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# Minimum Wage and Corporate Investment: Evidence from Manufacturing Firms in China

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## Abstract

This paper studies how minimum wage policies affect capital investment using the industrial census of manufacturing firms in China, where minimum wage policies vary across counties. Exploiting minimum wage policy discontinuities at county borders, we find that minimum wages increase capital investment. The investment response to minimum wages is stronger for firms that are labor-intensive, that have more room for technological improvement, and that cannot sufficiently pass on labor costs to consumers. A natural experiment based on county jurisdictional changes further assures the causal relationship.

*"The introduction of new techniques by the entrepreneurs is the more common source of increased labor productivity (caused by minimum wage legislation)."*

George J. Stigler (1946)

## I. Introduction

As a core element of labor policies and a controversial issue in the political arena, minimum wage (MW) policies have received much attention in the economics literature.[1] Recent discussions raise concern that the rise of the MW induces firms to carry out labor-displacement capital investments, which may eventually harm workers.[2] Despite the intuitive appeal of such concern, little is known about how and to what extent MWs shape firm-level policies. Further, it is firms that ultimately pick up wage bills. Understanding more about firms' responses to MW policies can provide some new perspectives on the real economic consequences of MW policies.

In this study, we empirically investigate how MWs affect firms' capital investments. Existing studies suggest that the effects of MWs on investments can be governed by two competing hypotheses. The capital-labor substitution hypothesis predicts a *positive* effect of MWs on firm investments. The increase in the price of labor relative to the price of capital implies a decrease in the demand for labor. MW hikes, by making labor more costly, induce firms to adopt more capital- and labor-saving technologies to replace MW-earning workers in routine tasks (e.g., Autor, Levy, and Murnane (2003), Acemoglu and Restrepo (2020)). This argument is consistent with anecdotal evidence. For instance, Foxconn, a major assembler of Apple's iPhone, responds to rising wages by investing in robots to reduce its reliance on labor. A Foxconn factory based in Kunshan, Jiangsu

[1] See Neumark and Wascher (2008), Card and Krueger (2015), and Clemens (2019) for reviews.
[2] See https://www.forbes.com/sites/panosmourdoukoutas/2019/09/01/15-minimum-wageapps-order-kiosks-and-robots-will-make-it-irrelevant-for-the-fast-food-industry/#62d9e81328ae

Province, had managed to automate about 60,000 jobs, according to a report by CNBC in 2016.[3]

In contrast, MW hikes can *decrease* corporate investments. By increasing labor costs, MW increases can lower the future cash flows of new investment projects, rendering the new projects less attractive. In addition, higher MWs can increase a firm's distress risk because a higher wage share makes firms less able to cope with economic recessions (Fazzari, Hubbard, Petersen, Blinder, and Poterba (1988), Favilukis, Lin, and Zhao (2019)). Taken together, lower free cash flows and higher distress risks caused by MW hikes lead firms to forgo investment projects they would otherwise have launched. As theories offer mixed guidance, the question of how MWs affect capital investment decisions is, therefore, an empirical one.

This paper attempts to answer this question using a sample of Chinese manufacturing firms. Our findings support the *capital-labor substitution hypothesis*. We find that MWs have a significant positive effect on capital investments. A 10% increase in MWs corresponds to the investment level increasing by 1.22% of total assets in the following year, amounting to 8.83% of the investment variable's sample mean. Further evidence shows that the investment response to MWs extends over one year, suggesting that the MW-induced substitution effect persists for an extended period.

Empirically identifying the causal effects of MWs on firms' investments is challenging owing to a variety of endogeneity problems. An important concern is that MW changes can be triggered by local economic trends, which simultaneously cause changes in firms' investments. To overcome this concern, the primary identification of this study relies on the unique MW policies in China, which vary across counties. Using the abrupt changes in MW policies at county borders, we examine the effects of MWs on firms that are located around these borders. The advantage of the design is that economic shocks and other spatially correlated confounding factors can penetrate county borders and thus impact firms on the other side of the borders at a similar magnitude, while MW policies do not cross such borders. Hence, cross-border neighbors can establish a reasonable

[3] https://www.cnbc.com/2016/05/22/rise-of-the-robots-60000-workers-culled-from-just-one-factory-as-chinas-struggling-electronics-hub-turns-to-artificial-intelligence.html

counterfactual investment response to MW policies. Comparing this counterfactual response with the investment response on the other side of the border allows us to account for unobservable local economic trends that apply to firms on both sides of the border. Our identification strategy resembles that used by Dube, Lester, and Reich (2010).

To implement the analysis, we construct all contiguous county pairs in China and, within each pair, retain firms located within a short distance (i.e., 50km) from the shared border.[4] This distance restriction further ensures the homogeneity of the local economic trends shared by the cross-border neighbors. We add to the county-pair specification various firm controls, macroeconomic controls, firm fixed effects, and, most importantly, county-pair-by-year fixed effects that can effectively remove the common trends, observable or unobservable, applying to firms on both sides of the borders.[5]

While the county-pair regression design can account for local economic trends around county borders, omitted variables that vary within counties across time remain a challenge for identification. Specifically, we are concerned that local governments may endogenously choose MWs according to the growth of local firms. For instance, a local government may increase MWs when it sees strong growth in local firms. If this is the case, what is captured in the regression can be local firm growth simultaneously correlating with firm investments and MWs. Since we have controlled the profitability of firms in the regression, the past growth of local firms is of less concern. But it is plausible that MWs change in response to firms' anticipated growth, which is not controlled in the regression.

We construct two measures for the expected growth of local firms. The first measure is based on the industry market-to-book ratio of the contemporaneous same-industry public firms in China. A substantial literature in finance indicates that the market-to-book ratio can reasonably reflect the market expectation of a firm's growth potentials (e.g., Fama and French (1995), Penman (1996)).

[4] We pinpoint each firm's precise location by converting the address information into geographic coordinates (longitude and latitude).

[5] We thank an anonymous referee for suggesting this fixed-effect model, which significantly improves the paper.

The second measure is the GDP growth forecasts made by the local governments. Because of the significant role of local firms in driving the local economy, we argue that the GDP forecasts can largely reflect the governments' assessment of the growth potential of local firms. Our main findings are robust to controlling the two growth measures of firms.

We further strengthen the causal relation using a natural experiment based on between-county border change events. As part of the administrative division reform since the beginning of the 2000s, some between-county borders were redrawn. Such border change entails the reassignment of the administration of some streets, villages, or towns to neighboring counties, along with the firms located in the reassigned areas. The firms (*Treat*) that changed their administrative counties in the border change events could experience different MW policies relative to the former same-county firms (*Control*) that did not change their administrative counties. The experiment concerns only treated firms experiencing an increase of MWs in the border change events.[6] This exogenous increase in MWs allows us to tease out the causal effects of MWs using a difference-in-difference regression design. We find that treated firms, on average, increased their investment around these border change events more than the control firms, further strengthening the causal relation between MWs and firms' investments.

To address the concern that border change decisions are related to local MW policies, we carry out a falsification test using border changes between counties of the same MWs. As such, the MWs of treated firms and control firms did not diverge after the border change events. The falsification test indicates that the investment of treated firms did *not* vary significantly relative to that of control firms around the border change events. The falsification test result alleviates the concern that the border change decisions correlate with local MW policies.

To provide additional evidence on the capital-labor substitutional hypothesis, we explore the heterogeneous effect of MWs along three industry dimensions and one geographic dimension. The first dimension concerns labor intensity. Since labor-intensive firms are more likely to be exposed

[6] There are border change events in which low-MW counties acquired parts of contiguous high-MW counties. However, no firm in our sample is involved in such cases

to local MWs, we expect them to invest more following MW changes. The next dimension we examine is product market power. When facing MW increases, firms are incentivized to pass increased labor costs on to their downstream consumers. However, the pass-through of increased labor costs can be constrained by their bargaining power. Hence, we postulate that firms with weak product market power are less able to shift increased labor costs and respond more to MW changes. The third dimension is industry technology development. We expect that the investment response to MW changes is concentrated in less technologically advanced industries, as these industries have more room to make use of existing and mature technologies, which are potentially easier to adopt. Lastly, we examine property rights protection. We hypothesize that firms located in areas with better property rights protection invest more in response to MW changes, as their investment can be better protected by law. Our analyses show that the four sets of cross-sectional tests generate results consistent with our expectations.

To understand what types of investment firms make, we collect information on machinery and equipment imports from the China Customs Database. Such import data can show firm technology investment because, in developing countries, one way for firms to achieve technological improvements is by importing more technologically advanced machinery and equipment (Eaton and Kortum (2001), Caselli and Wilson (2004)). We find that MW changes are accompanied by more machinery and equipment imports. The findings provide some suggestive evidence that MWs induce technological improvements.

An important implication of the substitution hypothesis is that the increase in capital investment is associated with the displacement of workers. Consistent with the predicted dynamics of the substitution of capital and labor, our analysis shows that firms faced with increased MWs adopt a higher capital-to-labor ratio and experience lower employment growth.

Using the sample of Chinese manufacturing firms is suitable to answer this question for several reasons. First, using firms' detailed address information enables us to identify their precise geographic locations. The location identification permits better control of local economic trends in the county-pair design. Second, a big challenge in studying the MW effect on firms' behaviors is that a firm can be subject to more than one MW policy if it operates in several places that adopt different MWs. Our sample can largely overcome this problem as more than 95% of the sample firms run a single plant (Ma, Tang, and Zhang (2014)). Third, manufacturing industries usually feature a relatively high investment intensity, which makes the effect of the MWs on investments more likely to be captured empirically. Using rich financial information contained in the firm data, we can precisely measure investments and other financial characteristics (e.g., total assets, profitability) for sample private firms as we do for publicly held firms.

Meanwhile, our empirical set-up also has two limitations. First, the sample is not representative of all industries in the economy and cannot be thought of as being equivalent to the U.S. Longitude Business Database in terms of the completeness of coverage. Manufacturing industries are different, in many ways, from the hotels and restaurants that are widely investigated in MW studies (e.g., Aaronson (2001), Card and Krueger (1994), and Dube et al. (2010)). The findings of this study may not be generalizable to different industries.

