Improving or Disappearing: Firm-Level Adjustments to Minimum Wages in China*

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June, 2018

Abstract

We here consider how Chinese firms react to higher minimum wages, exploiting the 2004 minimum-wage Reform in China. After this reform, we find that the wage costs for surviving firms that were more exposed to minimum-wage hikes rose, but also that their productivity significantly improved, allowing them to absorb the cost shock without any change in their profitability and with limited job losses. Our results are robust to pre-existing trend analysis and an IV strategy. However, the survival probability of the firms that were most exposed to minimum-wage hikes fell after the Reform. Firm-level productivity gains partly came from better inventory management and greater investment in capital, at the cost of a reduction in firm-level cash.

Keywords: minimum wages, firm-level performance, productivity, China.
JEL codes: J38, O12, O14.

*This is a substantially revised version of a paper previously circulated as "The cleansing effect of minimum wages - Minimum wages, firm dynamics and aggregate productivity in China", IRES DP 2014-15. We thank the editor and two anonymous referees for very helpful comments. We are also grateful to Nicolas Berman, Daniel Hamermesh, Ann Harrison, Vernon Henderson, Carl Lin, Yi Lin, Beata Javorcik, Mathieu Parenti, William Parienté, Fabien Postel-Vinay, Stuart Rosenthal, Marlon Seror, Daniel Sturm and Eric Verhoogen, as well as to many conference and seminar participants, for fruitful discussions. This paper benefited from the financial support of the program "Investissemement d'Avenir" (reference ANR-10-LABX-14-01) of the French government.

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1 Introduction

Minimum wages are widely used across the world as a tool for redistribution or a way of producing higher wages in countries where the bargaining powers of employers and employees are rather unbalanced. Recently, countries such as Germany, the US and the UK have strengthened their minimum-wage policies: a national minimum wage was implemented for the first time in Germany in 2015, Barack Obama called for a significant rise in minimum wages in his 2014 State of the Union address, and the UK government announced in Spring 2016 that the minimum wage would increase by 40% over the next five years. In less-developed economies, the riots in Bangladesh and Cambodia in recent years reflect the considerable social demand for a more equal distribution of the benefits of growth. In China, polls reveal that concerns about inequality have grown, as "*roughly eight-in-ten have the view that the rich just get richer while the poor get poorer*" (Pewresearch Center, 2012).

These policy evolutions have added fuel to the already-heated debate on the effect of minimum wages on workers and firms, with some academics concluding to only modest effects if any (see Dube et al., 2010 and Allegretto et al., 2011), while others emphasize the negative effect of minimum wages for some particular employment types, such as low-skilled and young workers (see Neumark et al., 2014, for example).

In this paper we use balance-sheet data on more than 350,000 industrial firms to analyze the firm-level response to minimum-wage hikes in China, where the minimum wage is set at the city level. Our empirical strategy exploits the 2004 Reform of minimum wages that imposed large increases in minimum wages and greater enforcement across Chinese cities. We focus on the performance growth and exit of firms present on the market before the minimum-wage hike. We thus do not address here the question of the macroeconomic effects of the policy, which would also require the analysis of entry. We first show that the 2004 Reform was binding, and made minimum wages in China tighter: local minimum wages rose faster after the Reform, and the share of Chinese firms complying with the local minimum wage increased dramatically. Moreover, the fraction of firms paying average wages close to the local minimum wage increased after the Reform. No such trends were found before 2004: while minimum wages barely imposed any constraints on firms before 2004, the Reform made minimum wages more stringent. The Reform thus can be seen as a shock in terms of how minimum wages are enforced and bind in China, and we would expect firms to respond to this shock.

We carry out a difference-in-differences analysis to estimate the effect of the 2004 Reform on firm-level outcomes. We consider two periods in the data: a Reform period (2004 to 2007) and a pre-Reform period (1998 to 2003). In each year, we identify two groups of firms, the exposed and the non-exposed. Absent worker-level information, exposed firms are defined as those whose average wage (total wage bill over employment) in year t-1 was

lower than the local minimum wage in t (as in Harrison and Scorse, 2010, and Draca et al., 2011). We then compare the performance growth in the firms that were exposed to minimum-wage hikes (relative to the non-exposed firms) after the Reform, relative to the same performance gap in the pre-Reform period. We account for firm-level time-invariant characteristics through firm fixed effects and we also control for firm-level time-varying characteristics (size, productivity, exports, ownership etc.), as well as city-year and sector-year fixed effects. Local shocks are thus taken into account, as well as any potential correlation between the exposure to minimum-wage hikes and firm-level time-invariant and observable time-varying characteristics.

Looking at the immediate effects of the minimum-wage hikes (i.e. in the year of exposure), we find that average wages in exposed firms rose faster after the 2004 Reform, as compared to the pre-Reform period. Profitability remains unaffected, while, in line with the existing literature, the negative effect on employment is modest. The main explanation is that surviving exposed firms significantly improved their productivity. We also document heterogeneity in treatment effects depending on the intensity of exposure: intuitively, the premium in terms of wage and productivity growth is larger for firms with a greater gap between their baseline average wage and the subsequent city minimum wage. Our results are robust to various tests of the validity of our identification assumptions and the definition of our estimation sample. We check that our findings are robust to pre-trend analysis and an IV strategy based on a Bartik-type instrument for exposure to the minimum wage. We also analyze possible firm-level repercussions beyond the first year of exposure to post-Reform minimum-wage hikes. We find that the wage and productivity premia continue in the medium run (our data allows us to investigate effects up to three years after treatment), while the average employment repercussion remains negative. However, productivity gains go beyond reducing the number of employees, so that the value added of surviving exposed firms increases in the medium run following the Reform. Our key message thus remains valid in the medium run: surviving exposed firms significantly improved their productivity following the Reform, allowing them to absorb the cost shock without any change to their profitability and with limited falls in employment.

We then go further into the analysis of the mechanisms. We find evidence of a greater exit probability for exposed firms after the Reform. Productivity gains seem to partly come from better management practices, in particular regarding inventory management and productivity-enhancing investments, with the capital-labor ratio rising faster for surviving exposed firms after the Reform. We show that this comes at the cost of lower firm-level cash. The competing explanations for the effects we measure do not appear likely. In particular, the data is not compatible with lower fringe benefits to compensate for higher wages, or the substitution of less-paid/less protected migrants for incumbent workers. Last, what we

measure is revenue-based productivity, which captures both physical productivity and prices (Foster et al., 2008). Absent price data, we show that the firm-level response to the Reform is homogeneous across sectors with different degrees of competition and price-elasticities of demand. This suggests that firm-level adjustments through higher prices instead of greater productivity is unlikely.

To the best of our knowledge, this is the first contribution to highlight firm-level productivity reactions to the minimum wage. Note that this reaction is probably larger in developing countries, in which inefficiencies remain pervasive (Hsieh and Klenow, 2009, Brandt et al., 2013). The small employment reactions to minimum wages in developed countries that are commonly found in the literature reflect different firm-level channels of adjustment, such as lower profits (Draca et al., 2011 in the UK, for example) and higher prices (Aaronson, 2001 in the US and Canada, for example).

China is a useful case to analyze for a number of reasons. First, China, the fastest-growing economy over the past fifteen years (Song et al. 2011), has become a key player in the global economy; as such, understanding the determinants of its competitiveness and industrial dynamics is of interest for both developed and developing countries. Moreover, China is a showcase in terms of low wages: in 2004, the average Chinese monthly manufacturing wage was 141 Dollars, versus 342 Dollars in Mexico and over 2,500 Dollars in the US.[1] Finally, as shown in Figures 1 and 2 in the Appendix, there is considerable variation in both the level and growth-rate of the minimum wage in the 261 Chinese cities in our final sample.[2] In 2003, the monthly minimum wage ranged from 170 Yuan (20 Dollars) in Eerduosi and Hulunbeier (Inner Mongolia) to 600 Yuan (72 Dollars) in Shenzhen; on the other hand, the 2003-2007 rise in the minimum wage was above 200% in cities from Inner Mongolia, while the figure was only 20% or slightly more in some other cities whose wage standards in 2003 were already quite high, as in Xinjiang and Guangdong provinces, for example.

Our work contributes to the literature in a number of ways. First, it adds to the debate on the effect of minimum wages on employment. Although raising the wage floor should theoretically increase the wages of low-paid workers and adversely affect employment (Borjas, 2004), evidence (largely from the US) points to little or no employment effect of minimum wages (Card and Krueger, 1994; Dickens et al., 1999; Dube et al., 2010; see Schmitt, 2013, for a review).[3] However, the results here continue to be debated (see Dube et al., 2010;

[1] Authors' calculations based on LABORSTA ILO data: `http://laborsta.ilo.org/STP/guest`.

[2] China is divided into four municipalities (Beijing, Tianjin, Shanghai and Chongqing) and 27 provinces, which are further divided into prefectures. As is common in the literature, we use the terms city and prefecture interchangeably, even though prefectures include both an urban and a rural component.

[3] One of the potential explanations for the lack of an employment effect is that the percentage of workers earning the minimum wage in the countries in question is very small, i.e. under 5% (Neumark et al., 2004), and that the changes in the minimum wage have only been small (often lower than the inflation rate). The situation in China is radically different. Since the promulgation of the new minimum-wage regulations in

Allegretto et al., 2011 and Neumark et al., 2014, for example). We revisit the question using data from Chinese factories, which are often considered as a symbol of "low-cost" production. There is already some work on China in this respect; this has mainly relied on aggregate or semi-aggregate data and has produced mixed results, with some analyses finding negative employment effects and others no significant effect.[4] We differ from most of this existing work in our use of firm-level data, so that we can directly link firm-level outcomes to changes in the local minimum wage. Exploiting this firm-level data allows us to investigate various margins of adjustment to minimum wages beyond employment effects. Note however that the firm-level data covers only the largest firms. Since these firms account for a large share of aggregate output (see Section 3), this restriction is certainly not an issue for our productivity and profitability analysis; we might however miss part of the employment effects if small firms are more sensitive to minimum-wage changes (even though our result of relatively modest employment effects is overall in line with the literature).

Our second contribution is that our analysis of non-employment outcomes allows us to ask why we find in our sample limited firm-level employment effects of the 2004 minimum wage Reform in China. There are four main ways in which firms can react to higher minimum wages, supposing they comply with them: employment, profit, prices and productivity (Schmitt, 2013; Hirsch et al., 2015). However, absent reliable firm-level information, rigorous joint empirical evidence on these effects is scarce. A few notable exceptions include Draca et al. (2011), who show that British firms adapted to the introduction of a national minimum wage in 1999 by reducing their profit margins, leaving employment and productivity unaffected, and Harasztosi and Lindner (2015), who find only a small negative employment effect of a massive minimum wage rise in Hungary, with most of the adjustment occurring through higher firm-level prices. In this paper, we propose a careful evaluation of the various ways in which Chinese firms may have adjusted to the 2004 Reform, including the number of employees, productivity, profitability and survival. We also interpret the lack of heterogeneity in our effects by the sectoral-level degree of competition and price-elasticity of demand

2004, local governments have been required to implement frequent and substantial increases in minimum wages. The latest illustration is the pledge under China's 12^{th} Five-Year Plan to raise minimum wages by at least 13% annually. Such substantial upward adjustments in minimum wages can be expected to have sizeable repercussions on firms and workers.

[4]Ni et al. (2011) find some negative effects on overall employment in the prosperous coastal provinces and some positive effects in the less-developed interior provinces. Wang and Gunderson (2012) focus on the employment to population ratio for migrants and find the opposite result (a negative effect in non-coastal zones and no effect in the fast growing Eastern regions). Fang and Lin (2015), who combine county-level minimum-wage panel data with a longitudinal household survey, find evidence that minimum-wage changes led to significant adverse effects on employment in the Eastern and Central regions of China, especially for women and young and less-educated workers. Huang et al. (2014) use firm-level data and find a small negative employment effect. Our work differs in that we use balance-sheet data from industrial firms and consider non-employment outcomes. We also focus on the 2004 Reform, which provides us with an original estimation strategy to address endogeneity problems.

as evidence against an interpretation of our results in terms of firm-level price adjustment. That the channels of adjustment vary across studies should not be seen as a contradiction; it rather suggests that the way firms adjust to minimum wages depends on the local context.

Finally, we also contribute to the literature on the effects of labor laws and labor standards in developing countries. Harrison and Scorse (2010) find that anti-sweatshop activism increased wages without harming employment in the Indonesian footwear and textile industries, while higher minimum wages reduced employment. We here focus on minimum wages but extend our analysis to the entire manufacturing sector. On the other hand, Magruder (2013) finds that increases in local minimum wages tended to favor formal work and real demand in Indonesia in the 1990s; his results are in line with a Big Push model, where the minimum wage helps coordinate development at a high wage level. We do not take any stance in our paper on the possible consumption and macroeconomic effects of minimum wages in China; we rather focus on the channels through which firms adjust to the cost shock, in a context where both domestic and export demands were rising. In this respect, recent research by Duflo et al. (2011), Bloom et al. (2013) and Atkin et al. (2015) shows that firms in developing countries might not adopt the best production technologies and management practices, even though the gains from doing so might be substantial. For example, the treated (provided with management consultancy) Indian textile firms in the experiment in Bloom et al. (2013) saw a rise in productivity of 17%. This issue does not only apply to small firms.[5] The monetary or utility costs of changing technology/practices and organizational barriers to change can explain this resistance. Lack of attention to change in management is also emphasized as an explanation by Beaman et al. (2014) in the case of Kenya. This will be all the more true that easy access to cheap labor provides few incentives to pay the monetary and non-monetary adoption costs. Our results suggest that, by raising production costs, minimum wages might change the incentives for surviving firms to pay these adoption costs of better technologies and/or management practices. This link we emphasize between wages and technology choice is reminiscent of Acemoglu and Shimer (2000), who show that firms that offer higher wages fill job openings more rapidly, and so are willing to make larger irreversible investments in complementary inputs such as capital.

