The Dynamics of Labor Market Frictions, Input Substitution, and Corporate Investment *

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ABSTRACT

Labor market frictions shape corporate investment by constraining firms' ability to adapt to labor cost shocks. We examine how structural shifts—driven by technological change, globalization, union decline, and import competition—have altered firms' investment responses to minimum wage increases. Since the early 1980s, investment sensitivity to wage changes has declined from strongly negative to economically insignificant. This pattern reflects greater input substitutability: firms more exposed to automation, offshoring, or weaker labor institutions are better able to absorb cost shocks. Our findings offer new insight into how evolving labor frictions affect capital allocation and suggest broader implications for other corporate decisions.

KEYWORDS: LABOR MARKET FRICTIONS, CORPORATE INVESTMENT, INPUT SUBSTITUTION, AUTOMATION, GLOBALIZATION, U.S.-CHINA

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I. Introduction

The sensitivity of corporate investment to labor cost shocks in the U.S. has declined dramatically since the early 1980s. In the 1980s and 1990s, increases in the minimum wage significantly reduced firm investment, but since 2000, this relationship has effectively vanished. We argue that this shift reflects a fundamental transformation of labor markets: U.S. firms today face fewer constraints in responding to domestic wage pressure due to greater capital-labor substitutability, expanded access to foreign labor, and declining union power.

We examine this phenomenon through the lens of production theory, emphasizing two complementary channels of substitution. First, technological advances have increased the elasticity of substitution between capital and labor, enabling firms to automate instead of reducing investment when wages rise. Second, globalization has made it easier to substitute foreign for domestic labor, allowing firms to offshore production while maintaining capital intensity. Declining unionization has further enhanced firms' capacity to pursue these strategies. Together, these developments have weakened the previously strong negative link between wages and investment.

We begin by documenting how firms' investment responses to minimum wage increases have evolved between 1984 and 2017. To measure this sensitivity—hereafter "investment-wage sensitivity (IWS)—we augment standard investment regressions (e.g., Fazzari, Hubbard and Petersen, 1988) with a minimum wage variable. The IWS serves as a reduced-form, outcome-based proxy of how tightly a firm faces substitution constraints in responding to labor cost shocks, reflecting the elasticity of substitution. The contrast is stark: before 2000, minimum wage hikes significantly reduced capital expenditures, with an estimated sensitivity of –0.038 corresponding to a 24.6% decrease in investment relative to the sample mean, for a one-standard-deviation increase in the minimum wage. After 2000, this effect is essentially zero (0.001) and statistically insignificant.[1,2]

[1] Consistent with the framework and findings in Gustafson and Kotter (2022), we find more significant results when focusing on industries that are more exposed to minimum wage labor.

[2] We confirm and refine this finding using a rolling window estimator to construct a time series of IWS. The IWS declines steadily from the mid-1990s through the 2010s, a trend that is not present when we re-estimate the same model using a simulated minimum wage series randomly assigned to states. This placebo check confirms that the

To understand what drives this dramatic change, we exploit plausibly exogenous variation in firms' exposure to three key forces: labor-saving technological innovation, increased access to foreign labor, and declining unionization. Our empirical strategy compares how investment-wage sensitivities evolve for firms with high versus low exposure to each factor. We hypothesize that firms with greater substitution possibilities, whether through automation, offshoring, or reduced labor constraints, will experience sharper declines in sensitivity over time.

We first examine whether automation has weakened firms' investment responses to labor cost shocks by increasing their ability to substitute capital for labor. Drawing on evidence that routine-intensive jobs are especially susceptible to automation (Graetz and Michaels, 2017; Acemoglu and Restrepo, 2019), we measure industry exposure to labor-saving technological change based on the share of routine-task employment. Comparing firms in high- and low-exposure industries before and after 2000, we find that firms more exposed to automation experienced a pronounced decline in investment-wage sensitivity over time. This pattern supports the view that technological advances have increased capital-labor substitutability, reducing the impact of wage shocks on investment.

Next, we examine whether globalization, specifically increased access to low-cost foreign labor, contributed to the decrease in sensitivity to investment wages. Our analysis focuses on the 1999 U.S.–China bilateral trade agreement, which improved legal protections for U.S. firms operating in China and reduced the costs of shifting production abroad. In a nested constant elasticity of substitution (CES) framework, this policy change increased firms' effective elasticity of substitution between domestic and foreign labor by removing frictions that had previously constrained offshoring. Consistent with this mechanism, we find that firms already operating in China before the agreement experienced a pronounced weakening of investment-wage sensitivity afterward, while firms without such exposure remained highly sensitive. Firms that entered China after the agreement show similar patterns, supporting the interpretation that globalization, by raising firms' ability to substitute across labor markets, played a pivotal role in decoupling investment from domestic wage shocks.

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observed decrease is not an artifact of sample composition or mechanical trends.

We then examine whether declining union power has contributed to the weakening of investment-wage sensitivity. As unionization rates have fallen (Açıkgöz and Kaymak, 2014), firms have gained greater capacity to substitute other inputs for local employment in response to wage shocks. To test this mechanism, we analyze the impact of right-to-work laws, which limit unions' ability to require membership and dues. We find that firms headquartered in states adopting these laws experienced a pronounced reduction in investment-wage sensitivity compared to firms in states without such legislation. Moreover, this decline is most pronounced in states where union coverage fell most steeply after adoption, suggesting that the erosion of bargaining power—rather than legal change alone—plays a key role. These results support the view that weakening institutional labor protections has expanded firms' effective options for mitigating labor cost shocks.

Finally, we examine whether rising import competition, particularly from China, has contributed to the decline in investment-wage sensitivity. China's accession to the World Trade Organization (WTO) in 2001 led to a surge in Chinese exports to the U.S., intensifying competitive pressure across many industries. Unlike direct access to foreign labor, this channel operates through product markets: greater competition limits firms' ability to pass rising labor costs onto consumers, increasing the incentive to adopt labor-saving strategies. Consistent with this mechanism, we find that firms in industries more exposed to Chinese imports experienced a pronounced weakening of investment-wage sensitivity after China's WTO entry, while less-exposed firms showed little change. These results underscore that globalization reshapes investment behavior through multiple channels, including expanding firms' ability to substitute across labor markets and intensifying product market competition that constrains cost pass-through.

Together, these four empirical tests show that the decline in investment sensitivity to minimum wage shocks is not uniform across firms but instead reflects clear structural differences in their exposure to globalization, technological change, and labor market institutions. Firms with greater capacity to substitute or reallocate away from domestic labor exhibit a pronounced weakening of the investment response to wage increases, while firms with fewer options remain more constrained.

Our study contributes to several strands of literature. First, we offer new insights into the literature on the real effects of minimum wage policy. Prior studies report mixed findings: some show that minimum wage increases reduce investment (e.g., Cho, forthcoming; Gustafson and Kotter, 2022), while others find positive or null effects (e.g., Geng, Huang, Lin and Liu, 2022; Hau, Huang and Wang, 2020). We help reconcile these results by demonstrating that the impact of wage regulation on investment depends critically on context, specifically, where firms sit along multiple dimensions of substitution constraints, including their ability to substitute across inputs, their exposure to globalization and import competition, and the strength of institutional labor protections. Firms operating in more constrained contexts exhibit strong negative investment responses to wage increases, while those with greater capacity to adapt through automation, foreign sourcing, or more flexible labor arrangements show attenuated or no response. This heterogeneity helps explain why prior studies have reported divergent results.

Second, we contribute to the corporate finance literature that examines how labor market frictions shape firms' investment and financing policies. Prior work shows that labor institutions, including union bargaining power and employment protections, affect firms' capital structure, employment, and investment decisions (e.g., Agrawal and Matsa, 2013; Bai, Fairhurst and Serfling, 2020; Bena, Ortiz-Molina and Simintzi, 2022; Jeffers, 2024; Matsa, 2010; Simintzi, Vig and Volpin, 2015). While previous research often uses fixed effects or cross-sectional designs that do not explicitly model how these frictions change over time, we demonstrate that they have evolved substantially over the past several decades, driven by globalization, technological change, declining union power, and increased product market competition. Our analysis shows that shifts in the degree of labor substitutability over time are central to understanding variation in investment responses to wage shocks. This dynamic perspective complements earlier research by showing how evolving frictions shape firms' responses over time.

Third, we provide firm-level evidence that complements macroeconomic research on labor's declining share of national income (Karabarbounis and Neiman, 2014; Gutiérrez and Piton, 2020). While much of this literature documents aggregate trends such as the falling labor share and increasing markups, we show that declining worker bargaining power is also evident in the microeconomic response of firms to wage regulation. Our results highlight how structural shifts in labor market power affect not only wages but also firms' capital allocation strategies and responsiveness to policy.

Fourth, our findings provide new empirical insight into how the elasticity of substitution between inputs—both capital and labor, and domestic and foreign labor—shapes firms' responses to cost shocks. Although this concept is central to production theory and models of firm behavior, it has received limited attention in empirical corporate finance. By documenting how structural forces such as globalization and automation alter firms' effective substitutability across inputs, we show that changes in these elasticities play a pivotal role in mediating investment behavior and adapting to labor cost shocks.

Finally, our results speak to the growing literature on monopsony power in labor markets. While much of this research emphasizes wage-setting frictions and employment outcomes, we show that monopsony-like dynamics also influence corporate investment behavior. As firms gain greater substitutability through automation, offshoring, or declining union constraints, their investment decisions become less constrained by domestic labor costs. This perspective extends the implications of labor market power beyond wages to capital allocation and broader firm strategy.

II. Framework, Data, and the Decline in Investment–Wage Sensitivity

This section lays out the conceptual, institutional, and empirical foundations of our analysis. In Section II.A, we define investment–wage sensitivity (IWS) as a reduced-form measure of how firms adjust capital investment in response to minimum wage–induced labor cost shocks, and we explain how it reflects underlying substitutability and institutional constraints. Section II.B provides background on the structure of U.S. minimum wage regulation, emphasizing the features that generate cross-state and temporal variation in wage mandates. Section II.C describes our data sources, sample construction, and variable definitions. Section II.D presents our estimation strategy for measuring IWS, including our baseline regression specification and rolling window approach. Section II.E documents a sharp decline in IWS over time and across firm types, motivating the hypothesis that structural changes have weakened firms' sensitivity to labor costs. Finally, Section II.F presents additional tests and robustness checks to validate the patterns observed and rule out alternative explanations

A. Investment–Wage Sensitivity as a Measure of Firm Substitution Constraints

Understanding how firms' responsiveness to labor cost shocks has evolved is central to evaluating how labor market frictions shape corporate behavior, capital allocation, and labor outcomes. These dynamics are increasingly shaped by structural forces such as institutional change, globalization, and technological progress.

We study this evolution empirically by focusing on a firm-level measure of investment–wage sensitivity (IWS), which captures how capital spending responds to mandated increases in labor costs. The premise is straightforward: when labor costs rise due to exogenous wage mandates, firms may adjust investment depending on their capacity to substitute inputs across labor, capital, or geography. A firm that cannot easily substitute capital for labor or reassign production from domestic to foreign labor will experience a reduction in the marginal product of capital. This depresses the returns to investment. More intuitively, rising labor costs increase the input costs of investment projects, reducing their expected net present value (NPV) and making them less likely to be undertaken.

We interpret variation in IWS as a reduced-form, outcome-based proxy for the tightness of a firm's substitution constraints in responding to labor cost shocks. This proxy reflects the elasticity of substitution between capital and labor, between domestic and foreign labor, as well as institutional frictions that constrain firms' ability to adjust factor inputs. In standard production theory, these elasticities govern how easily a firm can re-optimize its input mix in response to cost shocks—either by shifting from labor to capital or by adjusting sourcing strategies across labor markets. When these elasticities are low, firms face tighter constraints: capital and labor behave as complements, or labor sourcing is geographically rigid. In such cases, wage increases reduce the marginal productivity of capital, lowering the return to new investment. More intuitively, rising labor costs increase the input costs of investment projects, lowering their expected net present value (NPV) and making them more likely to be deferred or canceled. When substitution is easier, the firm can absorb the shock without disrupting investment plans, yielding a weaker IWS.

We emphasize that IWS is not a structural parameter estimated from a specific production function. Rather, it is a revealed, empirical measure of how firms behave in the face of policy-induced wage shocks. It provides a practical tool to study how the tightness of labor constraints and the firm's ability to substitute have evolved in response to shifting institutional and economic conditions. In the sections that follow, we use this measure to document a secular decline in IWS over time, and to investigate whether this trend reflects greater input substitutability due to technological advances, globalization, or weakened institutional frictions.

B. Minimum Wage Regulation: Institutional Details

The Fair Labor Standards Act (FLSA) establishes the federal minimum wage standards for U.S. employees. Coverage extends to workers whose employers engage in interstate commerce and have annual sales of at least $500,000. It also covers hospitals, schools, and government agencies regardless of their dollar volume of business. Even if an enterprise does not meet the sales threshold, its employees are still covered if they are individually engaged in interstate commerce during the workweek. In addition, the Act governs overtime compensation, recordkeeping, and youth employment for both private and public sector workers. Since its enactment in 1938, the FLSA has been amended numerous times, primarily to raise the federal minimum wage. The FLSA is administered by the Department of Labor's Wage and Hour Division and provides protection to more than 143 million workers (about 93% of the civilian labor force in the United States) at more than 9.8 million establishments as of July 2009.

There are also many states with their own minimum wage regulations. Some states fix their minimum wage rates at the federal rate, link the rates to inflation, adjust the rates based on the scheduled legislative action, or combine the first three options. The federal minimum wage standards may not match the state minimum pay rates. When an employee is subject to both regulations, Section 18 of the FLSA states that the employee is entitled to the higher of the two standards. The federal and state minimum wage rates fluctuate at different intervals and in different amounts. These changes are depicted in Figure B.1 for a sample of three geographically distant states: California, Connecticut, and Illinois. As shown in Figure B.1, the timing of minimum wage changes varies at the federal level and across the states. In our estimation of sensitivities, we exploit this staggered timing of changes in minimum wage rates with an assumption that changes in the minimum wage rates are orthogonal to firm outcomes.

As outlined earlier, each state has its own process for adjusting minimum wage rates. One method is to index minimum wage rates to inflation. This adjustment procedure is problematic for our identification strategy because inflation might directly affect corporate investment.[3] To address this concern, we exclude all firms headquartered in states that index minimum wage rates to inflation.[4] The second mechanism for adjusting minimum wages is to include specific future dates for specific minimum wage rates in legislation. Our identifying assumption could be violated if such legislation were driven by local economic conditions (e.g., the lawmakers' anticipation of improvement in investment opportunities). To address this, we include state-by-year fixed effects in our conditional analyses to account for the possibility of state-level minimum wage policies being endogenous to local economic conditions.[5]

[3]In principle, there are two conflicting effects of inflation on corporate investment (Hochman and Palmon, 1983): depreciation allowances and interest deductions effects. First, since depreciation allowances are determined by historical costs (not current nominal values), the real tax advantage of depreciation declines with inflation. Second, the real tax benefit of interest rises with inflation because firms deduct interest expenses at nominal interest rates (not at real rates). Thus, the question of whether inflation influences corporate investment positively or negatively is one of the empirical studies (Feldstein, 1982).

[4]These 15 states are Alaska, Arizona, Colorado, Florida, Michigan, Minnesota, Missouri, Montana, Nevada, New Jersey, Ohio, Oregon, South Dakota, Washington, and Vermont. Our results are robust to including those states in our analysis.

[5]One approach used in the literature (e.g., Dube, Lester and Reich, 2010; Allegretto, Dube and Reich, 2011; Geng et al., 2022) to deal with endogeneity is to focus on border counties. However, we opt not to compare cross-

The third adjustment mechanism used by states is to set their minimum wage rates based on the federal rate. This mechanism is similar to specifying rates through state law. We emphasize that changes in *federal* minimum wage regulations may be seen as exogenous to the *state-level* macroeconomic conditions that could influence individual firm outcomes. Insofar as the federal minimum wage regulation is orthogonal to state-level economic circumstances, the identification enables us to isolate the impact of unobservable state-level macroeconomic shocks on capital expenditures. To directly control for the macroeconomic conditions of the U.S. economy that may facilitate a change in federal minimum wage laws, we include year fixed effects in our investment regressions.

C. Data and Sample Construction

We obtain the historical changes in minimum wages for non-farm private sector employment under state laws for all U.S. states from the Tax Policy Center.[6] These data are sourced from the Wage and Hour Division of the U.S. Department of Labor and from the *Monthly Labor Review* by the Bureau of Labor Statistics and span the 1983 to 2014 time period. For 2015–2017, we hand-collect the data from the U.S. Department of Labor.[7] To match this time period, we construct a sample of firms in Compustat following the sample selection criteria of Almeida, Campello and Galvao (2010). We exclude observations from financial institutions (SIC codes 6000–6999) and those with negative *Tobin's q*. We also eliminate firm-year observations with asset or sales growth exceeding 100% because these firms exhibit large jumps in business fundamentals in terms of size and sales, and these jumps are typically caused by major corporate events, such as mergers and acquisitions or reorganizations. The last step is to discard extremely tiny companies with capital of less than $10 million because linear investment models might not be suitable for such companies,

border county pairs since neighboring counties across the state border may not be the best control group (Neumark, Salas and Wascher, 2014). Intuitively, those cross-border counties do not necessarily possess a similar business or regulatory environment that is subject to different state-level policies (e.g., state tax rates, local government spending on infrastructure, state-level labor regulations, etc.). Moreover, we do not observe corporate investment at the county level and firms do not usually operate along geographical borders.

[6] https://www.taxpolicycenter.org/statistics/state-minimum-wage-rates

[7] https://www.dol.gov/whd/state/stateMinWageHis.html

as described in Gilchrist and Himmelberg (1995). Using the consumer price index for all urban consumers (CPI-U), we convert all dollar-valued variables into December 2014 constant dollars. The final sample has 59,096 firm-year observations.

Table I provides summary statistics of the main variables used in this study. The firm-year-level data consists of 59,096 firm-year observations from 1984 to 2017, consisting of 6,376 firms. We define investment rates for firm i in state s in year t as capital expenditures (I) normalized by the beginning-of-year capital stock (K) in which capital stock is measured as property, plant, and equipment. This variable is named as $Investment$ $(= \frac{I_{i,s,t}}{K_{i,s,t-1}})$. The sample mean and median of investment rates are 24.5% and 18.5%, respectively, which implies that the empirical distribution of *Investment* is right-skewed. Cash flow is calculated as earnings before extraordinary items plus depreciation (CF), normalized by the beginning-of-the-year capital stock: $Cash\ Flow = \frac{CF_{i,s,t}}{K_{i,s,t-1}}$. $Cash\ Flow$ is also right-skewed and has a very high standard deviation (89.5%). $Tobin's\ q$ is a proxy for investment opportunities, which is measured as the ratio of the market value of assets to the book value of assets.

For state-level variables, we report their descriptive statistics based on 1,190 state-year observations from 1983 to 2016. We define $w_{s,t-1}$ as the minimum hourly wage rate for year t−1 in state s. The average of $w_{s,t-1}$ in nominal dollars is \$5.31, and its standard deviation is \$1.54. $w_{s,t-1}$ displays both cross-sectional variation (across states) and time-series variation (within-state variation). The across-state standard deviation (the cross-sectional standard deviation of state-level time-series averages) of $w_{s,t-1}$ is \$0.25, and the within-state standard deviation (the average of time-series standard deviations for all states) is \$1.52.[8] These numbers indicate that there is considerable within-state variation in minimum wage rates.

We define $w_{i,s,t-1}$ as the minimum hourly wage rate for year t−1 in state s where firm i's headquarters is located, which is used in our regressions. We obtain information about firms'

[8]The absence of investment-wage sensitivity in the post-2000 period might be explained if within-state variation in minimum wage rates decreased with time. For the later sample period, however, the within-state standard deviation is higher: \$0.72 (\$1.00) for the pre-2000 (post-2000) period.

headquarters from the Compustat data that provide the latest headquarters location.[9] In our dataset, there are many state-years that have more than one minimum wage rate in effect during the year. In these cases, we compute a weighted average minimum wage rate where the weights are given by the number of days the minimum wage rate is in effect. The average of $w_{i,s,t-1}$ is \$5.28, and its (overall) standard deviation is \$1.59 (not reported in the table). The detailed definition of each variable is provided in Appendix A.

[Insert Table I here.]