Second, Chinese manufacturing firms feature a relatively low level of technological penetration. Ample existing and mature technologies are available for Chinese firms to use, which, to some extent, facilitates the substitution process. The argument is supported by a cross-sectional test in Section III.G.3. This feature helps reconcile our findings with two contemporaneous U.S. studies (Gustafson and Kotter (2018), Cho (2018)), which find no evidence of MW-induced substitution. Viewed from this perspective, we consider that our findings mainly pertain to firms in developing economies, which usually have low technology adoption levels.

This study adds to the literature on the dynamics of capital and labor in the era of automation. Previous studies suggest that the availability of low-skilled labor leads to lower capital investment and impedes automation and innovation (Lewis (2011), Hornbeck and Naidu (2014)). The adoption of new technologies (e.g., the use of computers or robots) can change the demand for labor skills (Autor et al. (2003)) and reduce employment, particularly for routine manual work (Acemoglu and Restrepo (2020)). Complementing these studies, we note that MW policies can accelerate the substitution of labor for capital and increase firms' productivity.

This study also contributes to the growing literature on labor and finance. Despite the importance of the MW policy, its implication on firms' behaviors is relatively underexplored in the finance literature. Our study adds to the discussion of how labor policies are related to research topics in finance (e.g., Michelacci and Quadrini (2009), Bova and Yang (2017), Mueller, Ouimet, and Simintzi (2017), Ellul, Pagano, and Schivardi (2018)). By examining the effects of MWs on firms' investment, our analysis is related to recent studies on the effect of the labor force on investment (Besley and Burgess (2004)), merger and acquisition (Ma, Ouimet, and Simintzi (2016)), employee turnover (Aldatmaz, Ouimet, and Van Wesep (2018)), financing (e.g., Simintzi, Vig, and Volpin (2015), Lin, Schmid, and Xuan (2018), Ellul and Pagano (2019)), household finance (Aaronson, Agarwal, and French (2012), Agarwal and Qian (2014), Agarwal, Ambrose, and Diop (2019)), and corporate innovation (Chang Fu, Low, and Zhang (2015), Bradley, Kim, and Tian (2017), Liu, Mao, and Tian (2017))

## II. Empirical Design and Data

### *A. Minimum Wage Regulation in China*

MW policies in China provide us with a unique and rich institutional setting in which to study the effect of labor on corporate investment policies. MW provisions came into force in 1994 and arose from the international minimum wage-fixing obligations assumed by China upon ratifying the International Labor Organization (ILO) Convention No. 26.3, requiring China to create a minimum wage-fixing mechanism for its workforce. MW policies in China are not identical across the country. The Labor Law authorizes provincial governments to set local MWs, which can vary across counties of the same province. County governments can also negotiate local MWs with their respective provincial authorities and therefore have a substantial influence on local MW policies (Casale and Zhu (2013)). Provincial authorities are responsible for reviewing these policies and monitoring policy enforcement.

In 2004, the Ministry of Human Resources and Social Security (MOHRSS) issued new MW provisions aimed at amending the existing MW law. According to MOHRSS, the provisions improved the MW law in the following four respects. First, the protection of MW laws extended to workers employed by private entities that conduct not-for-profit activities. Second, the local MW policies started to specify the hourly MW rate for part-time workers. Third, the penalty for non-compliant enterprises increased significantly from 20–100% to 100–500% of wage shortfalls. Fourth, the local government increased the frequency of MW adjustment. We discuss MW compliance further in Section III.A.

MW data in this study come directly from the MOHRSS and the Chinese Academy of Labor and Social Security. The dataset covers the MWs of all counties in China between 1996 and 2012. MW policies in China stipulate wages monthly. To match the firms' financial data reported annually, we construct a yearly MW measure by multiplying December MWs by 12 and use the annualized MWs to predict capital investment for the following year.

[Figure 1 about here]

As shown in Figure 1, nationwide MWs, in both nominal and real terms, demonstrate rapid growth over the sample period, a pattern consistent with the rising real wage in China over the past two decades (Li, Li, Wu, and Xiong (2012)). The average MW in China was CNY2,292 per year in 1998, which increased to CNY11,329 in 2012. On average, 56.8% of counties change the MW annually, albeit at different magnitudes; 45.5% of counties increase the MW by more than 10%, while 21% of counties raise it by more than 20%. Some counties have a more aggressive MW adjustment: 6.4% of counties increase the MW by more than 30%.

[Figure 2 about here]

MWs in China present large cross-sectional and intertemporal variations. Figure 2 shows the geographical distribution of MWs across China in four selected sample years. For each chosen year, we sort counties by MWs into quintiles marked by different colors. Most counties change quintiles over the years, as illustrated by their color change, suggesting a county's MW changes substantially over time relative to those of its peers.

### *B. Firm-Level Data*

We obtain firm-level financial data for 1998–2013 from the Chinese Industrial Enterprise Dataset (CIED) released by the National Bureau of Statistics (NBS). The NBS conducts an annual survey of all industrial firms with annual sales exceeding CNY5 million until 2009 (about US$750,000 at the 2009 exchange rate) and CNY20 million thereafter (about US$3 million at the 2009 exchange rate). According to Brandt, Van Biesebroeck, and Zhang (2012), manufacturing firms surveyed in 2004 represent more than 90% of total manufacturing output and more than 70% of employment in manufacturing industries, demonstrating the comprehensive coverage of our dataset.

The 2010 CIED data are not available to us. To overcome this issue, we supplement the CIED with administrative data drawn from the Annual Tax Survey (ATS) conducted by the Ministry of Finance. The ATS reports detailed financial information for the surveyed private firms in 2007–2011. The data supplementation process requires extensive firm name-matching between the CIED and ATS to ensure we do not count firms that appear in both databases twice. In cases where two databases do not agree on the variables of commonly covered firms, we use values reported by the CIED. After assembling the minimum wage data, the firm-year data used in this study spans from 1998 through 2013; 2012 is the last year for the right-hand-side variables in the regression due to the use of one-year lagged control variables.

The survey data usually have many missing or abnormal values and need to be cleaned before usage. All financial variables used in this study are winsorized at the 1% level to eliminate outliers. We further eliminate the utilities sector (four-digit industry codes 4400–4499 and 4600–4699), which is usually under strict regulation that may limit firms' investment behavior. The detailed data filtering process is reported in Appendix 2.

Our data provides detailed firm ownership information, which allows us to categorize firms into three broad ownership types: i) private; ii) state-owned; and iii) foreign ownership. Our analysis focuses on private firms featuring relatively homogeneous characteristics to avoid any selection bias introduced by the ownership type.[7]

### C. Summary Statistics

Table 1 presents descriptive statistics for the regression variables. The dependent variables in this study are measured in a one-year lead relative to the explanatory and control variables on the right-hand side of the baseline regression. The mean of the outcome variable INVESTMENT indicates that an average firm's capital expenditure per year is 13.7% of its total assets, demonstrating that the firms make substantial capital expenditures over the sample period.

[Table 1 about here]

Panel B reports county-level variables based on 30,491 distinct county-year observations, with the necessary information available during our sample period. The analysis uses annualized MW based on December MW. The average annual MW in the sample is CNY5,501. Panel C also reports statistics for other firm-level control variables. The average sample firm has an asset size of CNY262.4 million, net fixed assets relative to total assets of 36.3%, and a moderate return on assets (ROA) of 13.3%. Detailed definitions of variables are provided in Appendix 1.

### D. Empirical Strategy

Identifying the effects of MWs on capital investment is challenging, as the MW determinants may not be orthogonal to economic fundamentals. To overcome the endogeneity problem, we take advantage of the discontinuities of MW policies at county borders and directly compare the investment behavior of firms located on borders of contiguous counties that may have different MWs. Contiguous counties act as good controls because their geographical proximity tends to minimize the heterogeneities of their economic environments while exhibiting variations in MWs. The identification of all contiguous county pairs is based on a digital map of China, obtained from

[7] State-owned firms and foreign firms are more complicated than the private firms studied in this sample. For example, investment by state-owned firms is likely to be guided and influenced by governments (Cull and Xu (2003), Huang, Li, Ma, and Xu (2017), Ru (2018)), which, in the meantime, make the MW policies. We also exclude foreign firms, as their investments are likely to be determined by their overseas parent firms.

the China Data Center of the University of Michigan. We eliminate cross-province county pairs to prevent the introduction of province-level regulatory patterns and business cycles. As a county can border several neighboring counties and thus appear in multiple county pairs, a firm-year located in such a county can repeatedly appear in the dataset; each instance is identified by a distinct county pair in our regression sample.

We further refine the sample by restricting it to border firms to ensure, to the greatest extent possible, that the economic condition is similar for firms on both sides of the border. To identify border firms, we first pinpoint the precise firm location by converting the address information provided in the CIED[8] into two-dimensional geographic coordinates (longitude and latitude) using a major geocoding service provider in China.[9] We next remove from a county pair any firm located outside a specified distance from the shared border of the county pair (within 100, 75, or 50km). The largest sample (100km) consists of 4,066,095 observations, including 268,610 distinct firms and 4,631 contiguous county pairs.

We model capital investment as a function of MWs and estimate the following regression specification:

$$(1)\quad INVESTMENT_{i,c,p,t+1} = \beta_0 + \beta_1 ln(MW)_{c,t} + \beta_2 X + \theta_{p,t} + \sigma_{k,t} + \delta_i + \varepsilon_{i,t+1},$$

where subscripts $i$, $c$, $p$, $k$ refer to a firm, a county, a contiguous county pair, and an industry, respectively. The dependent variable INVESTMENT is capital investment measured by the change in net fixed assets plus their current depreciation relative to total assets. ln(MW) denotes the log value of local MWs. $X$ is a vector of the firm-level control variables, including the log of total assets, ln(ASSETS), the ratio of tangible assets to total assets, TANGIBILITY, and return on assets, ROA. To control for the expected growth of a county's firms, we include a county-level firm growth measure based on the market-to-book ratio of the contemporaneous same-industry public

[8] The firm census data provide the detailed location of a firm's headquarters. It is possible that large firms may have multiple plants located in different locations. To mitigate this concern, in a table available upon request, we show that our results remain unaffected when firms in the upper 10% of the size distribution are dropped.
[9] The geocoding service is from Gaode Map, a subsidiary of Alibaba Group.

firms. To account for the possible effects of macroeconomic conditions, we control several economic variables, including the log of GDP per capita, ln(GDP_PER_CAPITA), the growth rate of GDP, ΔGDP, the growth rate of foreign direct investment, ΔFDI. The macroeconomic variables are obtained from the China City Statistical Yearbooks and measured at the level of the city that administers the corresponding county. We lag all explanatory and control variables by one year to reduce the risk of reverse causality.