The remainder of the paper is structured as follows. The next section describes the Chinese minimum-wage system and some stylized facts on the evolution of minimum wages. Section 3 presents the firm-level data we use and reports some descriptive statistics to show that the 2004 Reform rendered minimum wages binding in China. Section 4 then sets out our empirical strategy, while Section 5 displays our firm-level results. We carry out a number of robustness checks in Section 6, including pre-trend analysis and IV, and analyze the medium-run effects of the Reform. Section 7 digs deeper into the mechanisms underlying

[5]The Indian firms participating in the experiment in Bloom et al. (2013) employed 100 to 1000 workers.

the productivity effect and tests for alternative explanations of this effect. Last, Section 8 concludes.

2 The minimum-wage system and the 2004 minimum-wage Reform in China

This section presents a brief history of minimum wages in China and some details about the 2004 reform.

2.1 A brief history of the minimum wage in China

The history of the Chinese minimum wage started in 1984 when the country acknowledged the "Minimum Wage Treaty" of the International Labor Organization. However, the government did not then immediately impose any obligations in terms of wage standards. Some provinces started experimenting with minimum wages at the end of the 1980s (Guangdong and Shenzen, for example), but it was only in 1993 that China introduced national minimum-wage regulation. This legislation was officially added to the Chinese Labor Law in July 1994 (Lin and Yun, 2016). As Chinese provinces have very different living standards, China does not have one national minimum wage: minimum wages are rather established following a decision process involving both national and local authorities. Each province, municipality, autonomous region, and even district can set its own minimum wage according to local conditions and based on national guidelines.[6] In particular, the Central Government asks local authorities to take the cost of living, household size, average wages, labor productivity, unemployment and economic development at the local level into account when setting minimum wages. Typically, following the national requirements, provincial governments set out multiple minimum-wage classes for the region as a whole, and each city and county in the region chooses the appropriate minimum-wage level based on local economic conditions and living standards. The Minimum Wage Regulations stipulate that the minimum wage in each locality should be at least 40 percent of the local average wage. This process is still applied, and in the latest round of minimum-wage increases, for example, Zhejiang set out four minimum-wage classes for the entire province, with some top-tier cities such as Hangzhou, Ningbo and Wenzhou choosing the highest minimum wage (Class A), and other cities settling on the next-highest minimum wage (Class B) such as Jinhua, Shaoxing and Taizhou, or on the other classes (Lishui and Zhoushan in Class C and Jiaxing in Class D).

[6]The definition of the minimum wage may also vary across locations. Beijing, Shanghai, Jiangsu, Shanxi and Henan do not include social-security payments and public-housing funds when calculating the minimum wage, while other provinces do. In unreported results, which are available upon request, we check that our main message holds when excluding the former locations.

In the 1990s, minimum wages increased quite slowly in China, and not all workers were covered (those in self-employed businesses and State-owned enterprises were not, for example). Moreover, penalties for non-enforcement were only low, suggesting that minimum wages may not have been binding or enforced. The Chinese authorities, concerned by the growing inequality within and across cities that accompanied the rapid growth in the country, set out new minimum-wage rules in March 2004. One of the explicit aims of the 2004 Reform was to increase living standards, in particular in cities where these were the lowest. The 2004 Reform introduced a number of changes: more workers were covered by the minimum wage, minimum wages were adjusted more frequently (at least once every two years) and an hourly minimum wage was created for part-time workers.

The new rules threaten violators with increased penalties: these quintupled from 20-100% of the wage owed before the Reform to 100-500% post-Reform. The definition of what employers can include as part of the wage before comparing it to the legal minimum was also revised (Wang and Gunderson, 2012). They cannot include overtime pay or statutory bonuses to compensate for night work or the hardship of working in hazardous environments (extreme temperatures, toxicity etc.). The controls regarding firm compliance with labour regulations were further tightened in 2004 following the promulgation of the Decree on Labour Inspection by the State Council. Inspections by the government labor authorities are the main tool for the enforcement of minimum-wage rules. The decree specifies the responsibilities and duties of the labour inspection service, and strengthens law enforcement. Its article 5 states "*that local people's government at the county level or at any level above shall strengthen labor security supervision work. The expenses needed in labor security supervision shall be included into the public finance budget at the current level.*"

Of course, these new rules cannot completely prevent firms from non-compliance or using strategies to offset the inflation of labor costs induced by the Reform. Some papers suggest that the pass-through of minimum wages onto labor costs might not be complete for some Chinese firms that reduce the fringe benefits they used to provide as well as overtime pay (Wang and Gunderson, 2015; Ye et al., 2015). However, this does not necessarily invalidate our approach or contradict our results. First, even though pass-through is not complete, as long as labor costs increase we should expect some repercussions on firm-level outcomes. Second, the above contributions do not directly investigate firms' wage-setting behavior during the period of the 2004 Reform, and both focus on a reduced set of firms as compared to our analysis here. Finally, a number of exercises in Section 7 show that, on average, any offsetting effects coming from lower fringe benefits or the substitution of migrants for regular workers do not seem to be of major importance for the manufacturing firms in our sample.

2.2 Stylized facts on the evolution of minimum wages following the 2004 Reform

Before turning to the firm-level analysis, we provide some statistics on the evolution of minimum wages in China. The minimum-wage data at the prefecture level come from various official websites such as China Labour Net. The data contain monthly minimum wages for full-time employees and hourly minimum wages for part-time employees by city and year from 1998 to 2007. Since we do not have information on the total number of hours worked, we use only the former in our regression analysis. City-level minimum wages can be adjusted multiple times in a given year. We define the city-level minimum wage in a year as the highest value of the minimum wage in that year and city.

The Reform imposed a massive rise in city-level minimum wages. Figure 1 shows that city-level nominal minimum wages rose continuously over the 1998-2007 period. The vertical line in 2003 indicates the change in slope and the clear acceleration of minimum-wage growth following the implementation of the Reform in March 2004. While the average annual growth rate of city-level minimum wages was 9.2% between 1998 and 2003 (with a median of 0%), this rose to 15.5% between 2003 and 2007 (with a median of 10.2%), in line with more substantial and frequent adjustments in city-level minimum wages. We might wonder whether these nominal minimum-wage rises were actually canceled out by inflation, with finally little wage pressure on firms. We do not have city-level price indices, but if we use provincial price indices we find that the change in city-level real minimum wages is fairly similar to that in nominal wages. City-level real minimum wages rose by 9.3% per annum on average before the 2004 Reform (with a median of 1.4%) and by 12% after the Reform (with a median figure of 7.4%). Further descriptive statistics in Section 3.2 focus on the ratio of firm-level average wages to the city level minimum wage and thus avoid any effect of local prices. Our identification strategy will also account for city-level inflation through the inclusion of city-year fixed effects.

3 Data and stylized facts on firm-level wage adjustments after the 2004 Reform

This section presents the data and shows that the 2004 Reform made minimum wages more stringent in China.

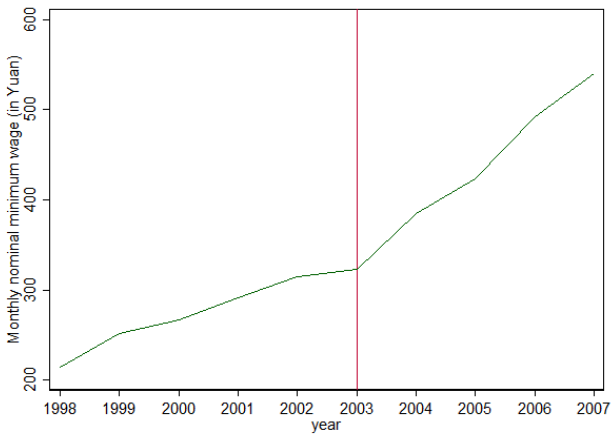

Figure 1: The change in city-level nominal minimum wages

3.1 Data

Our firm-level data come from the annual surveys conducted by the National Bureau of Statistics (NBS) in China. These firm-level surveys include balance-sheet data for all industrial State-owned and non-State firms with sales of over 5 million Yuan.[7] The industries here include mining, manufacturing and public utilities. A comparison to the 2004 full census of industrial firms reveals that these firms account for 20% of all industrial firms, employ roughly 71% of the industrial workforce and generate 91% of output and 98% of exports (Brandt et al., 2012).[8] We use information on the number of employees, production, capital, profits, intermediate inputs and the total wage bill.[9] While labor productivity is our main productivity measure throughout the paper (defined as value-added per worker), we also calculate a firm-level TFP index. To do so, we estimate Cobb-Douglas production functions following the approach developed by Levinsohn and Petrin (2003). We estimate different production functions for each of the 2-digit industries using the firm-level data.

Intermediate inputs are used as a proxy for unobserved variables (entrepreneur characteristics or macroeconomic shocks) which could determine the levels of both employment

[7]This official threshold could make us suspect that firms might disappear from the sample not because of their bankruptcy but because their size falls below the threshold; this could in particular affect the results on the effect of minimum wages on firm-level survival. However, careful analysis of the data suggests that the churning in the sample due to the minimum-size threshold is virtually nonexistent (Brandt et al., 2014). Not only is the sales threshold not strictly enforced, but the rapid growth of the manufacturing sector in China over the period also makes sales reductions relatively rare; the threshold is thus unlikely to become binding for firms that are already above it.

[8]We follow the routine developed by Brandt et al. (2012) to link firms over time using a unique numerical identifier.

[9]These data aggregate up almost perfectly to the totals for the same variables reported in the Chinese Statistical Yearbook.

and capital on the one hand and output on the other.[10]

One limitation of our data is that we do not have information at the worker level. This means that we are not able to use the distribution of wages within the firm to measure firm-level exposure to minimum wage hikes. We use instead the firm-level average wage (the ratio of the total wage bill to the number of employees), as in the few other papers on the topic that also use firm-level data (see Harrison and Scorse, 2010 and Draca et al., 2011, for example). We have balance-sheet data for all of the years from 1998 to 2007. We carry out basic dataset cleaning: we only consider firms with five or more employees, we drop observations with zero or negative wages, material inputs, capital and value-added, and we drop the top and bottom 1% of firms in terms of average wages and average wage growth. We also only keep cities with at least 20 firms over the entire period. Our empirical analysis is carried out on over 350,000 firms that were active at some point between 1998 and 2007.

3.2 Minimum wage binding and enforcement in China

Most work on the effect of minimum wages has had to address two key issues. First, it can be difficult to estimate minimum-wage effects on firm-level outcomes if the change in the minimum wage is only small: firm-level wages might rise faster than the minimum wage, making minimum wages non-binding. Another issue, more specific to developing countries, is the extent to which minimum wages are enforced. Massive non-compliance or firm-level offsetting strategies can make it difficult to identify minimum-wage effects (see for example Bell, 1997, for Mexico and Strobl and Walsh, 2003, for Trinidad and Tobago). The 2004 Chinese Reform has a number of advantages with respect to these two issues. We showed in the previous section that the rise in minimum wages after the 2004 Reform was massive. Moreover, even though neither enforcement nor binding are directly observable, we show in this subsection that the data suggests that firms were both more likely to comply and more constrained by minimum wages after the Reform.

We define the share of complying firms in a given year t as the share of firms paying an average wage at least equal to the city-level minimum wage in this same year t.[11] This annual share rises from an average of 87.5% between 1998 and 2003 to 93.1% from 2004 to 2007. Hence, despite the acceleration in local minimum wage rises following the 2004 Reform, Chinese firms have become more likely to comply with the rules. It is still difficult

[10] The results, available upon request, provide credible elasticities. The coefficient on labor is on average lower than that usually found in the literature, but this is not surprising for a developing country such as China where worker productivity is quite low.

[11] Our data include the total wage bill and the number of workers, but not the number of hours worked. Our measure of firm-level average wages is sensitive to the presence of part-time workers in the firm. However, as long as part-time intensity remains constant over time, the change in the share of firms with average wages below the city-level minimum wage can be interpreted as a change in firm-level compliance with the minimum wage.

for the moment to assess whether this greater compliance reflects more enforcement (due to the strengthening of controls and the reinforcement of penalties in case of non-enforcement) or average wages rising faster than minimum wages over the period, so that minimum wages did not really bind. The evidence based on the distribution of firm-level average wages points towards more enforcement.

The left-hand side of Figure 2 shows that firm-level average wages rose continuously over the period: their distribution gradually shifts to the right from 1998 to 2006 (we do not show all of the years to keep the graphs readable). It might thus well be the case that minimum wage changes go hand-in-hand with the "natural" dynamics of firm-level wages over the period. To have an idea of how binding minimum wages are, the right-hand side of the figure shows the ratio of firm-level average wages to the city-level minimum wage. This ratio cancels out city-level prices that might drive both firm-level average wages and city-level minimum wages. If minimum wages increasingly bind over time, this ratio should increasingly be concentrated at around 1, so that more and more firms pay average wages in the vicinity of the minimum wage. Moreover, in the case of greater enforcement, the distribution should exhibit a smaller share of firms in which the ratio of the average wage to the city minimum wage is below 1. This is not what we see before the Reform: the distribution of the firm-level average wages to the city minimum wage ratio is fairly stable from 1998 to 2002. However, this distribution changes significantly after the 2004 Reform: in line with the figures noted above, the share of firms paying average wages below the city minimum wage falls (compliance increases), and the share of firms paying average wages just above the city minimum wage rises.