D. Measuring Investment-Wage Sensitivity

We investigate how firms respond to changes in labor costs by focusing on the sensitivity of corporate investment to mandated changes in state-level minimum wages. To estimate firm responses, we augment the standard Fazzari et al. (1988) investment regression with a minimum wage variable:

$$\frac{I_{i,s,t}}{K_{i,s,t-1}} = \alpha_i + \alpha_t + \beta_1\, Tobin's\, q_{i,s,t-1} + \beta_2\, \frac{CF_{i,s,t}}{K_{i,s,t-1}} + \beta_3\, w_{i,s,t-1} + \beta_4\, X_{i,s,t-1} + \epsilon_{i,s,t}, \quad (1)$$

where i, s, and t index firms, states, and years. The dependent variable is the investment rate (capital expenditures normalized by lagged capital stock) and the key independent variable, $w_{i,s,t-1}$, is the state-level minimum wage in the year prior to investment, matched to the location of the firm's headquarters. $Tobin's\ q$ and CF (cash flow) are the standard investment regression controls. α_t is a set of year fixed effects, which absorb time-varying macroeconomic shocks faced by all firms; and α_i is a set of firm fixed effects, which absorb time-invariant unobservable firm characteristics.

[9] We obtain robust results using the data on historical headquarters states provided by Bai et al. (2020) and the University of Notre Dame's Software Repository for Accounting and Finance (Loughran and McDonald, 2016). Another concern is that a firm's operation may be geographically dispersed, hence its headquarters location does not necessarily reflect a relevant minimum wage rate for which most of its workforce is located. To address this concern, we count the number of times each 10-K mentions a U.S. state name and use the relative state counts for each state as a proxy for a firm's operational intensity in each state (García and Norli, 2012). We then restrict firm-year observations to have a state with an operational intensity greater than 50% and use that state to identify a firm's corresponding minimum wage rate. Since the mandatory filing via the EDGAR system was implemented after May 1996, we use the information in 1997 for the years before 1997. We confirm the declining sensitivity.

We also control for state-level macro variables, $X_{i,s,t-1}$, including real GDP growth rates, log of population, and unemployment rates.[10] The definitions of all variables are provided in Appendix A. In our estimation, we cluster standard errors of regression coefficients at the state level, instead of the firm level. Given that the minimum wage laws vary by state, potential correlations in unobserved factors that affect different firms in the same state may lead to inconsistent estimates of standard errors. Hence this method accounts for cross-firm correlations of error terms within a state, which is more general than firm-level clustering.

The coefficient of interest, β_3, is the investment-wage sensitivity (IWS) which captures the sensitivity of firm investment to labor cost shocks. The economic interpretation is straightforward. A negative and significant β_3 indicates that increases in the minimum wage reduce the expected returns to investment, likely by raising operating costs and reducing the net present value (NPV) of marginal projects. In classical production theory, this pattern is consistent with low capital-labor substitutability, that is, when labor and capital are complements, increasing labor costs reduce the incentive to invest. Conversely, if capital and labor are more substitutable or if firms can reallocate production elsewhere, we expect investment to be less sensitive (i.e., a smaller negative or statistically insignificant β_3.). We treat β_3 as an empirical proxy for firm-level input substitution margins in absorbing mandated wage increases, a measure shaped by capital-labor substitution elasticities, bargaining conditions, and access to external labor pools.

E. The Decline in Investment-Wage Sensitivity Over Time

Before turning to our causal analyses, we document several empirical patterns that motivate our focus on structural forces affecting input substitution margins. To start, we first measure investment-wage sensitivity by estimating equation (1) over the full sample period. Column (1) in Panel A of Table II reports that the coefficient on the minimum hourly wage rate ($w_{i,s,t-1}$) is

[10]The political orientation of state governors may affect the state-level minimum wage policy and the business environment that affects corporate investment decisions. Therefore, using the data collected from the National Governors Association, we include an indicator for the political parties of state governors (Democrat, Republican, or Independent) and confirm that the results are robust. Using real minimum wage also yields qualitatively similar results.

negative and statistically significant at the 10% level. The estimate of –0.017, the investment-wage sensitivity, indicates that when a state's minimum wage increases, firms headquartered in that state reduce their investment rates. The magnitude of the regression coefficient implies that the effect is economically large. Following a one-standard-deviation increase in the minimum wage ($1.588), firms reduce their investment rates by 270 basis points ($= 1.588 \times -0.017$). This 270 basis point reduction corresponds to an 11.0% decrease, relative to the sample mean investment rate (24.5%). In spite of the sizable economic impact of minimum wage on investment, the statistical significance of the result is quite weak (at the 10% level).

Turning to time variation, columns (2) and (3) report results, respectively, from estimating equation (1) for the first- and second-half of the sample: subperiods 1984–2000 and 2001–2017. The results show a dramatic drop in the sensitivity of investment to minimum wage increases. More specifically, column (2) shows that, for the first half of the sample period, the estimated investment-wage sensitivity is –0.038 (more than twice the estimate for the full sample period) and statistically significant at the 1% level. A one-standard-deviation increase in the minimum wage leads firms to reduce their investment rates by 603 basis points, which corresponds to a 24.6% decrease relative to the sample mean. In sharp contrast, column (3) shows that for the second half of the sample, firm-level investment is no longer sensitive to minimum wage increases: the estimated investment-wage sensitivity is 0.001 and statistically insignificant at conventional levels. The investment-wage sensitivities between the two sub-periods are statistically different at the 1% level of significance (χ^2-statistic = 12.52).[11]

This trend is consistent with the rising elasticity of substitution between capital and labor documented in the literature (Cantore, Levine, Pearlman and Yang, 2015; Chirinko and Mallick,

[11]The economy of the United States has transitioned toward service and technology-based enterprises (e.g., toward white-collar jobs). Corrado and Hulten (2010) document that the share of intangible capital out of firms' total capital has increased from 25% (1973–1994) to 34% (1995–2007). This change could account for the lack of investment-wage sensitivity in the later sample period. However, we do not observe any substantial changes in industry composition in the two sub-periods: for instance, manufacturing industries (NAICS codes = 31, 32, or 33) account for 47% (45%) of firm-year observations in the sample from 1984 to 2000 (from 2001 to 2017). Also, we obtain similar results using the firm-year observations with zero R&D expenses.

2017; La Grandville, 2017). The pattern is also in line with a sharp increase in manufacturing firms' monopsony power (as measured by wedges between marginal revenue products of labor and wages) since the early 2000s (Yeh, Macaluso and Hershbein, 2022). We further examine this time-varying nature of investment-wage sensitivity and conduct a formal structural break analysis in Appendix B.A. We acknowledge that the estimated sensitivity may identify a general-equilibrium effect that reflects the potential product or labor market spillovers. For example, workers may cross the border to pursue the minimum wage hike in the neighboring state, affecting corporate investment decisions. Although such spillovers are possible, there is little empirical evidence in the literature for their existence or quantitative importance (Aaronson, French, Sorkin and To, 2018).

F. Additional Evidence and Robustness Tests

1. Minimum Wage Sensitive Industries and Time-varying Local Economic Conditions

To lend further credence to our estimation framework, we examine whether minimum wage sensitive industries exhibit more pronounced negative investment-wage sensitivities. We identify industries that are most subject to minimum wage changes using the percentage of workers who are paid at or below the federal minimum wage. The data are obtained from the Labor Force Statistics (2017) by the Bureau of Labor Statistics. The minimum wage sensitive industries include manufacturing, retail trade, leisure and hospitality, and services, accounting for 95% of minimum-wage workers in the private sector. We reestimate Panel A of Table II by interacting the minimum wage variable with an indicator for minimum wage sensitive industries.

[Insert Panel B of Table B.2 here.]

The results are reported in column (1) in Panel B of Table B.2. The estimated coefficient on the interaction term is –0.006 and statistically significant at the 1% level, verifying our identification strategy. This result is consistent with findings documented in Gustafson and Kotter (2022). Furthermore, to the extent that low-skill workers (many of whom are low-wage) are more vulnerable to replacement (Acemoglu and Restrepo, 2018), the declining sensitivity documented in Panel A would be more pronounced for those minimum wage sensitive industries. The results in columns (2) and (3) support this view. The estimated coefficient on the interaction term is negative in the pre-2000 period whereas it is positive in the post-2000 period. These results imply that the decline in investment-wage sensitivity over time is largely driven by the industries most subject to minimum wage policy. To address a concern that variations in minimum wage rates might be endogenous to state economic conditions, we include state-by-year fixed effects in columns (4)–(6). The results are similar to those in columns (1)–(3), confirming that our sensitivity estimates are less likely to be affected by unobserved time-varying state-specific economic conditions.

2. A Decrease in the Share of Minimum-Wage Workers

In estimating the sensitivity, we ideally want to consider the following two-stage system:

$$\frac{I_{i,s,t}}{K_{i,s,t-1}} = \beta\, Wage_{i,s,t} + \epsilon_{i,s,t}, \qquad (2)$$
$$Wage_{i,s,t} = \delta\, w_{i,s,t-1} + \eta_{i,s,t},$$

where we omit the fixed effects and control variables for ease of exposition and $Wage$ indicates the average hourly wages of workers in firm i. If $w_{i,s,t-1}$ is orthogonal to $\epsilon_{i,s,t}$, an IV regression of investment on wages using the minimum wage variable as an instrument would recover the sensitivity, β. In contrast, a reduced-form regression of investment on the minimum wage variable would identify $\beta \times \delta$ where δ reflects a fraction of minimum-wage or low-skilled workers. Since we have limited information on $Wage$, we employ the reduced-form approach in estimating the sensitivity[12]. Therefore, the declining sensitivity may be driven by a decreasing share of minimum-wage workers over our sample period, even if substitutability remains stable over time. To check this possibility, we reestimate the investment-wage sensitivity for industries with a non-decreasing percentage of minimum-wage workers. Comparing the fraction of minimum wage workers for the years 2003 and 2017, we identify the following industries with the non-decreasing percentage:

[12] Only about 10% of firm-year observations in Compustat have non-missing values of the total labor costs.

mining, manufacturing, retail trade, and transportation and utilities.[13]

[Insert Panel C of Table B.2 here.]

The results are documented in Panel C of Table B.2. The estimated sensitivities are similar to those obtained in Panel A. A formal statistical test to evaluate the null hypothesis of the equality of sensitivities between the two sub-periods is rejected at the 5% level. Thus, declining sensitivity is not a manifestation of a decreasing fraction of minimum-wage workers.

3. Robustness Tests

We conduct additional robustness checks to validate our estimates of investment-wage sensitivities. First, we implement a placebo test for the pre-2000 sample using a pseudo minimum wage variable constructed by randomly reassigning firms to different states while preserving the distribution of firms and the timing of state-level wage changes. The distribution of estimated coefficients from 1,000 simulations centers near zero, and the actual coefficient from our data lies far in the lower tail, suggesting that the observed investment-wage sensitivity is not driven by chance. Second, we address potential measurement error in $Tobin's\ q$ by applying higher-order cumulant estimators, as proposed by Erickson, Jiang and Whited (2014). The investment-wage sensitivity remains statistically significant and economically larger in the pre-2000 period, while remaining negligible post-2000, consistent with our baseline estimates. Finally, we test the strict exogeneity assumption by including the one-period lead value of the minimum wage variable in the investment regressions, following Wooldridge (2011) and Grieser and Hadlock (2019). The coefficient on the lead variable is insignificant across all specifications, supporting the validity of our empirical setting. These additional results are documented in Appendix B.

[13] The data on the percentage of minimum-wage workers are available from 2002; however, the industry classification in 2002 was different from that in other years.

III. Structural Drivers of Declining Investment-Wage Sensitivity

The preceding section documents a substantial decline in investment–wage sensitivity (IWS) since the late 1990s, particularly among firms in low-wage, labor-intensive industries. In this section, we examine the structural forces that contributed to this shift. Our central hypothesis is that declining IWS reflects a relaxation of labor-related constraints—driven by increases in firms' substitutability and the erosion of institutional barriers to adjustment. We test this hypothesis using four sources of variation that plausibly affect the firm's input substitution capacity: (i) technological change, proxied by exposure to automatable labor tasks; (ii) globalization, captured through China-related foreign affiliate exposure; (iii) declining unionization, measured through union presence and right-to-work laws; and (iv) product market competition, captured via industry-level import penetration. Each of these mechanisms allows us to test whether firms with greater substitution capacity exhibit systematically lower IWS in the post-2000 period.

A. *Automation and Capital–Labor Substitutability*

We begin by examining how firms' increasing exposure to automation has influenced their investment responsiveness to labor cost shocks over time.[14] Our hypothesis is that firms in industries more exposed to automation have had an increasing ability to substitute capital for labor, thereby weakening the link between mandated wage increases and investment. This fits naturally within our elasticity of substitution framework: as automation becomes more feasible, the elasticity of substitution between capital and labor increases, providing firms with greater options when dealing with labor cost shocks. More specifically, firms will attempt to substitute labor with capital if the productivity per unit of capital cost exceeds the productivity per unit of labor cost, other things equal.

[14] Acemoglu and Restrepo (2019) document that robots competing against humans reduce employment and wages for workers in the U.S. local labor markets between 1990 and 2007.

In order to examine how technology affects investment-wage sensitivity, we use the observation that routine-intensive jobs are particularly susceptible to replacement by new robot technologies (Graetz and Michaels, 2017). Specifically, we measure the extent to which industries are subject to technological change (automation displacing labor) using an industry-level share of routine-task labor, $Exposure_{tech}$.

To construct $Exposure_{tech}$, we closely follow the procedure employed in Zhang (2019).[15] We first define the routine-task intensity (RTI) score for each occupation as

$$RTI_k = ln(T_k^{routine}) - ln(T_k^{abstract}) - ln(T_k^{nonroutine\ manual}) \tag{3}$$

where $T_k^{routine}$, $T_k^{abstract}$, and $T_k^{nonroutine\ manual}$ are the routine, abstract, and nonroutine manual task skill levels (scaled from 1 to 10) required by occupation k obtained from the revised fourth edition of the *Dictionary of Occupational Titles* by the U.S. Department of Labor. We classify workers as routine-task labor if their occupations' RTI score falls in the top quintile of the RTI distribution. Next, we obtain data on the number of employees and their wages for each occupation-industry pair in the year 1999, from the *Occupational Employment Statistics* by the Bureau of Labor Statistics. $Exposure_{tech}$ is the proportion of routine-task labor costs to the total industry labor costs in the year 1999. The higher the $Exposure_{tech}$ variable, the greater the likelihood that automation would replace labor in that industry.

Using this industry-level measure of exposure to technological change ($Exposure_{tech}$), we define $Exposure_{tech,i}$ as firm i's exposure to technological change, as of 1999, in two ways: (i) As a continuous variable, we set $Exposure_{tech,i}$ to be equal to $Exposure_{tech}$ for the three-digit SIC industry to which firm i belongs; (ii) As an indicator variable, we set $Exposure_{tech,i} = 1$ for firms if their continuous $Exposure_{tech,i}$ measure is above the median value of the $Exposure_{tech,i}$ distribution, and zero otherwise. Firms with $Exposure_{tech,i}$ = 1 are termed as firms with *High*

[15]We thank Miao Ben Zhang for making the data available on his website (`https://www.miaobenzhang.com`).

Exposure to technological change, and the firms with $Exposure_{tech,i}$ = 0 are termed as firms with *Low Exposure* to technological change, respectively. We then estimate a difference-in-differences regression that is similar to equation (5).

$$\frac{I_{i,s,t}}{K_{i,s,t-1}} = \alpha_i + \alpha_t + \beta_1\ Tobin's\ q_{i,s,t-1} + \beta_2\ \frac{CF_{i,s,t}}{K_{i,s,t-1}} + \beta_3\ w_{i,s,t-1} + \beta_4\ Post \times w_{i,s,t-1} \qquad (4)$$
$$+ \beta_5\ Exposure_{tech,i} \times Post \times w_{i,s,t-1} + \beta_6\ Exposure_{tech,i} \times w_{i,s,t-1}$$
$$+ \beta_7\ Exposure_{tech,i} \times Post + \beta_8\ X_{i,s,t-1} + \epsilon_{i,s,t}.$$

We interact $w_{i,s,t-1}$, the minimum wage, with two variables, $Exposure_{tech,i}$ and $Post$ (indicating the time period after 2000). Following Acemoglu and Restrepo (2019), we assume that the post-2000 period is more technologically advanced than the pre-2000 period. The coefficient of the triple interaction term ($Exposure_{tech,i} \times Post \times w_{i,s,t-1}$) captures difference-in-differences in investment-wage sensitivity after the year 2000 across firms that are differentially susceptible to technological change in the 21st century. We include the same set of control variables used in Panel A of Table II. We also include all interaction terms of these control variables with $Exposure_{tech,i}$ and $Post$ to capture differential effects of control variables on investment after the year 2000 across firms with different degrees of exposure.

[Insert Table III here.]

Table III presents the estimates of the relation between technological change exposure and changes in investment-wage sensitivity. We first discuss the regression results using $Exposure_{tech,i}$ as an indicator variable. Column (1), Panel A of Table III estimates that the difference-in-differences in the investment-wage sensitivity after the year 2000 between the *High Exposure* and *Low Exposure* firms (the triple interaction term, $Exposure_{tech,i} \times Post \times w_{i,s,t-1}$) is 0.020. This estimate is economically large and significant at the 1% level. We interpret this effect as follows.

For the *High Exposure* firms, the pre-2000 investment-wage sensitivity is calculated as the sum of regression coefficients: $\beta_3 + \beta_6 = -0.018 - 0.012 = -0.030$ with a t-statistic of –2.91, both economically and statistically very significant. This means, these firms significantly reduced their investment in response to mandated changes in the minimum wage before the year 2001. For the same group of firms, the post-2000 investment-wage sensitivity is calculated as the sum of four regression coefficients: $\beta_3 + \beta_4 + \beta_5 + \beta_6 = -0.018 + 0.001 + 0.020 - 0.012 = -0.009$ with a t-statistic of −1.89. The estimated investment-wage sensitivity is economically close to zero and statistically significant marginally at the 10% level. This means, the *High Exposure* firms virtually do not adjust their investment in response to mandated changes in the minimum wage after the year 2000.

We now turn to the *Low Exposure* firms (the omitted group). For these firms, the investment-wage sensitivity in the pre-2000 period is the regression coefficient $\beta_3 = -0.018$ with a t-statistic of −2.17, both economically and statistically significant. *Low Exposure* firms significantly adjusted their investment in response to mandated changes in the minimum wage before the year 2000. For the same firms, the investment-wage sensitivity in the post-2000 period is calculated as the sum of two regression coefficients: $\beta_3 + \beta_4 = -0.018 + 0.001 = -0.017$ with a t-statistic of −2.66. The estimated investment-wage sensitivity continues to be economically significant with almost the same magnitude as before, and strongly statistically significant. The estimation results in column (1) suggest that technological change-induced automation replacing labor is significantly associated with the change in investment-wage sensitivity observed over our sample period 1984–2017. In Column (2), we estimate equation (4) using $Exposure_{tech,i}$ as a continuous variable. Our inferences are qualitatively unchanged, and we discuss these results in Figure II (a).

[Insert Figure II (a) here.]

In this figure, the firms with *Low* (*High*) *Exposure* to technological change are defined using $Exposure_{tech,i}$ as a continuous variable, as follows: firms with $Exposure_{tech,i}$ value one-standard deviation below (above) the mean value of the variable are *Low* (*High*) *Exposure* firms respectively. The figure shows that the *Low Exposure* firms do not change their investment-wage sensitivity over the entire sample period. The *High Exposure* firms eliminate the investment-wage sensitivity in the second half of the sample period because of technological change-induced substitution of labor with capital.

B. Globalization and Substitutability of Foreign for Domestic Labor

We next examine whether improved access to foreign labor, another source of rising substitutability, has dampened firms' responsiveness to domestic wage shocks. Our tests focus on the 1999 U.S.–China bilateral trade agreement, which expanded U.S. firms' ability to capture profits from Chinese operations by improving legal protections and reducing capital repatriation frictions.[16, 17] In effect, the agreement facilitated the ability of U.S. firms to establish and expand operations in China, effectively broadening their access to low-cost foreign labor. In doing so, it increased firms' alternative options in the face of domestic labor cost shocks, enhancing both the feasibility of substituting foreign for domestic labor and firms' bargaining power through the credible threat of reallocating capital outside the U.S. to take advantage of cheaper labor.[18]

To identify firms that are more likely to benefit from the enhanced ability to access cheap Chinese labor, we focus on U.S. firms with subsidiaries in China, following the strategy employed in Bena and Simintzi (2019). Their prior presence in China positions them to capitalize on the policy change and expand more rapidly and efficiently. Since firms may endogenously choose to operate in China after the agreement, we begin by first focusing on U.S. firms with at least one subsidiary in China as of 1997, two years prior to the bilateral agreement, and then consider those firms that newly established subsidiaries in China following the agreement. To identify U.S. firms

[16]For instance, relaxing foreign ownership restrictions, eliminating foreign exchange balancing requirements, removing local content requirements, lifting requirements of any kind including offsets, transfer of technology, or requirements to conduct research and development in China, etc.

[17]Devereaux and Lawrence (2004) show that the bilateral agreement was largely unexpected due to strong opposition in the U.S. Congress making it useful as a means of identifying a causal relation between U.S. worker bargaining power and investment-wage sensitivity. Ceglowski and Golub (2012) show that relative unit labor costs in Chinese manufacturing vis-a-vis the U.S. (which accounts for relative productivity, relative wages, and real exchange movements) were about 22% in 1998, based on the World Bank estimates.