The specification uses a rich set of fixed effects. The inclusion of county-pair-by-year fixed effects $\theta_{p,t}$ captures time-varying spatial heterogeneities operating around the shared border of two contiguous counties. The industry-by-year fixed effects $\sigma_{k,t}$ are responsible for controlling time-varying industry trends. Firm dummies $\delta_i$ restrict the source of MW identification to within-firm intertemporal variation. We cluster robust standard errors at the county-pair level.

## III. Main Evidence

This section presents the empirical results of our analysis on the effects of MW policies on capital investment. We begin with a discussion of the enforcement of MW policies in China. We then present the baseline findings, followed by a set of robustness tests and cross-sectional tests. The potential mechanisms behind our baseline findings are also discussed.

### *A. The Enforcement of MW Policies*

Understanding the enforcement of MW policies is important, as our research builds on the premise that MW adjustments represent a meaningful change in the labor costs of local firms. In other words, if capital-labor substitution is the working mechanism, the transmission of MW effects to a firm's investment would be impossible without having first affected the firm's wage cost.

Prior studies show that Chinese firms are largely compliant with MW policies. For example, during 2005–2007, which is in the middle of our sample period, Gan, Hernandez, and Ma (2016) use firm-level data and show that the percentage of firms with average wages below local MWs was between 5% and 7%. In a more recent survey, Ye, Gindling, and Li (2015) collect the salary information of 521,501 employees in Chinese manufacturing sectors in 2009. They conclude that only 3.5% of surveyed workers earn less than local MWs.

Next, we calculate the MW compliance rate using the Urban Household Income Survey released by the National Bureau of Statistics. The survey was carried out yearly in 18 provinces from 2002–2006. We compare the individual income information drawn from the survey with the local MWs. The comparison indicates that 7% of surveyed individuals reported incomes below the local MWs, consistent with the non-compliance rate shown in prior studies.

[Table 2 about here]

Last, we test whether local MWs correlate systematically with firms' total wage expenditure. The regression model adopts the county-pair design resembling the specification in equation (1). In Table 2, the statistically significant coefficient of ln(MW) in the 50km sample suggests that a 10% increase in MWs is associated with an increase in total wages of 1.37%. The strong positive correlation between MWs and firms' wage bills suggests that MW policies are indeed binding, consistent with evidence found in existing studies.

### *B. Baseline Results*

Table 3, Panel A reports OLS regression results using the specification in equation (1) in three samples that respectively restrict to firms within 100, 75, and 50km of the borders. We find that MWs represent a positive and statistically significant explanatory variable for corporate investments, and the results are robust across the board. The economic magnitude of MWs is also sizable. For instance, in the 100km sample, the point estimate of ln(MW) in Column 1 indicates that a 10% increase in MW implies an increase in *Investment* of 1.22% of its total assets, amounting to 8.83% of the average *Investment* in the sample. In Columns 2 and 3, where we respectively use 75km and 50km samples, the coefficient estimate of ln(MW) remains quantitatively and qualitatively similar. The baseline regression results are consistent with the capital-labor substitution hypothesis, which suggest that firms increase their capital investment to offset rising labor costs induced by the MW increase.

[Table 3 about here]

Next, we explore the investment response to MWs in the long term. Using the average investment in year $t + 2$ and year $t + 3$ as the dependent variable, we find that the positive association between MWs and the investment remains, as shown in Panel B of Table 3. The weaker economic magnitude relative to that in the one-year result suggests the investment response to MWs decays over time. Recognizing that the MW variable can be serially correlated, we further pursue a change regression specification that uses the change in MWs as the independent variable. We offer more discussion on the change specification in Section III.D.

### *C. Endogeneity Issues*

In this section, we carry out a battery of robustness checks to further reinforce the causal relation between MW policies and firms' investments. The baseline specification is useful in controlling the local economic trend that is common to firms on both sides of the border. Remaining confounding variables that vary within counties across time still exist. We are concerned that local governments may endogenously change MWs in response to local firms' evolution. For example, local governments may increase MWs when they see prosperous local firms, hoping the workers can capture some of the gains. Despite the regression controlling firms' profitability, we recognize that the profitability measure can capture firms' growth only in the past, whereas the MW policies may well reflect the expected growth of local firms in the future. We use the following tests to mitigate this concern.

#### *1. Confounding Variables*

We use two measures for expected growth that separately capture the growth anticipation of the financial market and that of local governments. The first one, as we briefly discussed in section II.D, is a county's firm growth potential calculated using the information on publicly listed firms. This growth measure takes advantage of the forward-looking nature of the capital market. We include this measure as a key control variable for all regressions.

The second measure aims to capture governments' expectations of local business growth. Controlling governments' expectations is important in capturing the element of economic outlook embedded in MWs because they are responsible for making local MW policies. To achieve this goal, we collect the GDP growth forecast made by local governments. Since local firms play a significant role in driving the local economy, it is reasonable to expect that the GDP growth forecast largely reflects the governments' expectations of local business growth. The GDP forecast data come from the annual government work reports. In these reports, Chinese local governments usually release the GDP forecast for the coming year. Since GDP forecast data are not in a machine-readable format, we collect the data manually from the work reports issued by local governments.[10] We control the GDP forecasts made by local governments in the baseline regression. The results in Panel A of Table 4 indicate that our main findings still hold.

We also test the relation between MWs and the growth of a county's firms. We separately proxy the growth of a county's firms by average lagged ROA and the two expected growth measures discussed above. The regression results are reported in Columns 1–4 of Table A1 in Appendix 3. The evidence suggests that MWs are not significantly associated with any measure of local firm growth. We also test the relation between MWs and local firm investment. The regression results in Columns 5–6 indicate that local firm investments do not appear to significantly covary with the changes in MWs. Our results are consistent with the findings of Hau, Huang, and Wang (2020) that corporate policies have limited explanatory power in predicting the timing of MW changes in China.

Unobservable industry trends can be an important source of endogeneities. The literature of development economics suggests that some Chinese manufacturing industries present a strong pattern of industrial clustering such that same-industry firms usually locate close to one another (Sonobe and Otsuka (2006), Long and Zhang (2011)). It is, therefore, plausible that a county adjusts its MW according to the trends of industries that concentrate heavily within it. If so, our

[10] About 80% of the observations in the sample can be matched to a local GDP forecast. GDP forecast data have missing values in the early years, as some local governments did not mention specific forecast numbers. We use city-level GDP forecast data, as county-level data are extremely limited.

results may pick up industry trends. To rule out that possibility, we include industry-by-year fixed effects for all major regressions.

[Table 4 about here]

We next deal with city trends. In China, a city is a higher administrative authority than a county. In the county-pair design, if two counties in a pair come from different cities, the difference in their MW policies may coincide with other city-level policies that correlate with firm investments. To mitigate this concern, we eliminate county pairs straddling two cities and rerun the regression in county pairs where both counties were administered by the same city. The regression results tabulated in Panel B of Table 4 indicate that the effects of MWs are qualitatively similar to the baseline results.

To ensure that our result is not affected by the differential MW compliance rates across areas, we exclude from the sample the bottom quartile of cities with the worst MW compliance rate according to the Urban Household Income Survey. We cannot calculate a county-level compliance rate because there is no county information for the surveyed subjects. For this reason, our calculation is at the city level, which is a higher level of administration than for a county. As shown in Table A2 of Appendix 3, the baseline results still hold in the refined sample.

### *2. Natural Experiment*

To strengthen the probability of a causal relation between MWs and firms' investments, we conduct a natural experiment using the redrawing of the shared borders between contiguous counties. Starting in the early 2000s, some between-county borders were redrawn such that the administration of some villages, streets, or towns was transferred to neighboring counties; this was also the case for firms located in the transferred areas. Thus, firms transferred to a new county could have a different MW from the former same-county firms that did not change administrative counties. The experiment concerns only treated firms experiencing an increase of MWs in the border change events. As a result, the exogenous changes in MWs due to the border changes give us a chance to identify the causal effects of MWs, as the MW changes used in the experiment have less to do with the local governments' assessment of the growth prospect of local firms.

We draw all border change information in 1998–2013 from the Chinese Ministry of Civil Affairs.[11] Not all the border change events involve firms appearing in our sample. We carefully use firms' location information to identify affected firms in the border change events. This process yields 45 events that include at least one sample firm. Some 20 out of 45 events involved border change events transferring firms from a low-MW county to a high-MW one. The remaining 25 events occurred between counties that adopted the same MWs.[12] Our experiment focuses on the 20 events, where the outgoing firms (*Treat*) experienced an increase in MWs relative to their former same-county peers (*Control*).

To balance the number of treated firms and control firms in the sample, we perform a Mahalanobis matching that maps each treated firm to up to five same-event control firms in the year before the border change events based on the firm-level characteristic variables used in the baseline specification. The control firms should also operate in the same industry as the corresponding treated firms. The resulting sample consists of 13 events involving 103 treated firms and 354 control firms.

We apply a difference-in-difference regression design with a six-year window spanning from two years before the border change to four years after. The difference-in-difference regression is specified as follows:

$$(2) \quad INVESTMENT_{i,t} = \beta_0 + \beta_1 TREAT_i \times POST_t + \beta_2 X + \sigma_{k,t} + \delta_i + \varepsilon_{i,t},$$

where TREAT is a dummy variable equal to one if the firm *i* is a treated firm, and otherwise zero. POST is a time dummy set to one in the years following the border change events. The control variables are the same as those used in the baseline specification.

[11] The information can be found at http://xzqh.mca.gov.cn/description?dcpid=1

[12] We note that there is only one event in which an area was transferred from a high-MW to a low-MW county. But this event does not involve any sample firms, and thus cannot be used in our experiment.

Table 5 reports the regression results of the experiment. Panel A presents the summary statistics of firm-years used in the analysis. Panel B diagnoses the quality of the Mahalanobis matching by comparing treated and control firms in the matched sample in the year leading up to the border change events. Across all matching variables, the difference in their mean is statistically indifferent from zero, which assures the quality of the matching procedure. Panel C tabulates regression results. Columns 1 and 2 present the regression results for the specification in equation (2). In Column 1, the significantly positive coefficient estimate of the interaction term $\text{TREAT}_i \times \text{POST}_t$ suggests that, on average, the treated firms experienced more investments than control firms by 9.8 percentage points.