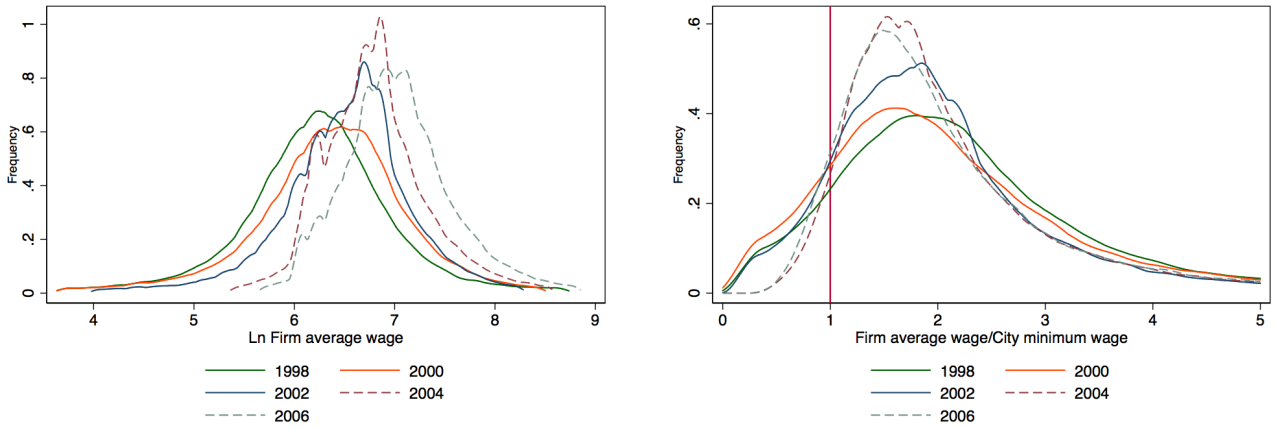

Figure 2: The distribution of firm-level average wages from 1998 to 2006

This graphical analysis is confirmed by the figures in Table 1. These show that the share of non-complying firms in China ($\frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} \leq 1$) fell by 5.6 percentage points following the

Reform. The share of firms paying average wages more than twice the local minimum wage also fell by 1.4 percentage points, while the share of firms whose average wage is between one and two times the local minimum wage rose by 7 percentage points.

Table 1: The distribution of firm-level average wages (in % of active firms)

	$\frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} < 1$	$1 \leq \frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} < 1.25$	$1.25 \leq \frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} < 1.5$	$1.5 \leq \frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} < 2$	$\frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} \geq 2$
1998-2003	12.5	8.5	10.7	23.2	45.1
2004-2007	6.9	9.6	13.4	26.4	43.7

For complying firms, we then calculate the ratio of firm-level average wages to the city-level median wage, to take into account wage differences across cities, and rank firms by wage deciles based on this ratio. We can further see from Table 2 that, among complying firms, the ratio of firm-level average wages to the city-level minimum wage fell after the Reform, especially in the middle of the distribution; this is evidence of compression in the distribution of firm-level average wages after the 2004 Reform.

The increase in the share of complying firms, and the fall in the firm-level average wages to city-level minimum wage ratio show that the distribution of firm-level average wages clearly tilted towards the local minimum wage following the Reform. This wage compression after the reinforcement of minimum-wage standards is in line with Katz and Krueger (1992) and Lee (1999) for the US, and suggests both greater enforcement and a more binding minimum wage in China after 2004.

Table 2: Average firm-level wage to city minimum wage ratio among complying firms

	Avg $\frac{\text{Firm Avg Wage}}{\text{City Min. Wage}}$				
Deciles $\frac{\text{Firm Avg Wage}}{\text{City Med. Wage}}$	1	3	5	7	9
1998-2003	1.22	1.60	1.96	2.39	3.38
2004-2007	1.22	1.54	1.85	2.28	3.31

We also find that the average wage in low-wage firms rose much faster after the implementation of the 2004 Reform. Table 3 presents regression results on the correlation between initial firm-level average wages and subsequent firm-level average wage growth: firm-level average wage growth between t-1 and t is regressed on firm-level average wages in t-1 and its interaction with a dummy for the years in the Post-Reform period (t$\geq$2004). We hence consider whether wage growth differs depending on baseline wages, and whether this difference changed after the implementation of the 2004 Reform. All of the regressions include city-sector fixed effects and year dummies.

The results in column (1) show that in the firms that survive from one year to the next, the correlation between initial wages and subsequent firm-level wage growth is negative and

Table 3: Firm-level average wage growth and initial average wages

Dependent variable	$\Delta_{t-1,t}$ Ln firm wage		
	(1)	(2)	(3)
Ln Firm av. wage$_{t-1}$	-0.480^a	-0.530^a	-0.550^a
	(0.015)	(0.013)	(0.015)
Ln Firm av. wage$_{t-1}$ × 2002-2003 period			0.049^a
			(0.011)
Ln Firm av. wage$_{t-1}$ × Reform	-0.049^a	-0.051^a	-0.030^a
	(0.009)	(0.009)	(0.011)
City-Sector fixed effects	Yes	Yes	Yes
Firm-level controls	No	Yes	Yes
Year dummies	Yes	Yes	Yes
Observations	1,292,028	1,289,621	1,289,621
R-squared	0.26	0.27	0.28

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls in columns (2) and (3) include firm-level employment, wage and labor productivity, as well as dummies for State-owned firms, foreign firms and exporting firms measured in year t-1. Reform is a dummy for t≥2004. "2002-2003 period" is a dummy for the years 2002 and 2003, i.e. the two years prior to the Reform.

significant, suggesting convergence whereby wages grew faster in low-paying firms. The negative and significant coefficient on the interaction between initial average wages and the Reform period dummy indicates that this catch-up was larger after the 2004 Reform. These results do not change in column (2) when we control for additional firm-level variables such as initial size, productivity, export status and ownership.[12]

Column (3) further interacts the initial firm-level average wage with a dummy for the two years before the Reform (2002 and 2003). The coefficient on this interaction term is positive and significant, showing that, if anything, convergence slowed down in 2002 and 2003 as compared to the 1998-2001 period. The faster convergence in firm-level average wages following the 2004 Reform is not then just the continuation of a pre-existing trend. Overall, without claiming any causality from minimum wages for the moment, these exploratory results show that low-wage firms faced greater upward pressure on wages after the 2004 Reform.

The 2004 minimum wage Reform then produced a sharp rise in local minimum wages in China. We also observe greater compliance and wage compression in the bottom tail of the distribution. Finally, after the Reform low-wage firms converged faster in terms of average wages. These results all suggest that minimum wages became more binding and/or better enforced after the 2004 Reform, generating a significant cost shock for firms. We hence exploit this Reform to examine how Chinese firms reacted to higher and tighter minimum

[12]The coefficient on initial wage alone might also capture some reversion-to-the-mean effects when firm-level wages are subject to temporary shocks or measurement error. As long as these effects remain constant over time, the coefficient on the interaction term adequately provides information on how the 2004 minimum wage Reform affected the convergence between low- and high-wage firms.

wages.

3.3 The measure of exposure to minimum wages and descriptive statistics

We here provide some descriptive statistics on the Chinese firms that were exposed to minimum-wage hikes. As already mentioned, our definition of exposed firms is constrained by data limitations. We follow previous work and define "exposed firms" as those whose average wage at t-1 was below the local minimum wage set at time t (the same estimation logic appears in the respective analyses of Indonesian and British data in Harrison and Scorse, 2010, and Draca et al., 2011, respectively). The rationale behind this definition is that these firms are obliged to raise their baseline wages in order to comply with the new city-level minimum wage. In the absence of information on worker-level wages, our exposure measure is potentially noisy: in reality, some fraction of employees will not be exposed to the minimum-wage rise in "exposed" firms and *vice versa* for "non-exposed" firms. In the econometric analysis, we will account for this by distinguishing at some point exposed firms whose average wage at t-1 was far from the future minimum wage at t, firms whose wages were closer to this level, and non-exposed firms whose average wage at t-1 was just above the future minimum wage.

Table A-1 in the Appendix presents the descriptive statistics on survival and average wage growth for exposed and non-exposed firms, both pre- and post-Reform. The proportion of firms that survive from one year to the next after the Reform is lower for exposed firms (84%) than for non-exposed firms (89%).[13] Furthermore, over the 2004-2007 period, firm-level average wage growth is significantly higher for exposed firms, at 0.54 log points, as compared to only 0.11 log points for firms with higher initial average wages. A similar gap is found for median firm-level average wage growth. Finally, exposed firms experience higher productivity and lower employment growth after the Reform. The difference between exposed and non-exposed firms is generally slightly less striking in the years before the Reform period, except for employment growth where there is an even more negative differential between exposed and non-exposed firm. These simple descriptive statistics suggest that the gap in terms of performance growth between exposed and non-exposed firms changed following the Reform. Our econometric analysis below will try to assess whether these changes can be interpreted as causal. By way of contrast, note that the average growth rate of the minimum wage the firms face in their city in a given year was roughly the same for exposed and non-exposed firms over the two periods (even though, intuitively, higher for both types of firms during

[13]The survival rates we find here are a little higher but not that different from the plant-turnover rates found in other developing countries (Tybout, 2000).

the Reform period); this suggests that there was no systematic difference in the geographic distribution of exposed and non-exposed firms in our sample.

Table A-2 in the Appendix shows average firm characteristics in both the pre- and post-Reform periods. Average wages are between 2.5 and 3.5 times higher in non-exposed firms compared to exposed firms. Non-exposed firms are also much more productive, larger and more likely to export.

Finally, Table A-3 displays the share of exposed firms over the entire period at the 2-digit industry level. There is substantial heterogeneity across sectors in the proportion of firms whose average wage at t-1 was lower than the city-level minimum wage at t. As expected, there are fewer exposed firms in sectors with greater skill intensity such as the Manufacture of Telecommunication Equipment & Computers and the Manufacture of Instruments. The percentage of exposed firms is higher in Agri-food industries and lower skill-intensity manufacturing sectors such as the paper or textile industries.

4 Empirical strategy

Our empirical approach considers the introduction of more restrictive minimum wages in 2004 and estimates difference-in-differences. We compare the relative performance of "exposed" and "non-exposed" firms within cities and sectors before and after the 2004 Reform, accounting for firm-level time-invariant characteristics via firm fixed effects. It is important to note that the set of exposed firms changes over time: the set of firms whose average wage in the preceding year was below the current local minimum wage is different in each year. Exposure status is also not constant over time for a given firm, so that the exposure variables will not be multicollinear with the firm fixed effects.

Our baseline specification can be written as follows:

$$\mathrm{Y}^{f}_{c,k,t} = \alpha\,\mathrm{Exposed}^{f}_{t} + \beta\,\mathrm{Exposed}^{f}_{t} \times \mathrm{Reform}_{t} + Z^{f}_{t-1} + \mu_{f} + \nu_{c,t} + \kappa_{k,t} + \epsilon^{f}_{c,k,t} \qquad (1)$$

The firm outcomes $\mathrm{Y}^{f}_{c,k,t}$ are in turn the average wages, employment, productivity, value-added and profitability of firm f in city c and sector k at time t.

As noted above, $\mathrm{Exposed}^{f}_{t}$ is a dummy for the firm's average wage at t-1 being below the local minimum wage at t. Reform_{t} is a dummy for observations from 2004 onwards, i.e. coming from the post-Reform 2004-2007 period.[14] Our coefficient of interest is β. This measures the gap in performance growth between exposed and non-exposed firms in the post-Reform period (the first difference), relative to the exposed-non exposed gap in the pre-

[14] We do not need to introduce a dummy for post-Reform in the regression, as this would be collinear with our city-year and sector-year fixed effects.

Reform years (the second difference). Note however that the "total" performance differential between exposed and non-exposed firms after the Reform is measured by the sum $\alpha+\beta$. In the presence of firm fixed effects, μ_f, the estimate of β comes from sample firms that switch status from non-exposed to exposed and *vice versa* in 2004, 2005, 2006 and 2007; firms that are always exposed or never exposed to minimum-wage hikes over the period have an exposure status that is entirely absorbed by the firm fixed effects. We hence examine the extent to which firms' response to minimum-wage increases changes following policy-enforcement tightening in 2004. The specification in Equation (1) exploits yearly variations in the data and thus focuses on the immediate effects of the minimum-wage changes. Section 6.3 will also consider medium-run effects.[15]

There are a number of endogeneity issues in this difference-in-differences that we now address. First, exposed firms might have particular characteristics, in terms of their size and productivity for example, that also help determine their subsequent performance. We thus control for Z^f_{t-1}, a set of firm-level controls including employment, the capital-labor ratio, labor productivity, and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms in t-1.[16] Moreover, controlling for firm fixed effects and focusing on how the gap between "exposed" and "non-exposed" firms changes pre- and post-Reform will control for any time-invariant unobserved characteristics that are common to the firms that are exposed at some point over the 1998-2007 period.