[18]Using a similar empirical strategy, Bena and Simintzi (2019) show that improved access to cheaper Chinese labor led U.S. firms to reduce their process innovation activities in the U.S., i.e., cheaper Chinese labor substituted for cost-reducing process innovation activities at home.

with Chinese subsidiaries in our sample, we use hand-collected information from 10-K filings in 1997.

Our tests are based on the following difference-in-differences regression:

$$\begin{aligned}\frac{I_{i,s,t}}{K_{i,s,t-1}} &= \alpha_i + \alpha_t + \beta_1\, Tobin's\; q_{i,s,t-1} + \beta_2\, \frac{CF_{i,s,t}}{K_{i,s,t-1}} + \beta_3\, w_{i,s,t-1} \\ &+ \beta_4\, Agreement_t \times w_{i,s,t-1} + \beta_5\, China97_i \times Agreement_t \times w_{i,s,t-1} \\ &+ \beta_6\, China97_i \times w_{i,s,t-1} + \beta_7\, China97_i \times Agreement_t + \beta_8\, X_{i,s,t-1} + \epsilon_{i,s,t},\end{aligned} \tag{5}$$

where $China97_i$ is an indicator variable set to one if firm i has at least one subsidiary in China as of 1997, and zero otherwise; $Agreement_t$ is an indicator variable set to one for the time period after the agreement (including 1999), and zero otherwise; and $X_{i,s,t-1}$ includes the same set of control variables used in Panel A of Table II. We also include interaction terms of all control variables with $China97_i$ and $Agreement_t$.[19] The outcome of interest in this generalized difference-in-differences framework is β_5 which captures the change in investment-wage sensitivity for firms operating in China as of 1997 relative to firms not operating in China. As shown earlier, our baseline estimate of the investment-wage sensitivity prior to 1999 is negative, (Panel A of Table II). If firms with greater access to cheap Chinese labor increase their bargaining power over their workers, we expect that their investment will become less sensitive to minimum wage increases, and hence, we expect β_5 to be positive and significant.

[Insert Table IV here.]

Table IV presents the estimation results for equation (5). In column (1), the difference-in-differences in the investment-wage sensitivity (β_5) is 0.035 and significant at the 1% level. We interpret this finding as follows: For the treated firms with Chinese subsidiaries at the time of the agreement, the investment-wage sensitivity before the agreement is calculated as the sum of regression coefficients: $\beta_3 + \beta_6 = -0.017 - 0.028 = -0.045$ with a t-statistic of –3.73, both

[19]The $China$ and $Agreement$ indicators are absorbed by the firm and year fixed effects, respectively.

economically and statistically significant, i.e., prior to the agreement, the treated firms significantly adjusted their investment in response to mandated changes in the minimum wage. The investment-wage sensitivity for the treated firms after the agreement is calculated as the sum of four regression coefficients: $\beta_3 + \beta_4 + \beta_5 + \beta_6 = -0.017 + 0.005 + 0.035 - 0.028 = -0.005$ with a t-statistic of −0.86, both economically and statistically insignificant, indicating that the treated firms do not adjust their investment in response to mandated changes in the minimum wage after the agreement.

We now turn to the effect of the agreement on the control group of firms that did not have Chinese subsidiaries at the time of the agreement. For these firms, the investment-wage sensitivity before the agreement is the regression coefficient $\beta_3 = -0.017$ with a t-statistic of −2.14, both economically and statistically significant; prior to the agreement, the control firms, like the treated firms, significantly adjusted their investment in response to mandated changes in the minimum wage. The investment-wage sensitivity for the control firms after the agreement is calculated as the sum of two regression coefficients: $\beta_3 + \beta_4 = -0.017 + 0.005 = -0.012$ with a t-statistic of −1.92. Thus, in contrast to the treated firms, the estimated investment-wage sensitivity for the control firms continues to be economically and statistically significant (albeit smaller in magnitude than before).

[Insert Figure II (b) here.]

Figure II (b) summarizes the results of column (1). It plots the changes in investment-wage sensitivities around the bilateral agreement for the treated and control firms. The solid dots indicate the point estimates of the sensitivity from the regression, and the vertical lines around these point estimates denote the corresponding 95% confidence intervals for the point estimates. The figure shows that the 1999 U.S.-China bilateral agreement significantly shifted bargaining power away from U.S. workers to U.S. firms because of the treated firms' improved access to cheap labor in China. As a result, these U.S. firms' investment decisions are not sensitive to minimum wage shocks. Together, these findings are consistent with the hypothesis that greater access to cheaper Chinese labor increases the bargaining power of employers vis-a-vis their workers causing a significant decline in the investment response to minimum wage increases.[20]

In Table IV column (2), we also examine the changes in investment-wage sensitivity of those select firms that changed their operational status in China, following the U.S.-China bilateral agreement. These firms newly established their subsidiaries in China after the agreement. Since setting up operations in a new country is not instantaneous, we reasonably choose 2004 (five years after the agreement) as the year to check the operational status of these firms in China. Specifically, we define $China04_i$ as an indicator variable set to one for those firms without any subsidiary in China as of 1997, but have at least one subsidiary in China as of 2004, and zero otherwise. We then introduce this indicator variable with its interactions in equation (5). In column (2), the omitted group consists of firms that have no operations in China; that is, $China04$ = $China97$ = 0. For firms that changed their operational status in China after the agreement ($China04$ = 1), the results are as follows: The investment-wage sensitivity changes from –0.031 ($= -0.016 - 0.015$, a t-statistic of –2.18) before the agreement to 0.000 ($= -0.031 + 0.002 + 0.029$, a t-statistic of 0.06) after the agreement. The result is consistent with the view that firms move their operations to China after the agreement to source cheap labor for their operations and eliminate their investment-wage sensitivity.[21] Figure B.3 plots these results.

Figure B.3 is also consistent with the revealed preference theory, which can be used to analyze the China subsidiary choices of firms and to compare the influence of the U.S.-China bilateral agreement on firm behavior. Firms with operations in China in 1997 before the agreement presumably had the most to gain by reducing the negative impact of minimum wage shocks on investment and were willing to incur costs of doing business in China, even before the agreement. Consistent

[20]We also check the robustness of our results for firms with at least one subsidiary in China as of 1998, one year prior to the bilateral agreement. The results documented in Panel A of Table B.5 in Appendix B are qualitatively similar to those reported in Table IV.

[21]To track U.S. firms' entry into China after the bilateral agreement, we also construct a time-varying indicator, $China_{i,t}$, that takes a value of one if firm i has at least one subsidiary in China in year t, and zero otherwise. Since comprehensive reporting of subsidiary information in the EDGAR (Electronic Data Gathering, Analysis, and Retrieval) database from the U.S. Securities and Exchange Commission (SEC) is only available starting from 1997, we use information as of 1997 for all years prior to 1997. Using this time-varying indicator instead of $China97$, we continue to obtain similar results. These robustness results are reported in Panel B of Table B.5 in Appendix B.

with this view, these firms had the most negative investment-wage sensitivity of all firms in our sample before the agreement and also gained the most by eliminating this investment-wage sensitivity after the agreement. Firms with operations in China in 2004 but not in 1997 also gained, presumably because the agreement lowered their costs of doing business in China enough to overturn their earlier decision of not having a Chinese subsidiary. Indeed, these firms had a negative impact of minimum wage shocks on investment (but not as much as the firms that were operating in China as of 1997) and were willing to incur costs of doing business in China, only after the agreement lowered their costs to do business in China. This group of firms was successful in eliminating their investment-wage sensitivity after the agreement. Finally, the firms with no operations in China before and after the agreement had their negative investment-wage sensitivity virtually unchanged after the agreement.

C. Unionization and Constraints on Labor Cost Adjustment

The ability to substitute inputs, whether via automation or offshoring, depends not only on technology or geography, but also on institutional frictions. We consider this by examining the effect of unionization on the sensitivity of firm investment to minimum wage shocks. It has been well documented that labor union membership in the United States has been declining over the last 50 years (Açıkgöz and Kaymak, 2014). We investigate whether the increase in employer bargaining power associated with this trend contributes to the significant decline in investment-wage sensitivities by enabling firms to more flexibly respond to minimum wage increases.

We begin with a descriptive analysis. In Figure I, we plot the 15-year moving averages of (i) annual union coverage and (ii) investment wage sensitivity from 1984–2017.[22] The figure shows a clear negative relationship; as union coverage declines, investment-wage sensitivity increases (becomes less sensitive). To provide a measure of this relationship, we estimate a univariate time-series regression of IWS on *Union Coverage*. The estimated coefficient on *Union Coverage* is

[22] *Union Coverage* is defined as the percentage of private-sector workers covered by a collective bargaining agreement; the data is from Hirsch and Macpherson (2003). The time series of investment-wage sensitivities are from the rolling-window estimates shown in Panel A of Table B.1

–0.769 and is statistically significant with a t-statistic of –4.24.[23] This inverse relationship provides preliminary evidence that union strength limits firms' ability to substitute away from labor in response to wage shocks. Although this result is not causal, it is sensible and consistent with the hypothesis that declining unionization plays a role in explaining changes in investment-wage sensitivity.

To provide causal evidence, we exploit the staggered passage of state-level right-to-work (RTW) laws as an exogenous source of variation in union strength (e.g., Matsa, 2010; Chava, Danis and Hsu, 2020). In states with RTW legislation, employees cannot be compelled, by unions or employers, to join unions or pay membership dues as a condition of employment. As a result, these laws limited union access to financial resources and manpower, weakening their bargaining power. We define RTW as an indicator variable equal to one if a firm is headquartered in a state that has passed RTW legislation as of year t; and zero otherwise.[24] To test whether weakened union power causes corporate investment to be less sensitive to minimum wage shocks, we interact the minimum wage variable $w_{i,s,t-1}$ in the investment regression with RTW. The coefficient on this interaction term captures the effect of unionization on investment-wage sensitivity.

[Insert Table V here.]

The results, reported in Table V, are consistent with the hypothesis that, by increasing the input substitution capacity with which firms can respond, declining union power has led to a reduction in the sensitivity of firm investment to minimum wage increases. More specifically, in column (1), the estimated coefficient of $w_{i,s,t-1}$ is –0.013 and statistically significant at the 5% level. The estimate on the interaction term ($RTW \times w_{i,s,t-1}$) is 0.026 and statistically significant at the 10% level. This suggests that after the passage of RTW laws, corporate investment is less sensitive to minimum wage increases. The investment-wage sensitivity before and after the passage of the

[23] We use the Newey-West standard error, which is robust to heteroskedasticity and autocorrelation up to 14 lags, to take into account the fact that the dependent variable is estimated using the overlapping samples. The R^2 of this regression is 0.724, which suggests that changes in union coverage density can explain almost three-quarters of the variation in changes in investment-wage sensitivity over our sample period.

[24] Following Chava et al. (2020), we remove states that introduced RTW laws before 1984, which is the beginning of our sample period.

law is illustrated in Figure II (c). The figure shows that the investment-wage sensitivity becomes positive after the passage of RTW law (0.013 = –0.013 + 0.026) although it is not statistically significant at the conventional level. We conclude that the passage of RTW laws contributed to the significant decline in investment-wage sensitivity for firms affected by these laws in our sample.

[Insert Figure II (c) here.]

In column (2), we introduce an additional indicator variable, $Large\ Decline_{RTW}$, to isolate the effects of states with a large decline in union coverage around the RTW adoption year. We compute the three-year average of state-level union coverage before and after the RTW adoption year. The difference between these values measures the change in union coverage around the passage of RTW laws which is expected to be negative. We then define $Large\ Decline_{RTW}$ as an indicator variable set to one if a state (where a firm is headquartered) experienced a below-median change around the adoption year, and zero otherwise. The estimate on the interaction term ($RTW \times Large\ Decline_{RTW} \times w_{i,s,t-1}$) is 0.043 and statistically significant at the 1% level. This result supports our hypothesis that weakening union power causes corporate investment decisions to be less sensitive to minimum wage increases by allowing firms to pursue alternative options. We also note that the coefficients on RTW are positive and significant in both columns, which indicates that the passage of RTW laws also has a direct positive impact on investment. These results are consistent with findings in Hirsch (1992), Fallick and Hassett (1999), and Chava et al. (2020).

Overall, the results in this section suggest that weakening union power during 1984–2017 is an important causal mechanism that explains the significant decline in investment-wage sensitivity by enabling firms to more easily adopt mechanization or substitute foreign workers.[25]

[25] In Appendix C, we conduct a simple counterfactual exercise to gauge the overall economic effects of a minimum wage increase on the workforce, taking into account job losses due to investment cuts. The results indicate that the opportunity cost of job losses resulting from the investment cut is much larger than the benefit of wage increase for the pre-2000 period in which the investment-wage sensitivity is negative. We note that this analysis is simplistic in that it does not take into account the general-equilibrium effects of the minimum wage increase on factor or output prices.

D. Import Competition and Constraints on Labor Cost Passthrough

We next examine how rising import competition, which has limited U.S. firms to pass through their increasing domestic labor costs, enabled them to adopt alternative options such as automation or offshore production. We focus on the surge in Chinese exports to the U.S. following China's accession to the World Trade Organization (WTO) in 2001. Devereaux and Lawrence (2004) note that after the passage of the U.S.-China bilateral agreement in November 1999, the Clinton Presidential administration announced its strong support for Permanent Normal Trade Relations (the PNTR bill) with China. After a ten-month-long effort, the U.S. House and Senate passed the PNTR bill into law in September 2000, which would be in force once China's accession to the WTO was completed. In December 2001, China became the 143rd member of the WTO, and the U.S. extended the PNTR status to China as of January 2002. The net effect of these laws was to grant China's producers access to the U.S. market and thus intensify the import competition for U.S. firms across many sectors of the economy.

Product market competition affects a firm's ability to raise its product prices in response to labor cost shocks (e.g., Harasztosi and Lindner, 2019; Agarwal, Ayyagari and Kosová, 2024). In a more competitive industry (higher import penetration ratio), firms are less able to shift rising labor costs to their consumers through an increase in prices without losing their competitive advantage (e.g., without causing a large drop in output). Such product market pressure increases the payoff to substitution, giving exposed firms stronger incentive to replace costly domestic labor, for instance, by shifting to a capital-intensive production process (Dai and Qiu, 2023) or offshoring. Therefore, we hypothesize that the investment-wage sensitivity of U.S. firms is eliminated after China's export surge (i.e., after 2001), and this effect is more pronounced for firms in industries that are highly exposed to Chinese import competition. We use this demand-driven shock to U.S. labor markets to identify the causal effect of China's export surge on changes in investment-wage sensitivity.

We define a U.S. industry's exposure to imports from China, $Exposure_{UC}$, as the log of the import penetration ratio if an industry is classified to be in the tradable sector, and zero otherwise.

The import penetration ratio is defined as the ratio of U.S. imports from China to total U.S. expenditure on goods in which U.S. expenditure is measured as U.S. gross output plus U.S. imports minus U.S. exports.[26] We use the log transformation of the import penetration ratio due to its highly right-skewed distribution. Following Mian and Sufi (2014), we classify a four-digit NAICS industry as tradable if the amount of its imports and exports is at least $10,000 per worker, or if the total amount exceeds $500M.[27]

To test our hypothesis, we estimate the following difference-in-differences regression, which is similar to equation (5):

$$\begin{aligned}\frac{I_{i,s,t}}{K_{i,s,t-1}} &= \alpha_i + \alpha_t + \beta_1\, Tobin's\; q_{i,s,t-1} + \beta_2\, \frac{CF_{i,s,t}}{K_{i,s,t-1}} + \beta_3\, w_{i,s,t-1} + \beta_4\, WTO_t \times w_{i,s,t-1} \\ &+ \beta_5\, Exposure_{UC,i} \times WTO_t \times w_{i,s,t-1} + \beta_6\, Exposure_{UC,i} \times w_{i,s,t-1} \\ &+ \beta_7\, Exposure_{UC,i} \times WTO_t + \beta_8\, X_{i,s,t-1} + \epsilon_{i,s,t}.\end{aligned} \tag{6}$$

We define $Exposure_{UC,i}$ as firm i's exposure to imports from China, as of 1999 (two years prior to China's accession into the WTO), in two ways: (i) As a continuous variable, we assume it to be equal to $Exposure_{UC}$ for the four-digit NAICS industry to which firm i belongs. (ii) As an indicator variable, we set $Exposure_{UC,i} = 1$ for all firms in the industries with above-median $Exposure_{UC}$, and zero otherwise. WTO_t indicates the time period after China's WTO accession (including the year 2001).[28] $X_{i,s,t-1}$ includes the same set of control variables used in Panel A of

[26]Specifically, we construct the import penetration ratio as

$$IP_{j,1999} = \frac{M^{UC}_{j,1999}}{Y_{j,1999} + M_{j,1999} - E_{j,1999}}$$

where for each industry j, $M^{UC}_{j,1999}$ is U.S. imports from China in 1999; $Y_{j,1999}$ is industry shipments; $M_{j,1999}$ refers to industry imports; and $E_{j,1999}$ is industry exports. We thank Peter Schott for making the trade flows data used in his paper (Bernard, Jensen and Schott, 2006) available on his website.

[27]We thank the authors for making their full list of industry classification available in the Supplemental Material (Mian and Sufi, 2014).

[28]Autor, Dorn and Hanson (2013) document that the import penetration ratio for Chinese goods rose from 0.6 percent in 1991 to 4.6 percent in 2007, with an inflection point in 2001, supporting our identification strategy.

Table II. $X_{i,s,t-1}$ also includes all interaction terms of these control variables with $Exposure_{UC,i}$ and WTO_t.[29] The coefficient of the triple interaction term ($Exposure_{UC,i} \times WTO_t \times w_{i,s,t-1}$) captures the difference-in-differences in investment-wage sensitivity before and after 2001 across firms that are subject to a different degree of import competition. Our hypothesis predicts β_5 to be positive.

[Insert Table VI here.]

The first column of Table VI estimates equation (6) using $Exposure_{UC,i}$ as an indicator variable. The difference-in-differences in the investment-wage sensitivity (β_5) after China's accession to the WTO (WTO accession, hereafter) between treated ($Exposure_{UC,i}$ = 1) and control ($Exposure_{UC,i}$ = 0) firms is 0.041 and significant at the 1% level. For the treated firms, the investment-wage sensitivity before the WTO accession is calculated as the sum of regression coefficients: $\beta_3 + \beta_6 = -0.021 - 0.027 = -0.048$ with a t-statistic of −5.69, both economically and statistically significant, indicating that these firms reduced their investment in response to mandated changes in the minimum wage before the WTO accession. Their investment-wage sensitivity after the agreement is calculated as the sum of four regression coefficients: $\beta_3 + \beta_4 + \beta_5 + \beta_6 = -0.021 + 0.009 + 0.041 - 0.027 = 0.002$ with a t-statistic of 0.34. The estimated sensitivity is economically and statistically insignificant, indicating that the treated firms do not adjust their investment in response to mandated changes in the minimum wage after the WTO accession.

For the control group of firms, the investment-wage sensitivity before the WTO accession is the regression coefficient $\beta_3 = -0.021$ with a t-statistic of −2.05, both economically and statistically significant, suggesting that these firms significantly adjusted their investment in response to labor cost shocks before the WTO accession. Their investment-wage sensitivity after the WTO accession is calculated as the sum of two regression coefficients: $\beta_3 + \beta_4 = -0.021 + 0.009 = -0.012$ with a t-statistic of −1.65. The estimated sensitivity continues to be economically significant, although barely statistically significant, with a p-value of 0.109 (and smaller in magnitude than before). In

[29]The $Exposure_{UC,i}$ and WTO variables are absorbed by the firm and year fixed effects, respectively.

column (2), we estimate equation (6) using $Exposure_{UC,i}$ as a continuous variable. Our inferences are qualitatively unchanged.

One important concern about $Exposure_{UC,i}$ is that it may also be correlated with domestic shocks to U.S. industries that, in turn, affect U.S. import demand, especially if China were the least-cost producer of the demanded products. Therefore, to the extent that corporate investment may be correlated with unobserved shocks to U.S. product demand, the OLS estimates using $Exposure_{UC,i}$ as an RHS variable could be biased and inconsistent. To address this concern, following Autor et al. (2013), we instrument our exposure measure ($Exposure_{UC,i}$) with a non-U.S. trade exposure to Chinese imports ($Exposure_{OC,i}$) that is constructed using data on imports from China in eight other high-income countries excluding the United States.[30] The intuition behind this instrument is that other high-income countries are similarly exposed to China's export surge, which is mostly driven by supply shocks in China, with an assumption that (unobserved) import demand shocks are uncorrelated across high-income countries.[31]

Column (3) in Table VI reports the second stage two-stage least squares (2SLS) estimates of equation (6). The estimates are qualitatively similar to the OLS estimates in column (2). The 2SLS estimate of the coefficient on triple interaction term, the difference-in-differences in the investment-wage sensitivity, β_5 = 0.023 is statistically significant at the 1% level and larger than the corresponding OLS estimate of 0.019 in column (2). These results offer further evidence that globalization in its demand-driven forms has played a crucial role in shaping how firms respond to labor cost shocks, creating an urgency to implement alternative options like automation or offshore production under increasing competitive pressure.

