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

F. Mayneris, S. Poncet and T. Zhang

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

Florian Mayneris†, Sandra Poncet‡ and Tao Zhang§

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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 their employment and profitability were not affected. This came about due to significant productivity gains among surviving exposed firms. Our results are robust to pre-trend analysis and an IV strategy. However, the survival probability of firms 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 flow. We show that competing explanations are unlikely. In particular, there is no evidence of lower fringe benefits compensating for higher wages, the substitution of less-paid/less-protected migrants for incumbent workers, or firm-level adjustment through higher prices instead of higher productivity. This firm-level productivity adjustment to the minimum wage might be particularly relevant for developing countries where inefficiencies are still pervasive.

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

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†Université catholique de Louvain, IRES, CORE; florian.mayneris@uclouvain.be.
‡Paris School of Economics (University of Paris 1), CEPII and FERDI; sandra.poncet@univ-paris1.fr.
§Shanghai University of International Business and Economics; neotaoism@aliyun.com.

1 Introduction

Minimum wages are widely used across the world as a tool for redistribution or a way of ensuring 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, recent riots in Bangladesh and Cambodia 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) and others emphasizing the negative effect of minimum wage for some specific 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 nearly 200,000 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 response 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 require studying also 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.

We then rely on a difference-in-differences approach to estimate the effect of the 2004 Reform on firm-level outcomes. We consider two periods in the data, a Reform period (2003-2005) and a pre-Reform period (2001-2003), and identify two groups of firms, exposed firms and non-exposed firms. Absent worker-level information, exposed firms are defined as the firms in which the average wage (total wage bill over employment) in year t is lower than the local minimum wage in $t + 2$ (as in Harrison and Scorse, 2010, and Draca et al., 2011). We then compare performance growth in exposed firms relative to non-exposed firms after the Reform, relative to the pre-Reform period. We control for firm-level initial characteristics (size, average wage, productivity, export, ownership etc.), as well as city-

sector, 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-varying characteristics.

We find that minimum-wage hikes raised the average wage in exposed firms between 2003 and 2005 without any fall in employment, as compared to the 2001-03 reference period. The main explanation is that surviving exposed firms significantly improved their productivity following the Reform, allowing them to absorb the cost shock without any change in their employment or profitability. The productivity-growth premium for exposed firms from the Reform is 3.1 percentage points, i.e. 12.5% of the measured gap in productivity growth between exposed and non-exposed firms. These results are robust to pre-trend analysis, the inclusion of firm fixed effects, and an IV strategy based on a Bartik-type instrument for exposure to the minimum wage. However, we find evidence of higher exit probability for very low-wage (and thus most 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 flow. 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, the substitution of less-paid/less protected migrants for incumbent workers, or firm-level adjustment through higher prices instead of greater productivity. Last, what we measure is the revenue-based productivity, which captures both physical productivity and prices (Foster et al., 2008). Absent price data, we still show that the firm-level response to the Reform is homogeneous across sectors with different degrees of competition. Together with the absence of any effect of the Reform on profitability, this suggests that firm-level adjustment through higher prices instead of greater productivity is unlikely (Draca et al., 2011; Harasztosi and Lindner, 2015).

To the best of our knowledge, we are the first to highlight this firm-level productivity reaction to the minimum wage. Note that it is probably larger in developing countries, in which inefficiencies remain pervasive (Hsieh and Klenow, 2009, Brandt et al., 2013). The small employment reactions to the minimum wages in developed countries usually found in the literature might reflect different firm-level channels of adjustment such as lower profits (Draca et al., 2011 in the UK for example) or 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, average monthly wages in manufacturing were 141 Dollars in China, versus 342 Dollars in Mexico and over 2,500 Dollars in the US.[1]

Second, 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 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 2003-2005 rise in the minimum wage was 147% in Eerduosi and Hulunbeier, while it remained constant in some other cities which in 2003 already adhered to the new wage standards introduced in the 2004 Reform.

Our work here 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 effects 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; Allegretto et al., 2011 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.[4] We differ from this existing work by the use of much more detailed data, so that we can directly link firm-level outcomes to changes in the local minimum wage. Huang et al. (2014) also use firm-level data to measure the effect of local minimum wages on employment but do not consider

[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 been only small (often lower than the inflation rate). The situation in China is radically different. Since the promulgation of the new minimum-wage regulations in 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. Our work differs in that we use balance-sheet data from industrial firms to consider non-employment outcomes. We also focus on the 2004 Reform, which provides us with an original estimation strategy to address endogeneity problems.

non-employment outcomes such as firm-level productivity and profitability.