We might also worry that local authorities set the minimum wage according to the local business cycle. As noted in Section 2.1, this is actually explicitly encouraged in the national guidelines. We address this issue by adding a number of fixed effects. We include city-year fixed effects $\nu_{c,t}$ to capture any time-varying shocks affecting both the minimum-wage decisions of local authorities and firm performance in city c at time t. We also include sector-year fixed effects $\kappa_{k,t}$ to control for shocks that affect all firms in a given sector in a given period. Our final sample covers 480 sectors and 261 cities over the 1998-2007 period. The sectors k are defined using the Chinese sectoral classification at the 4-digit level.

Finally, it could be that local authorities set minimum wages based on the particular business cycles of low-wage firms. The inclusion of firm-level fixed effects and firm-level time-varying controls partly addresses the different dynamics of firm over time. We carry

[15] These correspond to a maximum horizon of three years for the effect of exposure to minimum-wage hikes in the post-Reform period, i.e. the effect of being exposed in 2004 on firm-level performance up to 2007.

[16] The dummies vary over time, hence are not absorbed in the firm-fixed effects. We do not control for the firm-level average wage in t-1 as exposed firms are by definition the lowest-wage firms in their city. Among all of these control variables, employment in t-1 is key. Not controlling for it affects the results. On the opposite, controlling for employment only (without the other firm-level controls) gives results almost identical to our benchmark results. This is because the correlation between initial average wage (partly captured by the exposed dummy) and the other initial characteristics of the firms is not so high. In particular, firm-level employment appears to be a significant determinant of performance growth which needs to be controlled for to adequately measure the effect of the Reform.

out additional exercises to address this issue. In particular, we trim low- and high-wage firms to ensure greater comparability between our "exposed" and "non-exposed" firms, and test for pre-existing trends. We also rely on a Bartik (1991)-type instrument to account for any remaining endogeneity of the Exposed_t^f dummy to local shocks affecting low-wage firms. The results are robust to all of these checks.

We cluster standard errors at the city level to account for possible autocorrelation between firms from the same city (Moulton, 1990).

5 Results

This section discusses the results regarding the short-run effects of the 2004 Reform.

5.1 Baseline results

Table 4 shows the results from the estimation of Equation (1). Firm-level average wages are the dependent variable in column (1). The following columns report the estimates for firm-level employment, labor productivity, value-added, TFP and profitability.

Our key variable of interest is the interaction between the firm-level Exposed dummy and the Reform dummy. This shows the repercussions from the tightening of minimum-wage enforcement in 2004. Controlling for initial firm characteristics, the negative and significant coefficients on the Exposed dummy on its own indicate that exposed firms under-performed before the 2004 Reform compared to non-exposed firms in every dimension, including wages. In particular, there was no catching-up in wages for exposed firms suggesting, together with the statistics in Section 3.2 on (the absence of) compliance, that these firms were not put under much pressure by the minimum-wage legislation. Had minimum wages rules been enforced, we would have seen a positive coefficient on the exposed dummy, at least in the wage equation. In the remaining tables of the paper, we will not show the coefficients on the Exposed dummy in order to lighten the presentation.

This situation is completely reversed after the 2004 Reform, as demonstrated by the positive coefficient on the interaction between Exposed and Reform in column (1) of Table 4: the 2004 Reform led to a relative rise in average wages in the surviving exposed firms. All else equal, the average wage gap between exposed and non exposed firms rose by 13.5% ($\simeq e^{0.127}$-1) following the Reform compared to its pre-Reform level: while exposed firms tended to experience more negative trends before the Reform, they seem to be forced to catch up in terms of wages after the Reform so that they have almost no differential anymore compared to non-exposed firms ($e^{0.127-0.100}$-1$\simeq$ 2.7%). The F-test shown at the foot of each column tests the difference in trends between exposed and unexposed firms after the reform. The

null hypothesis is that the sum of the coefficients on the exposed dummy and the exposed × Reform dummy equals zero. The probabilities (below 0.05) indicate that this equality is rejected at the 5% confidence level (with the exception of profitability). Our estimates clearly indicate that the 2004 Reform was binding and put more wage pressure on low-wage firms than before. The rise in local minimum wages thus allowed laggard firms to achieve wage growth trends that are superior to those experienced by other enterprises.

Column (2) considers the possible employment repercussions of this non-negligible wage shock. The negative coefficient on the Exposed dummy again suggests that exposed firms had structurally lower employment growth than non-exposed firms. However, we find no significant additional employment effect after the Reform: firms that were exposed to the minimum-wage hike in 2004 onwards did not react by hiring relatively less or firing relatively more workers on average than exposed firms did pre-Reform.

The following three columns in Table 4 help us understand why higher minimum wages increased labor costs without reducing employment. Columns (3)-(5) show the estimates of Equation (1) for labor productivity (value added per worker), value-added and firm-level total factor productivity (calculated following the procedure suggested by Levinsohn and Petrin, 2003). Higher minimum wages were associated with significant relative productivity gains in exposed firms, with their value-added rising faster than that in non-exposed firms. The estimates in column (3) suggest an increase in the labor-productivity premium of exposed firms post-Reform of 3.7%. This is sizeable, equal to 15% of the average gap in annual labor productivity growth between exposed and non-exposed firms over the 2004-2007 period.[17] Since the gap was negative before the Reform (-2.3%), this means that all else equal, following the Reform the labor productivity trends in exposed firms were higher than those of non-exposed firms by 1.4%($\simeq e^{0.037-0.023}$).

Last, column (6) considers the effect of the tightening of minimum wages on firm profitability, finding no significant effect of the Reform in this dimension.[18]

A number of channels can explain why productivity adjusts to the minimum wage. Firms can substitute skilled for unskilled workers for example (with total employment unchanged).[19] We do not have information on the skill composition of firms in each year and thus cannot test this. However, we show in Section 7.1 that other (non exclusive) explanations are also at play, such as capital investment and changes in management practices.

[17] As shown in Table A-1 in the Appendix, labor productivity grows each year by 0.38 and 0.16 log points on average in exposed and non-exposed firms respectively, so that $\frac{0.037}{e^{0.38-0.16}-1} \simeq 0.15$.

[18] This does not come as a surprise given the size of the effects; considering that the share of wages in firm-level value-added for the median firm in our sample is 25%, the 3.8 ($e^{0.037}$-1) percentage point increase in labor productivity compensates for the 13.5 ($e^{0.127}$-1) percentage point increase in wages, leaving profitability unchanged: $0.25 \times 0.135 \simeq 0.034$.

[19] Giuliano (2013) finds substitution effects between high- and low-quality teenagers and young adults in the US retail industry.

Table 4: Minimum wages and firm outcomes: baseline

Dependent variable	Firm outcome$_t$					
	Ln wage (1)	Ln employment (2)	Ln labor productivity (3)	Ln value added (4)	Ln TFP (LP) (5)	Profit over output (6)
Exposed Firm	-0.100^a	-0.072^a	-0.023^a	-0.094^a	-0.078^a	0.014
	(0.006)	(0.004)	(0.007)	(0.006)	(0.006)	(0.025)
Exposed Firm × Reform	0.127^a	-0.005	0.037^a	0.032^a	0.031^a	0.010
	(0.009)	(0.006)	(0.008)	(0.007)	(0.007)	(0.024)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
F-test $\alpha + \beta = 0$	16.68^a	213.79^a	4.42^b	144.07^a	92.43^a	0.97
Proba > F	0.001	0.001	0.036	0.001	0.001	0.325
Observations	1,207,197	1,207,197	1,205,208	1,205,208	1,102,080	1,205,419
R-squared	0.75	0.93	0.78	0.86	0.85	0.35

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm at t-1 being lower than the local minimum wage at t. Reform is a dummy for t≥2004. The Reform dummy is absorbed in the sector-year and city-year fixed effects. The F-test shown at the foot of each column tests the equality in absolute terms of the estimated coefficients on Exposed Firm and Exposed Firm × Reform. The probabilities (below 0.05) indicate that this equality is rejected at the 5% confidence level.

Table A-4 in the Appendix checks that our results continue to hold when discarding firms at the top and bottom of the wage distribution to ensure greater comparability between our "exposed" and "non-exposed" firms. There is no established rule on how to define the correct group of firms; we decide to drop firms whose average wage (normalized by the median observed in the city) is above the median observed among non-exposed (high-wage) firms, and below the first decile of exposed (low-wage) firms, as very low average wages may at least partly reflect misreporting. The results are qualitatively similar to those in our benchmark analysis.

5.2 The degree of exposure

We noted above that the "exposed" dummy is a noisy measure of exposure to minimum wages, as some fraction of employees will not be exposed to higher minimum wages in these "exposed" firms and some employees will be in "non-exposed" firms. Despite the lack of data on the distribution of wages within firms, we propose to extend our empirical approach beyond the simple difference between exposed and non-exposed firms, and consider heterogeneity in treatment intensity. In Table 5 this is given by the difference between the firm's baseline average wage and the city minimum wage: the greater is this difference, the more exposed the firm is (with a larger share of low-wage workers, or lower wages) and the larger we expect the wage and productivity effects to be. We thus further decompose

Table 5: Intensity of exposure

Dependent variable	Firm outcome$_t$					
	Ln wage	Ln employment	Ln labor productivity	Ln value added	Ln TFP (LP)	Profit over output
	(1)	(2)	(3)	(4)	(5)	(6)
Just above the threshold firm	-0.076^a	-0.040^a	-0.024^a	-0.064^a	-0.058^a	-0.021
	(0.004)	(0.003)	(0.005)	(0.006)	(0.007)	(0.051)
Slightly exposed firms	-0.109^a	-0.069^a	-0.031^a	-0.099^a	-0.085^a	0.022
	(0.005)	(0.004)	(0.007)	(0.007)	(0.007)	(0.040)
Highly exposed firms	-0.143^a	-0.118^a	-0.027^b	-0.143^a	-0.118^a	-0.014
	(0.011)	(0.007)	(0.011)	(0.010)	(0.009)	(0.031)
Just above the threshold firm × Reform	0.082^a	-0.014^a	0.031^a	0.017^b	0.021^a	0.026
	(0.005)	(0.004)	(0.007)	(0.008)	(0.008)	(0.049)
Slightly exposed firm × Reform	0.126^a	-0.011^b	0.040^a	0.029^a	0.031^a	-0.008
	(0.009)	(0.005)	(0.009)	(0.008)	(0.008)	(0.035)
Highly exposed firm × Reform	0.180^a	-0.007	0.051^a	0.044^a	0.041^a	0.053
	(0.014)	(0.009)	(0.012)	(0.011)	(0.011)	(0.036)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,207,197	1,207,197	1,205,208	1,205,208	1,102,080	1,205,419
R-squared	0.75	0.93	0.78	0.86	0.85	0.35

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. Reform is a dummy for t≥2004. The Reform dummy is absorbed in the sector-year and city-year fixed effects.

exposed firms into two groups: "Highly-exposed", whose initial average wage is below the median wage in exposed firms in the city, and the other "Slightly-exposed " firms. We also investigate what happens to firms with average wages only slightly above the future city-level minimum wage, since a significant fraction of their employees are almost certainly exposed to the minimum wage. We here look at the bottom tier of non-exposed firms, i.e. firms whose average wage at t-1 was between 1 and 1.3 times the subsequent local minimum wage at t: we refer to these as "Just above the threshold". If the firm's initial average wage is a good proxy for its exposure to the minimum-wage hike, we expect our key coefficients to fall in size from "Highly exposed" to "Just above the threshold".

The results in Table 5 confirm this intuition. "Highly-exposed" firms are more affected by the Reform than "Slightly-exposed" firms in terms of average wages and productivity. We also find significant but smaller coefficients for "Just above the threshold" firms. This is consistent with two (non mutually exclusive) explanations: firms with average wages just above the subsequent city-level minimum wage have some workers who are directly affected by the new minimum wage; or the firms are indirectly affected via spillover effects of the Reform on wages just above the minimum level, as found in a number of papers in the minimum-wage literature (e.g. Grossman, 1983; Card and Krueger 1995; Neumark and Wascher, 2008; Autor et al., 2016).

From a quantitative perspective, average wages rise by 8.5% following the Reform for firms with an average wage at t-1 that is between 1 and 1.3 times the subsequent local minimum wage at t; this premium rises to 13.4% points for "Slightly-exposed firms" and 19.7% in "Highly-exposed firms" (taking the exponential of the coefficient). The same ranking is found for productivity. We do find a small negative employment effect for firms just above and below the minimum wage threshold, i.e. those for which productivity increases the least following the Reform. By contrast, the firms that are the most exposed to minimum wage increases do not adjust employment after 2004. We thus here qualify our initial assessment of the effect of the Reform on employment in surviving firms: while there is no average effect, some exposed firms did experience modest falls in employment. The effects on employment and labor productivity combine into an overall positive effect on value added in exposed firms, with this effect rising, as for wages and productivity, with exposure. The estimated effect on profitability remains insignificant for all of the exposed firms, whatever their degree of exposure.

6 Robustness checks

We show in this section that our results are robust to a number of checks regarding the validity of our identification assumptions. We also investigate the medium-run impacts of the 2004 Reform.