[Table 5 about here]

To assess the dynamic effect of MWs, we estimate the following regression:

$$(3) \quad INVESTMENT_{i,t} = \beta_0 + \sum_{m=-2}^{m=4} \beta_1^m TREAT_i \times TIME_m + \beta_2 X + \sigma_{k,t} + \delta_i + \varepsilon_{i,t}.$$

The dummy variable $\text{TIME}_m$ marks the *m*th year relative to the event year of border changes. The diagram in Figure 3 plots the estimated values of $\beta_1^m$, with the vertical segment below and above the dots denoting 95% confidence intervals. The coefficient $\beta_1^0$ for the treatment effect in the event years ($\text{TREAT}_i \times \text{TIME}_0$) is subsumed by the fixed effects.

[Figure 3 about here]

It is plausible that the observed county border changes may result from political and economic considerations. Our experiment can be contaminated to the extent that these economic and political confounding factors systematically correlate with MW policymaking as well as firm investments. We search the literature and government documents for the rationale underlying the border change decision but fail to find an answer, partly due to the opaque policymaking process in China.

To mitigate effects arising from these confounding omitted variables, we perform a placebo test using border change events where two counties adopted the same MW policies. In other words, the firms assigned to a new county due to the border changes experienced *no* change in MW policies relative to the former same-county firms that did not change their administrative counties. We argue that if any unobservable factor affecting the design of border changes coincide with firms' investment, we should obtain coefficients akin to those obtained in the primary experiment test. We repeat the specification in equation (2) using the placebo test sample and tabulate regression results in Columns 3 and 4, Panel C of Table 5. The coefficients of the interaction term are statistically indistinguishable from zero in both columns, suggesting that border change decisions are less likely to be driven by local MW policies.

### *D. Change Regression Specification*

We investigate the investment response to MWs using a change specification that regresses investment on the changes in MWs. The change specification can account for the serial correlation of the MW variable such that the regression results can better reflect the horizon over which the effect of MWs occurs.

The analysis first uses the change specification to explore the effect of the changes in MWs on one-year lead investments. The specification is similar to that in equation (1) except that the key independent variable is replaced with changes in the log of MWs, $\Delta\ln(MW)$. In Panel A of Table 6, the regression results indicate that the MW changes feature a significantly positive explanatory variable for the investment in the next year, consistent with the baseline regression results.

[Table 6 about here]

Next, we apply the change specification to examine the long-run effect of the MWs. Specifically, the change specification regresses the average firm's investment in year $t + 2$ and $t + 3$ on changes in MWs from year $t - 1$ to $t$. In Panel B of Table 6, the results indicate that the positive effects of the changes in MWs on investments extend beyond one year, consistent with the results using the level specification. To provide further evidence on the longevity of the MW effect, we plot the effect of MW changes on investments measured in different years relative to MW changes. As shown in Figure A1 of Appendix 3, the investment response still exists five years after MW changes, consistent with the long-lasting effect of MWs.

### *E. Downward Variations in Minimum Wages*

We note that a few Chinese counties in the sample experienced a downward variation in MWs. We isolate these MW-decreasing counties and investigate how firms' investment activities respond to the downward change in MWs.

We perform an event study using a sample of counties experiencing a downward variation in minimum wages. We only include minimum-wage-decreasing counties that had steady minimum wages for at least two years before the wage-decreasing year and maintained the decreased minimum wages for at least two years afterward. The event study uses the county-pair design that pairs each minimum-wage-decreasing county with a contemporaneous contiguous county that did not reduce the minimum wage during the event window.

The regression sample consists of 74 county pairs involving 21 treated counties and 11,249 firm-years. We label (*Treat*) firms situated in the MW-decreasing counties and (*Control*) firms in the paired counties. On average, the MW-decreasing counties experienced a downward variation in MWs of 8.5%. Relative to those of the neighboring control counties, the changes in MWs of the treated counties were -13%. The regression specification is specified as:

$$(4) \quad INVESTMENT_{i,t} = \beta_0 + \beta_1 TREAT_i \times POST_t + \beta_2 X + \theta_p + \sigma_{k,t} + \delta_i + \varepsilon_{i,t},$$

where the firm and macroeconomic control variables in the vector $X$ are the same as those in equation (1). Table A3 of Appendix 3 tabulates the regression results. We find that the changes in investment for the treated firms were, on average, lower than those for the control firms by 9.2–10.3% of total assets. The results suggest that firms slow down capital investments when the local MWs decrease, consistent with the capital-labor substitution hypothesis.

### *F. Labor Migration and Firm Relocation*

The identification of the county-pair design may be at risk of being contaminated if firms and workers mobilize in response to MW changes. We argue that MW changes are not likely to cause manufacturing firms to relocate as the high relocation cost can make it less economically viable. For example, other than standard relocation expenditure, the relocation may also incur a high bureaucracy cost in acquiring new land parcels and satisfying a series of regulatory requirements (in areas such as environmental assessment, fire control, taxation, etc.). Such bureaucratic friction can be particularly costly in relation to a local government with which firms have never previously engaged. Besides, the MW of the low-MW county may outgrow that of the high-MW county in the future, which may further prevent firms from relocating.

While MW workers may flow from a low-MW county to its high-MW neighbor in a pair, we argue that MW-induced migration tends to downward bias our findings, if it exists. This is because the identification of the MW effect relies on the MW-induced difference in the wage levels between two counties in a pair. The migration of workers potentially drives up (down) the wage level in the low-MW (high-MW) county due to the decreased (increased) labor supply, which narrows the difference in the wage level within a county pair and thus weakens the treatment effect of the MW in the design. Further, if the difference in MWs between two contiguous counties drives the labor migration between them, we should be able to mitigate the migration effect by controlling the MWs of both counties in the pair. In the untabulated result, we find that the investment response to MWs remains robust after controlling the MWs of the contiguous counties in the county pair.

### *G. Heterogeneities*

We hypothesize that firms increase capital investment in response to MW hikes. However, the degree of the investment response to a unit change in MWs can vary by firms or industries. In this section, to provide more evidence on the capital-labor substitution story, we examine four dimensions of cross-sectional heterogeneity where the substitution effect is likely greater.

#### *1. Minimum Wage Sensitivity*

The intensity of MW worker usage is the first dimension we investigate. Since firms hiring more

MW workers tend to be hit harder by MW hikes, we expect that the MW effect on investment mostly concentrates in MW-labor-intensive firms. Due to the lack of payroll information on individual workers, we use two wage-based and one labor-skill-based variable to estimate a firm's or an industry's reliance on MW workers from different perspectives. The first is the firm's average wage defined as its total wage expenditure divided by the number of employees. A higher average wage implies that the firm is less exposed to MW shocks. We average this measure within a firm across the sample period and split the sample based on the within-firm average. In Columns 1 and 2, Panel A of Table 7, we find that the investment response is greater in the group of the low firm average wage.

The second is a measure of industry labor intensity, which is defined as the average ratio of wage expenditure to total assets across the industry's firms. A higher ratio means that an industry has a higher labor intensity. We average the labor intensity measure within an industry across the sample period and assign sample industries to high and low groups of labor intensity based on the median value of the within-industry average. In Columns 3 and 4, the regression results indicate that the effect of MWs appears to be stronger in the sample of the high labor-intensity group.

[Table 7 about here]

We also construct a third measure based on the labor skill level following the idea that firms hiring more unskilled workers are more sensitive to MW changes. We use a 2004 survey on employee education information across industries. We define an industry as more MW sensitive if it hires a higher proportion of workers having no college or higher qualifications in 2004. Columns 5 and 6 indicate that the effects of MWs on firms' investments largely concentrate in industries hiring more low-skilled workers.

Following Cleary (1999), we test the difference in MW coefficients in each subsample analysis and report the *p*-value in the bottom of the table. The results show that the MW coefficients between the two subsamples are significantly different at the 1% level for the three sets of tests discussed above. Collectively, the cross-sectional analysis results are consistent with our expectation that the investment response for MW hikes is stronger for firms more exposed to MW shocks, consistent with the capital-labor substitution hypothesis.

### *2. Product Market Power*

We then examine the role of product market power in shaping the firms' response to MW hikes. Existing studies document that firms seek to shift rising labor costs to consumers by increasing product prices. For example, Aaronson (2001) shows that local restaurant prices increase following MW hikes. The extent to which firms can shift MW increases may vary significantly. We argue that product market power is one such determining variable. Firms with stronger product market power possess greater bargaining powers with respect to their downstream clients and thus are more able to pass on rising labor costs, which weakens the incentive to displace their MW workers. As such, we expect that the investment response to MW changes is weaker when a firm's product market power is stronger.

We proxy an industry's market power by the Lerner Index (or the price-cost margin). A higher Lerner Index means that an industry has a higher price-cost margin and thus greater product market power. The Lerner Index of a three-digit industry $k$ is defined as:

$$LI_k = \frac{1}{N_k} \sum_{i \in k} \frac{OPERATING\_PROFIT_i - FINANCIAL\_COST_i}{SALE_i}, \quad (5)$$

where $N_k$ is the number of firms in the industry $k$.

We average the measure for product market power within an industry across the sample period and sort the sample industries into subsamples by the within-industry average market power. We tabulate the results in Panel B of Table 7. In Columns 1 and 2, the coefficients of ln(MW) are significantly larger in the subsample of low product market power than those in the subsample of high product market power, consistent with our expectation that a higher product market power deters firms from automating workers in the wake of MW hikes.

### *3. Technology Development*

The third dimension of MW heterogeneity is technological development. Less technologically advanced firms have more room to make use of existing and mature technologies. This means the potential difficulty and cost of carrying out labor automation is lower for less technologically advanced firms than for more technologically advanced firms. As such, we expect that MW-induced substitution between capital and labor is more likely to arise in those less technologically advanced firms, where marginal technological improvements can be made at lower costs.

We measure technological advancement using patenting information at the industry level. Focusing on the industry-level innovation measure is more informative in that it mitigates the influence of firm-level noises. A Chinese industry is regarded as more technologically advanced if its technology gap with its U.S. counterpart is smaller, and vice versa. Benchmarking against U.S. industries makes sense because, in general, U.S. firms possess the most advanced technologies of their respective industries. Empirically, we measure the technology gap by the difference in total patent output through the sample period between a Chinese industry and its U.S. counterpart scaled by the total number of patents filed in a US industry. Patent data come from Patent Statistical Database (PATSTAT), which includes all patents filed around the world.

