronger for establishments in industries employing larger shares of low-wage or routine occupations. We also document that the estimated effect is concentrated in establishments whose parent firms are medium-sized (employment is between 50 and 499), and significant state minimum wage increases have little impact on IT budget for establishments whose parent firms are small (employment ≤ 49) or large (employment ≥ 500). We also show that IT budget responses are stronger for establishments with lower IT capital or labor productivity in pre-event periods.

Our results show that higher IT budgets materialize into higher IT capital. Specifically, the number of installed PCs per employee increases at treatment establishments following state minimum wage events. Our estimations suggest that an average treatment establishment in our sample purchases around two more PCs after significant minimum wage increases, relative to control establishments.

Finally, our results suggest that significant state minimum wage increases on average lead to an economically small decrease in total employment in treatment establishments. We further show that the employment effect is the largest for establishments in industries with the highest employment shares of routine occupations and is stronger for establishments with lower labor productivity 1 year prior to minimum wage events. Our results strengthen the interpretation that state minimum wage increases could induce establishments to automate labor to countervail increased labor costs.

Appendix A. Variable Definitions

IT_BUDGET/REVENUE (%): The ratio of IT budget to the average revenue during years prior to minimum wage events at the establishment level, expressed in percentage points. Source: CiTDB.

HARDWARE/REVENUE (%): The ratio of hardware budget to the average revenue during years prior to minimum wage events at the establishment level, expressed in percentage points. Source: CiTDB.

SOFTWARE/REVENUE (%): The ratio of software budget to the average revenue during years prior to minimum wage events at the establishment level, expressed in percentage points. Source: CiTDB.

SERVICES/REVENUE (%): The ratio of service-related budget to the average revenue during years prior to minimum wage events at the establishment level, expressed in percentage points. Source: CiTDB.

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COMMUNICATIONS/REVENUE (%): The ratio of communications-related budget to the average revenue during years prior to minimum wage events at the establishment level, expressed in percentage points. Source: CiTDB.

IT_BUDGET ($000): The level of IT budget at the establishment level, expressed in thousands of 2018 dollars. Source: CiTDB.

EMPLOYMENT: The level of total employment at the establishment level. Source: CiTDB.

REVENUE ($ Million): The level of revenue at the establishment level, expressed in millions of 2018 dollars. Source: CiTDB.

PC_PER_EMPLOYEE: The ratio of number of personal computers (PCs) to total employment at the establishment level. Source: CiTDB.

LABOR_PRODUCTIVITY: The ratio of revenue to total employment at the establishment level. Source: CiTDB.

log(1 + ESTABLISHMENT_AGE): The natural logarithm of one plus establishment age. If the age of an establishment is missing in a year, we replace log (1 + ESTABLISHMENT_AGE) as −1. Source: CiTDB.

ESTABLISHMENT_AGE_MISSING: A dummy variable is equal to 1 if the age of an establishment is missing in a year, and equal to 0 otherwise. Source: CiTDB.

TREATED: For each minimum wage event, TREATED is a dummy variable equal to 1 if an establishment is located in the state experiencing a significant minimum wage increase, and equal to 0 if an establishment is located in an adjacent state that does not experience any minimum wage increase between up to 3 years before and up to 4 years after the event year. Source: CiTDB and David Neumark's website at https://sites.socsci.uci.edu/~dneumark/datasets.html.

POST: For each minimum wage event, POST is a dummy variable equal to 1 for the years after the event year, and 0 otherwise. Source: CiTDB and David Neumark's website at https://sites.socsci.uci.edu/~dneumark/datasets.html.

YEAR $-\tau$: For each minimum wage event, YEAR $-\tau$ is a dummy variable equal to 1 for the τth year before the event year, and equal to 0 otherwise. Source: CiTDB and David Neumark's website at https://sites.socsci.uci.edu/~dneumark/datasets.html.