Our second contribution is that our analysis of non-employment outcomes allows us to ask why we find such small 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: employment, profit, prices and productivity (Schmitt, 2013; Hirsch et al., 2015). However, absent reliable firm-level information, rigorous empirical evidence on such 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 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. However, Draca et al. (2011) do not find firm-level adjustments in terms of productivity, while Harasztosi and Lindner (2015) do not test the productivity channel. In this paper, we propose a careful evaluation of the various ways in which Chinese firms may have adjusted to the 2004 Reform, including 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 the absence of effect on firm-level profitability as evidence against any interpretation in terms of firm-level price adjustment.

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 hurting 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, 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. 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. The link 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.

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

The remainder of the paper is structured as follows. The next section describes the data we use and the Chinese minimum-wage system; it shows that the 2004 Reform rendered minimum wages binding in China. Section 3 then presents our empirical strategy and some descriptive statistics, while Section 4 displays our firm-level results. We carry out a number of robustness checks in Section 5, including pre-trend analysis and IV. Section 6 digs deeper into the mechanisms underlying the productivity effect and tests for alternative explanations of our results. Last, Section 7 concludes.

2 Data and Minimum-Wage Reform in China

2.1 Data

The data on minimum wages at the prefecture level come from various official websites such as China Labour Net.[6] 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 include only the former in our regression analysis.[7] City-level minimum wages can be adjusted several times in a given year. We define the city-level minimum wage in a year as the highest value the minimum wage takes in that year and city.

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 over 5 million Yuan. 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, intermediate inputs and the total wage bill.[9] We do not have information at the worker-level. The only wage information we have is thus the firm-level average wage (the ratio of the total wage bill to the number of employees, as in Draca et al., 2011, for example). We have balance-sheet data for all years from 1998 to 2007. However, in order to assess the effect of the 2004 minimum-wage reform, we focus on the evolution of firm-level performance

[6]This is accessible at http://www.labournet.com.cn/ and provides information on national labour and personnel rules.

[7]The hourly minimum wages are in any case calculated by the authorities based on monthly minimum wages.

[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.

between 2003 and 2005 and compare this to the pre-Reform change between 2001 and 2003 (as discussed in Section 3.1): our main estimation sample thus comprises the years 2001, 2003 and 2005. We will also use firm-level information in 1999 to test for pre-trends. 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. The dataset after cleaning contains nearly 200,000 firms active in 2001 and/or 2003.

2.2 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 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.[10] 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. 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, including Jiaxing, Jinhua and Taizhou, settling on the next-highest minimum wage (Class B).

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)

[10]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.

and 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, thus set out new minimum-wage rules in March 2004. One of the explicit aims of the 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), an hourly minimum wage was created for part-time workers, and non-enforcement penalties rose from 20-100% of the wage owed before the Reform to 100-500% post-Reform. The 2004 Rules were thus designed to increase the binding and enforcement of minimum wages. This is what we aim to assess in the next subsection.

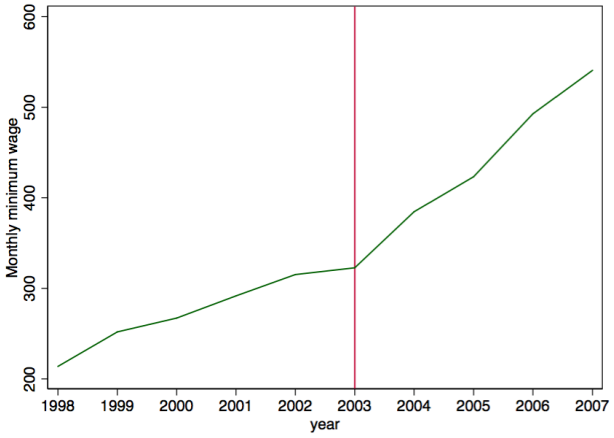
2.3 Minimum wage binding and enforcement in China

Most work on the effect of minimum wages has had to address two key issues. First, it can be difficult to estimate the 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 makes 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 show in this section that the rise in minimum wages after the 2004 Reform is massive. Moreover, even though neither enforcement nor binding are directly observable, the data suggests that firms were more constrained by minimum wages after the Reform.