[30]These countries are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland. We thank David Dorn for making the data available on his website (`http://www.ddorn.net/data.htm`).

[31]Since the U.S. total expenditure may be endogenous to import shocks, we check the robustness of our results by measuring industry total expenditure one year prior to 1999 and obtain similar results. We also estimate the predictive power of (non-U.S.) high-income-country instrument ($Exposure_{OC}$) for U.S. trade exposure to Chinese imports ($Exposure_{UC}$) by regressing $Exposure_{UC}$ by industry (n = 386) on the corresponding $Exposure_{OC}$ for the year 1999. The estimated coefficient on the instrument is 1.12 (The first stage $F - statistic$ = 49.68), and the adjusted R-squared is 0.87. These results confirm the strong predictive power (in the first-stage regression) of other high-income countries' trade exposure on the U.S. trade exposure to Chinese imports.

[Insert Figure II (d) here.]

For graphical illustration, we present the 2SLS estimates for two groups of firms in Figure II (d). The first group is U.S. firms with no exposure to import competition with $Exposure_{UC,i} = 0$. The second group is U.S. firms with $Exposure_{UC,i}$ value that is one standard deviation above the sample mean. Figure II (d) shows that the magnitude of the sensitivity of the non-vulnerable firms is lower than the investment-wage sensitivity of the more vulnerable firms in the pre-WTO accession period. In the post-WTO accession period, both groups of firms move towards eliminating their sensitivity. The non-vulnerable firms now have a lower magnitude of sensitivity than before (but still statistically significant). However, the more vulnerable firms have no investment-wage sensitivity after the WTO accession.

In supplementary analysis, we explore whether the changes in investment-wage sensitivity due to import competition vary by firm position within an industry. We find that the decline in sensitivity is more pronounced for industry leaders, consistent with theories suggesting that dominant firms are more likely to respond to cost shocks by investing to preserve market position (Gutiérrez and Philippon, 2017). These results are presented in Table B.6 of Appendix B.[32]

Across all four tests, we find consistent evidence that firms with greater input substitutability exhibit lower sensitivity of investment to wage shocks. These results reinforce the central message of the paper: structural changes in labor market institutions and technologies have fundamentally altered the investment behavior of firms in response to rising labor costs.

IV. Conclusion

This study examines how structural changes in labor markets have reshaped firms' investment responses to labor cost shocks. Through analysis of firm-level investment behavior following minimum wage increases, we document a striking decline in investment sensitivity over time. In-

[32] Using the data on new entry by Wal-Mart into the local markets, Khanna and Tice (2000) find similar results: larger and more profitable incumbents invest more (i.e., expansion in the number of stores) in response to Wal-Mart's entry, while highly levered incumbents shrink (i.e., retrenchment in the number of stores).

vestment responded negatively and significantly to minimum wage hikes in the 1980s and 1990s, but this effect disappears after 2000. Using natural experiments and cross-sectional variation in structural exposures, we attribute this shift to increasing employer substitution margins driven by globalization, technological advancement, and declining unionization. These forces have collectively expanded firms' options for managing rising labor costs without reducing capital investment.

From a production theory perspective, the diminished investment response to labor cost shocks reflects an increase in the effective elasticity of substitution between capital and labor and between foreign and domestic labor. When substitution was more constrained, wage increases lowered the marginal return to capital and triggered sharp reductions in investment. In contrast, as firms gained the capacity to automate, offshore, or restructure in response to wage pressures, their investment became less sensitive to these shocks.

Our findings contribute to the labor and finance literature by demonstrating that labor market constraints evolve over time. While prior studies document how labor market frictions shape firm financing, investment, and innovation decisions, we extend this work by showing that the influence of these frictions has declined over time—a shift consistent with theoretical predictions from production economics. Specifically, our findings support the hypothesis that rising capital-labor substitutability and enhanced foreign-domestic labor substitutability have weakened the traditional "scale effect" whereby higher labor costs depress investment. Our analysis thus provides a structural explanation for the mixed findings in prior research regarding investment responses to minimum wage changes.

This work also contributes to research on the decline in labor's share of U.S. national income since the 1980s. Much of this literature attributes the decline to weakening worker bargaining power relative to employers, a trend driven by the same labor market transformations we examine. While existing research has primarily examined the macroeconomic implications of declining worker power, our study provides firm-level microfoundations that illuminate these broader trends.

Our analytical framework opens several avenues for future research. First, researchers could examine whether rising substitution elasticity persists under emerging geopolitical conditions that may reintroduce frictions to global labor markets. Reshoring trends, increasing trade protectionism, and supply chain resilience concerns could potentially reverse the trajectory we document. Second, extending this analysis to other labor cost shocks—such as mandated benefits or occupational licensing requirements—could test whether similar mechanisms operate across different policy contexts. Finally, this research provides a methodological template for examining dynamic interactions between labor market institutions and firm behavior, with implications for both production theory and corporate finance.

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Table I: Descriptive Statistics

This table provides descriptive statistics of the main variables used in this study. The firm-year sample consists of 59,096 firm-year observations from 1984 to 2017. $Investment$ is measured as capital expenditures normalized by the beginning-of-year capital stock (property, plant, and equipment). $Cash\ Flow$ is calculated as earnings before extraordinary items plus depreciation, normalized by the beginning-of-the-year capital stock. $Tobin's\ q$ is a proxy for investment opportunities, which is measured as the ratio of the market value of assets to the book value of assets, where the market value of assets is defined as total assets plus market equity minus book equity. For state-level variables, we report their descriptive statistics based on 1,190 state-year observations. $w_{s,t-1}$ is the minimum wage at time $t-1$ in state s. For 1983–2014, we obtain the historical changes in minimum wages for non-farm private sector employment under state and federal laws from the Tax Policy Center. These data are sourced from the Wage and Hour Division of the U.S. Department of Labor and from the *Monthly Labor Review* by the Bureau of Labor Statistics. For 2015–2017, we hand-collect the data from the U.S. Department of Labor. Under Section 18 of the Fair Labor Standard Act, when an employee is subject to both the federal and state minimum wage laws, the employee is entitled to the higher of the two standards. The across-state variation is the cross-sectional standard deviation of state-level time-series averages of $w_{s,t-1}$ whereas the within-state variation is the average of time-series standard deviations for all states. $GDP\ growth$ is state-level annual growth rate (in percentage) of real GDP from the Bureau of Economic Analysis; $Population$ is intercensal estimates of the resident population (in thousands) for each state from the U.S. Census Bureau; $Unemployment$ is state-level unemployment rate (in percentage) from the Bureau of Labor Statistics. The definitions of all variables are provided in Appendix A.

Variables	Mean	Median	Std.Dev.	# of Obs.
Firm-Year-Level Data				
$Investment$	0.245	0.185	0.217	59,096
$Cash\ Flow$	0.379	0.269	0.895	59,096
$Tobin's\ q$	1.641	1.331	0.978	59,096
State-Year-Level Data				
$w_{s,t-1}$ ($)	5.307	5.150	1.535	1,190
$w_{s,t-1}$ ($) (across-state variation)			0.248	
$w_{s,t-1}$ ($) (within-state variation)			1.516	
$GDP\ Growth$ (%)	2.576	2.400	2.801	1,190
$Population$ (thousands)	5,687	3,506	6,736	1,190
$Unemployment$ (%)	5.792	5.400	2.002	1,190

Table II: Decline in Investment-Wage Sensitivity

Panel A presents fixed effect OLS regressions of corporate investment on minimum wages in equation (1). The dependent variable is *Investment*, measured as capital expenditures normalized by the beginning-of-the-year capital stock (property, plant, and equipment). $w_{i,s,t-1}$ is the minimum wage at time $t-1$ in state s where firm i's headquarters is located. For 1983–2014, we obtain the historical changes in minimum wages for non-farm private sector employment under state and federal laws from the Tax Policy Center. These data are sourced from the Wage and Hour Division of the U.S. Department of Labor and from the *Monthly Labor Review* by the Bureau of Labor Statistics. For 2015–2017, we hand-collect the data from the U.S. Department of Labor. Under Section 18 of the Fair Labor Standard Act, when an employee is subject to both the federal and state minimum wage laws, the employee is entitled to the higher of the two standards. We exclude 15 states that have indexed their minimum wage rates to inflation for the identification reason discussed in Section II.B. We measure *Cash Flow* as earnings before extraordinary items plus depreciation normalized by the beginning-of-the-year capital stock and *Tobin's q* as a ratio of the market value of assets to the book value of assets. We also control for state-level macro-variables: real GDP growth rates, log of population, and unemployment rates. The definitions of all variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The full sample period runs from 1984 to 2017. H_0: (2)[$w_{i,s,t-1}$] - (3)[$w_{i,s,t-1}$] = 0 is based on a two-tailed test with χ^2-statistics in squared bracket.

Panel A. Minimum Wage and Corporate Investment: Full Sample, Pre-, and Post-2000

	Dependent Variable: $Investment_{i,s,t}$		
	Full Sample	1984 to 2000	2001 to 2017
	(1)	(2)	(3)
$w_{i,s,t-1}$	-0.017*	-0.038***	0.001
	(0.009)	(0.012)	(0.003)
$Cash\ Flow$	0.043***	0.098***	0.029***
	(0.002)	(0.005)	(0.002)
$Tobin's\ q$	0.063***	0.066***	0.053***
	(0.003)	(0.003)	(0.003)
$GDP\ growth$	0.002**	0.003**	0.002**
	(0.001)	(0.001)	(0.001)
ln($Population$)	-0.108	-0.145*	-0.192***
	(0.065)	(0.076)	(0.049)
$Unemployment$	0.0002	0.001	0.002
	(0.001)	(0.001)	(0.002)
H_0: (2)[$w_{i,s,t-1}$] - (3)[$w_{i,s,t-1}$] = 0		-0.039*** [12.52]	
Firm and Year FEs	Yes	Yes	Yes
# of Firm-Year Obs.	59,096	31,408	27,688
Adjusted R^2	0.140	0.135	0.122

Table II: Decline in Investment-Wage Sensitivity (continued)

Panel B reestimates Panel A by including an indicator for minimum wage sensitive industries ($MW\ Ind$) and its interaction with the minimum wage variable. We identify industries that are most subject to minimum wage changes using the percentage of workers who are paid at or below the federal minimum wage, which is obtained from the Labor Force Statistics (2017) by the Bureau of Labor Statistics. The minimum wage sensitive industries include manufacturing, retail trade, leisure and hospitality, and services. Panel C reestimates Panel A within the subsample of industries with a non-decreasing fraction of minimum-wage workers by comparing the years 2003 and 2017. The definitions of all other variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The full sample period runs from 1984 to 2017.

Panel B. Conditional Analyses on Minimum Wage Sensitive Industries

	Dependent Variable: $Investment_{i,s,t}$					
	Full Sample	1984 to 2000	2001 to 2017	Full Sample	1984 to 2000	2001 to 2017
	(1)	(2)	(3)	(4)	(5)	(6)
$w_{i,s,t-1}$	-0.013	-0.023**	-0.004			
	(0.009)	(0.010)	(0.005)			
$w_{i,s,t-1} \times MW\ Ind$	-0.006***	-0.020***	0.007*	-0.004**	-0.018***	0.009**
	(0.002)	(0.006)	(0.004)	(0.002)	(0.005)	(0.004)
$MW\ Ind$	0.031***	0.064***	-0.040	0.018*	0.051**	-0.049
	(0.010)	(0.024)	(0.036)	(0.010)	(0.021)	(0.036)
Controls / Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	No	No	No
State-by-Year FEs	No	No	No	Yes	Yes	Yes
# of Firm-Year Obs.	59,096	31,408	27,688	59,096	31,408	27,688
Adjusted R^2	0.140	0.136	0.123	0.427	0.454	0.488

Panel C. Industries with Non-decreasing Fraction of Minimum-Wage Workers

	Dependent Variable: $Investment_{i,s,t}$		
	Full Sample	1984 to 2000	2001 to 2017
	(1)	(2)	(3)
$w_{i,s,t-1}$	-0.018*	-0.035**	0.003
	(0.010)	(0.016)	(0.003)
H_0: (2)[$w_{i,s,t-1}$] - (3)[$w_{i,s,t-1}$] = 0		-0.038** [4.99]	
Controls / Firm and Year FEs	Yes	Yes	Yes
# of Firm-Year Obs.	40,389	22,136	18,253
Adjusted R^2	0.141	0.134	0.121

Table III: Automation and Capital-Labor Substitutability

This table presents difference-in-differences regressions of investment on minimum wages interacted with two variables, $Exposure_{tech,i}$ and $Post$, in equation (4). We measure the extent to which industries are subject to technological change using an industry-level share of routine-task labor ($Exposure_{tech}$). To construct this variable, we follow the procedure employed in Zhang (2019). We first define the routine-task intensity (RTI) score for each occupation as $RTI_k = ln(T_k^{routine}) - ln(T_k^{abstract}) - ln(T_k^{nonroutine\ manual})$ where $T_k^{routine}$, $T_k^{abstract}$, and $T_k^{nonroutine\ manual}$ are the routine, abstract, and nonroutine manual task skill levels (scaled from 1 to 10) required by occupation k. Each occupation's required skill level data are obtained from the revised fourth edition of the *Dictionary of Occupational Titles* by the U.S. Department of Labor. We classify workers as routine-task labor if their occupations' RTI score falls in the top quintile of the RTI distribution. We then construct $Exposure_{tech}$ as the proportion of routine-task labor costs to the total industry labor cost. We obtain data on the number of employees and their wages for each occupation-industry from the *Occupational Employment Statistics* by the Bureau of Labor Statistics. With the industry-level share of routine-task labor, we define $Exposure_{tech,i}$ as firm i's exposure to technological change, as of 1999 in two ways: (i) As a continuous variable, we set $Exposure_{tech,i}$ to be equal to $Exposure_{tech}$ for the three-digit SIC industry to which firm i belongs; (ii) As an indicator variable, we set $Exposure_{tech,i} = 1$ for firms if their continuous $Exposure_{tech,i}$ measure is above the median value of the $Exposure_{tech,i}$ distribution, and zero otherwise. $w_{i,s,t-1}$ is the minimum wage at time $t-1$ in state s where firm i's headquarters is located; $Post$ indicates period after 2001. The coefficient of the triple interaction term ($Exposure_{tech,i} \times Post \times w_{i,s,t-1}$) captures difference-in-differences in investment-wage sensitivity after the year 2001 across firms that are differentially susceptible to technological change in the 21st century. In all columns, we include the same set of control variables used in Panel A of Table II and their interaction terms with $Exposure_{tech,i}$ and $Post$. In column (1), we define *Low* (*High*) *Exposure* group as firms with $Exposure_{tech,i} = 0$ (1) as an indicator variable. In column (2), we define *Low* (*High*) *Exposure* group as firms with $Exposure_{tech,i}$ (as a continuous variable) value one-standard deviation below (above) the mean value of $Exposure_{tech,i}$. $Exposure_{tech,i}$ and $Post$ variables are absorbed by firm and year fixed effects, respectively. The definitions of all variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The sample period runs from 1984 to 2017.

Dependent Variable →	$Investment_{i,s,t}$	
Exposure to Technological Change →	Dummy	Continuous
	(1)	(2)
$w_{i,s,t-1}$	-0.018**	-0.016*
	(0.008)	(0.009)
$Post \times w_{i,s,t-1}$	0.001	-0.001
	(0.008)	(0.010)
$Exposure_{tech,i} \times Post \times w_{i,s,t-1}$	0.020***	0.073**
	(0.006)	(0.031)
$Exposure_{tech,i} \times w_{i,s,t-1}$	-0.012**	-0.049*
	(0.005)	(0.025)
$Exposure_{tech,i} \times Post$	0.083	0.405
	(0.114)	(0.507)
Investment Sensitivity to Minimum Wage [t-stat]		
Before (*Low Exposure*)	-0.018** [-2.17]	-0.019** [-2.19]
After (*Low Exposure*)	-0.017** [-2.66]	-0.015** [-2.32]
Before (*High Exposure*)	-0.030*** [-2.91]	-0.030*** [-3.10]
After (*High Exposure*)	-0.009* [-1.89]	-0.010* [-1.88]
Controls / Interaction of Controls	Yes	Yes
Firm and Year FEs	Yes	Yes
# of Firm-Year Obs.	36,213	36,213
Adjusted R^2	0.176	0.176

Table IV: Globalization and Substitutability of Foreign for Domestic Labor: 1999 U.S.-China Bilateral Agreement

Column (1) presents difference-in-differences regressions of investment on minimum wages interacted with two indicators, $China97$ and $Agreement$, in equation (5). $w_{i,s,t-1}$ is the minimum wage at time $t-1$ in state s where firm i's headquarters is located. $China97$ is an indicator variable set to one if a firm has at least one subsidiary in China as of 1997, two years prior to the U.S.-China bilateral agreement in 1999, and zero otherwise; $Agreement$ indicates the time period after the agreement (including 1999). We use hand-collected information from 10-k filings to identify U.S. firms' Chinese subsidiaries. The coefficient of the triple interaction term ($China97 \times Agreement \times w_{i,s,t-1}$) captures difference-in-differences in investment-wage sensitivity after the agreement between treated ($China97$ = 1) and control ($China97$ = 0) firms. In column (2), we introduce another group by defining $China04$ as an indicator variable set to one for firms without any subsidiary in China as of 1997 but having at least one subsidiary as of 2004 (five years after the agreement), and zero otherwise. The omitted group consists of firms that have no operations in China, that is, $China97 = China04 = 0$. In all columns, we include the same set of control variables used in Panel A of Table II and all interaction terms of these control variables with $China97$ (or $China04$) and $Agreement$ indicators. $China97$ ($China04$) and $Agreement$ indicators are absorbed by firm and year fixed effects, respectively. The definitions of all variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The sample period runs from 1984 to 2017.

Dependent Variable →	$Investment_{i,s,t}$	
	Two Groups	Three Groups
	(1)	(2)
$w_{i,s,t-1}$	-0.017**	-0.016*
	(0.008)	(0.008)
$Agreement \times w_{i,s,t-1}$	0.005	0.002
	(0.005)	(0.006)
$China97 \times Agreement \times w_{i,s,t-1}$	0.035***	0.038***
	(0.010)	(0.010)
$China04 \times Agreement \times w_{i,s,t-1}$		0.029**
		(0.014)
$China97 \times w_{i,s,t-1}$	-0.028***	-0.029***
	(0.008)	(0.009)
$China04 \times w_{i,s,t-1}$		-0.015
		(0.012)
$China97 \times Agreement$	-0.093	-0.051
	(0.179)	(0.192)
$China04 \times Agreement$		0.408*
		(0.239)
Investment Sensitivity to Minimum Wage [t-stat]		
Before (omitted group)	-0.017** [-2.14]	-0.016* [-1.98]
After (omitted group)	-0.012* [-1.92]	-0.014** [-2.34]
Before ($China04$ = 1)		-0.031** [-2.18]
After ($China04$ = 1)		0.000 [0.06]
Before ($China97$ = 1)	-0.045*** [-3.73]	-0.045*** [-3.71]
After ($China97$ = 1)	-0.005 [-0.86]	-0.005 [-0.96]
Controls / Interaction of Controls / Firm and Year FEs	Yes	Yes
# of Firm-Year Obs.	59,096	59,096
Adjusted R^2	0.157	0.158

Table V: Unionization and Constraints on Labor Cost Adjustment: Passage of Right-to-Work (RTW) Laws

This table presents difference-in-differences regressions of investment on minimum wages interacted with an RTW indicator variable. We use the passage of right-to-work (RTW) laws to measure the weakening power of labor unions. We define RTW as an indicator variable that assumes the value one, if the state where a firm is headquartered has passed RTW legislation as of year t, and zero otherwise. $w_{i,s,t-1}$ is the minimum wage at time $t-1$ in state s where firm i's headquarters is located. We remove states that introduced an RTW law before 1984, which is the beginning of our sample period. In column (2), we introduce an additional indicator, $Large\ Decline_{RTW}$, to isolate the effects of states with a large decline in union coverage around the RTW adoption year. We compare the three-year average of state-level union coverage before and after the RTW adoption year. The difference between these values measures the change in union coverage around the passage of RTW laws which is expected to be negative. We then define $Large\ Decline_{RTW}$ as an indicator variable set to one if a state (where a firm is headquartered) experienced a below-median change in union coverage rate around the adoption year, and zero otherwise. In all columns, we include the same set of control variables used in Panel A of Table II and all interaction terms of these control variables with RTW. $Large\ Decline_{RTW}$ is absorbed by firm fixed effects. The definitions of all variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The sample period runs from 1984 to 2017.