YEARτ: For each minimum wage event, YEARτ is a dummy variable equal to 1 for the τth year after the event year, and equal to 0 otherwise. Source: CiTDB and David Neumark's website at https://sites.socsci.uci.edu/~dneumark/datasets.html.

GDP_PER_CAPITA_GROWTH_RATE: Growth rate of real GDP per capital at the state level. Source: the Bureau of Economic Analysis (BEA) Regional Economic Accounts.

HPI_GROWTH_RATE: Growth rate of annual housing price index (HPI) at the state level. Source: the Federal Housing Finance Agency (FHFA).

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Appendix B. Additional Figures and Tables

FIGURE B1

Map of Treatment and Border Control Counties

Figure B1 reports the map of treatment and border control counties in the sample.

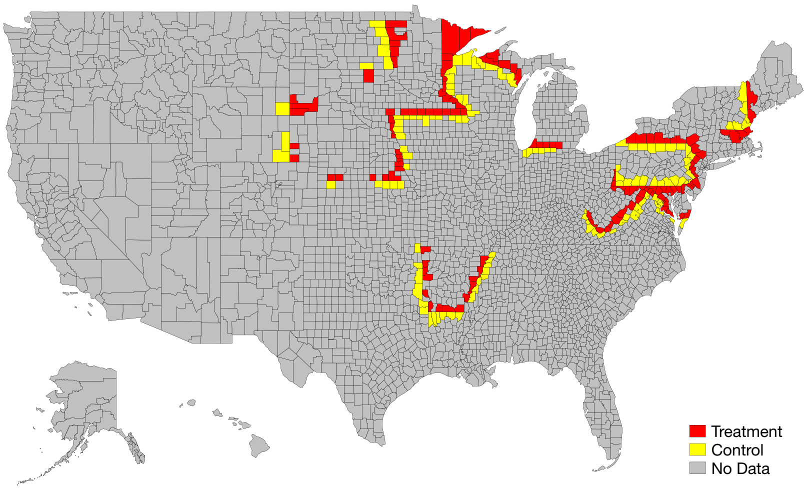

FIGURE B2

Trends of IT Budget for Treatment and Control Establishments

Figure B2 reports the trends of IT budget for treatment and control establishments separately. For each period relative to the event year of a minimum wage event, we first calculate the average IT budget for treatment and control establishments within each pair of border counties and then calculate the average IT budget for treatment and control establishments across all pairs of border counties and across all minimum wage events.

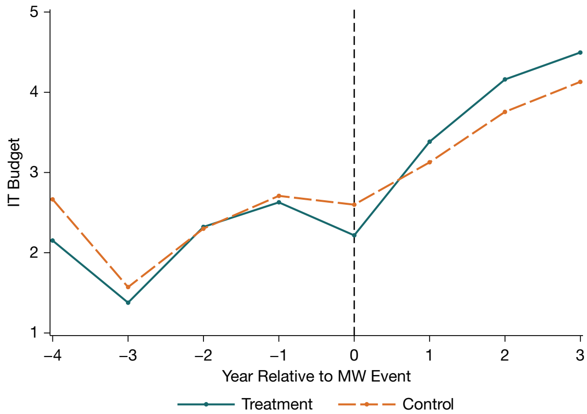

FIGURE B3

Trends of Total Employment for Treatment and Control Establishments

Figure B3 reports the trends of the natural logarithm of total employment for treatment and control establishments separately. For each period relative to the event year of a minimum wage event, we first calculate the average natural logarithm of employment for treatment and control establishments within each pair of border counties and then calculate the average natural logarithm of employment for treatment and control establishments across all pairs of border counties and across all minimum wage events.