2.3.1 Minimum wages rose sharply in China after the 2004 Reform

The Reform imposed a massive rise in city-level minimum wages. Figure 1 shows that city-level minimum wages rose continuously over the 1998-2007 period. The vertical line in 2003 allows visualizing a change in slope and a clear acceleration of minimum-wage growth from the implementation of the Reform in March 2004 onwards. 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. One might fear that these nominal minimum-wage rises may actually be 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 evolution of city-level real minimum wages is rather

similar to that of nominal ones. 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%). Anyway, note that our identification strategy will account for city-level inflation through the inclusion of city-year fixed effects.



Average minimum wage
(in Yuan)

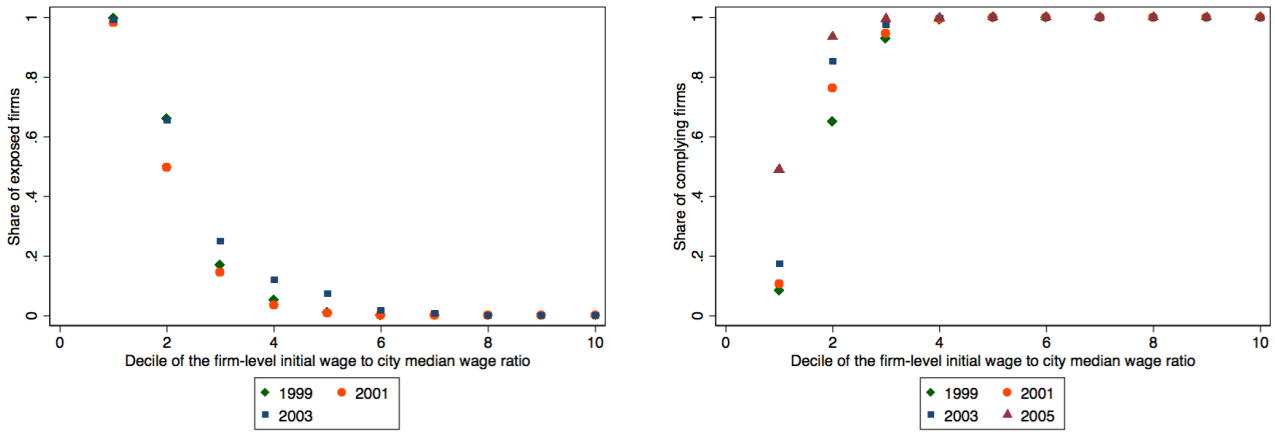
Figure 1: The change in city-level minimum wages

2.3.2 Firm-level exposure to minimum-wage growth and compliance with the minimum wage

We now investigate the degree to which Chinese firms are exposed to minimum-wage hikes. We define "exposed firms" over two-year windows as those in which the average wage at t is below the future local minimum wage at $t+2$. These firms are obliged to raise their wages in order to comply with the new city-level minimum wage. As we do not have information on worker-level wages, our exposure measure is thus 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. However, this is the best way to define exposure with firm-level data (the same estimation logic appears in the respective analysis of Indonesian and British data in Harrison and Scorse, 2010, and Draca et al., 2011, respectively). We discuss below how we deal with this issue in the econometric analysis.

We 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 decile based on this ratio. We then calculate for each decile the share of exposed firms in 1999, 2001 and 2003. Unsurprisingly, the graph on the left-hand side of Figure 2 shows that, whatever the period, the share of exposed firms is almost one in the first decile, but on the contrary close to

zero from the sixth decile onward. In between, the graph clearly shows that the share of exposed firms rises sharply from 1999 and 2001 to 2003. Note that the share of exposed firms in a given year t does not look like a step function. This is due to the heterogeneity of minimum wages across cities: for a given year t and a given initial average-wage decile, some firms might be exposed to minimum wage hikes in $t+2$ but others not, depending on the minimum-wage increase decided by the local authorities. For a given city however, the share of exposed firms in a given period will be a step function. Overall, the average share of exposed firms in cities moved from 22.2% in 1999 to 19.7% in 2001 and 24.6% in 2003. Among the cities in our sample, the 95th centile of the share of exposed firms was roughly 40% in both 1999 and 2001, but 50% in 2003. We can thus conclude that one direct consequence of the large rise in minimum wages following the Reform is that more firms were exposed to minimum-wage growth.