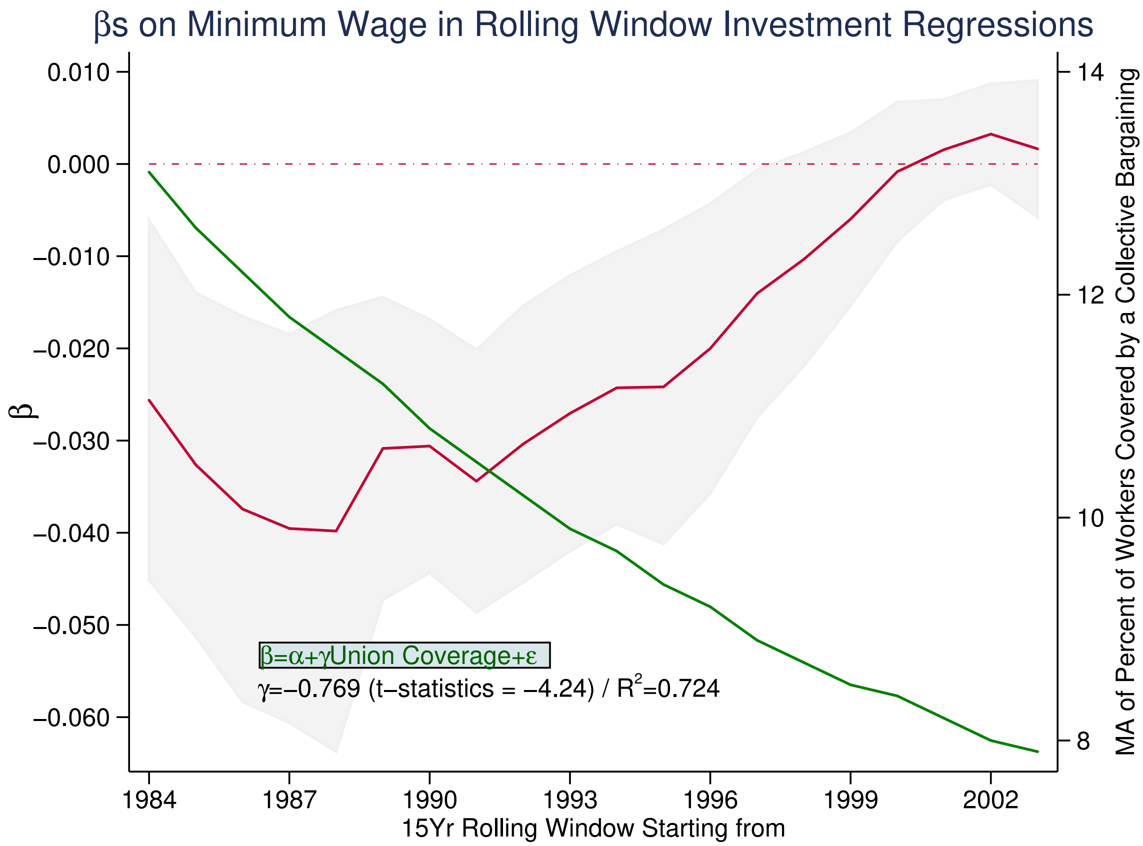
Dependent Variable →	$Investment_{i,s,t}$	
		States With Large Decline in Union Coverage Around the Adoption Year
	(1)	(2)
$w_{i,s,t-1}$	-0.013**	-0.012**
	(0.005)	(0.005)
$RTW \times w_{i,s,t-1}$	0.026*	0.011
	(0.015)	(0.009)
$RTW \times Large\ Decline_{RTW} \times w_{i,s,t-1}$		0.043***
		(0.009)
$Large\ Decline_{RTW} \times w_{i,s,t-1}$		0.002
		(0.007)
RTW	0.751*	1.208***
	(0.385)	(0.419)
$RTW \times Large\ Decline_{RTW}$		-0.306***
		(0.055)
Controls / Interaction of Controls	Yes	Yes
Firm and Year FEs	Yes	Yes
# of Firm-Year Obs.	37,111	37,111
Adjusted R^2	0.144	0.144

Table VI: Import Competition and Constraints on Labor Cost Passthrough

This table presents difference-in-differences regressions of investment on minimum wages interacted with $Exposure_{UC,i}$ and WTO in equation (6). $w_{i,s,t-1}$ is the minimum wage at time $t-1$ in state s where firm i's headquarters is located. WTO indicates the time period after China's entry into the World Trade Organization in 2001. For each U.S. industry, we first define its exposure to imports from China, $Exposure_{UC}$, as the log of the import penetration ratio (Bernard et al., 2006) if the industry is classified to be in the tradable sector(Mian and Sufi, 2014), and zero otherwise. We then measure firm i's exposure as of 1999 ($Exposure_{UC,i}$), two years prior to China's accession to the WTO, in two ways: (i) As a continuous variable, we set $Exposure_{UC,i} = Exposure_{UC}$ for the four-digit NAICS industry to which firm i belongs; (ii) As an indicator variable, we set $Exposure_{UC,i} = 1$ for firms in the industries with above-median $Exposure_{UC}$, and zero otherwise. The coefficient of the triple interaction term ($Exposure_{UC,i} \times WTO \times w_{i,s,t-1}$) captures difference-in-differences in investment-wage sensitivity before and after the year 2001 across firms that are differentially exposed to import competition. In column (3), we instrument for $Exposure_{UC,i}$ with $Exposure_{OC,i}$, Chinese import exposure for eight other high-income countries. In all columns, we include the same set of control variables used in Panel A of Table II and all interaction terms of these control variables with $Exposure_{UC,i}$ and WTO variables. In column (1), we define *No* (*High*) *Exposure* firms as firms with $Exposure_{UC,i} = 0$ (1) as an indicator variable. In columns (2) and (3), we define *No* (*High*) *Exposure* firms as firms with zero (one standard deviation above the mean) value of $Exposure_{UC,i}$ as a continuous variable. $Exposure_{UC,i}$ and WTO variables are absorbed by firm and year fixed effects, respectively. The definitions of all variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The sample period runs from 1984 to 2017.

Dependent Variable →	$Investment_{i,s,t}$		
	OLS		2SLS
Exposure to Import Competition →	Dummy	Continuous	Continuous
	(1)	(2)	(3)
$w_{i,s,t-1}$	-0.021**	-0.025**	-0.024**
	(0.010)	(0.011)	(0.010)
$WTO \times w_{i,s,t-1}$	0.009	0.013	0.012
	(0.008)	(0.008)	(0.008)
$Exposure_{UC,i} \times WTO \times w_{i,s,t-1}$	0.041***	0.019***	0.023***
	(0.004)	(0.006)	(0.006)
$Exposure_{UC,i} \times w_{i,s,t-1}$	-0.027***	-0.008**	-0.015***
	(0.004)	(0.003)	(0.004)
$Exposure_{UC,i} \times WTO$	-0.094	-0.048	-0.013
	(0.090)	(0.050)	(0.074)
Investment Sensitivity to Minimum Wage [t-stat]			
Before (*No Exposure*)	-0.021** [-2.05]	-0.025** [-2.35]	-0.024** [-2.30]
After (*No Exposure*)	-0.012 [-1.65]	-0.013 [-1.59]	-0.012 [-1.57]
Before (*High Exposure*)	-0.048*** [-5.69]	-0.040*** [-4.45]	-0.047*** [-4.92]
After (*High Exposure*)	0.002 [0.34]	0.006 [0.66]	0.001 [0.06]
Controls / Interaction of Controls	Yes	Yes	Yes
Firm and Year FEs	Yes	Yes	Yes
# of Firm-Year Obs.	46,168	46,168	46,104
Adjusted R^2	0.152	0.151	0.152

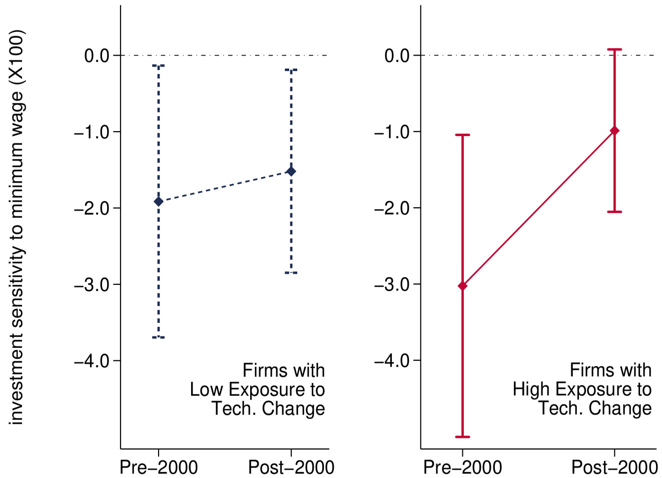
Figure I: Investment-Wage Sensitivity Since the Early 1980s



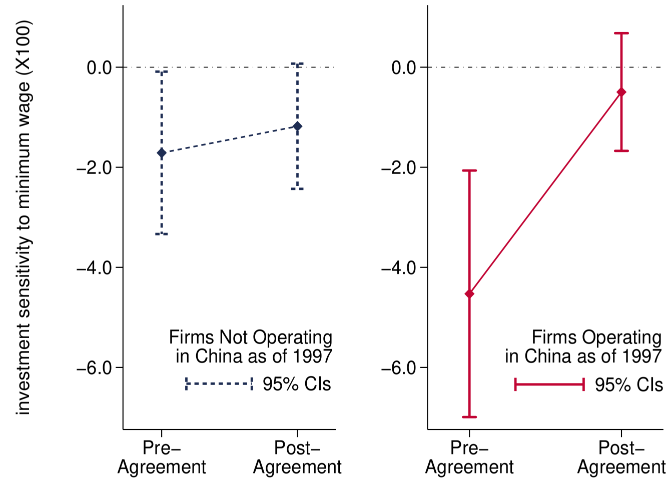
In this figure, the red line plots the time-series of estimated investment-wage sensitivity (β) from the 15-year rolling window investment regressions in Panel A of Table B.1. The grey shaded area indicates the 95% confidence interval. The x-axis refers to the starting years of the 15-year rolling windows. The estimated coefficients and t-statistics are tabulated in Panel A of Table B.1. The green line plots the 15-year moving average of union coverage (defined as the percentage of private-sector workers that are covered by a collective bargaining agreement) using the same sample windows as used in rolling window investment regressions. The union coverage data come from Hirsch and Macpherson (2003). We estimate a univariate time-series regression of investment-wage sensitivity at time t on the 15-year moving average of the annual union coverage. The estimated coefficient on union coverage is –0.769 with t-statistics of –4.24 based on the Newey-West standard error that is robust to heteroskedasticity and autocorrelation up to 14 lags. The R^2 of the regression is 0.724.

Figure II: Input Substitutability and Changes in Investment-Wage Sensitivity

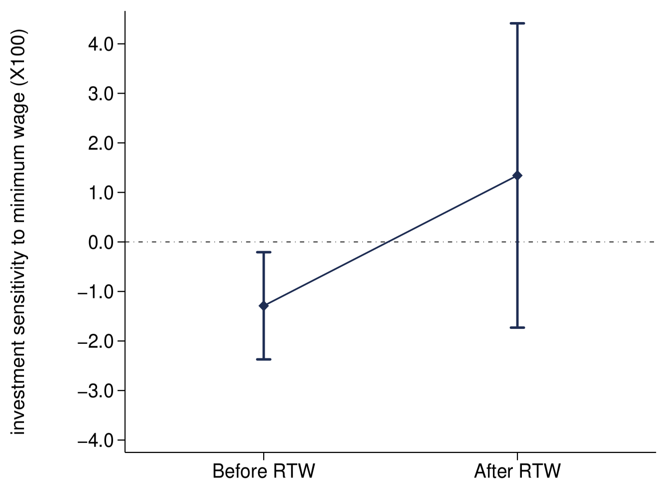
(a) Technological Change / Automation



(b) 1999 U.S.-China Bilateral Agreement



(c) Declining Union Power: Right-to-Work (RTW) Laws



(d) Chinese Import Competition

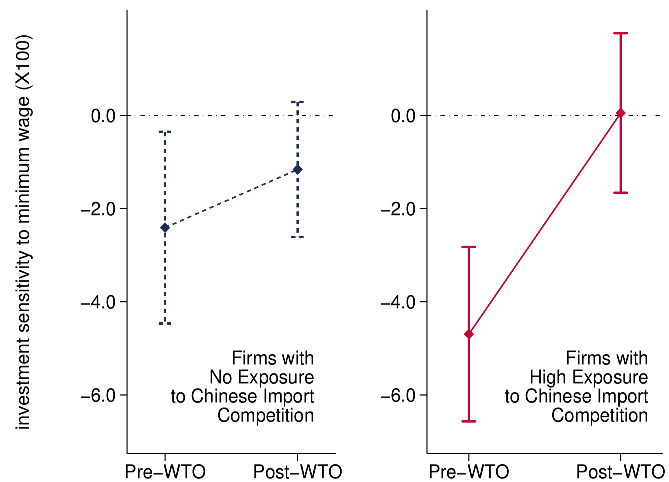


Figure II: Input Substitutability and Changes in Investment-Wage Sensitivity (continued)

These figures plot changes in investment-wage sensitivity for two groups of firms that are identified in Tables IV to V. Figure (a) is based on the estimates in column (2) of Table III. We measure the extent to which industries are subject to technological change using an industry-level share of routine-task labor ($Exposure_{tech}$). To construct this variable, we follow the procedure employed in Zhang (2019). We first define the routine-task intensity (RTI) score for each occupation as $RTI_k = ln(T_k^{routine}) - ln(T_k^{abstract}) - ln(T_k^{nonroutine\ manual})$ where $T_k^{routine}$, $T_k^{abstract}$, and $T_k^{nonroutine\ manual}$ are the routine, abstract, and nonroutine manual task skill levels (scaled from 1 to 10) required by occupation k. Each occupation's required skill level data are obtained from the revised fourth edition of the *Dictionary of Occupational Titles* by the U.S. Department of Labor. We classify workers as routine-task labor if their occupations' RTI score falls in the top quintile of the RTI distribution. We then construct $Exposure_{tech}$ as the proportion of routine-task labor costs to the total industry labor cost. We obtain data on the number of employees and their wages for each occupation-industry from the *Occupational Employment Statistics* by the Bureau of Labor Statistics. With the industry-level share of routine-task labor, we define firm i's exposure ($Exposure_{tech,i}$) to technological change as of 1999 as $Exposure_{tech}$ for the three-digit SIC industry to which firm i belongs. We define firms with low (high) exposure to technological change as firms with $Exposure_{tech,i}$ value one-standard deviation below (above) the mean value of $Exposure_{tech,i}$. Figure (b) is based on the estimates in column (1) of Table IV. The first group consists of firms that do not operate in China as of 1997 ($China97 = 0$), two years prior to the 1999 U.S.-China bilateral agreement; the second group consists of firms operating in China as of 1997 ($China97 = 1$). We identify firms operating in China (i.e., having at least one subsidiary in China) using hand-collected information from 10-K filings on U.S. firms' Chinese subsidiaries. Dots indicate the estimated investment-wage sensitivity, and the vertical lines around these point estimates are 95% confidence intervals. Figure (c) is based on the estimates in column (1) of Panel B in Table V. We use the passage of right-to-work (RTW) laws to measure the weakening power of labor unions. Specifically, we identify firms that are headquartered in a state as having weak (strong) union power if the state has (not) passed the RTW legislation. Figure (d) is based on the estimates in column (3) of Table VI. We define a U.S. industry's exposure to imports from China, $Exposure_{UC}$, as the log of the import penetration ratio if an industry is classified to be in the tradable sector, and zero otherwise. We define firm i's exposure ($Exposure_{UC,i}$) to imports from China as of 1999, two years prior to China's accession to the World Trade Organization, as $Exposure_{UC}$ for the four-digit NAICS industry to which firm i belongs. We define the first group as firms with no exposure to import competition ($Exposure_{UC,i} = 0$) and the second group as firms with one-standard-deviation above the average exposure ($Exposure_{UC,i} = \mu + \sigma$). To capture supply-driven components in U.S. imports from China, we instrument for $Exposure_{UC,i}$ with $Exposure_{OC,i}$ in which $Exposure_{OC,i}$ is Chinese import exposure for eight other high-income countries. The detailed definitions of all variables are provided in Appendix A.

For Online Publication

Appendices

Appendix A. Variable Definitions

Variables	Definition [Compustat designations where appropriate]
Investment	Capital expenditures [CAPX] normalized by the beginning-of-the-year capital stock (property, plant, and equipment) [PPENT]
$w_{i,s,t-1}$	Minimum wage at time $t-1$ in state s where firm i's headquarters is located; We use the historical changes in minimum wages under state laws reported by the Tax Policy Center, which uses data from the Wage and Hour Division of the U.S. Department of Labor and from the *Monthly Labor Review* by the Bureau of Labor Statistics. In cases where an employee is subject to both the state and federal minimum wage laws, the employee is entitled to the higher of the two under Section 18 of the Fair Labor Standard Act.
Cash Flow	Earnings before extraordinary items [IB] plus depreciation [DP] normalized by the beginning-of-the-year capital stock [PPENT]
Tobin's q	The ratio of the market value of assets to book value of assets [AT] where the market value of assets is defined as total assets [AT] plus market equity minus book equity in which market equity is defined as common shares outstanding [CSHO] times fiscal-year closing price [PRCC_F]; book equity is calculated as stockholders' equity [SEQ] minus preferred stock liquidating value [PSTKL] plus balance sheet deferred taxes and investment tax credit [TXDITC] when available minus post-retirement assets [PPROR] when available
GDP growth	State-level annual growth rate of real GDP from the Bureau of Economic Analysis
ln(Population)	Log of intercensal estimates of the resident population for each state from the U.S. Census Bureau
Unemployment	State-level unemployment rate from the Bureau of Labor Statistics
$MW\ Ind$	An indicator variable set to one for firms in minimum wage sensitive industries. We identify those industries using the percentage of workers who are paid at or below the federal minimum wage, which is retrieved from the Labor Force Statistics from the Current Population Survey (2017) by the Bureau of Labor Statistics. The minimum wage sensitive industries include manufacturing, retail trade, leisure and hospitality, and services. They account for 95% of minimum-wage workers in the private sector.
Agreement	An indicator variable for the time period after the U.S.-China bilateral agreement in 1999 (including 1999)
China97	An indicator variable set to one for firms with at least one subsidiary in China two years prior to the U.S.-China bilateral agreement in 1999, and zero otherwise. We use hand-collected information from 10-k filings to identify U.S. firms' Chinese subsidiaries.
China04	An indicator variable set to one for firms without any subsidiary in China as of 1997 but having at least one subsidiary in China as of 2004 (five years after the U.S.-China bilateral agreement in 1999), and zero otherwise

Post	An indicator for the time period after 2001
$Exposure_{tech,i}$	We first measure the extent to which industries are subject to technological change using an industry-level share of routine-task labor ($Exposure_{tech}$). To construct this variable, we follow the procedure employed in Zhang (2019). We first define the routine-task intensity (RTI) score for each occupation as $RTI_k = ln(T_k^{routine}) - ln(T_k^{abstract}) - ln(T_k^{nonroutine\ manual})$ where $T_k^{routine}$, $T_k^{abstract}$, and $T_k^{nonroutine\ manual}$ are the routine, abstract, and nonroutine manual task skill levels (scaled from 1 to 10) required by occupation k. Each occupation's required skill level data are obtained from the revised fourth edition of the *Dictionary of Occupational Titles* by the U.S. Department of Labor. We classify workers as routine-task labor if their occupations' RTI score falls in the top quintile of the RTI distribution. We then construct $Exposure_{tech}$ as the proportion of routine-task labor costs to the total industry labor cost. We obtain data on the number of employees and their wages for each occupation-industry from the *Occupational Employment Statistics* by the Bureau of Labor Statistics. Using this industry-level measure of exposure to technological change ($Exposure_{tech}$), we define $Exposure_{tech,i}$ as firm i's exposure to technological change, as of 1999 in two ways: (i) As a continuous variable, we set $Exposure_{tech,i}$ to be equal to $Exposure_{tech}$ for the three-digit SIC industry to which firm i belongs; (ii) As an indicator variable, we set $Exposure_{tech,i} = 1$ for firms if their continuous $Exposure_{tech,i}$ measure is above the median value of the $Exposure_{tech,i}$ distribution, and zero otherwise.
RTW	An indicator variable set to one if the state where a firm is headquartered has passed the right-to-work (RTW) legislation as of year t, and zero otherwise
$LargeDecline_{RTW}$	We first compute the three-year average of state-level union coverage before and after the RTW adoption year. The difference between these values measures the change in union coverage around the passage of RTW laws. We then define $LargeDecline_{RTW}$ as an indicator variable set to one if a state (where a firm is headquartered) experienced a below-median change around the adoption year and zero otherwise
WTO	An indicator for the time period after China's entry to the World Trade Organization (WTO) in 2001
$Exposure_{UC,i}$	We first define a U.S. industry's exposure to imports from China ($Exposure_{UC}$) as Chinese import penetration ratio (Bernard et al., 2006) if firms are classified as tradable sector (Mian and Sufi, 2014), and zero otherwise. We then define firm i's exposure to imports from China as of 1999 ($Exposure_{UC,i}$) in two ways: (i) As a continuous variable, we set it to be equal to $Exposure_{UC}$ for the four-digit NAICS industry to which firm i belongs; (ii) As an indicator variable, we set $Exposure_{UC,i} = 1$ for firms in the industries with above-median $Exposure_{UC}$, and zero otherwise.
$Exposure_{OC,i}$	Chinese import exposure for eight other high-income countries excluding the United States (including Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland)
Leader	An indicator variable set to one for firms with above-median *Tobin's q*, sales [SALE], or total assets [AT] for each industry (SIC two-digit) as of 1999, two years prior to China's entry to the WTO, and zero otherwise

Appendix B. Additional Tables and Figures

A. *Rolling Window Regressions and Structural Break Analysis*

To examine dynamic changes in investment-wage sensitivity over time, we run 15-year rolling window regressions. Specifically, we estimate equation (1) in the main text for twenty sub-sample periods. The first sample period runs from 1984 to 1998 and the last sample period runs from 2003 to 2017. We then obtain a time series of twenty estimates of investment-wage sensitivity and corresponding t-statistic (Panel A of Table B.1). The pattern is clear: investment-wage sensitivity is negative and strongly significant for all 15-year sub-samples with starting dates from 1984 to 1998. The negative sensitivity (in magnitude) peaks in the samples from 1987 to 2001 and from 1988 to 2002. The coefficient then steadily decreases in magnitude (while continuing to be statistically significant) for all 15-year sub-periods with starting dates from 1988 to 1998. However, after 1998, the sensitivity is statistically and economically zero in all the recent 15-year sub-periods which start from 1999 to 2003. This pattern is displayed in Figure I in the main text. The red line plots the time-series of estimated sensitivity and the grey shaded area, the 95% confidence intervals.