In Columns 3 and 4 of Panel B, we tabulate the regression results in subsamples split based on the median of the technology advancement measure. The results show that the MW coefficient is greater in the subsample of less technologically developed industries (those with a larger technology gap with their U.S. counterparts), consistent with our expectation that the investment response to MW hikes is more likely to take place in industries where the marginal costs for technological improvement are lower. In addition, the finding helps reconcile our paper with two U.S. studies (Gustafson and Kotter (2018), Cho (2018)). The higher degree of technology penetration in the U.S. can be a contributing factor as to why no substitution effect is found in the sample of U.S. firms.

***4. Property Rights Protection***

Finally, we examine how property rights protection affects firms' investment response to MW changes. Prior studies show that firms' investment decisions critically depend on their ability to legitimately reap the proceeds of investments (Johnson, McMillan, and Woodruff (2002)). In contrast, in a dysfunctional legal environment, governments—or more accurately politicians—

may extract rents from a firm's investment proceeds. This rent-seeking behavior, labeled the "grabbing hand" by Shleifer and Vishny (2002), can severely depress firms' investment in an economy. We extend this idea to our analysis by examining whether a better legal environment for property rights protection facilitates capital investment induced by MW hikes. We predict that firms are less likely to invest in labor automation if their incentives to invest have already been discouraged by poor property rights protection.

We use the Investment Climate Survey conducted by the World Bank in 2005 to measure the degree of regional property rights protection. The survey collects a range of firm-specific information from a large sample of representative firms. According to Cull, Li, Sun, and Xu (2015), the survey covers 12,400 manufacturing firms located in 120 cities of all Chinese provinces except Tibet. For this study, we use firms' responses to question J7, which asks firms' confidence for the security of property rights when commercial disputes arise. We construct a city-level measure of property rights protection by averaging the firms' response to question J7 across surveyed private firms of the same city.

We report the regression result of the cross-sectional tests by the city property rights protection index in Columns 5 and 6 of Panel B. Consistent with our hypothesis, the coefficient of ln(MW) is statistically and economically significant in the high-protection-index subsample, but its statistical significance diminishes in the low-index subsample. Our findings show that the legal environment for property rights protection plays an important role in the interplay of labor market policies and capital investments.

## IV. Other Firm-Level Responses

In this section, we provide additional evidence to support the capital-labor substitution hypothesis by examining how MW changes affect the capital-to-labor ratio, employment, and machinery and equipment imports.

### *A. Capital-Labor Substitution*

Our analysis so far indicates the positive effects of MWs on corporate investment. But little evidence is provided about the dynamics of substitution of capital and labor. The subsequent analysis supplies more evidence from the perspectives of firm capital-to-labor ratio and employment outcome.

We follow Acemoglu and Finkelstein (2008) and define the capital-to-labor ratio as the ratio of tangible capital stock to employment. We estimate the effects of MWs on capital-to-labor ratio using the following regression specification:

$$(6)\quad In(K/L)_{i,t+1} = \beta_0 + \beta_1 ln\ (MW)_{c,t} + \beta_2 X + \theta_{p,t} + \sigma_{k,t} + \delta_i + \varepsilon_{i,t+1},$$

where ln(K/L) is the log of the capital-to-labor ratio. Other parts of the model specification follow the convention of equation (1).

[Table 8 about here]

We report the regression results in Panel A of Table 8. The coefficient estimates of ln(MW) are statistically significant at the 1% level across the board. The estimated coefficient of ln(MW) in the 50km sample indicates that a 10% increase in MWs leads to a 1.75% increase in the capital stock relative to the labor force.

Next, we estimate the employment outcome of MW hikes using the employment growth from $t$ to $t + 1$ as the outcome variable. In Panel B of Table 8, we find that MWs significantly decrease firms' employment growth. In the 50km sample, the estimated coefficient of ln(MW) implies that a 10% increase in MWs decreases employment growth by 0.44 percentage points, which corresponds to 9.36% of average employment growth in the sample. Our findings are consistent with the work of Burkhauser, Couch, and Wittenburg (2000), which documents negative MW elasticities of employment. Collectively, the findings on capital-to-labor ratio and employment further support the capital-labor substitution hypothesis.

### *B. Machinery and Equipment Imports*

We examine firms' importing activities in relation to machinery and equipment. The information on such imports provides not only more granular evidence on what types of investment firms make but also some suggestive evidence on the source of technological improvement. Eaton and Kortum (2001) and Caselli and Wilson (2004) indicate that most of the world's capital is produced in a small number of R&D-intensive countries, while the rest of the world generally imports its equipment. Hence, the evidence on machinery and equipment imports helps shed light on a source of labor-saving technology for the sample firms.

[Table 9 about here]

To test this idea, we collect data on machinery and equipment imports from the China Customs Database, which contains detailed import and export information on firms, including item specifications, transaction value, and quantity. The database uses Harmonized Commodity Codes to identify imported items and codes machinery products as 84 or 85. We match the identified imported machinery items manually with the CIED firms and calculate the total value of imported machinery and equipment for each firm-year. We estimate the following specification:

$$(7)\quad MACHINE_{i,t+1} = \beta_0 + \beta_1 ln\,(MW)_{c,t} + \beta_2 X + \theta_{p,t} + \sigma_{k,t} + \delta_i + \varepsilon_{i,t+1}$$

The dependent variable is the value of imported machinery and equipment divided by total assets. The other elements of the specification are defined in the same way as the equation (1). In Table 9, the regression results indicate that MWs are positively associated with imports of machinery and equipment. The evidence shows how firms make the technology-improvement investment needed to adapt to adverse labor market shocks.

## V. Concluding Remarks

MW provisions have been a controversial public policy. The debate surrounding the effects of MWs on employment started with the first legislation in the U.S. in 1938 and remains inconclusive, even though it has lasted over 80 years.[13] However, the effects of MW policies on firms' behaviors

[13] Two academic studies investigating recent MW hikes in Seattle reach opposite conclusions. See https://www.economist.com/news/finance-and-economics/21724802-two-studies-their-impact-seattle-reach-opposite-

are largely neglected in the finance literature. In this study, we examine how MW policies impact firms' capital investments. Our findings indicate that firms, rather than passively absorbing the costs brought about by MW hikes, react actively to offset rising labor costs by automating routine tasks previously performed by MW-earning workers, consistent with the capital-labor substitution hypothesis. Further, our analysis shows that the investment response to MWs is greater for firms that are MW-sensitive, technologically underdeveloped, cannot sufficiently pass on labor costs to consumers, and located in regions with better property rights protection.

The analysis uses combined census data and tax survey data for Chinese manufacturing firms from 1998–2013. The identification strategy is based on the discontinuities of MW policies at county borders. A key advantage of this empirical strategy is that it enables us to isolate the treatment effects of MWs while controlling for omitted spatial heterogeneities that may bias the regression estimates. To sharpen the identification of the study, we undertake a natural experiment using county border changes in the early 2000s; this yields results consistent with our baseline analysis.

This study extends the MW literature by investigating the effects of MWs on the behavior of firms. In showing that firms adapt to MW changes by coordinating their employment policies with investment policies, it provides a fresh perspective on the economic consequences of MW policies. We believe it would be fruitful for future studies to provide more cross-country evidence on the mechanisms through which firms respond distinctly to MW policies. Given the growing labor costs in developing countries, it would also be interesting to understand cross-border capital and labor allocations within multinational firms when one country increases the MW relative to another.

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## Table 1
## Summary Statistics

The summary statistics for all regression variables are reported in Table 1. The sample includes 843,918 firm-year observations with 268,610 distinct private Chinese manufacturing firms located within 100km of the borders of contiguous county pairs. Panel A presents the descriptive statistics of the firm-level dependent variables used in the analysis. INVESTMENT is the change of net fixed assets from year $t$ to $t + 1$ plus current depreciation in year $t + 1$, and then divided by total assets in year $t$. ln(K/L) is the log of net fixed assets divided by the number of employees. MACHINE is the value of machinery and equipment imports divided by total assets. All main dependent variables in Panel A are measured in a 1-year lead relative to right-hand-side variables reported in Panels B and C. Panel B reports the county-level variables. After dropping missing observations in economic variables, the final sample includes 30,491 county-year observations with essential firm-level and regional variables in year $t$ and $t + 1$. MW, measured in thousands, is annualized minimum wages based on December minimum wages. Other firm-level control variables reported in Panel C include the log of total wage in a firm, ln(WAGE); the log value of total assets, ln(ASSETS); the ratio of net fixed assets over total assets, TANGIBILITY; and the return on assets, ROA. The detailed variable definitions are given in Appendix 1.

| | N | Mean | S.D. |
|---|---|---|---|
| *Panel A. Main Dependent Variables, Firm-Year Level (measured in year t + 1)* | | | |
| INVESTMENT | 843,918 | 0.137 | 0.419 |
| ln(K/L) | 831,875 | 4.322 | 1.341 |
| EMPLOYMENT | 843,918 | 0.047 | 0.646 |
| MACHINE | 828,947 | 0.003 | 0.030 |
| | | | |
| *Panel B. County-Year Level Variables (measured in year t)* | | | - |
| MW | 30,491 | 5.501 | 2.572 |
| ln(MW) | 30,491 | 1.599 | 0.463 |
| $\Delta\ln(MW)_t$ | 30,291 | 0.107 | 0.121 |
| | | | |
| *Panel C. Other Firm-Year Level Variables (measured in year t)* | | | |
| ln(WAGE) | 830,626 | 7.569 | 1.114 |
| ln(ASSETS) | 843,918 | 10.175 | 1.158 |
| TANGIBILITY | 843,918 | 0.363 | 0.227 |
| ROA | 843,918 | 0.133 | 0.224 |

**Table 2**
**Minimum Wages and Firms' Wage Bills**

Table 2 reports the regression results of corporate investment on firms' total wage bills. The estimation is based on samples of firms that are respectively located within 100km, 75km, and 50km of borders of contiguous county-pairs during the 1998–2013 period. County pairs that straddle two provinces are excluded. ln(WAGE) is the log value of total wage expenditure in year $t$ + 1. The independent variable, ln(MW), is the log value of annual minimum wages. The firm-level control variables include the log value of lagged total assets, ln(ASSETS); a lagged tangibility measure, TANGIBILITY; and return on assets, ROA. Macroeconomic variables are measured at the level of cities that administer the relevant county and include the log value of GDP per capita, the growth rate of GDP, and foreign direct investment growth. The regression specifications have controlled for county-pair-by-year and firm fixed effects. The robust standard errors are clustered at the county-pair level and reported in brackets. *, **, and *** indicate the statistical significance at the 10%, 5%, and 1% levels, respectively.