Exposed firms

Complying firms

Figure 2: Exposure to minimum-wage rises and compliance with the minimum wage

The right-hand side of Figure 2 shows that not only did exposure to minimum-wage growth rise following the 2004 Reform but so also did compliance with the minimum wage. 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] We can see that compliance in the lowest deciles of firm-level average wages to the city-level median wage ratio increases over the period (and always equals one from the fifth decile onward). However, after the 2004 Reform, there is a large jump for low-wage firms: the share of firms in the first decile complying with the minimum wage rose from around 20% in 2003 to over

[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 minimum-wage enforcement.

40% in 2005. Prior to 2004, average wages were at least equal to the city-level minimum wage in roughly 88% of active firms; this figure rose to 94% after 2004.

It is still difficult 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 in the next subsection points towards more enforcement.

2.3.3 Chinese firms appear more constrained by minimum wages following the 2004 Reform

The left-hand side of Figure 3 shows that firm-level average wages rose continuously over the period: their distribution gradually shifts to the right from 1999 to 2005. It might thus well be the case that minimum wage changes went hand-in-hand with the "natural" dynamics of firm-level wages. 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 allows to wash away 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 concentrate around 1, so that more and more firms pay average wages in the vicinity of the minimum wage. This is not what we observe before the Reform: the distribution of the firm-level average wages to the city minimum wage ratio is stable or, if anything, shifts to the right from 1999 to 2003. However, in 2005, this distribution significantly shifts to the left: there is a compression of firm-level average wages towards the level of the city minimum wage following the 2004 Reform.

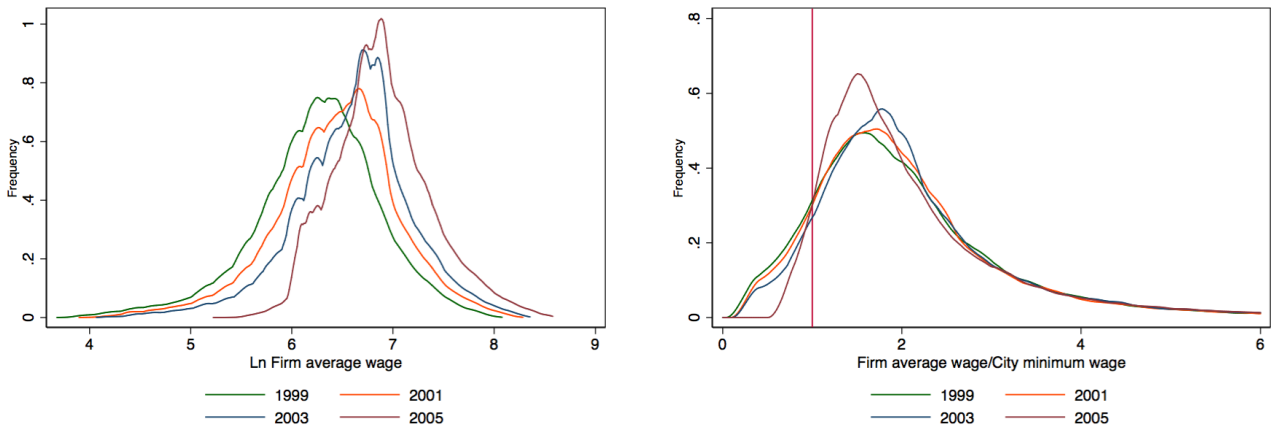


Figure 3: The distribution of firm-level average wages from 1999 to 2005

Table 1: Distribution of firm-level average wages (% of active firms)

	$\frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} < 1$	$1 \leq \frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} < 1.25$	$1.25 \leq \frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} < 1.5$	$1.5 \leq \frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} < 2$	$\frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} \geq 2$
1999-2003	.12	.09	.11	.24	.44
2004-2005	.06	.1	.15	.28	.41

The figures in Table 1 show that the share of non-complying firms ($\frac{\text{Firm Avg Wage}}{\text{City Min. Wage}} \leq 1$) fell by six percentage points following the Reform. The share of firms paying average wages more than twice the local minimum wage also fell by two percentage points, while the share of firms in which the average wage is between one and two times the local minimum wage rose by eight percentage points. We can also see from Table 2 that, among complying firms, the value of the ratio of firm-level average wages to the city-level minimum wage fell after the Reform in the middle of the distribution (from the 3rd to the 7th decile of the distribution); this was on the contrary very stable before the 2004 Reform.