We now formally test for a regime shift in the relation between corporate investment and minimum wage. We assume a single, known structural break and allow all the coefficients to change after the structural break date:

$$\frac{I_{i,s,t}}{K_{i,s,t-1}} = \alpha_i + \alpha_t + \beta_1 \, w_{i,s,t-1} + \beta_2 \, Z_{i,s,t-1} + d_t(k) \Big[\beta_3 \, w_{i,s,t-1} + \beta_4 \, Z_{i,s,t-1} \Big] + \epsilon_{i,s,t}, \tag{7}$$

where $Z_{i,s,t-1}$ is a set of firm- and state-level control variables used in equation (1), α_t is a set of year fixed effects, and α_i is a set of firm fixed effects. $d_t(k)$ equals one if t is greater than or equal to the assumed year of structural break k, and zero otherwise. We require at least five years of data for both the pre- and post-break periods in our estimation, and hence k runs from 1989 to 2013. We adopt two statistical methods to identify the best fit model: Akaike's information criterion (AIC) and Bayesian (or Schwarz's) information criterion (BIC). The lower the value of the criteria, the better the quality of the model. In Panel B of Table B.1, we plot AIC and BIC as a function of assumed break year k. Both AIC and BIC achieve their minimum value if $k = 1999$, indicating that a break date of 1999 best fits the data. These results are also consistent with the rolling window regression results.

B. Additional Robustness Tests

1. Placebo Test

We perform a placebo test to check whether a *pseudo* minimum wage increase affects investment. Specifically, we repeat the estimation of equation (1) for the pre-2000 sample using a pseudo minimum wage variable. To construct the pseudo minimum wage variable ($w_{i,s,t-1}^{Pseudo}$), we randomly assign a firm i to a state s by ensuring that the distribution of the number of firms in each state is identical to our main sample. The timing of the state-level minimum wage changes is also identical to our main sample. We define $w_{i,s,t-1}^{Pseudo}$ as the minimum wage at time t–1 in state s where firm i's *hypothetical* headquarters is located. Once all firms i in the sample are assigned in this manner, we estimate equation (1) using the simulated data and store the coefficient on $w_{i,s,t-1}^{Pseudo}$. This procedure constitutes one run of simulation and is repeated 1,000 times, and a distribution of $w_{i,s,t-1}^{Pseudo}$ coefficients is generated.

Table II reports the empirical distribution of the coefficient on $w_{i,s,t-1}^{Pseudo}$. The mean and median of this distribution are 0.002 and 0.001, respectively, and both are close to zero. This suggests that, on average, there is no investment-wage sensitivity in our simulation. We plot the empirical distribution of the coefficient on $w_{i,s,t-1}^{Pseudo}$ in Figure B.2. The green line is the estimated nonparametric kernel density of the coefficient on $w_{i,s,t-1}^{Pseudo}$ coefficient. The red vertical line indicates the coefficient on $w_{i,s,t-1}$ (–0.038) obtained from the actual data (column (2) of Panel A of Table II). The actual value of –0.038 is far below –0.017 (the first percentile of the simulated distribution), suggesting that our estimated sensitivity in the 1984-2000 period is not likely due to chance.

2. Measurement Error in Tobin's q

Tobin's q, a proxy for investment opportunities, typically contains measurement error that can generate biased estimates of the investment-wage sensitivity. To mitigate this issue, Erickson et al. (2014) propose minimum distance estimators for a traditional errors-in-variables model that includes variables with and without measurement error.

Using these high-order cumulant estimators, we assess the robustness of the investment-wage sensitivity when the proxy for investment opportunities is subject to measurement errors. In columns (1–2) of Table B.3, we report the baseline fixed effect OLS estimates from Panel A of Table II for easier comparison. Columns (3–4) display the regression coefficients estimated using the higher-order cumulant estimators. Consistent with Erickson et al. (2014), the coefficients on *Tobin's q* (*Cash Flow*) based on the cumulant estimation are larger (smaller) than those from fixed effect OLS estimation. The investment-wage sensitivity continues to remain significant for the pre-2000 period, and economic significance increases, as the magnitude of the coefficient becomes larger (–0.041 compared to –0.038). The investment-wage sensitivity for the post-2000 period is virtually zero and insignificant, similar to what was obtained from the fixed effect OLS estimation (comparing the coefficient on $w_{i,s,t-1}$ across columns (2) and (4)).

3. Strict Exogeneity Assumption Tests

In equation (1), the consistency of the fixed effects estimator crucially depends upon the strict exogeneity assumption, as noted by Wooldridge (2011). The strict exogeneity assumption asserts that $E(\epsilon_{i,s,t}|w_{i,s,\tau}, \alpha_i) = 0$ for all t and τ. Therefore, we conduct strict exogeneity assumption tests for all our estimations in Table II, Panel A. As suggested by Wooldridge (2011) and Grieser and Hadlock (2019), we include the one-period lead value of the key variable of interest ($w_{i,s,t}$) in the investment regressions. Wooldridge (2011) notes that the coefficient on this lead variable is zero under the null hypothesis of strict exogeneity. We report the results of this exercise in Table B.4. In columns (1–3), we report the baseline fixed effect OLS estimates from Panel A of Table II for easier comparison. Columns (4–6) display the regression coefficients estimated using the procedure suggested by Wooldridge (2011). The estimated coefficient on $w_{i,s,t}$ is close to zero and statistically insignificant in all specifications. We, therefore, conclude that the strict exogeneity assumption is satisfied in our empirical setting.

C. Import Competition and Constraints on Labor Cost Passthrough: Industry Leaders vs. Laggards

Gutiérrez and Philippon (2017) document theoretical and empirical evidence that industry leaders invest more (compared to industry laggards) in response to a sharp increase in import competition after China's entry into the WTO. They argue that it is optimal for the leaders to invest more either because of the increased elasticity of substitution between different firms in the same industry or their desire to re-establish their leadership. In contrast, following a massive influx of Chinese products, the laggards are likely to exit or to downsize their investment. Motivated by their findings, we examine whether the differential changes in investment-wage sensitivities following China's accession to WTO are driven by industry leaders.

Following Gutiérrez and Philippon (2017), we identify leader (laggard) firms for each SIC industry as those firms with above-median (below-median) *Tobin's q* as of 1999, two years prior to China's accession to the WTO. We also identify leaders using different criteria: firms with above-median sales or total assets as of 1999. We estimate a regression model similar to equation (6) with an indicator variable $Leader$, which equals one if the firm is an industry leader (based on *Tobin's q*, sales or total assets) and zero otherwise. We also include the interaction terms of $Leader$ with WTO_t and $w_{i,s,t-1}$ in the regression, to examine the differential effects between leaders and laggards.

Columns (1), (2), and (3) in Table B.6 present the results using *Tobin's q*, sales, total assets respectively to identify industry leaders. In all three specifications, we find that industry leaders reduce the magnitude of their investment-wage sensitivity compared to industry laggards after China's accession to WTO. These results are consistent with Gutiérrez and Philippon (2017)'s evidence that industry leaders invest more (compared to industry laggards) in response to a sharp increase in import competition after China's entry into the WTO. Our findings suggest that a firm increases its capital expenditure to survive in a competitive environment (due to both increased import competition and a labor cost shock). This is also consistent with the findings documented in Bernard et al. (2006) that capital-intensive plants are more likely to survive and grow in the wake of import competition.

Table B.1: Rolling Window Regressions and Structural Break Analysis

Panel A repeats the estimation in Panel A of Table II in the main text using a 15-year rolling window sample that starts from 1984. The total number of estimated regressions is twenty. Standard errors in parentheses are robust to heteroskedasticity and clustered by state.

Panel A. Time-Varying Effects of Minimum Wage on Investment: 15-Year Rolling Window Regressions

Sample Period		β_3	t-statistics
From	To	(Coefficient on $w_{i,s,t-1}$)	
1984	1998	-0.026**	-2.667
1985	1999	-0.033***	-3.554
1986	2000	-0.037***	-3.638
1987	2001	-0.040***	-3.813
1988	2002	-0.040***	-3.380
1989	2003	-0.031***	-3.820
1990	2004	-0.031***	-4.521
1991	2005	-0.034***	-4.906
1992	2006	-0.030***	-4.115
1993	2007	-0.027***	-3.677
1994	2008	-0.024***	-3.340
1995	2009	-0.024***	-2.881
1996	2010	-0.020**	-2.592
1997	2011	-0.014**	-2.128
1998	2012	-0.010*	-1.813
1999	2013	-0.006	-1.296
2000	2014	-0.001	-0.224
2001	2015	0.002	0.585
2002	2016	0.003	1.206
2003	2017	0.002	0.451

Table B.1: Rolling Window Regressions and Structural Break Analysis (continued)

In Panel B, we test for a regime shift in the relation between corporate investment and minimum wage. We assume a single, known structural break and allow all the coefficients to change after the structural break year:

$$\frac{I_{i,s,t}}{K_{i,s,t-1}} = \alpha_i + \alpha_t + \beta_1 w_{i,s,t-1} + \beta_2 Z_{i,s,t-1} + d_t(k)\Big[\beta_3 w_{i,s,t-1} + \beta_4 Z_{i,s,t-1}\Big] + \epsilon_{i,s,t},$$

where i, s, and t index firms, states, and years; α_i and α_t is a set of firm and year fixed effects, respectively; $Investment$ $(= \frac{I_{i,s,t}}{K_{i,s,t-1}})$ is investment rates; $w_{i,s,t-1}$ is minimum wage at time t -1 in state s where firm i's headquarters is located; $Z_{i,s,t-1}$ is a set of the firm- and state-level control variables used in equation (1). $d_t(k)$ equals one if t is greater than or equal to the assumed year of structural break k, and zero otherwise. We require at least five years of data for both periods (pre- and post-break), and hence k runs from 1989 to 2013. We calculate the Akaike's information criterion (AIC) as $-2ln(L[k]) + 2p$ where $ln(L[k])$ is the maximized log-likelihood of the model in which the assumed structural break is year k and p is the number of parameters estimated. The Bayesian (or Schwarz's) information criterion is defined as $-2ln(L[k]) + pln(N)$ where N is the sample size. The figure plots the Akaike's and Bayesian information criteria for each assumed year of a structural break.

Panel B. Analysis of Structural Breaks: Single Known Break

Table B.2: Investment Sensitivity to Minimum Wage: Placebo Test

In this table, we repeat the estimation of column (2) of Panel A, using 1,000 simulated samples where we randomly assign each firm to a particular state. The table shows the empirical distribution of the coefficient on $w^{Pseudo}_{i,s,t-1}$. The definitions of all other variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The full sample period runs from 1984 to 2000.

Regression Coefficients from Bootstrapped Sample

	Dependent Variable: $Investment_{i,s,t}$ / Sample from 1984 to 2000										
	Col. (2) Panel A Table II	Mean	p1	p5	p10	p25	p50	p75	p90	p95	p99
$w_{i,s,t-1}$	-0.038	0.002	-0.017	-0.013	-0.010	-0.004	0.001	0.007	0.012	0.015	0.022

Table B.3: Investment Sensitivity to Minimum Wage: Measurement Error in *Tobin's q*

This table presents the results of regressing corporate investment on minimum wage using the linear high-order cumulant equations (Erickson et al., 2014) to address measurement error in *Tobin's q*. The dependent variables in all columns are *Investment*, measured as capital expenditures normalized by the beginning-of-the-year capital stock (property, plant, and equipment). We measure *Cash Flow* as earnings before extraordinary items plus depreciation normalized by the beginning-of-the-year capital stock and *Tobin's q* as a ratio of the market value of assets to the book value of assets. Columns (1–2) report the fixed effect OLS regression results in columns (2–3) of Panel A, Table II in the main text. In columns (1–2), standard errors in parentheses are robust to heteroskedasticity and clustered by state. In columns (3–4), bootstrapped standard errors that are robust to within-state correlation are reported in parentheses. ρ^2 is an estimate of the R^2 of the regression, and τ_Q^2 is an index of measurement quality, which ranges from 0 to 1, for the proxy variable with standard errors in parentheses. We set the highest order of cumulants to be five.

	Dependent Variable: $Corporate\ Investment_{i,s,t}$			
	OLS-FE		EJW High-order Cumulant Estimator	
	Pre-2000	Post-2000	Pre-2000	Post-2000
	(1)	(2)	(3)	(4)
$w_{i,s,t-1}$	-.038***	.001	-.041**	.005
	(.012)	(.003)	(.018)	(.005)
$Cash\ Flow$	.098***	.029***	.026**	.003
	(.005)	(.002)	(.011)	(.004)
$Tobin's\ q$	.066***	.053***	.268***	.220***
	(.003)	(.003)	(.021)	(.040)
$GDP\ Growth$	.003**	.002**	-.0003	.001
	(.001)	(.0008)	(.002)	(.0009)
$ln(Population)$	-.145*	-.192***	-.029	-.177
	(.076)	(.049)	(.096)	(.115)
$Unemployment$	.001	.002	-.001	.009
	(.001)	(.002)	(.003)	(.006)
Firm and Year FE	Yes	Yes	Yes	Yes
# of Firm-Year Obs.	31,408	27,688	31,408	27,688
Adjusted R^2	0.135	0.122		
ρ^2			0.257	0.268
τ_Q^2			0.317***	0.322***
			(0.023)	(0.023)

Table B.4: Investment Sensitivity to Minimum Wage: Strict Exogeneity Assumption Tests

In this table, we conduct strict exogeneity assumption tests for all estimations in Panel A of Table II in the main text. As suggested by Wooldridge (2011) and Grieser and Hadlock (2019), we include the one-period lead value of the key variable of interest (i.e., $w_{i,s,t}$) in the investment regressions. In columns (1–3) of this table, we report the baseline fixed effect OLS estimates from Panel A, Table II of the main text for easier comparison. Columns (4–6) present the estimated coefficients on $w_{i,s,t}$. The dependent variable is *Investment*, measured as capital expenditures normalized by the beginning-of-the-year capital stock (property, plant, and equipment). $w_{i,s,t-1}$ is the minimum wage at time $t-1$ in state s where firm i's headquarters is located. For 1983–2014, we obtain the historical changes in minimum wages for non-farm private sector employment under state and federal laws from the Tax Policy Center. These data are sourced from the Wage and Hour Division of the U.S. Department of Labor and from the *Monthly Labor Review* by the Bureau of Labor Statistics. For 2015–2017, we hand-collect the data from the U.S. Department of Labor. Under Section 18 of the Fair Labor Standard Act, when an employee is subject to both the federal and state minimum wage laws, the employee is entitled to the higher of the two standards. We measure *Cash Flow* as earnings before extraordinary items plus depreciation normalized by the beginning-of-the-year capital stock and *Tobin's q* as a ratio of the market value of assets to the book value of assets. We also control for state-level macro-variables. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The full sample period runs from 1984 to 2017. Note that the difference in the number of observations between columns (3) and (6) (consequently columns (1) and (4)) arises from some firms in the fiscal year 2017 of which fiscal year ended in 2018.

	Dependent Variable: $Corporate\ Investment_{i,s,t}$					
	Minimum Wage and Corporate Investment			Strict Exogeneity Assumption Tests		
	Full Sample	1984 to 2000	2001 to 2017	Full Sample	1984 to 2000	2001 to 2017
	(1)	(2)	(3)	(4)	(5)	(6)
$w_{i,s,t-1}$	-.017*	-.038***	.001	-.014*	-.038***	.004
	(.009)	(.012)	(.003)	(.007)	(.009)	(.003)
$w_{i,s,t}$				-.004	-.001	-.003
				(.006)	(.010)	(.005)
$Cash\ Flow$	.043***	.098***	.029***	.043***	.098***	.029***
	(.002)	(.005)	(.002)	(.002)	(.005)	(.002)
$Tobin's\ q$	.063***	.066***	.053***	.063***	.066***	.053***
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
$GDP\ growth$	.002**	.003**	.002**	.002**	.003**	.002**
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
$ln(Population)$	-.108	-.145*	-.192***	-.109	-.145*	-.195***
	(.065)	(.076)	(.049)	(.065)	(.075)	(.050)
$Unemployment$	.00002	.001	.002	-.00003	.001	.002
	(.001)	(.001)	(.002)	(.001)	(.001)	(.002)
Firm and Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
# of Firm-Year Obs.	59,096	31,408	27,688	59,043	31,408	27,635
Adjusted R^2	.140	.135	.122	.140	.135	.122

Table B.5: Robustness Results on 1999 U.S.-China Bilateral Agreement

Panel A repeats Table IV in the main text by using 1998 (instead of 1997) as the year to check the operational status in China. Column (1) presents a difference-in-differences regression of investment on minimum wages interacted with two indicators, $China98$ and $Agreement$. $w_{i,s,t-1}$ is the minimum wage at time $t-1$ in state s where firm i's headquarters is located. $China98$ is an indicator variable set to one if a firm has at least one subsidiary in China one year prior to the U.S.-China bilateral agreement in 1999, and zero otherwise; $Agreement$ indicates the time period after the agreement (including 1999). The coefficient on the triple interaction term ($China98 \times Agreement \times w_{i,s,t-1}$) captures difference-in-differences in investment-wage sensitivity after the agreement between treated ($China98$ = 1) and control ($China98$ = 0) firms. In column (2), we introduce another group by defining $China04$ as an indicator variable set to one for firms without any subsidiary in China as of 1998 but having at least one subsidiary as of 2004 (five years after the agreement), and zero otherwise. The omitted group consists of firms that have no operations in China, that is, $China98 = China04 = 0$. In all columns, we include the same set of control variables used in Table IV of the main text. $China98$ (or $China04$) and $Agreement$ indicators are absorbed by firm and year fixed effects, respectively. The definitions of all variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The sample period runs from 1984 to 2017.

Panel A. Identifying Treated Firms as of 1998

Dependent Variable:	$Investment_{i,s,t}$	
	Two Groups	Three Groups
	(1)	(2)
$w_{i,s,t-1}$	-.018**	-.017**
	(.008)	(.008)
$Agreement \times w_{i,s,t-1}$	.006	.003
	(.006)	(.006)
$China98 \times Agreement \times w_{i,s,t-1}$	.034***	.036***
	(.007)	(.007)
$China04 \times Agreement \times w_{i,s,t-1}$		.033*
		(.016)
$China98 \times w_{i,s,t-1}$	-.019***	-.020***
	(.006)	(.006)
$China04 \times w_{i,s,t-1}$		-.019
		(.015)
$China98 \times Agreement$	0.172	0.203
	(0.165)	(0.172)
$China04 \times Agreement$		0.318
		(0.227)
Investment Sensitivity to Minimum Wage [t-stat]		
Before (baseline group)	-0.018** [-2.20]	-0.017** [-2.05]
After (baseline group)	-0.012* [-2.01]	-0.014** [-2.40]
Before ($China04$ = 1)		-0.036** [-2.19]
After ($China04$ = 1)		-0.000 [-0.04]
Before ($China98$ = 1)	-0.037*** [-3.98]	-0.037*** [-3.92]
After ($China98$ = 1)	0.003 [0.75]	0.002 [0.62]
Controls / Interaction of Controls / Firm and Year FEs	Yes	Yes
# of Firm-Year Obs.	59,096	59,096
Adjusted R^2	.157	.158

Table B.5: Robustness Results on 1999 U.S.-China Bilateral Agreement (continued)

Panel B repeats Table IV in the main text by constructing a time-varying indicator, $China_{i,t}$. We define $China_{i,t}$ as an indicator variable set to one if firm i has at least one subsidiary in China in year t, and zero otherwise. $w_{i,s,t-1}$ is the minimum wage at time $t-1$ in state s where firm i's headquarters is located; $Agreement$ indicates the time period after the agreement (including 1999). We use hand-collected information from 10-k filings to identify U.S. firms' Chinese subsidiaries in every year. Since the year 1997 is the first year of comprehensive reporting of subsidiary information, we use information as of 1997 for all years prior to 1997. The coefficient on the triple interaction term ($China \times Agreement \times w_{i,s,t-1}$) captures difference-in-differences in investment-wage sensitivity after the agreement between treated ($China$ = 1) and control ($China$ = 0) samples. We include the same set of control variables used in Table IV in the main text. $Agreement$ indicator is absorbed by year fixed effects. The definitions of all variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The sample period runs from 1984 to 2017.