| | 1 | 2 | 3 |
|---|---|---|---|
| Distance to the county border | < 100km | < 75km | < 50km |
| Dep. var. | | $\ln(\text{WAGE})_{t+1}$ | |
| $\ln(\text{MW})_t$ | 0.121*** | 0.127*** | 0.137*** |
| | [0.034] | [0.034] | [0.040] |
| N | 4,005,930 | 3,846,176 | 2,993,121 |
| $R^2$ | 0.872 | 0.873 | 0.875 |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

## Table 3
## Baseline Regressions

Table 3 reports the regression results of corporate investment on minimum wages. Panel A shows the baseline estimation in samples of firms that are respectively located within 100km, 75km, and 50km of borders of contiguous county pairs during the 1998–2013 period. County pairs that straddle two provinces are excluded. INVESTMENT is the change of net fixed assets from year $t$ to $t$ + 1 plus current depreciation in year $t$ + 1 scaled by total assets in year $t$. ln(MW) is the log value of local minimum wages. The firm-level control variables include the log value of lagged total assets, ln(ASSETS); a lagged tangibility measure, TANGIBILITY; and return on assets, ROA. The regression also controls for the expected growth of a county's firms based on the market-to-book ratio of the contemporaneous same-industry public firms. Macroeconomic variables are measured at the level of cities that administer the relevant county and include the log value of GDP per capita, the growth rate of GDP, and foreign direct investment growth. Panel B reports results using the average investment of year $t$ + 2 and year $t$ + 3 as the dependent variable. All regression specifications have controlled for fixed effects at the county-pair-by-year, industry-by-year, and firm levels. The robust standard errors are clustered at the county-pair level and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively.

| | 1 | 2 | 3 |
|---|---|---|---|
| Distance to the county border | < 100km | < 75km | < 50km |
| *Panel A. Baseline Estimation* | | | |
| Dep. var. | | $\text{INVESTMENT}_{t+1}$ | |
| $\ln(\text{MW})_t$ | 0.122*** | 0.120*** | 0.121*** |
| | [0.020] | [0.020] | [0.022] |
| N | 4,066,095 | 3,903,398 | 3,038,156 |
| $R^2$ | 0.531 | 0.530 | 0.529 |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |
| *Panel B. Minimum Wage Effect in the Long Run* | | | |
| Dep. var. | | $\text{AVG\_INVESTMENT}_{t+2 \text{ to } t+3}$ | |
| $\ln(\text{MW})_t$ | 0.051*** | 0.060*** | 0.077*** |
| | [0.017] | [0.018] | [0.020] |
| N | 1,531,540 | 1,477,992 | 1,166,613 |
| $R^2$ | 0.648 | 0.646 | 0.646 |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

# Table 4
# Robustness Checks

Table 4 presents the robustness checks for our baseline estimation. Panel A tabulates the results for regressions controlling for the GDP growth forecast made by local governments. Panel B reports the results using a sample restricting to county pairs in which both counties are administered by the same city. All regression specifications have controlled for fixed effects at the county-pair-by-year, industry-by-year, and firm levels. The robust standard errors are clustered at the county-pair level and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively.

| | 1 | 2 | 3 |
|---|---|---|---|
| Distance to the county border | < 100km | < 75km | < 50km |
| *Panel A. Controlling for GDP Forecast* | | | |
| Dep. var. | INVESTMENT$_{t+1}$ | | |
| ln(MW)$_t$ | 0.139*** | 0.137*** | 0.130*** |
| | [0.024] | [0.024] | [0.026] |
| GDP_FORECAST$_t$ | 0.596*** | 0.613*** | 0.632*** |
| | [0.199] | [0.212] | [0.226] |
| N | 3,485,093 | 3,353,632 | 2,614,083 |
| $R^2$ | 0.558 | 0.556 | 0.556 |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |
| *Panel B. Within the Same Cities* | | | |
| Dep. var. | INVESTMENT$_{t+1}$ | | |
| ln(MW)$_t$ | 0.086*** | 0.083*** | 0.095*** |
| | [0.026] | [0.026] | [0.026] |
| N | 2,722,769 | 2,658,285 | 2,226,984 |
| $R^2$ | 0.533 | 0.533 | 0.533 |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

## Table 5
## A Natural Experiment Based on County Border Changes

Table 5 reports the effect of minimum wages (MWs) on firms' investment in a natural experiment in which the border dividing two neighboring counties was redrawn. We define as "*Treat*" firms that were reassigned to a different county following the border change and as "*Control*" those same-county firms that did not change their administrative counties. We match each treated firm with up to five corresponding control firms using a Mahalanobis match approach by a range of firm-level characteristics and identify 103 treated firms that adopted a higher MW than the would-be MW in absence of border changes, and 354 control firms. Panel A shows the summary statistics for firm-years used in the regression. Panel B reports the balance test results in the year preceding the border change event. The difference-in-differences regression results are reported in Panel C. Columns 1 and 2 tabulate results for the main experiment tests. Dummy variable TREAT marks the treated firms. Dummy variable POST marks the period after county border changes. Columns 3 and 4 show results for a falsification test in which the treated firms were assigned to new counties that adopt the same MW policies as their original counties. The standard errors are clustered at the firm level and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively.

*Panel A. Summary Statistics*

| Variables | N | Mean | S.D. |
|---|---|---|---|
| INVESTMENT | 4,918 | 0.129 | 0.419 |
| TREAT × POST | 4,918 | 0.097 | 0.295 |
| TREAT | 4,918 | 0.130 | 0.336 |
| POST | 4,918 | 0.672 | 0.469 |

*Panel B. Balance Tests*

| | Treated Firms | | Control Firms | | | |
|---|---|---|---|---|---|---|
| | N | Mean | N | Mean | Diff | *p*-Value |
| ln(ASSETS) | 103 | 9.29 | 354 | 9.21 | 0.08 | 0.40 |
| TANGIBILITY | 103 | 0.35 | 354 | 0.36 | -0.01 | 0.62 |
| ROA | 103 | 0.02 | 354 | 0.03 | 0.00 | 0.81 |

*Panel C. Difference-in-Differences Estimation*

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| | INVESTMENT $_t$ | | | |
| | Experiment Sample | | Placebo Tests | |
| TREAT × POST | 0.098** | 0.138** | -0.007 | 0.022 |
| | [0.043] | [0.056] | [0.022] | [0.019] |
| N | 4,918 | 4,918 | 7,882 | 7,882 |
| $R^2$ | 0.383 | 0.649 | 0.397 | 0.669 |
| Firm & economic controls | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes |
| Year | Yes | No | Yes | No |
| Industry × year FE | No | Yes | No | Yes |

## Table 6
## Robustness on Regression Specifications

Table 6 reports the regression results using a change specification that regresses firm investments on the change in minimum wages. The dependent variable is the one-year lead investment variable in year $t + 1$ in Panel A and the average investment of year $t + 2$ and $t + 3$ in Panel B. Δln(MW) is the change in the log of minimum wages from year $t$ -1 to $t$. All regression has controlled for the same set of firm-level control variables and macroeconomic variables as in Table 3. Also controlled for are fixed effects at the county-pair-by-year, industry-by-year, and firm levels. The robust standard errors are clustered at the county-pair level and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively.

| | 1 | 2 | 3 |
|---|---|---|---|
| Distance to the county border | < 100km | < 75km | < 50km |
| *Panel A. One-Year MW Effect Using the Change Specification* | | | |
| Dep. var. | | $INVESTMENT_{t+1}$ | |
| $\Delta\ln(MW)_t$ | 0.039** | 0.039** | 0.054*** |
| | [0.016] | [0.017] | [0.019] |
| N | 4,054,864 | 3,892,672 | 3,030,048 |
| $R^2$ | 0.532 | 0.531 | 0.530 |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |
| *Panel B. Long-Run MW Effect Using the Change Specification* | | | |
| Dep. var. | | $AVG\_INVESTMENT_{t+2 \text{ to } t+3}$ | |
| $\Delta\ln(MW)_t$ | 0.026** | 0.031** | 0.034** |
| | [0.013] | [0.013] | [0.015] |
| N | 1,526,163 | 1,472,904 | 1,162,773 |
| $R^2$ | 0.650 | 0.647 | 0.647 |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

## Table 7
## Heterogeneous Effects of Minimum Wages

Table 7 reports cross-sectional regression results of corporate investment. Panel A presents three pairs of subsample regression results by the MW sensitivity, which is separately proxied by the firm average wage, the industry labor intensity, and the industry labor skill level. Panel B reports three additional cross-sectional tests by i) product market power, which is proxied by the Lerner Index; ii) technology gap, which measures an industry's technological development by comparing the industry's patent output with that of its U.S. counterpart; and iii) property rights protection, which is a city-level property rights protection index constructed using a firm survey by the World Bank in 2005. We also report the *p*-value for the difference test that the null hypothesis states the estimated coefficients of ln(MW) in two subsamples are not significantly different. The standard errors are clustered at the county-pair level, industry-by-year, and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively. The detailed definitions for variables are in Appendix 1.