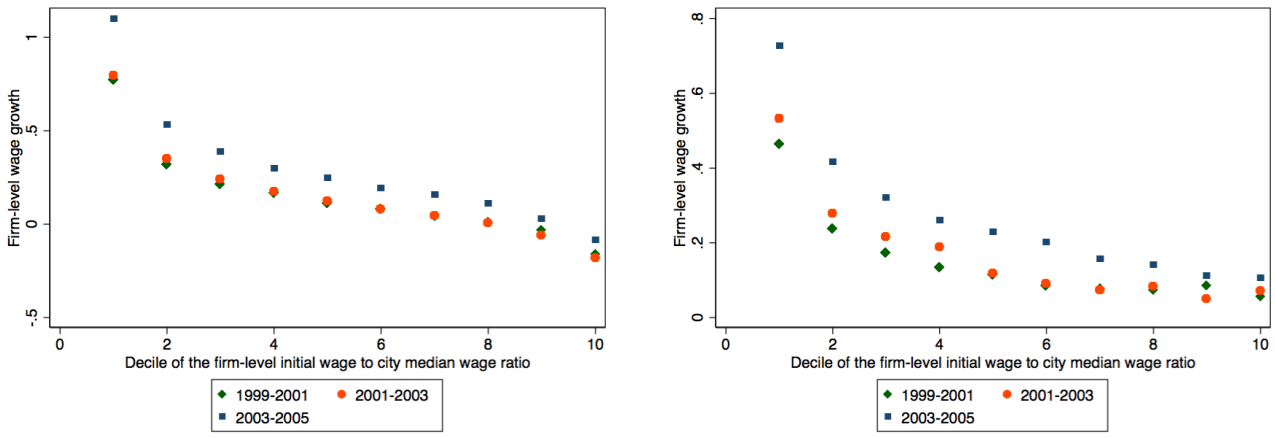
The increase in the share of complying firms, and the fall in the firm-level average wages to the city-level minimum wage ratio show that the distribution of firm-level average wages clearly tilts towards the local minimum wage following the Reform. This wage compression after the reinforcement of minimum-wage 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 to city minimum wage ratio among complying firms

	Avg $\frac{\text{Firm Avg Wage}}{\text{City Min. Wage}}$			
	1999	2001	2003	2005
1st decile	1.22	1.2	1.22	1.2
3rd decile	1.58	1.56	1.6	1.51
Median	1.91	1.90	1.91	1.79
7th decile	2.31	2.29	2.3	2.21
9th decile	3.21	3.18	3.23	3.17

We also find that the average wage in low-wage firms rose much faster after the implementation of the 2004 Minimum Wage Rules. Figure 4 depicts the unweighted and weighted averages of firm-level average wage growth by decile of the firm-level wages to the city median wage ratio.[12] Firm-level average wages clearly rose faster between 2003 and 2005, i.e. following the Reform, especially in the lowest deciles. Low-wage firms are thus put under more pressure following the imposition of the New Minimum Wage Rules. This further suggests that the higher growth in minimum wages after 2004 did not just reflect the natural growth in firm-level average wages.

[12]Using firm-level employment as weights.



Unweighted average

Weighted average

Figure 4: Firm-level average wage growth by initial wage decile

This conclusion is confirmed by the exploratory regressions in Table 3. We here present the correlation between initial firm-level average wages and subsequent firm-level average wage growth. We focus on two periods: 2001-03 and 2003-05, where the latter corresponds to the Reform period. We then regress firm-level average wage growth between t and $t+2$ on firm-level average wages in t and its interaction with a dummy for the Reform period (t=2003). All regressions include city-sector fixed effects and period dummies. The coefficient of interest is that on the interaction between initial average wages and the Reform period dummy.

The results in column (1) show that in the firms that survive for two years, the initial wage is negatively correlated with subsequent firm-level wage growth, suggesting convergence whereby wages grow faster in low-paying firms. This catch-up is much 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, export status, ownership etc.[13]

Column (3) includes an additional wave of data (1999-2001). Compared to the 1999-2001 period, low-wage surviving firms have higher wage growth during the 2001-2003 period, but the size of this coefficient is less than one third of that on the 2004 Reform. The convergence in firm-level average wages following the 2004 Reform does not then simply reflect pre-existing trends. Overall, without claiming any causality from minimum wages for the moment, these exploratory results show that low-wage firms experienced greater wage pressure after the 2004 Reform.