Panel B. Fully Allowing for Entry into China after the Agreement

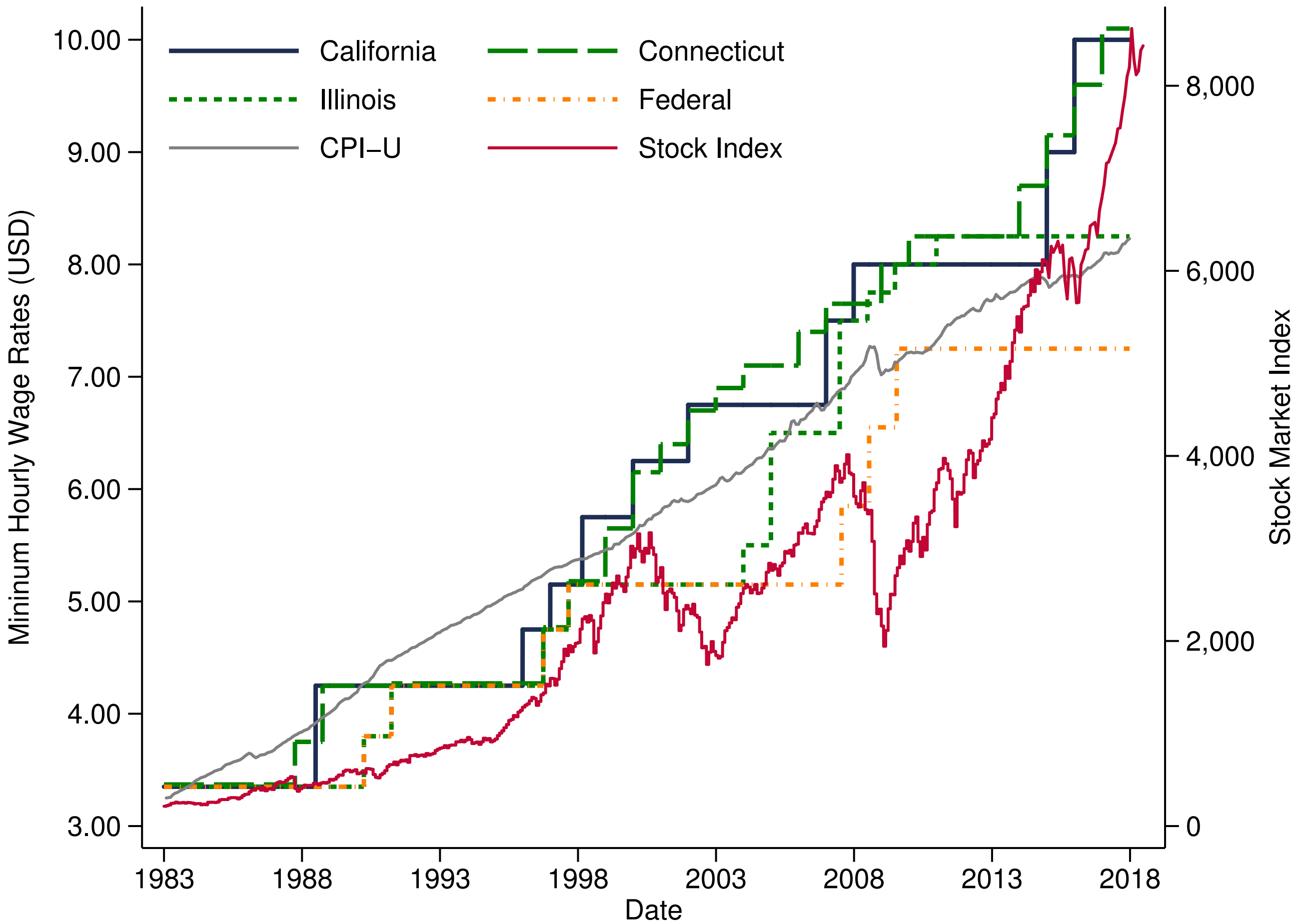
Dependent Variable:	$Investment_{i,s,t}$
	(1)
$w_{i,s,t-1}$	-.018**
	(.008)
$Agreement \times w_{i,s,t-1}$	.006
	(.006)
$China \times Agreement \times w_{i,s,t-1}$	.026***
	(.009)
$China$	0.196
	(0.160)
$China \times w_{i,s,t-1}$	-.020***
	(.007)
$China \times Agreement$	0.064
	(0.222)
Investment Sensitivity to Minimum Wage [t-stat]	
Before (baseline group)	-0.018** [-2.19]
After (baseline group)	-0.012* [-2.02]
Before ($China$ = 1)	-0.038*** [-3.61]
After ($China$ = 1)	-0.006 [-1.02]
Controls / Interaction of Controls	Yes
Firm and Year FEs	Yes
# of Firm-Year Obs.	59,096
Adjusted R^2	.158

Table B.6: Import Competition and Constraints on Labor Cost Passthrough: Industry Leaders vs. Laggards

This table presents difference-in-difference regressions of investment on minimum wages interacted with two indicators, $Leaders$ and WTO. $Leaders$ is an indicator variable set to one for firms with above-median *Tobin's q*, sales, or total assets for each two-digit SIC industry as of 1999, two years prior to China's entry to the WTO, and zero otherwise. $Leaders$ and WTO indicators are absorbed by firm and year fixed effects, respectively. *Laggards* indicates firms that are not industry leaders (i.e., $Leaders = 0$). The definitions of all variables are provided in Appendix A. Standard errors in parentheses are robust to heteroskedasticity and clustered by state. The sample period runs from 1984 to 2017.

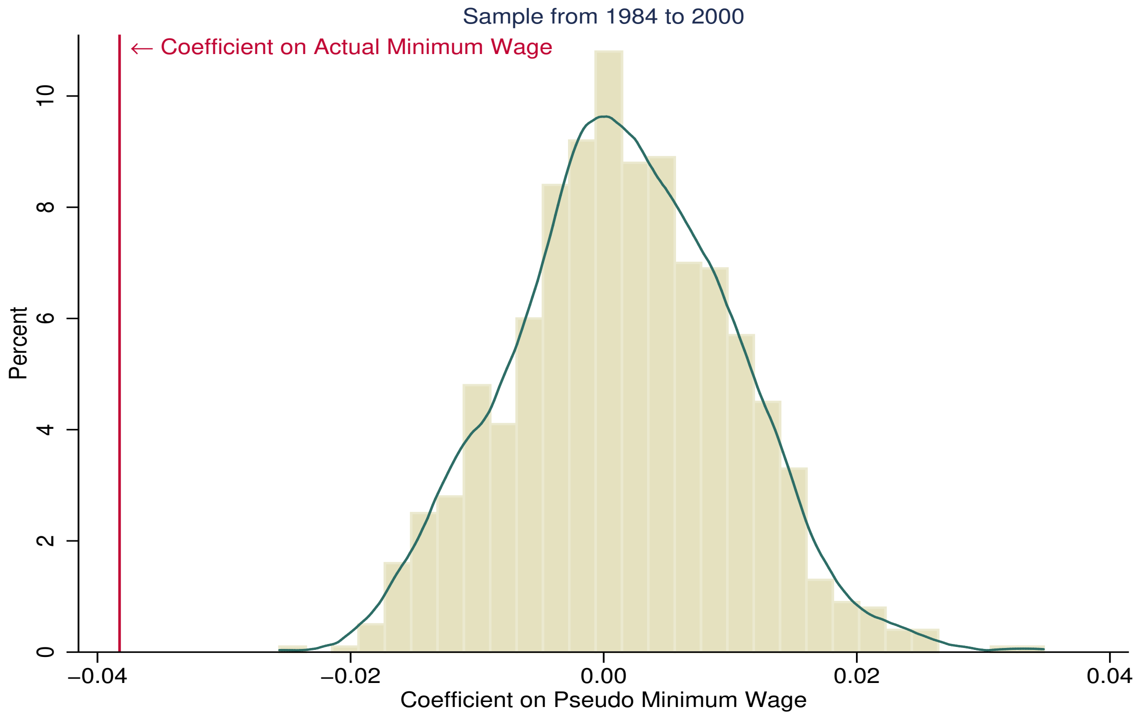
Dependent Variable →	$Investment_{i,s,t}$		
Leaders vs. Laggards Based on →	Tobin's q	Sales	Total Assets
	(1)	(2)	(3)
$w_{i,s,t-1}$	-0.016**	-0.009	-0.013
	(0.007)	(0.010)	(0.010)
$WTO \times w_{i,s,t-1}$	-0.002	-0.009	-0.006
	(0.008)	(0.009)	(0.009)
$Leader \times WTO \times w_{i,s,t-1}$	0.021***	0.036***	0.031***
	(0.005)	(0.005)	(0.007)
$Leader \times w_{i,s,t-1}$	-0.015**	-0.028***	-0.022***
	(0.006)	(0.005)	(0.005)
$Leader \times WTO$	0.057	-0.013	-0.064
	(0.121)	(0.092)	(0.126)
Investment Sensitivity to Minimum Wage [t-stat]			
Before (*Laggards*)	-0.016** [-2.27]	-0.009 [-0.95]	-0.013 [-1.29]
After (*Laggards*)	-0.017*** [-2.92]	-0.018** [-2.56]	-0.019** [-2.62]
Before (*Leaders*)	-0.031*** [-3.16]	-0.037*** [-3.58]	-0.035*** [-3.73]
After (*Leaders*)	-0.012** [-2.11]	-0.009* [-1.73]	-0.010* [-1.77]
Controls / Interaction of Controls	Yes	Yes	Yes
Firm and Year FEs	Yes	Yes	Yes
# of Firm-Year Obs.	37,484	38,844	38,829
Adjusted R^2	0.179	0.178	0.178

Figure B.1: Minimum Hourly Wage Across some U.S. States (California, Connecticut, and Illinois), 1983-2017



This figure shows the time-series of minimum hourly wage rates for three states and the federal government as an example for the time period 1983 to 2017. For 1983–2014, we obtain the historical minimum wage rates for non-farm private sector employment from the Tax Policy Center. These data are originally sourced from the Wage and Hour Division of the U.S. Department of Labor and from the *Monthly Labor Review* by the Bureau of Labor Statistics. For 2015–2017, we hand-collect the data from the U.S. Department of Labor. We also plot the Consumer Price Index for All Urban Consumers (in a grey solid line) by setting the index value in January 1983 to a wage rate of $3.25 on the left axis and the value-weighted stock market index (NYSE/AMEX/NASDAQ) on the right axis.

Figure B.2: Investment Sensitivity to Minimum Wage: Placebo Test

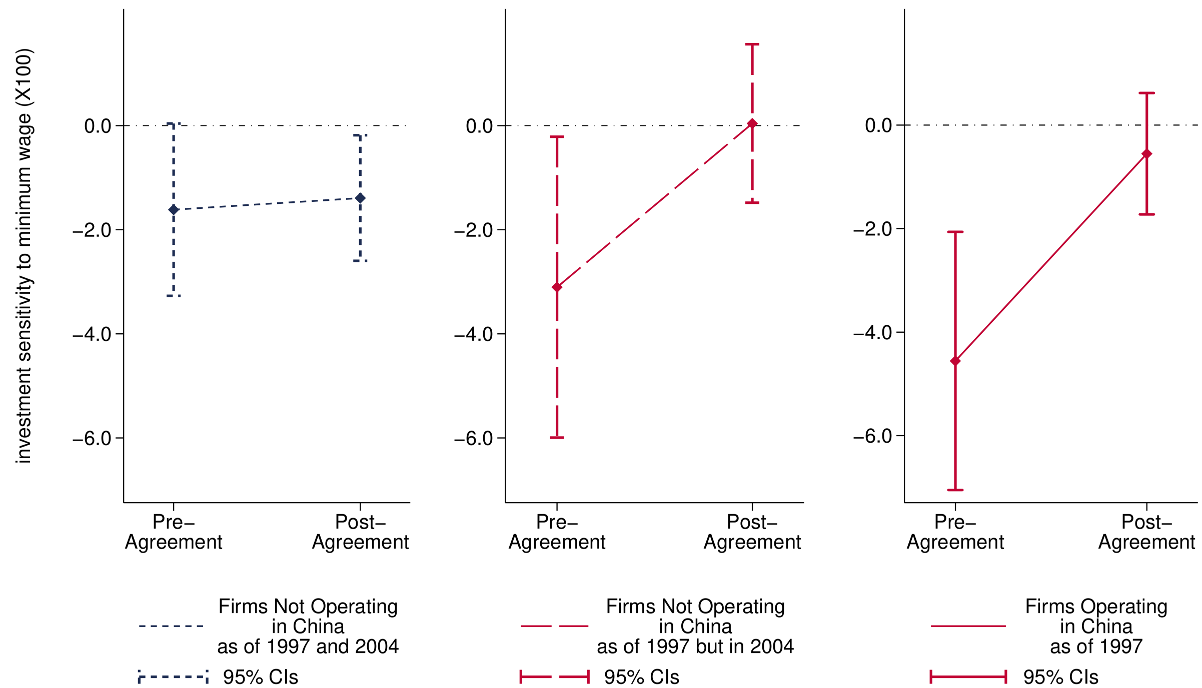


This figure is based on the following regressions:

$$\frac{I_{i,s,t}}{K_{i,s,t-1}} = \alpha_i + \alpha_t + \beta_1\, Tobin's\; q_{i,s,t-1} + \beta_2\, \frac{CF_{i,s,t}}{K_{i,s,t-1}} + \beta_3^{Pseudo}\, w_{i,s,t-1}^{Pseudo} + \beta_4\, X_{i,s,t-1}^{Pseudo} + \epsilon_{i,s,t},$$

where i, s, and t index firms, states, and years; α_i and α_t is a set of firm and year fixed effects, respectively; $Investment$ $(= \frac{I_{i,s,t}}{K_{i,s,t-1}})$ is investment rates; $Cash\ Flow = \frac{CF_{i,s,t}}{K_{i,s,t-1}}$ refers to cash flow; $Tobin's\ q_{i,s,t-1}$ is a proxy for investment opportunities; $X_{i,s,t-1}^{Pseudo}$ is a set of state-level macro-variables that are based on the pseudo state: real GDP growth rates, log of population, and unemployment rates. We construct a pseudo minimum wage variable ($w_{i,s,t-1}^{Pseudo}$), by randomly assigning each firm i to state s. Using the pseudo state, we define $w_{i,s,t-1}^{Pseudo}$ as the minimum wage rate at time $t-1$ in state s where firm i's *hypothetical* headquarters is located. We then estimate the investment regression and store the coefficient on $w_{i,s,t-1}^{Pseudo}$. This procedure is repeated 1,000 times, and a distribution of $w_{i,s,t-1}^{Pseudo}$ coefficients is generated. The figure displays the empirical distribution of the estimated coefficient on $w_{i,s,t-1}^{Pseudo}$. The green line is the estimated non-parametric kernel density. The red vertical line depicts the investment-wage sensitivity obtained from the actual data (column (2) of Panel A in Table II). The sample period runs from 1984 to 2000. Standard errors are clustered by state.

Figure B.3: 1999 U.S.-China Bilateral Agreement: Investment Sensitivity to Minimum Wage



These figures plot the changes in investment-wage sensitivity for three groups of firms before and after the 1999 U.S.-China bilateral agreement. The first group (in the left figure) consists of firms that do not operate in China as of 1997, two years prior to the agreement, and remain not operating in China as of 2004, five years after the agreement. The second group (in the middle figure) indicates firms not operating in China as of 1997 but operating in 2004. The last group (in the right figure) consists of firms operating in China as of 1997. We identify firms operating in China if firms have at least one subsidiary in China, using hand-collected information from 10-K filings on U.S. firms' Chinese subsidiaries. The dots indicate the estimated investment-wage sensitivity, and the vertical lines around these point estimates are the 95% confidence intervals. These estimates are based on column (2) of Table IV. The detailed definitions of all variables are provided in Appendix A.

Appendix C. Cost-Benefit Analysis of Minimum Wage Increase to Workforce: Counterfactual Analysis

What are the costs and benefits of raising the minimum wage? Especially during the presidential election years, this question draws substantial interest among policymakers, the press, and the public.[33] The growing interest in recent years does not only reflect the 2016 and 2020 presidential elections, but also indicates a heated debate over the recent legislative movement towards a $15 an hour minimum wage in large cities, for example, New York, Los Angeles, San Francisco, and Seattle. Many legislators and some expert economists, such as the Economic Policy Institute, mainly focus on the potential positive impact of minimum wage increases on alleviating income inequality, the ability of below-poverty-line workers to meet their basic needs, or the unemployment of low-skill workers. Experts from the Economic Policy Institute (2021) also claim that underpaid workers will spend much of their extra earnings, and this injection of wages will help stimulate the economy and spur greater business activity and job growth.[34] However, our findings in Panel A of Table II point out an important overlooked aspect of the minimum wage effect on the workforce through the investment cuts made by the firm. These investment cuts would lead to less new labor hired by the firm and lead to job losses among the workforce compared to a scenario where the minimum wage cuts were absent, and hence the firm made investments and hired new labor from the workforce.

To gauge the overall economic effects of a minimum wage increase, taking into account job losses due to investment cuts, we conduct a simple counterfactual exercise for an average firm. We compare the actual scenario where the firm faces minimum wage increases and responds by investment cuts with a hypothetical counterfactual. The counterfactual benchmark is a situation where a firm does not experience a minimum wage increase and hence would continue to make investments. As a caveat, we note that this cost-benefit analysis is a rough back-of-the-envelope calculation which is simplistic in that it does not take into account the general-equilibrium effects of the minimum wage increase on factor or output prices.

[Insert Table C.1 here.]

In Table C.1 Panel A1, we calculate the benefit of a minimum wage increase to the *existing*

[33] The Washington Post reported on Oct. 23, 2020, for example; "Biden wants to raise federal minimum wage. Trump doesn't. A look at their debate disagreement."

[34] The report, entitled 'Why the U.S. needs a $15 minimum wage', issued by the Economic Policy Institute on Jan. 26, 2021. `https://www.epi.org/publication/why-america-needs-a-15-minimum-wage/`

workforce. We assume conservatively that all hourly-paid workers would fully benefit from a minimum wage increase. For an average firm, we estimate the additional wages that the workforce earns from a minimum wage increase to be about \$2.90 million. This benefit is calculated as $\Delta w_{min} \cdot h \cdot L \cdot \rho_{hour}$ where Δw_{min} is the average annual change in minimum wage rates in our sample, h is the average annual hours actually worked per U.S. worker (obtained from *OECD Statistics* as of 2017), L is the average number of employees per firm (based on our sample as of the year 2017), and ρ_{hour} is the percent of hourly-paid workers out of total workers (obtained from the *Labor Force Statistics from the Current Population Survey* as of 2017).

In Table C.1 Panel A2, we calculate the cost of a minimum wage increase to the *future* workforce for the pre-2000 and post-2000 periods. We assume that the employment adjustment due to a change in the capital is based on the average number of workers per unit of capital stock. This cost is calculated as $\beta_3 \cdot \Delta w_{min} \cdot K \cdot (L/K) \cdot w$ where β_3 is the estimated investment-wage sensitivity in columns (2) and (3) of Panel A, Table II, K is the average lagged capital stock in million \$ (based on our sample as of the year 2017), L/K is the average number of workers per million \$ capital stock (based on our sample as of the year 2017), and w is the average annual income per U.S. worker (obtained from *OECD Statistics* as of 2017). In the earlier period (pre-2000), in which a minimum wage increase has a strong negative impact on investment, the opportunity cost of job losses resulting from the investment cut amounts to \$34.41 million. Since β_3 in the calculation is estimated with error, the 95% confidence interval of this point estimate is (−\$56.87, −\$11.86) million. Thus, for the entire workforce, it appears that the cost is much larger than the benefit for the pre-2000 period: the net cost to the workforce of a minimum wage increase at the average firm is \$31.51 million (the 95% confidence interval of (−\$53.97, −\$8.96) million). For the post-2000 period, this cost on average is negligible because there is no negative impact on investment for the average firm (the point estimate of β_3 is close to zero): the cost estimate is \$0.90 million with the 95% confidence interval of (−\$4.22, \$5.93) million. For the entire workforce, the net effect of a minimum wage increase is \$2.00 million, which is a noisy estimate as the 95% confidence interval of this estimate includes zero (the 95% confidence interval of (−\$1.32, \$8.83) million).