6.1 Pre-existing trends

Our setting here differs from the traditional framework where a policy change happens in a specific year, and affects once and for all a particular group of firms. In our case, there are two groups of firms in each year, those exposed and non-exposed to minimum-wage hikes, with the composition of these two groups changing from one year to the next depending on movements in city-level minimum wages and firm-level average wages. As minimum wages did not bind and were not enforced before the 2004 Reform, we identify the change in the average performance gap between the two groups of firms in the years after the Reform, as compared to the average gap between exposed and non-exposed firms in the pre-Reform period. Our difference-in-differences approach assumes that, all else equal, in the absence of the 2004 Reform this gap would have remained stable over the period. We cannot directly test this, but here propose two complementary approaches to address the question of pre-existing trends.

First, we add another dummy to identify the firms that are exposed to minimum-wage hikes in the two years preceding the 2004 Reform, i.e. firms whose average wage in 2001 or

2002 was lower than the subsequent city-level minimum wage in 2002 or 2003 respectively. By doing so, we can check whether these firms had already started to exhibit the patterns of results that we find for firms exposed to minimum-wage hikes after the Reform. In this specification, the coefficient on the "Exposed" dummy captures the gap between exposed and non-exposed firms in the years 1999-2001, which is now taken to be the reference period. The results in Table 6 show that the wage, productivity and value-added gains in exposed firms only appeared from 2004 onwards. Overall, the estimated coefficients on our key variable, Exposed × Reform, are virtually unchanged, confirming that the effects captured by our double differences reflect a change in the performance growth of exposed firms exactly coinciding with the 2004 Reform.

Table 6: Accounting for pre-existing trends (1)

Dependent variable	Firm outcome$_t$					
	Ln wage (1)	Ln employment (2)	Ln labor productivity (3)	Ln value added (4)	Ln TFP (LP) (5)	Profit over output (6)
Exposed Firm	-0.082^a	-0.087^a	-0.009	-0.098^a	-0.082^a	0.021
	(0.008)	(0.007)	(0.009)	(0.007)	(0.008)	(0.039)
Exposed Firm × 2002-03 Pre-reform	-0.038^a	0.032^a	-0.028^a	0.008	0.007	-0.016
	(0.008)	(0.009)	(0.010)	(0.010)	(0.010)	(0.032)
Exposed Firm × Reform	0.109^a	0.010	0.023^b	0.036^a	0.035^a	0.003
	(0.010)	(0.008)	(0.010)	(0.008)	(0.009)	(0.036)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,207,197	1,207,197	1,205,208	1,205,208	1,102,080	1,205,419
R-squared	0.75	0.93	0.78	0.86	0.85	0.35

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm at t-1 being lower than the local minimum wage at t. Reform is a dummy for t≥2004. "2002-03 Pre-reform" is a dummy for 2002 and 2003, i.e. the two years prior to the Reform. The Reform and Pre-reform dummies are absorbed in the sector-year and city-year fixed effects.

Table 7 proposes an exercise that is more focused on the dynamics of exposed firms before they became exposed. We here look at the performance gap in t-2 between the firms identified as exposed and non-exposed in t, controlling for their characteristics in t-1. In practice, we reproduce the results in Table 4, taking the dependent variable at t-2 instead of t. We hence ensure that the change in the performance gap between exposed and non-exposed firms post-Reform does not reflect pre-existing trends for these same firms before they were exposed in 2004-2007. The interaction term Exposed × Reform now measures the performance gap between exposed and non-exposed firms two years before the date at which exposure changed post-Reform. The coefficients on the interaction term are insignificant in this specification (except for the coefficient on wages, which is negative). Overall, our benchmark results do

not reflect pre-existing trends in the firms that became exposed post-2004.

Table 7: Accounting for pre-existing trends (2)

Dependent variable	Firm outcome$_{t-2}$					
	Ln wage	Ln employment	Ln labor productivity	Ln value added	Ln TFP (LP)	Profit over output
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed Firm	0.041^a	-0.083^a	-0.003	-0.086^a	-0.070^a	-0.014
	(0.009)	(0.008)	(0.007)	(0.006)	(0.006)	(0.009)
Exposed Firm × Reform	-0.028^a	0.016	-0.019	-0.003	-0.007	0.004
	(0.010)	(0.011)	(0.012)	(0.008)	(0.009)	(0.015)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	863,750	863,750	862,780	862,780	784,192	863,261
R-squared	0.67	0.92	0.75	0.85	0.84	0.17

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm at t-1 being lower than the local minimum wage at t. Reform is a dummy for t≥2004. The Reform dummy is absorbed in the sector-year and city-year fixed effects.

As an additional check for the accuracy of our specification, we consider the possible repercussions of a so-called Ashenfelter's dip problem,[20] which is quite common in the evaluation literature. Our definition of exposed firms based on average wage in year t-1 runs the risk of inappropriately including firms that face a transitory negative shock just before exposure. Indeed, firms that experience a bad shock in t-1 are more likely to exhibit an average wage that turns out to be smaller than the subsequent minimum wage in their city, and thus to be defined as exposed. The rise in firm-level average wage or productivity upon exposure could then well be explained by simple reversion to the mean for this subset of firms. In results (available upon request), we run our baseline estimates after dropping the observations for which a firm is not exposed, but will be exposed the year after; this removes the observations responsible for the Ashenfelter's dip issue. Point estimates are barely affected.

6.2 IV strategy

Even though our use of double differences is validated by the above pre-existing trend analyses, we may still worry that the minimum-wage hikes post-Reform reflect local shocks that occurred at the same time as the Reform, and are particular to low-wage firms (the city-year fixed effects already control for local shocks that are common to all firms in a given city). Since there is some noise in the exposure measure, we may also see attenuation bias in our fixed-effects estimates. We thus complement the difference-in-differences with IV estimation.

[20]See the survey by Heckman et al. (1999).

We appeal to the institutional features of the minimum-wage rules to instrument the "Exposed" dummy, assuming that the national guidelines were not designed to reflect particular local conditions. As explained in Section 2.1, China's Minimum Wage Regulations stipulate that minimum wages be no less than 40 percent of the average local wages.

One predictor of the local minimum wage could then be this lower bound (i.e. 40% of local average wages).[21] However, local average wages are themselves directly affected by minimum wages and partly reflect the potential shocks affecting low-wage firms that we wish to eliminate. As such, following the logic of Bartik (1991), we predict local average wages based on the city sectoral composition and the past wage growth observed for each sector in the rest of China. Equation (2) sets out the formula for this predicted local wage in city c at time t:

$$\widetilde{\text{Wage}}_t^{c^p} = \sum_k \frac{L_{c^p,k,t-2}}{L_{c^p,t-2}} \times \frac{\text{Wage}_{China\backslash p,k,t-1}}{\text{Wage}_{China\backslash p,k,t-2}} \times \text{Wage}_{c^p,k,t-1} \tag{2}$$

where $\text{Wage}_{China\backslash p,k,t-1}$ is the average wage in sector k at time t-1 across all Chinese locations bar the province p of city c^p, $\text{Wage}_{c^p,k,t-1}$ is the average wage in city c (from province p) and sector k at time t-1, and $\frac{L_{c^p,k,t-2}}{L_{c^p,t-2}}$ is the share of sector k in the overall employment of city c at time t-2.

$\widetilde{\text{Wage}}_t^{c^p}$ is thus the weighted sum of local sectoral wages in t-1, which we assume to grow between t-1 and t at the same pace as in the rest of China between t-1 and t-2. Since the information used to construct this instrument is based on the past structure of economic activities at the local level and past changes in wages in the rest of the country, $\widetilde{\text{Wage}}_t^{c^p}$ is assumed to be unrelated to any shocks specifically affecting local labor supply and demand, and thus local wages between t-1 and t. Our instrument for "Exposed" is then a dummy for firm average wages in year t-1 being lower than $0.4 \times \widetilde{\text{Wage}}_t^{c^p}$. The Exposed×Reform dummy is instrumented by the interaction between this IV and the dummy for the Reform period.[22]

The first stage of the IV results appears in Table A-5. The exposed dummy and the instrument described above are positively correlated. The size of the coefficient (0.739) is reassuring, showing that the IV is not completely collinear with the exposed dummy. We check that our instrumental variables are not weak. The F-tests associated with our instruments appear at the bottom of Table A-5. We also report the underidentification test at the bottom of Table 8. All of these tests pass.

The two-stage least squares estimates in Table 8 confirm the previous findings for all the

[21] Note that, in the same vein, Autor et al. (2016) use real statutory minimum wages as an instrument for the effective State-level minimum wage in the US.

[22] Given our definition of the instrument and the data we have, we can take this approach on data from 2000 to 2007, instead of 1999 to 2007 in our benchmark results.

Table 8: Minimum wages and firm outcomes: IV

Dependent variable	Firm outcome$_t$					
	Ln wage	Ln employment	Ln labor productivity	Ln value added	Ln TFP (LP)	Profit over output
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed Firm	-0.114^a	-0.105^a	-0.015	-0.118^a	-0.104^a	0.001
	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.023)
Exposed Firm × Reform	0.164^a	-0.023^b	0.037^a	0.015	0.023^b	0.035
	(0.012)	(0.010)	(0.012)	(0.010)	(0.010)	(0.039)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test	55.2^a	55.2^a	55.2^a	55.2^a	55.7^a	55.1^a
F-test $\alpha + \beta = 0$	29.33^a	145.39^a	3.60^b	108.32^a	80.02^a	0.46
Proba >F	0.001	0.001	0.059	0.001	0.001	0.60
Observations	1,111,660	1,111,660	1,109,866	1,109,866	1,102,080	1,110,089
R-squared	0.74	0.93	0.78	0.86	0.85	0.43

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm in t-1 being lower than the local minimum wage in t. Reform is a dummy for t≥2004. The Reform dummy is absorbed in the sector-year and city-year fixed effects. The instrument used for the "Exposed" dummy in the IV procedure is a dummy for firm average wages in t-1 being below the predicted minimum wage in t based on the 40% rule (see the text). The underidentification test is based on the Kleibergen-Paap rk LM statistic, with ^{a} indicating that the p-value (Chi-sq(2)) is below 0.01, suggesting that underidentification is rejected. The F-test shown at the foot of each column tests the equality in absolute terms of the estimated coefficients on Exposed Firm and Exposed Firm × Reform. The probabilities (below 0.05) indicate that this equality is rejected at the 5% confidence level.

firm-level outcomes. The coefficients are broadly unchanged.

Overall, taking the standard errors into account, the IV estimates are generally not significantly different from the benchmark results. Only for employment do we find now a negative effect, more in line with the heterogeneous effects depending on the initial firm average wage in Table 5. However, the negative employment impacts are relatively small and are only a fraction of the labor-productivity gains, so that the net effect on firm-level valued added is non-negative.

6.3 Medium-run effects

Our analysis so far has focused on the immediate effects of exposure to the 2004 minimum wage hikes. However, if adjusting employment is costly or some productivity improvements take time to materialize, we may observe some effects beyond the year of exposure. This section thus considers the medium-run impact of the Reform. To do so, we run our benchmark regression replacing the dependent variable by its values at t+1, t+2 and t+3 successively,

instead of at t.[23]

Table 9 shows the estimates for firm performance at t+1 (both fixed effects and IV). The results at t+2 and at t+3 appear in Tables A-6 and A-7 in the Appendix respectively, and are qualitatively similar. The medium-run average wage premium is very similar to that estimated in the short run. The average wage of exposed firms then increases upon exposure and subsequently evolves in the same way as in non-exposed firms. On the contrary, the productivity gains are larger in the medium run than in the short run. Some of the productivity gains for exposed firms after the Reform take one to three years to materialize. This is also the case for job losses. Employment in exposed firms, which is only modestly affected just after the Reform, declines more clearly in subsequent years (there is a significant negative coefficient in both the fixed-effects and IV regressions). This lagged employment response is consistent with the results in Wang and Gunderson (2011) using semi-aggregated Chinese data and Neumark (2001) for the US. This effect does however remain small in size, and below the effect on labor productivity, so that firm-level value added actually rises in the medium run following the Reform for surviving exposed firms. Finally, the absence of any profitability impact is confirmed.

That firm-level productivity and value-added continue to increase after the Reform beyond the year of exposure confirms and reinforces our interpretation of the short-run results: surviving exposed firms achieved continuous productivity gains after the 2004 Reform that allowed them to absorb the wage increase with limited job losses, and without reducing profitability. Our point estimates suggest an increase by 10% of the relative labor-productivity of exposed firms after the Reform. Combined with a decrease in the relative number of jobs by *circa* 5%, this allows exposed firms' value added to increase relatively to non exposed firms by around 5% on average in the three years following exposure to the minimum wage Reform.

7 Channels and alternative explanations

In this section we dig deeper into the mechanisms behind our short- and medium-run productivity effect, and discuss a number of alternative explanations.

7.1 The channels underlying productivity gains

We here present two types of results that further confirm that productivity was surviving firms' main margin of adjustment following the 2004 Reform. The first of these extends

[23]The time-span of our data (1998-2007) allows us to examine the effects for up to three years after the Reform (2005, 2006 and 2007).