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

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

Dependent variable	Δ Ln firm wage$_{t,t+2}$		
	(1)	(2)	(3)
Ln Firm av. wage	-0.532^a	-0.589^a	-0.561^a
	(0.016)	(0.013)	(0.010)
Ln Firm av. wage $\times$ 2001-2003 period			-0.062^a
			(0.008)
Ln Firm av. wage $\times$ Reform	-0.187^a	-0.188^a	-0.218^a
	(0.016)	(0.015)	(0.015)
City-Sector fixed effects	Yes	Yes	Yes
Firm-level controls	No	Yes	Yes
Year dummies	Yes	Yes	Yes
Time periods	2001-03 & 2003-05	2001-03 & 2003-05	1999-01 &2001-03 & 2003-05
Observations	225,224	224,788	306,608
R-squared	0.33	0.35	0.32

Standard errors in parentheses are clustered at the city level. ^{a}, ^{b} and ^{c} indicate significance at the 1%, 5% and 10% confidence levels. Firm-level controls in columns (2) and (3) include dummies for the firm-level decile in terms of initial employment, wages and labor productivity (normalized by the city median), as well as dummies for State-owned firms, foreign firms and exporting firms. Reform is a dummy for the 2003-2005 period. "2001-2003 period" is a dummy equal to 1 for the 2001-2003 period. All right-hand side variables are measured at t.

The 2004 minimum wage Reform then produced a sharper rise in local minimum wages in China. This increased the number of firms exposed to minimum-wage hikes. We also observe greater compliance and wage compression in the bottom tail of the distribution. Finally, after the Reform low-wage firms converge 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 thus exploit this Reform to examine how Chinese firms reacted to higher minimum wages.

3 Empirical strategy and summary statistics

3.1 Empirical strategy

Our empirical approach closely follows the introduction of more restrictive minimum wages in 2004 and takes a difference-in-difference approach. We compare the relative growth of "exposed" and "non-exposed" firms within cities and sectors before and after the 2004 Reform. The Reform period is defined as the two-year window 2003-05, centered around 2004, with the 2001-2003 period being used as the pre-Reform (and thus "pre-treatment") period.

Our baseline specification can be written as follows:

$$\Delta \mathrm{Y}^{f,c,k}_{t,t+2} = \alpha \, \mathrm{Exposed}^f_t + \beta \, \mathrm{Exposed}^f_t \times \mathrm{Reform}_t + Z^f_t + \mu_{c,k} + \nu_{c,t} + \kappa_{k,t} + \epsilon^f_{c,k,t} \qquad (1)$$

where Δ denotes a two-year difference. The outcomes ΔY^f are in turn the change in average

wages, employment, productivity and profitability.

Exposed_t^f is a dummy for the firm's average wage in year t being lower than the local minimum wage in $t+2$. This approach is similar to that used in the small number of papers considering the effects of minimum wages with firm-level data (Harrison and Scorse, 2010; Draca et al., 2011). As Section 2.3.2 above noted, in the absence of worker-level information, this measure of "exposure" is potentially noisy: in so-called "exposed" firms some employees will not be exposed to the Reform and *vice versa* for "non-exposed" firms. This measurement error should lead to attenuation bias. We address this issue in two ways. We first use an alternative exposure proxy, the difference between the city-level minimum wage in $t+2$ and firm-level average wages in t. We also consider in the robustness checks alternative thresholds for the dummy for exposed firms.

Reform_t is a dummy for observations coming from the 2003-2005 period (t=2003). Our coefficient of interest, β, thus measures the potential greater reaction of exposed firms relative to non-exposed firms in terms of performance growth (the first difference) in the post-Reform period compared to pre-Reform (the second difference). A number of endogeneity issues arise in this difference-in-differences that we now address.

First, exposed firms might have particular characteristics in terms of their size, productivity and of course wages (they are by definition the lowest-wage firms in their city) that also help determine their subsequent performance growth. We thus control for Z_t^f, a set of firm-level controls including dummies for the firm-level decile in terms of initial employment, wages and labor productivity (normalized by the median observed in the city), as well as dummies for State-owned firms, foreign firms and exporting firms. These are measured in 2001 or 2003 for firm-level performance growth in 2001-2003 and 2003-2005 respectively. Controlling for initial performance allows us to capture any potential firm convergence or divergence effects. The decile dummies allow for a potentially non-linear relationship between the initial characteristics and subsequent firm-performance growth. Moreover, focusing on how the gap between "exposed" and "non exposed" firms changes pre- and post-Reform allows us to control for any time-invariant unobserved characteristics that are common to all the exposed firms over the two periods. Most previous work in this area has relied on more aggregate data and so did not control for firm-level characteristics.