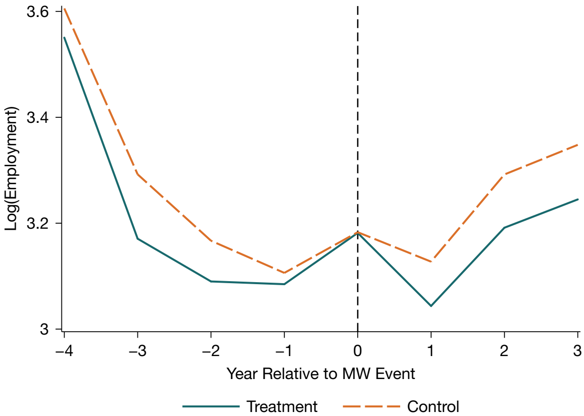

TABLE B1

State Minimum Wage Changes Between 2010 and 2018

Table B1 reports the state minimum wage changes between 2010 and 2018. The reported minimum wage changes are in nominal terms. The 13 minimum wage increases used in the analyses are in bold.

State	2010	2011	2012	2013	2014	2015	2016	2017	2018
AK	0.50	0.00	0.00	0.00	0.00	1.00	1.00	0.05	0.04
AL	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
AR	0.00	0.00	0.00	0.00	0.00	**0.25**	0.50	0.50	0.00
AZ	0.00	0.10	0.30	0.15	0.10	0.15	0.00	1.95	0.50
CA	0.00	0.00	0.00	0.00	1.00	0.00	1.00	0.50	0.50
CO	−0.03	0.11	0.28	0.14	0.22	0.23	0.08	0.99	0.90
CT	0.25	0.00	0.00	0.00	0.45	0.45	0.45	0.50	0.00
DC	0.00	0.00	0.00	0.00	**1.25**	1.00	1.00	1.00	0.75
DE	0.00	0.00	0.00	0.00	**0.50**	0.50	0.00	0.00	0.00
FL	0.00	0.06	0.36	0.12	0.14	0.12	0.00	0.05	0.15
GA	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
HI	0.00	0.00	0.00	0.00	0.00	0.50	0.75	0.75	0.85
IA	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
ID	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
IL	0.25	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
IN	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
KS	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
KY	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
LA	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
MA	0.00	0.00	0.00	0.00	0.00	**1.00**	1.00	1.00	0.00
MD	0.00	0.00	0.00	0.00	0.00	**1.00**	0.50	0.50	0.85
ME	0.00	0.00	0.00	0.00	0.00	0.00	0.00	**1.50**	1.00
MI	0.00	0.00	0.00	0.00	**0.75**	0.00	0.35	0.40	0.35
MN	0.00	0.00	0.00	0.00	**0.75**	1.00	0.50	0.00	0.15
MO	0.00	0.00	0.00	0.10	0.15	0.15	0.00	0.05	0.15
MS	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
MT	0.00	0.10	0.30	0.15	0.10	0.15	0.00	0.10	0.15
NC	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
ND	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
NE	0.00	0.00	0.00	0.00	0.00	**0.75**	1.00	0.00	0.00
NH	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

(continued on next page)

TABLE B1 (continued)

State Minimum Wage Changes Between 2010 and 2018

State	2010	2011	2012	2013	2014	2015	2016	2017	2018
NJ	0.00	0.00	0.00	0.00	**1.00**	0.13	0.00	0.06	0.16
NM	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
NV	0.00	0.70	0.00	0.00	0.00	0.00	0.00	0.00	0.00
NY	0.00	0.00	0.00	0.00	**0.75**	0.75	0.25	0.70	0.70
OH	0.00	0.10	0.30	0.15	0.10	0.15	0.00	0.05	0.15
OK	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
OR	0.00	0.10	0.30	0.15	0.15	0.15	0.50	0.50	0.50
PA	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
RI	0.00	0.00	0.00	0.35	0.25	1.00	0.60	0.00	0.50
SC	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
SD	0.00	0.00	0.00	0.00	0.00	**1.25**	0.05	0.10	0.20
TN	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
TX	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
UT	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
VA	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
VT	0.00	0.09	0.31	0.14	0.13	0.42	0.45	0.40	0.50
WA	0.00	0.12	0.37	0.15	0.13	0.15	0.00	1.53	0.50
WI	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
WV	0.00	0.00	0.00	0.00	0.00	**0.75**	0.75	0.00	0.00
WY	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

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