Table 9: Medium-run effects - 1-year lag

Dependent variable	Firm outcome$_{t+1}$											
	Ln wage		Ln employment		Ln labor productivity		Ln value added		Ln TFP (LP)		Profit over output	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposed Firm	-0.001	0.019^{a}	-0.038^{a}	-0.045^{a}	-0.018^{a}	-0.020^{b}	-0.055^{a}	-0.065^{a}	-0.050^{a}	-0.063^{a}	-0.001	-0.016
	(0.005)	(0.007)	(0.004)	(0.005)	(0.006)	(0.009)	(0.006)	(0.010)	(0.006)	(0.010)	(0.009)	(0.025)
Exposed Firm × Reform	0.112^{a}	0.164^{a}	-0.026^{a}	-0.052^{a}	0.055^{a}	0.082^{a}	0.029^{a}	0.031^{b}	0.037^{a}	0.045^{a}	0.058	0.104
	(0.006)	(0.009)	(0.006)	(0.009)	(0.009)	(0.013)	(0.009)	(0.012)	(0.008)	(0.011)	(0.046)	(0.092)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test		53.2^{a}		53.2^{a}		53.3^{a}		53.3^{a}		53.6^{a}		53.2^{a}
Observations	862,498	790,048	862,498	790,048	860,815	788,554	860,815	788,554	783,268	785,487	860,773	788,527
R-squared	0.72	0.73	0.92	0.93	0.79	0.79	0.86	0.87	0.86	0.86	0.45	0.32

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm at t-1 being lower than the local minimum wage at t. Reform is a dummy for t≥2004. The Reform dummy is absorbed in the sector-year and city-year fixed effects. The results from the IV regressions appear in the even columns following the strategy described in Section 6.2. The instrument used for the "Exposed" dummy in the IV procedure is a dummy for firm average wages in t-1 being below the predicted minimum wage in t based on the 40% rule (see the text). The underidentification test is based on the Kleibergen-Paap rk LM statistic, with ^{a} indicating that the p-value (Chi-sq(2)) is below 0.01, suggesting that underidentification is rejected.

the analysis to firms that did not survive. Under the productivity channel, firms that were unable to produce the required productivity improvements exited the market, so that the 2004 Reform reduced firm-survival probability.

The first three columns of Table 10 show the results from the estimation of Equation (1) with the dependent variable being the probability that the firm fails to survive between t-1 and t. Column (1) uses the dichotomic exposure measure, column (2) the categories of the various degrees of exposure, and column (3) an IV estimation of the type described in Section 6.2.[24]

In column (1), the difference-in-differences coefficient on the interaction term is positive and significant, confirming that, compared to non-exposed firms, the probability that exposed firms exit increases following the Reform. In column (2), the coefficients are positive for the all three exposure categories, but are smaller for the "Just above the threshold" firms. This confirms that the higher minimum wages and greater enforcement from the 2004 Reform reduced firm survival, especially for the firms that initially paid the lowest wages. The IV results in column (3) are of the same nature. As exposed firms are on average less productive than the others (see Section 3.3), the greater exit probability for exposed firms after 2004 suggests a cleansing effect of the 2004 Reform.

As for the surviving firms, a number of potential channels can lie behind productivity. In line with efficiency wages, higher wages can increase workers' job satisfaction and effort, and reduce labor-force turnover within firms, increasing overall productivity.[25] The cost shock could also trigger the adoption of better management or organizational practices. Note that the adoption costs of the latter may not be monetary. Recent work on South Asia has identified changes in management and production practices that can produce substantial productivity gains without requiring major investments in particular machinery or any other form of physical capital (Bloom et al., 2013; Atkin et al., 2015). Unfortunately, the firm-level data we use here do not include any information on job satisfaction, innovation or management practices. We can take an indirect approach and consider inventory reduction as an indicator of a change in management practices aimed at reducing costs and increasing productivity. The positive effect of inventory reduction on productivity has, for example, been shown in Japanese (Lieberman and Demeester, 1999) and Indian (Bloom et al., 2013) firms. Columns (4) to (6) of Table 10 show the relationship between the 2004 minimum wage Reform and the firm-level inventory to revenue ratio. The negative estimated coefficients on the interaction terms between our proxies for exposure and the Reform dummy suggest that

[24]Note that, except for survival, the fixed-effects and IV results in Table 10 are similar, since the 95% confidence intervals partly overlap.

[25]Bloom et al. (2015) explain that the rate of worker turnover has been the subject of growing concern in China over the past fifteen years, and Dube et al. (2007) use US data to show that minimum wages reduce turnover in the fast-food industry.

Table 10: Minimum wage channels

Dependent variable	Firm outcome$_t$											
	Exit			Ln Inventory over revenue			Ln Capital over labor			Ln Cash over value-added		
Estimator	OLS	OLS	IV	OLS	OLS	IV	OLS	OLS	IV	OLS	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposed Firm	0.026^a		0.034^a	0.007		0.005	-0.049^a		-0.063^b	0.042^a		0.038^b
	(0.003)		(0.004)	(0.007)		(0.009)	(0.015)		(0.028)	(0.011)		(0.015)
Exposed Firm × Reform	0.022^a		0.045^a	-0.041^a		-0.038^b	0.108^a		0.214^a	-0.037^a		-0.043^b
	(0.004)		(0.005)	(0.011)		(0.016)	(0.022)		(0.040)	(0.014)		(0.020)
Just above the threshold firm		0.004^c			0.019^a			-0.050^a			0.030^a	
		(0.002)			(0.007)			(0.011)			(0.008)	
Slightly exposed firms		0.017^a			0.017^b			-0.042^b			0.046^a	
		(0.003)			(0.008)			(0.016)			(0.013)	
Highly exposed firms		0.047^a			0.003			-0.091^a			0.058^a	
		(0.005)			(0.009)			(0.022)			(0.017)	
Just above the threshold firm × Reform		0.019^a			-0.034^a			0.094^a			-0.030^b	
		(0.003)			(0.010)			(0.015)			(0.013)	
Slightly exposed firms × Reform		0.028^a			-0.048^a			0.107^a			-0.036^b	
		(0.005)			(0.013)			(0.026)			(0.016)	
Highly exposed firms × Reform		0.026^a			-0.053^a			0.174^a			-0.057^b	
		(0.005)			(0.015)			(0.027)			(0.023)	
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test			54.9^a			55.2^a			55.2^a			54.6^a
Observations	1,418,938	1,418,938	1,306,633	1,139,047	1,139,047	1,068,195	1,207,197	1,207,197	1,111,660	936,766	936,766	971,769
R-squared	0.39	0.39	0.40	0.76	0.76	0.76	0.86	0.86	0.87	0.62	0.62	0.61

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. "Exit" is a dummy for the firm being active in t-1 but not in t. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio (except in columns 7 through 9) and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm in t-1 being lower than the local minimum wage in t. The "Exposed" dummy on its own is also introduced in columns (1), (3), (4), (6), (7), (9), (10) and (12). The dummies for "Exposed", "Just above the threshold", "Slightly exposed" and "Highly exposed" firms are also included in columns (2), (5), (8) and (11), but the results are not shown. Reform is a dummy for t≥2004. The Reform dummy is absorbed in the sector-year and city-year fixed effects. The results for the IV regressions appear in columns (3), (6), (9) and (12), following the strategy described in Section 6.2. The underidentification test is based on the Kleibergen-Paap rk LM statistic, with ^{a} indicating that the p-value (Chi-sq(2)) is below 0.01, suggesting that underidentification is rejected.

inventory-management practices improved in firms exposed to minimum wage hikes after the Reform.

Part of the adjustment may also work via physical-capital investment. Acemoglu and Shimer (2000) show, for example, that higher-wage firms fill their vacancies faster, and so are willing to make larger irreversible investments in complementary inputs such as capital. We hence measure the effect of the Reform on the firm-level capital-labor ratio (fixed assets for production purposes over employment). The difference-in-differences coefficients in columns (7) to (9) of Table 10 are positive and significant.

Our findings thus suggest that the 2004 Reform reduced inventory and boosted the capital-labor ratio in exposed firms, so that at least part of the minimum-wage productivity gains might reflect better management practices and greater physical investment. One notable feature of the Chinese economy is its dysfunctional financial system (Allen et al., 2005). Pervasive financial imperfections force Chinese firms to finance investment via internal savings (Song et al., 2011; Guariglia et al., 2011). As such, any efficiency-promoting investments made following the minimum-wage rise should reduce cash balances. Columns (10) to (12) of Table 10 show the difference-in-differences estimation of the cash to value-added ratio.[26] As expected, we find that firm-level cash falls post-Reform for exposed compared to non-exposed firms.

All of these effects are also valid in the medium run, except for the reduction in the cash-to-value added ratio which is not significantly affected in the years following exposure. As such, when exposed firms decide to invest to cope with higher minimum wages, they do so quickly. Also, the higher exit probability associated with the Reform gradually vanishes in the medium run, consistent with the productivity improvements after a few years being such that the cost shock is absorbed and no longer affects firm survival.[27]

We may wonder why exposed firms did not make these investments before if they are profitable. There are a number of explanations. Duflo et al. (2011), Bloom et al. (2013), Beaman et al. (2014) and Atkin et al. (2015) describe multiple barriers to technology adoption in developing countries: the utility cost, incomplete information, present bias, time constraints, lack of attention, and the procrastination or misalignment of incentives within firms might all prevent or delay the adoption of apparently profitable innovations. This could be all the more true that the availability of cheap labor provides little incentive to pay adoption costs. In our case, the substantially higher labor costs from the 2004 minimum-wage Reform may have increased the value of adopting new and better technologies or management practices.

Another question is why the low-productivity firms that improve after the minimum wage

[26]Cash is defined as the average current asset balance deducting net account receivables and inventory.
[27]The results are available upon request.

Reform were not forced out of the market by the entry or expansion of more productive firms. The same question arises in Bloom et al. (2013) regarding Indian textile firms. They suggest the limited managerial span of control within the firm as a barrier to the expansion of the best-performing businesses. Combined with low entry rates, this explains why some low-productivity firms survive. In the Chinese case, political connections with leaders of the Communist Party might also be part of the explanation, as emphasized by Khandelwal et al. (2013) in the context of export-license allocation before China's WTO entry.

Note that this productivity adjustment to the minimum wage is probably larger in developing countries, in which inefficiencies remain pervasive (Hsieh and Klenow, 2009, Brandt et al., 2013). Firms' reactions to minimum wages in developed countries might be very different. For example, Draca et al. (2011) find no significant productivity effects from the introduction of the UK minimum wage. As in our analysis of China above, they find no significant negative employment effects: UK firms seem to absorb higher labor costs through lower profits.

7.2 Alternative explanations

We now ask whether there are alternative explanations of the positive firm-level productivity effects that we attribute to the 2004 minimum wage Reform. All of the tables cited in this section are presented in the Appendix.

The role of migrants

First, firms might substitute migrants for local workers in order to absorb the cost shock from higher minimum wages. It is well-known that migrant workers, who are often illegal in the cities where they live, work more hours, receive lower hourly wages, and are less well covered by welfare and fringe benefits (see Du and Pan, 2009 for example). They are also more likely to be employed without contracts (which is legal for periods under three months) or as informal workers (Song et al., 2016a and 2016b). Such adjustments through informal workers and migrants are sometimes mentioned in the interviews run by Wang and Gunderson (2015) in their qualitative analysis of the effects of minimum wages in China. As migrants are then overall "cheaper" than local workers, firms can cut costs by hiring more of them. If firms do not declare their (potentially illegal) migrant and informal workers in the NBS surveys, this substitution should lead to a strong negative effect of minimum wages on firm-level employment. This is inconsistent with what we have shown above. On the contrary, if firms do declare their migrant workers, employment in exposed firms will not change relative to other firms, while the composition of employment will do so. As migrants work more hours than do local workers, the total hours in exposed firms should rise relative to those in non-exposed firms, which could explain the rise in labor productivity.

We do not have direct information on migrants in the firms' workforce. We can however propose an indirect test by looking for a differential minimum-wage effect in low- and high-migration cities. Our measure of migration intensity is the ratio of migrants to residents in the overall population. The number of migrants is calculated as the number of people without a local residence permit (*hukou*).[28]. We calculate migration intensity at the city-level in 2000 and 2005 using, respectively, the population census of 2000 and the 2005 mini population census. The results appear in Table A-8 in the Appendix. We consider our two proxies of firm productivity, labor productivity and TFP, and show OLS and IV results. The difference in difference terms are further interacted with a dummy for cities with migration intensity above the national median in 2000 (columns 1 to 4) and in 2005 (columns 5 to 8). The results reveal no difference in the relative change in productivity between exposed and non-exposed firms following the Reform by local migration intensity.

Along the same lines, we check that the higher average baseline wages in exposed firms after the 2004 Reform do not mask a compensating fall in fringe benefits. This would in particular be compatible with the substitution of migrants for regular workers, as migrants receive fewer fringe benefits. The first three columns of Table A-9 show the results from the estimation of Equation (1) with total wages per employee, including fringe benefits, as the dependent variable. In columns (4) to (6) this latter is rather the level of fringe benefits (or welfare pay) provided by firms to their employees in t.

The results confirm that exposed firms have larger relative growth in average total wages following the Reform. The size of these estimated coefficients is comparable to those for average baseline labor pay. The results in columns (4) to (6) underline that fringe benefits did not fall to compensate for higher wages.

Last, Du and Pan (2009) analyze two waves of China Urban Labor Surveys data from 2001 and 2005. They find that, all else equal (in particular controlling for age, skills etc.), migrant workers are more likely to be paid under the hourly minimum wage. However, this low-wage gap between migrants and local workers was smaller in 2005 than in 2001: the "cost advantage" of migrant workers thus fell after the 2004 Reform. This fits one of the Reform's objectives, which was to improve migrant coverage in terms of labor standards.