*Panel A. MW Sensitivity*

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| Distance to the county border | < 50km | | | | | |
| Dep. var. | $INVESTMENT_{t+1}$ | | | | | |
| | Firm Average Wage | | Industry Labor Intensity | | Unskilled Worker Percentage | |
| | High | Low | High | Low | High | Low |
| $\ln(MW)_t$ | 0.106*** | 0.136*** | 0.190** | 0.059*** | 0.187*** | 0.053** |
| | [0.025] | [0.028] | [0.027] | [0.025] | [0.027] | [0.026] |
| N | 1,440,246 | 1,597,910 | 1,694,348 | 1,343,808 | 1,673,779 | 1,328,498 |
| $R^2$ | 0.543 | 0.538 | 0.560 | 0.549 | 0.548 | 0.556 |
| $H_0$ : (1) = (2) | p < 0.01 | | | | | |
| $H_0$ : (3) = (4) | | | p < 0.01 | | | |
| $H_0$ : (5) = (6) | | | | | p < 0.01 | |
| Firm & economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Panel B. Other Dimensions*

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| Distance to the county border | < 50km | | | | | |
| Dep. var. | $INVESTMENT_{t+1}$ | | | | | |
| | Product Market Power | | Technology Gap | | Property Rights Protection | |
| | High | Low | High | Low | High | Low |
| $\ln(MW)_t$ | 0.061** | 0.142*** | 0.167*** | 0.065** | 0.150*** | 0.033 |
| | [0.030] | [0.025] | [0.027] | [0.027] | [0.051] | [0.035] |
| N | 927977 | 2110179 | 1,322,275 | 1,349,610 | 1,314,346 | 1,119,164 |
| $R^2$ | 0.583 | 0.540 | 0.560 | 0.550 | 0.514 | 0.538 |
| $H_0$ : (1) = (2) | p < 0.01 | | | | | |
| $H_0$ : (3) = (4) | | | p < 0.01 | | | |
| $H_0$ : (5) = (6) | | | | | p < 0.01 | |
| Firm & economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |

## Table 8
## Minimum Wages and the Capital-to-Labor Ratio

Table 8 reports the dynamics of capital-labor substitution in the samples of firms that are respectively located within 100, 75, and 50km of the borders of contiguous county pairs during the 1998–2013 period. County pairs that straddle two provinces are excluded. ln(K/L) is defined as the log value of net fixed assets divided by the number of employees. EMPLOYMENT is the employment growth from year $t$ to year $t$ + 1. ln(MW) is the log value of the local minimum wage. Firm-level control variables and macroeconomic variables are included. All regression specifications have controlled for fixed effects at the county-pair-by-year, industry-by-year, and firm levels. The robust standard errors are clustered at the county-pair level and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively.

*Panel A. Capital-to-Labor Ratio*

| | 1 | 2 | 3 |
|---|---|---|---|
| Distance to the county border | < 100km | < 75km | < 50km |
| Dep. var. | | $\ln(K/L)_{t+1}$ | |
| $\ln(MW)_t$ | 0.230*** | 0.219*** | 0.175*** |
| | [0.043] | [0.044] | [0.046] |
| N | 4007753 | 3847698 | 2995068 |
| $R^2$ | 0.738 | 0.739 | 0.743 |
| | | | |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

*Panel B. Employment*

| | 1 | 2 | 3 |
|---|---|---|---|
| Distance to the county border | < 100km | < 75km | < 50km |
| Dep. var. | | $\text{EMPLOYMENT}_{t+1}$ | |
| $\ln(MW)_t$ | -0.037** | -0.041** | -0.044** |
| | [0.018] | [0.018] | [0.021] |
| N | 4066095 | 3903398 | 3038156 |
| $R^2$ | 0.395 | 0.393 | 0.392 |
| | | | |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

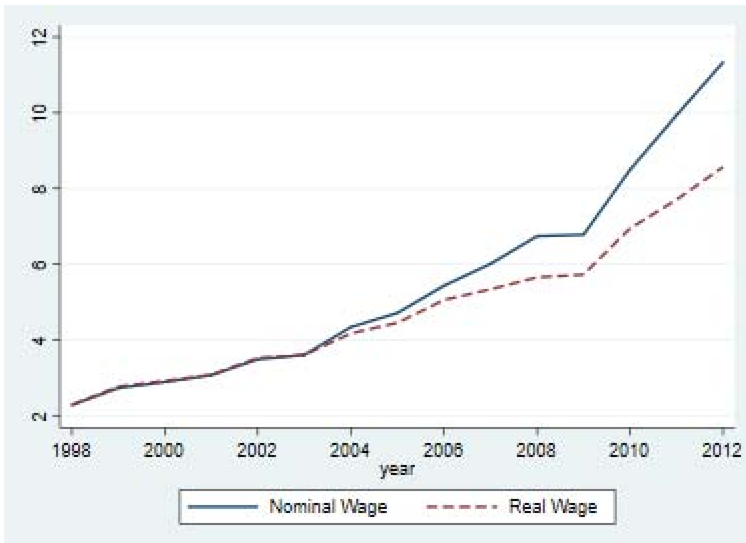
## Table 9. Minimum Wages and the Imports of Machinery and Equipment

Table 9 reports the regression results of machinery and equipment imports in the samples of firms that are respectively located within 100, 75, and 50km of the borders of contiguous county pairs during the 2000–2013 period. County pairs that straddle two provinces are excluded. MACHINE is the value of machinery and equipment imports divided by total assets. Firm-level control variables and macroeconomic variables are included. All regression specifications have controlled for fixed effects at the county-pair-by-year, industry-by-year, and firm levels. The robust standard errors are clustered at the county-pair level and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively.

| | 1 | 2 | 3 |
|---|---|---|---|
| Distance to the county border | < 100km | < 75km | < 50km |
| | | $MACHINE_{t+1}$ | |
| $\ln(MW)_t$ | 0.003*** | 0.003*** | 0.003*** |
| | [0.001] | [0.001] | [0.001] |
| N | 4,007,866 | 3,846,613 | 2,993,054 |
| $R^2$ | 0.535 | 0.536 | 0.548 |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

**Figure 1**
**The Trajectory of Minimum Wages in China**

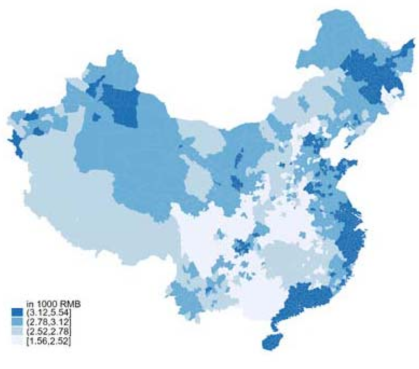
Figure 1 plots the average annualized minimum wages across Chinese counties over the sample period. The blue solid line indicates the nominal minimum wage and the dashed red line the real minimum wage in constant 1998 CNY (in thousands).



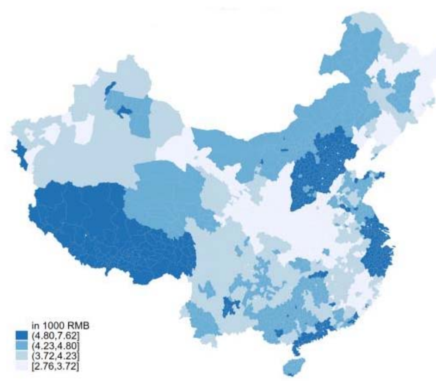
## Figure 2
## Spatial Distribution of Minimum Wages in China

Figure 2 plots the distribution of county-level minimum wages in China for four selected sample years. In each graph, all Chinese counties are sorted by their minimum wages into quintiles marked by different colors, with the darkest area corresponding to the top quintile.

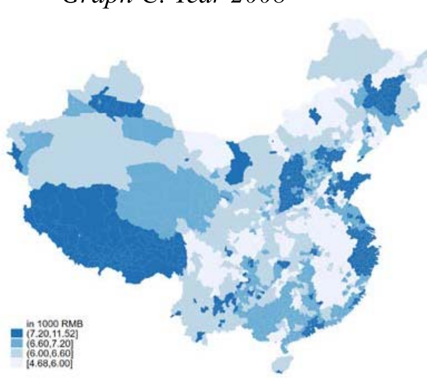
*Graph A. Year 2000*



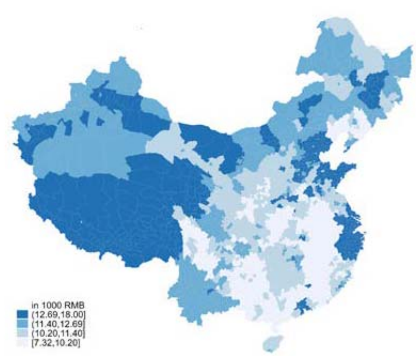

*Graph B. Year 2004*




*Graph C. Year 2008*

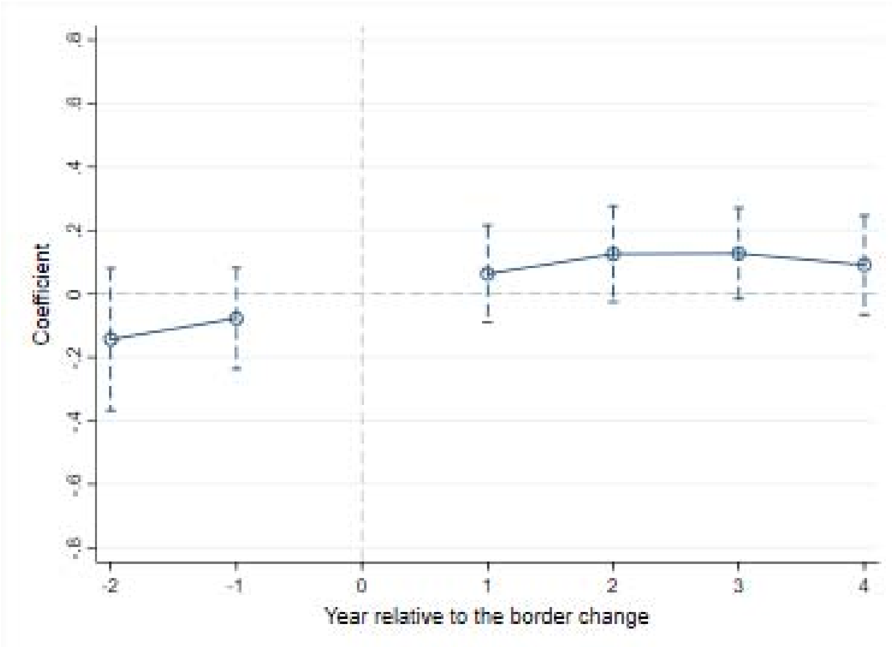



*Graph D. Year 2012*



## Figure 3
## Dynamic Effects of Minimum Wages on Investment Around Border Changes

Figure 3 shows the dynamic effect of minimum wages on firms' investments in a natural experiment. The dots on the graphs are based on the estimated $\beta_1^m$ in specification equation (3), with *m* corresponding to values on the x-axis. The vertical bars around the dots represent the 95% confidence intervals.