In Panel B of Table C.1 we summarize the results of the above counterfactual analysis by plotting the benefit, cost, and net cost (benefit) (along with the 95% confidence interval of the estimates) of a minimum wage increase to the total workforce based on calculations in Panels A1 and A2 in Table C.1. Figure (a) in Panel B is based on the estimated investment-wage sensitivity for the pre-2000 period, whereas figure (b) in Panel B is based on the estimated investment-wage sensitivity for the post-2000 period. These figures emphasize an overlooked but important negative effect of minimum wages on total employment through forgone corporate investment.

As a robustness check, we repeat our counterfactual analysis using different but reasonable parameter values (changes in the minimum wage, annual hours worked, the average number of employees, percent of hourly-paid workers, average capital stock, average labor to capital ratio, and average annual income per U.S workers) for the pre-2000 period. We obtain qualitatively similar results. The details of these calculations are reported in Table C.2.

[Insert Table C.2 here.]

The results in Table C.1 Panel A2 suggest that the net benefit/cost for the average firm due to a minimum wage increase in the post-2000 period is statistically indistinguishable from zero. However, as our results in Tables IV to V show, this average result masks important heterogeneity among firms. Firms that responded to the various economic shocks (and thus have no investment-wage sensitivity) do not impose any costs on the workforce due to minimum wage increases. They have moved their operations offshore; replaced labor with automation, and had weak unions to negotiate with. However, our analysis also identified a sizeable group of firms that did not respond to these economic shocks effectively. For the latter group of firms, the investment-wage sensitivity was still significantly negative. In Panel C of Table C.1 we summarize the counterfactual analysis for these groups of firms by plotting the benefit, cost, and net cost (benefit) (along with the 95% confidence interval of the estimates) of a minimum wage increase to the total workforce based on calculations similar to Panels A1 and A2 in Table C.1 for each of the economic shocks analyzed in Tables IV to V. We conclude from these results that there exist a sizeable group of U.S. firms as of today that produces significant negative effects of minimum wages on total employment through forgone corporate investment.

In Figure C.1 we graphically illustrate this additional source of employment reduction through the investment cut triggered by a minimum wage increase. $Demand_1$ represents the labor demand curve in the absence of the minimum wage increase policy. Equilibrium occurs when supply equals demand, which generates the competitive employment L^* and wage w^*. Once the government imposes a minimum wage (w_{min}), which is greater than w^*, firms demand less labor due to the increased cost of labor. L_{d1} will be the new level of employment that is lower than L^*. Our findings suggest that this might not be the whole story of the effect of a minimum wage increase. The investment cut resulting from the minimum wage increase will shift the demand curve to the left ($Demand_2$), which amplifies the employment reduction on top of imposing the minimum wage itself. L_{d2} will be the new level of employment that is lower than L_{d1}. Thus, $L_{d1} - L_{d2}$ is the additional unemployment due to the investment cut.

[Insert Figure C.1 here.]

In conclusion, our simple counterfactual exercise suggests that the proponents of minimum wage laws must consider the unintended negative effect of minimum wages on the workforce through corporate investment. In practice, increased minimum wages will have a number of other potential benefits over and above the increase in wages itself, such as reduced income inequality or satisfying the basic needs of low-skill workers, which are usually difficult to measure. These benefits also need to be considered in the cost-benefit analysis to obtain a more complete and accurate picture of welfare implications. Nevertheless, this article provides suggestive evidence that a minimum wage increase could ultimately dampen employment growth by stifling corporate investment.

Table C.1: Cost-Benefit Analysis of Minimum Wage Increase to Workforce: Counterfactual Analysis

These tables present a cost-benefit analysis of a minimum wage increase to the workforce using a simple counterfactual analysis for an average firm. We compare the actual scenario where the firm faces minimum wage increases and responds by cutting investment with a hypothetical counterfactual. The counterfactual benchmark is a situation where a firm does not experience a minimum wage increase and hence would continue to make investments. We note that this cost-benefit analysis is a rough back-of-the-envelope calculation that does not take into account the general-equilibrium effects of a minimum wage increase on factor or output prices. Panel A1 shows the benefit of a minimum wage increase to the existing workforce. Detailed information and data source of each parameter value is shown in *Description* column. We assume conservatively that all hourly-paid workers fully benefit from a minimum wage increase. Panel A2 calculates the opportunity cost (or benefit) of a minimum wage increase to the future workforce for the pre-2000 and post-2000 periods. We assume that the employment adjustment due to a change in the capital is based on the average number of workers per unit of capital stock.

Panel A1. Benefit of Minimum Wage Increase to Workforce (Pre- and Post-2000)

Value	Parameter / Calculation	Description
$0.15	Δw_{min}	average annual changes in minimum wage rates (full sample)
1,780	h	average annual hours actually worked per U.S. worker (*OECD.Stat* as of 2017)
18,614	L	average number of employees per firm (sample as of 2017)
58.3%	ρ_{hour}	percent of hourly-paid workers out of total workers (*Labor Force Statistics from the Current Population Survey* as of 2017)
M$2.90	$\Delta w_{min} \cdot h \cdot L \cdot \rho_{hour}$	additional wages that workforce earns from minimum wage increase

Panel A2. Cost (Benefit) of Minimum Wage Increase Due to Investment Cuts (Increases) (Pre- vs. Post-2000)

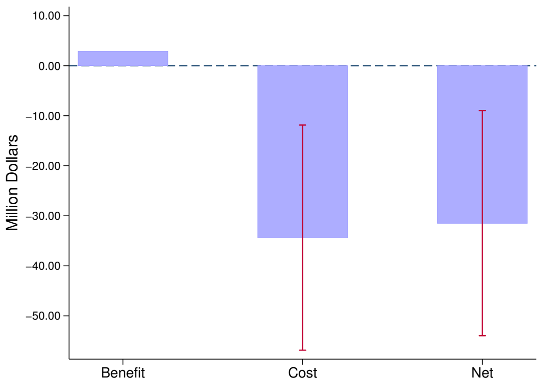
Value [95% Confidence Interval]		Parameter / Calculation	Description
Pre-2000	Post-2000		
-3.83% [-6.33, -1.32]%	0.01% [-0.47, 0.66]%	β_3	investment-wage sensitivity (columns (2) and (3) of Panel A, Table II)
M$3,409.21		K	average lagged capital stock in million $ (sample as of 2017)
30.44		L/K	average # of workers per million $ capital stock (sample as of 2017)
$57,715		w	average annual income ($) per U.S. worker (*OECD.Stat* as of 2017)
M$-34.41 M$[-56.87, -11.86]	M$0.90 M$[-4.22, 5.93]	$\beta_3 \cdot \Delta w_{min} \cdot K \cdot (L/K) \cdot w$	opportunity cost of job losses through investment cuts (or benefit of potential job openings via increased investment)

Table C.1: Cost-Benefit Analysis of Minimum Wage Increase to Workforce: Counterfactual Analysis (continued)

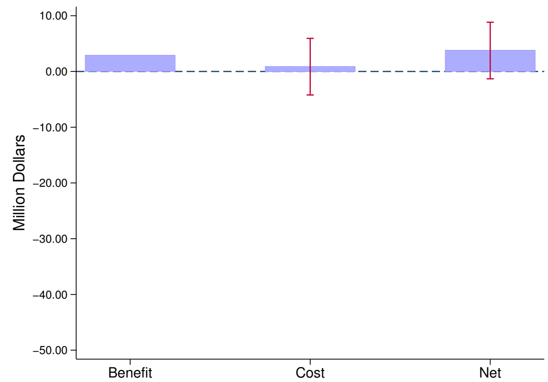
Panel B summarizes the benefit, cost, and net cost (benefit) of a minimum wage increase to the workforce based on the calculations in Panels A1 and A2. In Panel C, we repeat the same exercise to calculate the benefit, cost, and net cost (benefit) of a minimum wage increase to the workforce for the firms that are subject to a minimum wage shock based on Tables IV to V. The blue bars indicate each amount in million dollars, and the red vertical lines depict the 95% confidence intervals.

Panel B. Net Cost (Benefit) of Minimum Wage Increase to Workforce: Pre-2000 and Post-2000

(a) Pre-2000

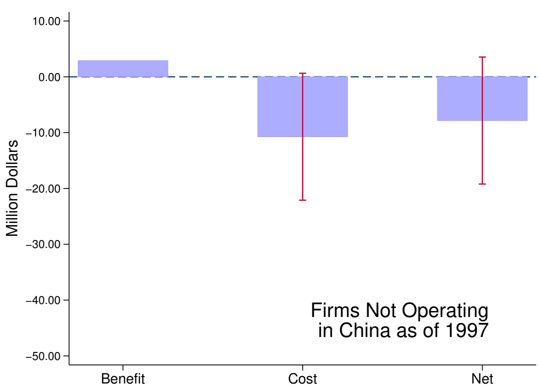


(b) Post-2000

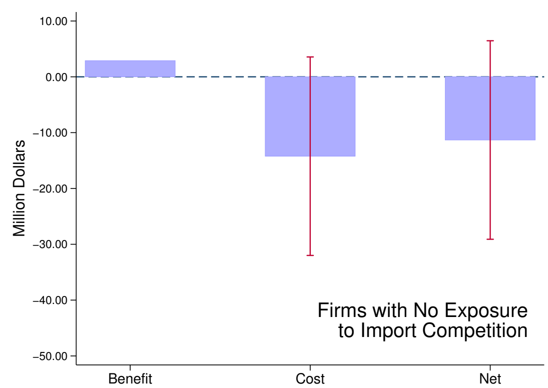


Panel C. Net Cost (Benefit) of Minimum Wage Increase to Workforce: For Firms that are Subject to a Minimum Wage Shock

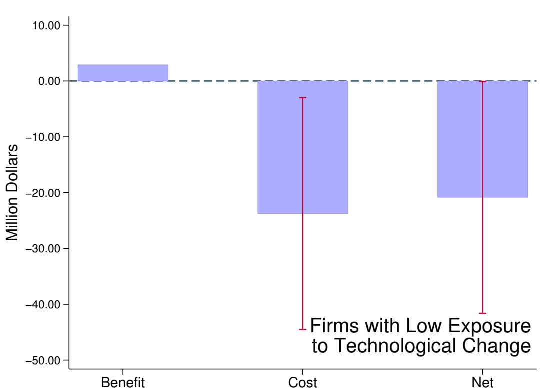
(a) 1999 U.S.-China Bilateral Agreement



(b) Chinese Import Competition



(c) Technological Change / Automation



(d) Declining Union Power

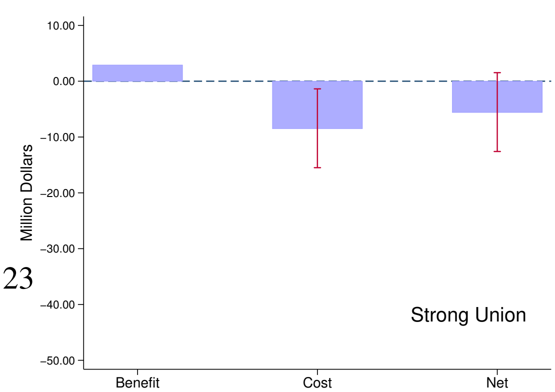


Table C.2: Cost-Benefit Analysis of Minimum Wage Increase to Workforce: Counterfactual Analysis Using Alternative Parameter Values

These tables repeat Panels A1 and A2, Table C.1 in the main text by using alternative parameter values that are used to conduct a cost-benefit analysis of a minimum wage increase to the workforce for an average firm. We compare the actual scenario where the firm faces minimum wage increases and responds by cutting investment with a hypothetical counterfactual. The counterfactual benchmark is a situation where a firm does not experience a minimum wage increase and hence would continue to make investments. We note that this cost-benefit analysis is a rough back-of-the-envelope calculation that does not take into account the general-equilibrium effects of a minimum wage increase on factor or output prices. Panel A1 shows the benefit of a minimum wage increase to the existing workforce. Detailed information and data source of each parameter value is shown in *Description* column. We assume conservatively that all hourly-paid workers fully benefit from a minimum wage increase. Panel A2 calculates the opportunity cost (or benefit) of a minimum wage increase to the future workforce for the pre-2000 and post-2000 periods. We assume that the employment adjustment due to a change in capital is based on the average number of workers per unit of capital stock.

Panel A1. Benefit of Minimum Wage Increase to Workforce (Pre- vs. Post-2000)

Value		Parameter / Calculation	Description
Pre-2000	Post-2000		
\$0.12	\$0.17	Δw_{min}	average annual changes in minimum wage rates (pre-2000 and post-2000 sample)
1,832	1,780	h	average annual hours actually worked per U.S. worker (*OECD.Stat* as of 2000 and 2017)
10,145	18,614	L	average number of employees per firm (sample as of 2000 and 2017)
59.6%	58.3%	ρ_{hour}	percent of hourly-paid workers out of total workers (*Labor Force Statistics from the Current Population Survey* as of 2002 and 2017)
M\$1.33	M\$3.28	$\Delta w_{min} \cdot h \cdot L \cdot \rho_{hour}$	additional wages that workforce earns from minimum wage increase

Panel A2. Cost (Benefit) of Minimum Wage Increase Due to Investment Cuts (Increases) (Pre- vs. Post-2000)

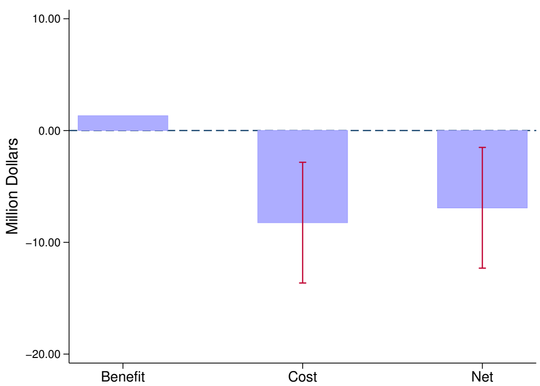
Value [95% Confidence Interval]		Parameter / Calculation	Description
Pre-2000	Post-2000		
-3.83% [-6.33, -1.32]%	0.01% [-0.47, 0.66]%	β_3	effect of unit minimum wage increase on investment rate (Columns (2) and (3) of Panel A, Table II)
M\$1,208.43	M\$3,409.21	K	average lagged capital stock in million \$ (sample as of 2000 and 2017)
28.18	30.44	L/K	average number of workers per million \$ capital stock (sample as of 2000 and 2017)
\$52,725	\$57,715	w	average annual income (\$) per U.S. worker (*OECD.Stat* as of 2000 and 2017)
M\$-8.25 M\$[-13.64, -2.84]	M\$1.02 M\$[-4.79, 6.72]	$\beta_3 \cdot \Delta w_{min} \cdot K \cdot (L/K) \cdot w$	opportunity cost of job losses through investment cuts (or benefit of potential job openings via increased investment)

Table C.2: Cost-Benefit Analysis of Minimum Wage Increase to Workforce: Counterfactual Analysis Using Alternative Parameter Values (continued)

Panel B summarizes the benefit, cost, and net benefit of the minimum wage increase to the workforce based on the calculations in Panels A1 and A2. Figure (a) plots the benefit, cost, and net benefit amounts that are based on the estimation from the pre-2000 sample period. Figure (b) plots the benefit, cost, and net benefit amounts that are based on the estimation from the post-2000 period. The blue bars indicate each amount in million dollars, and the red vertical lines depict the 95% confidence intervals.

Panel B. Net Cost (Benefit) of Minimum Wage Increase to Workforce: Using Different Parameter Values

(a) Pre-2000



(b) Post-2000

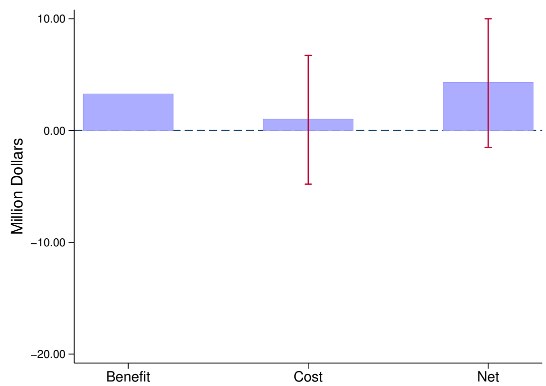
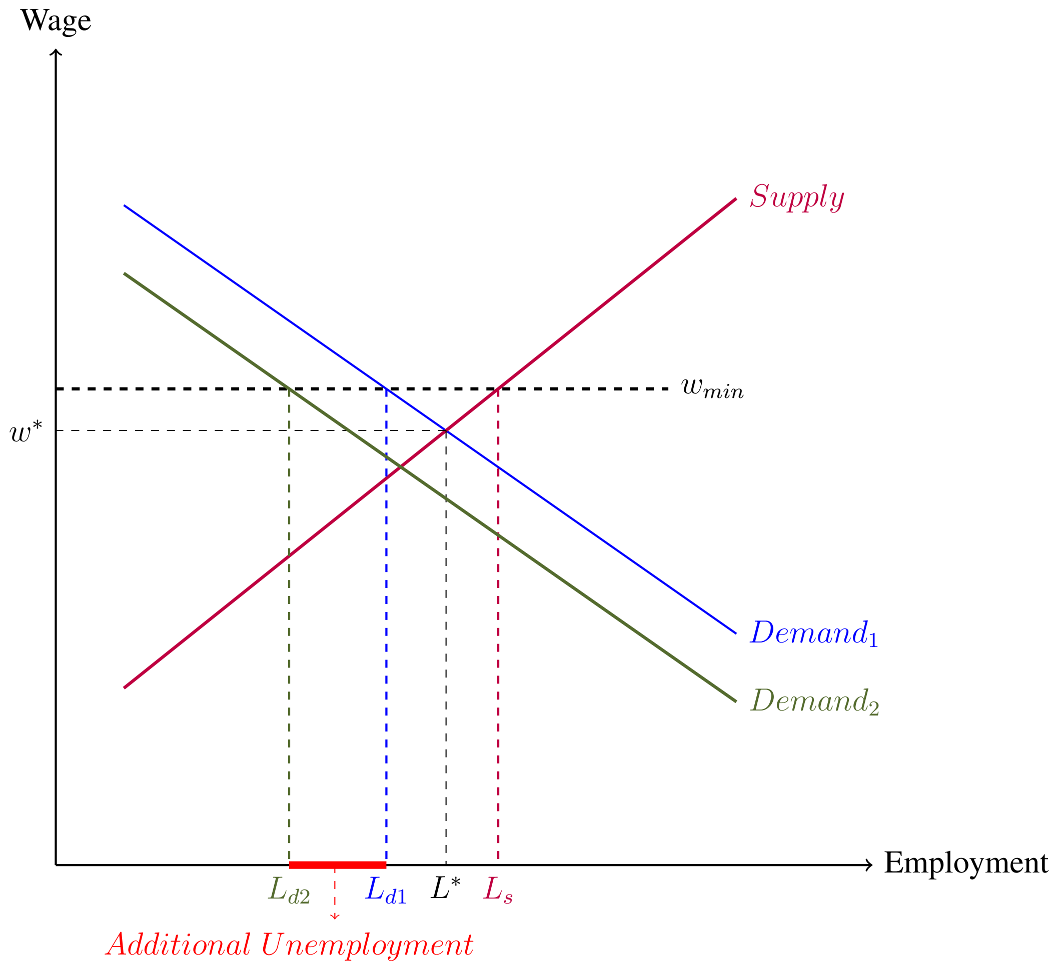


Figure C.1: Investment Cut and Unemployment



This figure illustrates an additional source of employment reduction through forgone corporate investment triggered by a minimum wage increase. $Demand_1$ represents the labor demand curve in the absence of the minimum wage increase policy. Equilibrium occurs when supply equals demand, which generates the competitive employment L^* and wage w^*. Once the government imposes a minimum wage (w_{min}), which is greater than w^*, firms demand less labor due to the increased cost of labor. L_{d1} will be the new level of employment that is lower than L^*. Our findings suggest that the investment cut resulting from the minimum wage increase will shift the demand curve to the left ($Demand_2$), which amplifies the employment reduction on top of imposing the minimum wage itself. L_{d2} will be the new level of employment that is lower than L_{d1}. Hence, $L_{d1} - L_{d2}$ is the additional unemployment due to the investment cut. As a caveat, we note that this illustration is simplistic in that it does not take into account the general-equilibrium effects of a minimum wage increase on factor or output prices.