Overall, these results do not provide any support for the hypothesis that exposed firms substituted migrants for local workers due to the higher and tighter minimum wages imposed by the 2004 Reform. They also show that, on average, there are no offsetting effects from lower fringe benefits after the Reform (which of course does not mean that this did not happen in some firms, as mentioned by some qualitative work).

[28]The hukou is a system of household registration which ties people to their original place of residence, essentially making migrant workers from the countryside illegal immigrants when they move to cities.

Number of hours worked

A second concern relates to the number of hours worked by employees in exposed firms. To absorb the cost shock of the 2004 Reform, firms that were the most exposed to higher minimum wages might have required their local and migrant workers to work longer hours. As we observe the number of employees, but not the hours they work, higher productivity post-2004 could reflect longer work hours in exposed firms. We cannot directly test this mechanism. However, Du and Pan (2009) show that work hours for both migrants and resident workers actually fell in China between 2001 and 2005. In 2001, migrants worked 73.4 hours per week on average in the informal sector and 60.8 hours in the formal sector, with the respective 2005 figures being 72.1 and 52.2. For local workers, these figures were 59.5 and 53.4 hours in 2001, and 44 and 43.5 in 2005. Despite these falling work hours, average firm-level output per worker rose by 25% between 2003 and 2005 in our data (46% in exposed firms and 21% in non-exposed firms, in both cases much faster than inflation). This could not have happened without better firm-level organization or greater worker efficiency. In addition, a recent contribution by Sun et al. (2015) shows that minimum wages reduced work hours in China, as in the previous UK results of Machin et al. (2003). This is in line with the qualitative evidence in Wang and Gunderson (2015): the reduction in the number of overtime hours is a channel of adjustment that is sometimes mentioned by the workers, employers and labor inspectors they interviewed. They also suggest that when workers do work overtime, they obtain days off instead of higher wages at the overtime rate. These results suggest that the actual rise in labor costs might be smaller than that implied by the minimum-wage rise for some of the exposed firms. However, this cannot explain the productivity effect that is robustly found across our various specifications; if anything, were exposed firms to reduce the number of (overtime) hours worked, this should lead to a downward bias in the measured productivity effect.

Given the above, changing hours does not seem a likely explanation of our results.

Pricing

A last potential explanation is that firms adjusted to the 2004 Reform by increasing their prices. With a sufficiently low price-elasticity of demand, firms can raise wages and not change their employment. With higher prices, firm labor productivity and TFP measured in value would also rise. We do not directly observe prices. However, were prices to explain our results, their adjustment would need to be different across sectors. We carry out two exercises in this respect.

First, under perfect competition, price equals marginal cost: additional labor costs such as those from higher minimum wages should be entirely passed on to consumers in the form of higher prices, leaving profitability unchanged (Draca et al., 2011). On the contrary, in less-

competitive industries firms bear part of the wage rise through lower mark-ups and so lower profits. Draca et al. (2011) show that profitability fell more in less-competitive industries following the introduction of the UK minimum wage at the end of the 1990s. We thus look at sectoral differences by the degree of competitive pressure. If price effects are at play, we should see smaller productivity rises for exposed firms in less-competitive industries. As in Draca et al. (2011) we split industries into "low" and "high" competition sectors according to the median level of the competition indicator. We follow Aghion et al. (2015) and measure competition by the Lerner Index, which assesses the size of markups relative to firm total value added.[29] The results from OLS and IV appear in the first two columns of Table A-10 for labor productivity and Table A-11 for firm TFP. We find no difference in the productivity impact of the 2004 Reform for low- and high-competition industries.

Second, conditional on the degree of competition, the extent to which labor costs can be passed on to consumers depends on the price-elasticity of demand: the higher this elasticity, the lower the pass-through. This should show up in our data as lower productivity gains after the Reform for firms in sectors with higher price-elasticity of demand. We use the price-elasticity of demand estimated by Broda et al. (2006) for China based on import data by 3-digit Harmonized-System (HS) categories. We split industries into "low" and "high" demand-elasticity sectors according to the median demand elasticity.[30] Columns 3 and 4 of Table A-10 for labor productivity and Table A-11 for firm TFP investigate the heterogeneity of productivity effects across "low" and "high" demand-elasticity sectors. We find no evidence that the impact of the 2004 Reform differs across those two groups: the triple interaction between Exposed, Reform and the high demand-elasticity dummy is never significant. Finally, the last two columns of both tables account for both competition and the price-elasticity of demand. Again, we find no significant heterogeneity in productivity gains considering both dimensions at the same time.

Overall, these results provide little support for output prices being behind the productivity effect we estimate.

8 Conclusion

We have here considered the repercussions of the Chinese 2004 minimum-wage rise on firm survival, employment, productivity and profitability. We identify the causal effect of minimum wages via a double-difference strategy, and show that our results pass a number of robustness checks including pre-existing trend analysis and IV. We find that immediately

[29]We thank Ann Harrison for sharing the Stata code used to calculate the Lerner index using the Chinese firm-level surveys.

[30]We thank Johannes Van Biesebroeck for sharing the match between Chinese industry codes and HS codes with us.

following higher minimum wages firm-level survival probability falls, and both wages and productivity significantly rise among the firms that were more exposed to the Reform, allowing surviving firms to maintain their profits and limit job losses. These productivity gains at least partly reflect better inventory management and greater capital investment, at the cost of lower cash. Overall, the minimum-wage Reform in China has produced a cleansing effect.

Our medium-run results complete a consistent picture: the 2004 minimum-wage policy not only immediately increased wages and productivity in exposed firms, it also continued to affect firm-level productivity in subsequent years. We do find slight negative employment effects in the medium run, but these were compensated by productivity gains, so that the value added of exposed firms increased and profits remained unaffected.

Given the multiple channels of firm-level reactions to minimum wages (employment, prices, productivity etc.), minimum-wage effects likely depend greatly on the local context. Our results show that in a fast-growing developing economy like China, where inefficiencies are still pervasive, the productivity channel might be particularly important.

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Appendix

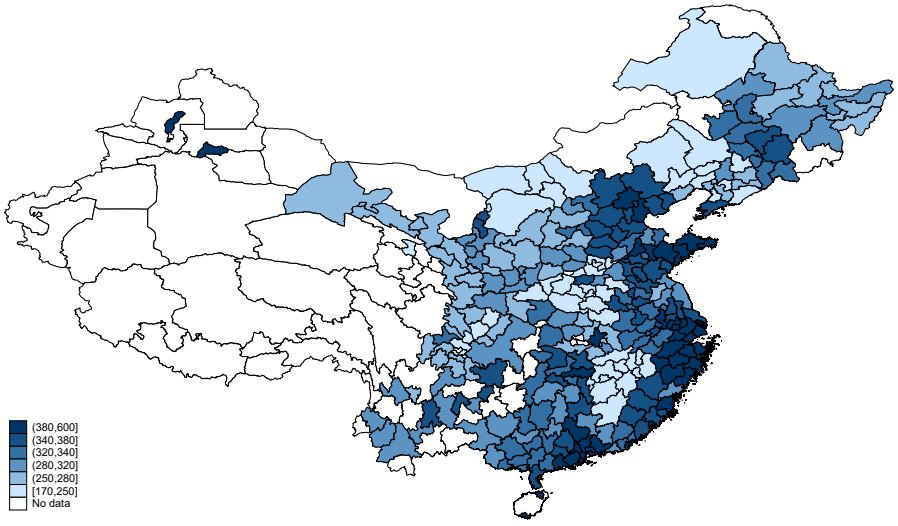

Figure 1: Monthly nominal minimum wages in 2003 (Yuan)

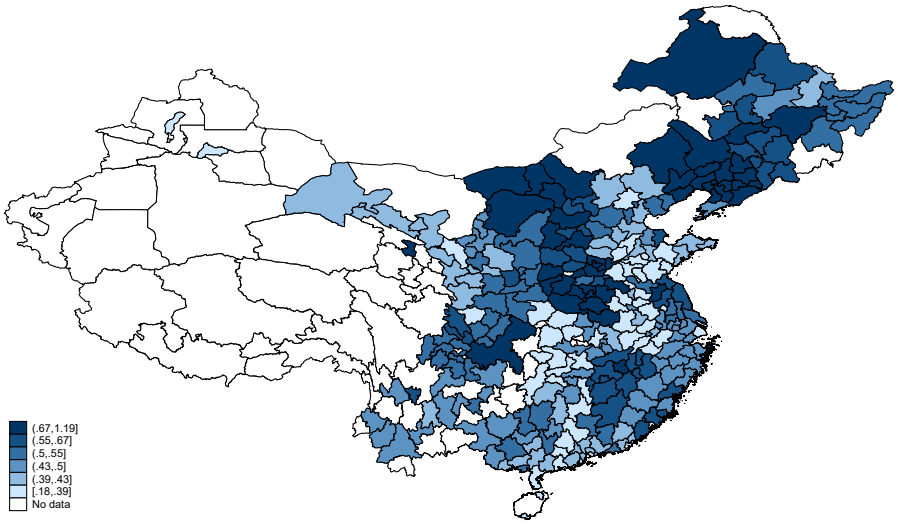

Figure 2: Δ Monthly nominal minimum wages 2003-07

Table A-1: Summary statistics on exposure and changes in firm performance

Firm type	Pre-Reform period 1998-2003 Exposed	Pre-Reform period 1998-2003 Non-exposed	Post-Reform period 2004-2007 Exposed	Post-Reform period 2004-2007 Non-exposed
Number present in our sample at t	101,312	538,728	106,999	788,239
Survival rate	0.78	0.88	0.84	0.89
	Surviving firms			
Mean Δ ln wage	0.48	0.00	0.54	0.11
Median Δ ln wage	0.39	0.03	0.47	0.09
SD Δ ln wage	0.75	0.50	0.54	0.43
Mean Δ ln labor productivity	0.25	0.05	0.38	0.16
Median Δ ln labor productivity	0.21	0.06	0.32	0.15
SD Δ ln labor productivity	1.05	0.82	0.94	0.80
Mean Δ ln employment	-0.18	0.01	-0.08	0.04
Median Δ ln employment	-0.02	0.00	0.00	0.00
SD Δ ln employment	0.59	0.45	0.47	0.39
	All firms			
Mean Δ ln Minimum wage	0.09	0.09	0.15	0.13
Median Δ ln Minimum wage	0.06	0.06	0.11	0.11
SD Δ ln Minimum wage	0.11	0.10	0.12	0.10

Authors' calculations from the NBS annual surveys. Δ indicates the change between t and t-1. See the main text for details.

Table A-2: Summary statistics on exposure and firm characteristics (in levels)

Firm type	Pre-Reform period 1998-2003 Exposed	Pre-Reform period 1998-2003 Non-exposed	Post-Reform period 2004-2007 Exposed	Post-Reform period 2004-2007 Non-exposed
Mean wage	234	823	457	1203
Mean labor productivity	33	59	69	103
Mean employment	283	325	200	243
Share of exporting firms	0.18	0.29	0.23	0.30

Authors' calculations from the NBS annual surveys. Wage is in Yuan, labor productivity in thousands of Yuan. See the main text for details.

Table A-3: Share of exposed firms by sector

Industry (2-digit)	Share of exposed firms
Manufacture of Beverages	0.21
Processing of Food from Agricultural Products	0.20
Manufacture of Food	0.20
Processing of Timber, Manufacture of Wood	0.17
Manufacture of Artwork and Other Manufacturing	0.16
Manufacture of Rubber Products	0.16
Manufacture of Leather, Fur etc.	0.16
Manufacture of Paper and Paper Products	0.15
Manufacture of Non-metallic Mineral Products	0.15
Manufacture of Cultural & Educational Products	0.15
Manufacture of Textile	0.15
Manufacture of Furniture	0.14
Manufacture of Wearing Apparel & Footwear	0.14
Printing, Reproduction of Recording Media	0.14
Recycling and Disposal of Waste	0.13
Manufacture of Plastic Products	0.13
Smelting and Pressing of Ferrous Metals	0.12
Manufacture of Metal Products	0.12
Manufacture of Medicines	0.12
Manufacture of Raw Chemical Materials & Chemical Products	0.12
Manufacture of Electrical Machinery and Equipment	0.11
Manufacture of Special Purpose Machinery	0.11
Processing of Petroleum & Coking	0.11
Smelting and Pressing of Non-ferrous Metals	0.11
Manufacture of Transport Equipment	0.10
Manufacture of General Purpose Machinery	0.10
Manufacture of Chemical Fibers	0.10
Manufacture of Instruments and Appliances & Office Machinery	0.10
Manufacture of Telecommunication Equipment & Computers	0.09
Manufacture of Tobacco	0.06

Authors' calculations from the NBS annual survey and Minimum-wage data. Exposed firms are those whose wage in t-1 was below the city-level minimum wage in t. The share of exposed firms is calculated as the average over the 1998-2007 period. See the main text for details.