## Appendix 1. Variable Definitions

*Firm-Level Variables*
$INVESTMENT_{i,t}$: Change of net fixed assets from year $t$ - 1 to $t$ plus current depreciation in year $t$ scaled by total assets in year $t$ – 1. Source: CIED

$\ln(K/L)_{i,t}$: Log value of net fixed assets divided by the number of employees. Source: CIED

$MACHINE_{i,t}$: The value of machinery and equipment imported by firm $i$ in year $t$ scaled by total assets. The type of imported product is identified by the Harmonized Commodity Code (HS code). We consider the imported product as machinery and equipment if the two-digit HS code is 84 or 85. Source: China Customs Database & CIED

$EMPLOYMENT_{i,t}$: The growth rate of employee numbers from year $t$ - 1 to year $t$. Source: CIED

$\ln(WAGE)_{i,t}$: Log value of a firm's total wage expenditure. Source: CIED

$\ln(ASSETS)_{i,t}$: Log value of total assets. Source: CIED

$TANGIBILITY_{i,t}$: The ratio of net fixed assets to total assets. Source: CIED

$ROA_{i,t}$: Return on assets defined as profits divided by total assets. Source: CIED

*County-Level Variables*
$\ln(MW)_{c,t}$: Log value of annualized minimum wage, defined as December minimum wages multiplied by 12. Source: MOHRSS

$\Delta\ln(MW)_{c,t}$: The change in the log value of minimum wages from year $t$ - 1 to year $t$. Source: MOHRSS

$COUNTY\_GROWTH\_POTENTIAL_{c,t}$: Expected growth of firms in county $c$ of year $t$. We first identify all industries in a county and, for each industry, calculate its growth potential using the average market-to-book ratio of the contemporaneous same-industry public firms in China. The expected growth of firms in county $c$ is size-weighted industry growth across county $c$'s industries, where the weight is the value of total asset size in each industry. Source: CIED and CSMAR

*Variables for Subsample Analysis*
FIRM_AVERAGE_WAGE (High/Low): The average wage per person within a firm. It is defined as the ratio of firm wage expenditure to employment. Source: CIED

INDUSTRY_LABOR_INTENSITY: Industry labor intensity is measured by the average ratio of total wage expenditure to assets across the firms in a 3-digit industry. We average an industry's labor intensity over the sample period and use the average intensity value to sort industries into high and low labor-intensive groups. Source: CIED

UNSKILLED_LABOR_PERCENTAGE: The percentage of unskilled workers in a 3-digit industry. Unskilled workers are defined as those who do not have a college degree or higher in 2004. All industries in the sample are sorted into high and low groups by the percentage of unskilled workers in an industry. Source: National Statistics Bureau

PRODUCT_MARKET_POWER: The product market power of a 3-digit industry. Following Aghion, Bloom, Blundell, Griffith, and Howitt (2005), we measure a firm's market power by the Lerner index, which is defined as

$$LI_{i,t} = \frac{OPERATING\_PROFIT_{i,t} - FINANCIAL\_COST_{i,t}}{SALE_{i,t}}.$$

The industry market power is the average Lerner index of the industry's firms. We first average the industry market power across the sample period and next use the within-industry average market power obtained in the last step to split the industries into high and low market power groups. Source: CIED

TECHNOLOGY_GAP: The technology gap measures the difference in the total number of granted patents during the sample period between an industry in China and the same industry in the U.S., which is then scaled by the total number of patents filed by the U.S. industry. We use the International Standard Industrial Classification of All Economic Activities (ISIC) to match U.S. and Chinese industries. We partition the sample by the median value of the technology gap measure across the sample industries. Source: PATSTAT & CIED

Property Rights Protection: The city-level contract enforcement index calculated using the question J7 in the Investment Climate Survey conducted by the World Bank in 2005. Source: World Bank Investment Climate Survey

## Appendix 2. Sample Construction

The firm-level data is constructed using two separate data sources. The first data set is the China Industrial Enterprise Database (CIED) between 1998 and 2013, where the data in 2010 is missing. The second dataset is the firm-level tax return data sourced from the Ministry of Finance of PRC between 2007 and 2011. These two comprehensive data sets contain noise and error. Consistent with previous studies, we apply the following criteria to clean our initial sample:

1. We drop firm-years in which the values of relevant accounting variables are missing or abnormal (zero or negative values). The variables are total assets, book value of fixed assets, operating revenues, and number of employees. We also exclude firm-years with operating statuses that are not reported as "Normal."
2. We drop firms in utility sectors that have the four-digit industry codes 4400–4499 and 4600–4699.
3. We drop firm-years reporting abnormal accounting values that contradict accounting principles. Specifically, we exclude firm-years in which

- the debt measures are not negative;
- the amount of current assets is larger than total assets;
- the amount of fixed assets is larger than total assets.

4. We drop firms that are smaller than a certain scale as small firms may not have reliable accounting systems. We exclude firm-years in which

- the number of employees is fewer than 30 or total wage expense is zero;
- total assets are lower than CNY5 million;
- sales value is lower than CNY5 million.

5. We drop firms that report incorrect or missing location codes. We drop firms with imprecise addresses that cannot be converted into coordinates using GIS techniques. We keep only firms that are located within 100km of the borders of contiguous county pairs.

The resulting sample consists of 843,918 firm-year observations, corresponding to 268,610 distinct private firms. The county pairs are constructed using the GIS map of China from 2002, provided by the China Data Center, University of Michigan. After combining the data of county pairs and dropping firms that are affected by border changes over time, we construct a sample containing 4,066,095 observations at the county-pair, firm, and year levels.

## Appendix 3

### Table A1. Minimum Wages and Firm Growth

Table A1 presents the regression of minimum wages on a county's firm growth potentials. COUNTY_ROA and COUNTY_INVESTMENT respectively measure the average ROA and average INVESTMENT across firms in a county. We prefix variables by Δ to denote changes in corresponding variables. GDP_FORECAST is the city-level GDP growth forecasts made by local governments. COUNTY_GROWTH_POTENTIAL is calculated based on the market-to-book ratio of the contemporaneous same-industry public firms in China. All regressions include a set of macroeconomic controls. Also included are county and province-by-year fixed effects. The robust standard errors are clustered at the county level and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dep. var. | $\ln(MW)_{t+1}$ | | | | | |
|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 |
| $COUNTY\_ROA_t$ | 0.011 | | | | | |
| | [0.009] | | | | | |
| $\Delta COUNTY\_ROA_t$ | | -0.000 | | | | |
| | | [0.005] | | | | |
| $GDP\_FORECAST_t$ | | | -0.058 | | | |
| | | | [0.052] | | | |
| $COUNTY\_GROWTH\_POTENTIAL_t$ | | | | -0.058 | | |
| | | | | [0.001] | | |
| $COUNTY\_INVESTMENT_t$ | | | | | 0.001 | |
| | | | | | [0.001] | |
| $\Delta COUNTY\_INVESTMENT_t$ | | | | | | -0.000 |
| | | | | | | [0.001] |
| N | 30,429 | 28,730 | 19,317 | 30,429 | 30,429 | 28,730 |
| $R^2$ | 0.992 | 0.992 | 0.992 | 0.992 | 0.992 | 0.992 |
| Economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Province × year FE | Yes | Yes | Yes | Yes | Yes | Yes |

## Table A2. Robustness on Minimum Wage Enforcement

Table A2 reports the results for the robustness tests on the minimum wage enforcement. The regression sample drops the bottom 25% of cities with the worst MW compliance rate based on income data from the Urban Household Income Survey. The specification of the regression is the same as that in Table 3. The robust standard errors are clustered at the county-pair level and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively.

| | 1 | 2 | 3 |
|---|---|---|---|
| Distance to the county border | < 100km | < 75km | < 50km |
| Dep. var. | | $INVESTMENT_{t+1}$ | |
| $\ln(MW)_t$ | 0.089*** | 0.086*** | 0.087*** |
| | [0.025] | [0.025] | [0.028] |
| N | 3,157,533 | 3,032,632 | 2,341,301 |
| $R^2$ | 0.536 | 0.535 | 0.534 |
| Firm & economic controls | Yes | Yes | Yes |
| County pair × year FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

## Table A3. Downward Minimum Wage Variation

Table A3 reports the difference-in-difference regression using a sample of counties that downward adjust their MWs in a year. We pair each MW-decreasing county with a neighboring county that experiences no downward variation in MWs in the event window. Treated firms are those located in the MW-decreasing counties, and control firms are in the paired county. This process produces a total of 74 county pairs, including 21 MW-decreasing counties. Panel A reports the summary statistics for observations used in this event study. The regression results are reported in Panel B. The firm-level control variables include the log of total assets, firm tangibility, and return on assets. Macroeconomic variables include the expected growth of a county's firms, the log of the city GDP per capita, the city GDP growth, and the growth rate of city foreign direct investment. All regression specifications have controlled for fixed effects at the county-pair-by-year, industry-by-year, and firm levels. The robust standard errors are clustered at the county-pair level and reported in brackets. *, **, and *** denote the statistical significance at the 10%, 5%, and 1% levels, respectively.

*Panel A. Summary Statistics*

| Variable | N | Mean | S.D. |
|---|---|---|---|
| INVESTMENT | 11249 | 0.131 | 0.282 |
| TREAT × POST | 11249 | 0.242 | 0.428 |
| TREAT | 11249 | 0.500 | 0.500 |
| POST | 11249 | 0.520 | 0.500 |

*Panel B. Difference-in-Differences Estimation*

| | 1 | 2 | 3 |
|---|---|---|---|
| Distance to the county border | < 100km | < 75km | < 50km |
| Dep. var. | | $INVESTMENT_t$ | |
| TREAT × POST | -0.092*** | -0.103*** | -0.093** |
| | [0.034] | [0.038] | [0.039] |
| N | 11,249 | 10,806 | 7,944 |
| $R^2$ | 0.782 | 0.792 | 0.817 |
| Firm & economic controls | Yes | Yes | Yes |
| County-pair FE | Yes | Yes | Yes |
| Industry × year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

## Figure A1. The Evolution of Investments Response to Minimum Wage Changes

Figure A1 plots the investment response to local minimum wage changes. We estimate the regression model specified in Table 6 in the 50km sample and plot the coefficients of minimum wage changes ($\Delta\ln(MW)$) when the dependent variable is the firm investment measured in different years relative to local minimum wage changes in year $t$. We use the average investment made in year $t + 2$ and year $t + 3$ so that the coefficient is comparable to that in Column 3, Panel B of Table 6. We also average the investment made in year $t + 4$ and $t + 5$. The vertical bars around the dots represent the 95% confidence intervals.