Table A-4: Minimum wages and firm outcomes: exposure dummy, restricted sample

Dependent variable	Firm outcome$_t$					
	Ln wage	Ln employment	Ln labor productivity	Ln value added	Ln TFP (LP)	Profit over output
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed Firm	-0.055^a	-0.065^a	-0.012^c	-0.076^a	-0.058^a	0.006
	(0.005)	(0.004)	(0.007)	(0.006)	(0.006)	(0.019)
Exposed Firm × Reform	0.083^a	0.001	0.026^a	0.027^a	0.020^a	-0.011
	(0.007)	(0.005)	(0.008)	(0.007)	(0.007)	(0.022)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	609,334	609,334	608,130	608,130	552,155	608,199
R-squared	0.73	0.93	0.79	0.85	0.84	0.40

The data cover the 1998-2007 period. The sample is restricted to firms below the median average wage observed among non-exposed firms (normalized by city-level median wages) and above the first decile average of wages observed among exposed firms. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio, and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm at t-1 being lower than the local minimum wage at t. Reform is a dummy for t≥2004. The Exposed dummy is controlled for but the results are not reported. The Reform dummy is absorbed in the sector-year and city-year fixed effects.

Table A-5: First stage: Instrumental-variable approach with exposed dummy

Dependent variable	Exposed dummy (1)	Exposed dummy× Reform (2)
Predicted Firm-exposed dummy	0.739[a]	-0.027[a]
	(0.014)	(0.002)
Predicted Firm-exposed dummy × Reform	-0.094[a]	0.685[a]
	(0.016)	(0.023)
Ln Firm employment	0.019[a]	0.007[a]
	(0.002)	(0.001)
Ln Firm labor productivity	-0.020[a]	-0.013[a]
	(0.002)	(0.002)
Firm profit over output	0.001	0.001
	(0.001)	(0.001)
Ln Firm-level capital-labor ratio	-0.001[a]	-0.001[a]
	(0.001)	(0.001)
State dummy	-0.004[c]	-0.001
	(0.003)	(0.002)
Foreign dummy	-0.006[a]	-0.002
	(0.002)	(0.002)
Export dummy	-0.004[a]	-0.003[c]
	(0.001)	(0.001)
Firm fixed effects	Yes	Yes
City-year fixed effects	Yes	Yes
Sector-year fixed effects	Yes	Yes
Observations	1,111,660	1,111,660
R-squared	0.68	0.65
Shea partial R-squared	0.51	0.46
Test of excluded instruments: F(2,260)	1604.8[a]	462.7[a]
Prob > F	0.00	0.00

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. "Exposed" is a dummy for the average wage in the firm at t-1 being lower than the local minimum wage at t. Reform is a dummy for t≥2004. Firm-level controls are measured at t-1. The predicted Firm exposed dummy is calculated following the approach in Bartik (1991) set out in Section 6.2.

Table A-6: Medium-run effects - 2-year lag

Dependent variable	Firm outcome$_{t+2}$											
	Ln wage		Ln employment		Ln labor productivity		Ln value added		Ln TFP (LP)		Profit over output	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposed Firm	0.029^a	0.040^a	-0.020^a	-0.015^a	-0.022^a	-0.025^a	-0.042^a	-0.041^a	-0.042^a	-0.044^a	0.021	0.017
	(0.004)	(0.006)	(0.003)	(0.005)	(0.006)	(0.009)	(0.005)	(0.009)	(0.006)	(0.009)	(0.019)	(0.026)
Exposed Firm × Reform	0.106^a	0.192^a	-0.037^a	-0.067^a	0.084^a	0.131^a	0.048^a	0.066^a	0.062^a	0.082^a	0.001	0.071
	(0.008)	(0.012)	(0.006)	(0.009)	(0.009)	(0.014)	(0.009)	(0.011)	(0.008)	(0.011)	(0.011)	(0.046)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test		54.4^a		54.4^a		54.5^a		54.5^a	54.6^a	54.4^a		
Observations	575,148	510,668	575,148	510,668	573,656	509,529	573,656	509,529	505,903	507,608	573,509	509,458
R-squared	0.73	0.73	0.93	0.93	0.79	0.79	0.87	0.88	0.87	0.87	0.42	0.44

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm in t-1 being lower than the local minimum wage in t. Reform is a dummy for t≥2004. The Reform dummy is absorbed in the sector-year and city-year fixed effects. The results from IV regressions appear in the even columns following the strategy described in Section 6.2. The instrument used for the "Exposed" dummy in the IV procedure is a dummy for firm average wages in t-1 being below the predicted minimum wage in t based on the 40% rule (see the text). The underidentification test is based on the Kleibergen-Paap rk LM statistic, with ^{a} indicating that the p-value (Chi-sq(2)) is below 0.01, suggesting that underidentification is rejected.

Table A-7: Medium-run effects - 3-year lag

Dependent variable	Firm outcome$_{t+3}$											
	Ln wage		Ln employment		Ln labor productivity		Ln value added		Ln TFP (LP)		Profit over output	
Estimator	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposed Firm	0.036^a	0.022^a	-0.001	0.011^b	-0.017^a	-0.027^a	-0.017^b	-0.015	-0.020^a	-0.021^b	0.017^c	0.017
	(0.005)	(0.007)	(0.004)	(0.005)	(0.006)	(0.008)	(0.007)	(0.009)	(0.007)	(0.009)	(0.010)	(0.013)
Exposed Firm × Reform	0.120^a	0.191^a	-0.043^a	-0.059^a	0.077^a	0.107^a	0.036^a	0.051^a	0.059^a	0.077^a	-0.060	-0.076
	(0.009)	(0.014)	(0.007)	(0.009)	(0.012)	(0.017)	(0.013)	(0.019)	(0.013)	(0.019)	(0.060)	(0.087)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test		57.1^a		57.1^a		57.3^a		57.3^a	57.6^a		57.3^a	
Observations	409,343	347,869	409,343	347,869	408,148	347,045	408,148	347,045	344,568	345,897	408,044	346,975
R-squared	0.74	0.73	0.93	0.94	0.80	0.81	0.88	0.89	0.88	0.88	0.31	0.34

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm at t-1 being lower than the local minimum wage at t. Reform is a dummy for t≥2004. The Reform dummy is absorbed in the sector-year and city-year fixed effects. The results from IV regressions appear in the even columns following the strategy set out in Section 6.2. The instrument used for the "Exposed" dummy in the IV procedure is a dummy for firm average wages in t-1 being below the predicted minimum wage in t based on the 40% rule (see the text). The underidentification test is based on the Kleibergen-Paap rk LM statistic, with ^{a} indicating that the p-value (Chi-sq(2)) is below 0.01, suggesting that underidentification is rejected.

Table A-8: Heterogeneity depending on migration intensity

Dependent variable	Firm productivity$_t$							
	Ln labor productivity	Ln TFP (LP)	Ln labor productivity	Ln TFP (LP)	Ln labor productivity	Ln TFP (LP)	Ln labor productivity	Ln TFP (LP)
Migration data	2000				2005			
Estimator	OLS		IV		OLS		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposed Firm × Reform	0.039^a	0.037^a	0.034^c	0.035^c	0.038^a	0.037^a	0.030	0.038^c
	(0.012)	(0.012)	(0.018)	(0.018)	(0.013)	(0.013)	(0.020)	(0.020)
Exposed × High-migration city × 2004 Reform	-0.005	-0.010	0.005	-0.018	-0.003	-0.010	0.010	-0.021
	(0.016)	(0.015)	(0.024)	(0.022)	(0.016)	(0.015)	(0.025)	(0.024)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test			54.3^a	54.2^a			59.3^a	59.0^a
Observations	1,205,208	1,102,080	1,109,866	1,102,080	1,205,208	1,102,080	1,109,866	1,102,080
R-squared	0.78	0.85	0.78	0.85	0.78	0.85	0.78	0.85

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm in t-1 being lower than the local minimum wage in t. "High-migration city" is a dummy for the share of migrants in the city population being above the median observed across cities. Columns 1 to 4 use migration data from the 2000 census, while columns 5 to 8 use data from the 2005 mini population census. Reform is a dummy for t≥2004. Apart from the interactions presented in the table, all the main effects of the relevant variables and other interactions are included in the regressions.

Table A-9: Minimum Wages and Fringe benefits

Dependent variable	Ln firm average total wage$_t$			Ln firm fringe benefits$_t$		
	OLS	OLS	IV	OLS	OLS	IV
	(1)	(2)	(3)	(4)		
Exposed Firm × Reform	0.124^a		0.159^a	0.091^a		0.115^a
	(0.009)		(0.011)	(0.010)		(0.016)
Just above the threshold firm × Reform		0.081^a			0.067^a	
		(0.005)			(0.008)	
Slightly-exposed firms × Reform		0.125^a			0.093^a	
		(0.008)			(0.012)	
Highly-exposed firms × Reform		0.175^a			0.131^a	
		(0.014)			(0.016)	
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test			57.1^a			57.1^a
Observations	1,207,177	1,207,177	1,111,641	976,316	976,316	894,160
R-squared	0.74	0.74	0.74	0.60	0.60	0.60

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm at t-1 being lower than the local minimum wage at t. Reform is a dummy for t≥2004. The "Exposed" dummy on its own is also included in columns (1) and (3). Dummies for "Just above the threshold", "Slightly exposed" and "Highly exposed" firms are also included in columns (2) and (4). The Reform dummy is absorbed in the sector-year and city-year fixed effects. Columns (3) and (6) report results for IV regressions following the strategy described in Section 6.2. The instrument used for the "Exposed" dummy in the IV procedure is a dummy for firm average wages in t-1 being below the predicted minimum wage in t based on the 40% rule (see the text). The underidentification test is based on the Kleibergen-Paap rk LM statistic, with ^{a} indicating that the p-value (Chi-sq(2)) is below 0.01, suggesting that underidentification is rejected.

Table A-10: Heterogeneity by sectoral competition and demand elasticity - Labor productivity

Dependent variable	Firm labor productivity$_t$					
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed × Reform	0.036^a	0.042^a	0.036^a	0.045^a	0.034^a	0.052^a
	(0.009)	(0.014)	(0.010)	(0.015)	(0.011)	(0.019)
Exposed × High demand-elasticity sector × Reform			0.001	-0.016	0.003	-0.019
			(0.013)	(0.020)	(0.015)	(0.025)
Exposed × Low-competition sector × Reform	0.002	-0.012			0.004	-0.015
	(0.011)	(0.018)			(0.015)	(0.025)
Exposed × Low-competition sector × High demand-elasticity sector × Reform					-0.003	0.001
					(0.021)	(0.039)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yess
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test		5.4e+04^a		4.5e+04^a		4.5e+04^a
Observations	1,205,208	1,109,866	1,205,208	1,109,866	1,205,208	1,109,866
R-squared	0.78	0.78	0.78	0.78	0.78	0.78

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm in t-1 being lower than the local minimum wage in t. Reform is a dummy for t≥2004. "Low-competition sector" is a dummy for sectors with an above-median Lerner index (measuring the size of markups: Aghion et al., 2015). "High demand-elasticity sector" is a dummy for sectors with demand elasticity above the median in our estimation sample (Broda et al., 2006). The Reform dummy is absorbed in the sector-year and city-year fixed effects. Apart from the interactions presented in the table, all of the main effects of the relevant variables and other interactions are included in the regressions. The results from IV regressions are displayed in the even columns following the strategy described in Section 6.2. The instrument used for the "Exposed" dummy in the IV procedure is a dummy for firm average wages in t-1 being below the predicted minimum wage in t based on the 40% rule (see the text). The underidentification test is based on the Kleibergen-Paap rk LM statistic, with ^{a} indicating that the p-value (Chi-sq(2)) is below 0.01, suggesting that underidentification is rejected.

Table A-11: Heterogeneity depending on sectoral competition and demand elasticity - TFP

Dependent variable	Firm TFP (LP)$_t$					
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed × Reform	0.027^a	0.020^c	0.036^a	0.026^c	0.027^b	0.014
	(0.007)	(0.011)	(0.009)	(0.014)	(0.011)	(0.018)
Exposed × Low-competition sector × Reform	0.010	0.008			0.020	0.028
	(0.011)	(0.018)			(0.015)	(0.026)
Exposed × High demand-elasticity sector × Reform			-0.011	-0.007	-0.001	0.010
			(0.013)	(0.023)	(0.015)	(0.027)
Exposed × Low-competition sector × High demand-elasticity sector × Reform					-0.025	-0.045
					(0.023)	(0.038)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yess
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test		5.3e+04^a		2.1e+04^a		2.1e+04^a
Observations	1,102,080	1,102,080	1,102,080	1,102,080	1,102,080	1,102,080
R-squared	0.85	0.85	0.85	0.85	0.85	0.85

The data cover the 1998-2007 period. Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. The firm-level controls include firm-level employment, labor productivity, the capital-labor ratio and profit over output, as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured at t-1. "Exposed" is a dummy for the average wage in the firm in t-1 being lower than the local minimum wage in t. Reform is a dummy for t≥2004. "Low-competition sector" is a dummy for sectors with an above-median Lerner index (measuring the size of markups: Aghion et al., 2015). "High demand-elasticity sector" is a dummy for sectors with demand elasticity above the median in our estimation sample (Broda et al., 2006). The Reform dummy is absorbed in the sector-year and city-year fixed effects. Apart from the interactions presented in the table, all the main effects of the relevant variables and other interactions are included in the regressions. The results from IV regressions are displayed in the even columns following the strategy described in Section 6.2. The instrument used for the "Exposed" dummy in the IV procedure is a dummy for firm average wages in t-1 being below the predicted minimum wage in t based on the 40% rule (see the text). The underidentification test is based on the Kleibergen-Paap rk LM statistic, with ^{a} indicating that the p-value (Chi-sq(2)) is below 0.01, suggesting that underidentification is rejected.