We might also worry that local authorities set the minimum wage depending on the local business cycle. As noted in Section 2.2, this is explicitly encouraged by the national guidelines. We address this concern by including a number of fixed effects. We include city-sector fixed effects, $\mu_{c,k}$: our estimations are thus based on the comparison of firms from the same city-sector, accounting for city-sector trends that are common to both the pre- and post-Reform periods. The sectors k are defined using the Chinese sectoral classification at the 4-digit level. We further include city-year fixed effects $\nu_{c,t}$ to capture any time-varying

shocks affecting both the minimum-wage decisions made by local authorities and firm growth in the city. We also include sector-year fixed effects to control for shocks affecting all Chinese firms in a given sector in a given period. Our final sample covers 480 sectors and 261 cities over the two periods.

Finally, it could be the case that local authorities set minimum wages based on the business cycles specifically faced by low-wage firms. The dummies for firm-level deciles in terms of initial employment, wages and labor productivity partly address this issue: these account for different dynamics over the two periods for firms from different size, wage and productivity classes. We conduct additional exercises to address this issue. We include an additional wave of data (1999-2001) to test for the existence of pre-trends affecting exposed firms in 2001-2003. We also run specifications including firm-level fixed effects (focusing in this case on firms that survive over the two periods). We finally alternatively rely on a Bartik (1991)-type instrument to further account for the potential remaining endogeneity of the Exposed_t^f dummy to local shocks affecting low-wage firms. The results are robust to all of these checks.

Note that to ensure greater comparability between "exposed" and "non-exposed" firms, we discard firms at the top and at the bottom of the wage distribution. There is no established rule on how to define the adequate group of firms; we decide to drop firms which 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 extremely low average wage may be due to misreporting. As explained in Section 3.2 this restriction ensures that exposed and non-exposed firms differ little in terms of observables, with the exception of wages. However, we show in the robustness checks that the results continue to hold in the entire sample of firms.

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

3.2 Descriptive statistics

Absent worker-level information, we calculate firm-level average wages as the firm's total wage bill divided by the number of employees.

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 at the 2-digit industry level following the approach developed by Levinsohn and Petrin (2003). Intermediate inputs are used as a proxy for unobserved variables (entrepreneur characteristics or macroeconomic shocks) which could determine the levels of both employment and capital on the one hand and output on the

other.[14]

In our sample, the average wages in 18.9% of firms are below the local minimum wage enforced two years later. This proportion of exposed firms rose from 16.5% in 2001 to 20.9% in 2003. Exposed firms being firms whose average wage in t is lower than the subsequent city-level minimum wage in $t+2$, they do not necessarily violate minimum wage rules: their average wage in t might well be above the current city-level minimum wage in t.

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 2003 firms that survived until 2005 is much lower for exposed firms (65%) than for non-exposed firms (79%).[15] Furthermore, 2003-2005 firm-level average wage growth is significantly higher for exposed firms, at 0.79 log points, as compared to only 0.16 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 less striking before the Reform period, except for employment growth which features an even more negative differential between exposed and non-exposed firms before the Reform. These simple descriptive statistics suggest that there is a negative correlation between "exposure" to the 2004 minimum-wage Reform and survival, and a positive correlation between "exposure" and the rise in firm-level average wages and productivity over the period. Our econometric analysis below will try to assess whether these correlations can be interpreted as causal. By way of contrast, note that the average growth rate of the minimum wage over this period was roughly the same for exposed and non-exposed firms, suggesting that there is no systematic difference in the geographic distribution of exposed and non-exposed firms in our sample.

Table 4 continues the descriptive analysis by regressing the "exposed" dummy on firm-level characteristics and city-year fixed effects for firms present in the surveys in 2001 and/or 2003. Columns (1) and (2) cover the entire sample of firms. The results in column (1) show that firms with average wages below the subsequent minimum level report (quite intuitively) lower productivity and are smaller. They are also less likely to be foreign firms and exporters. Column (2) further includes the firm average wage, which logically enters negatively. Controlling for wages, exposed firms now appear to be slightly more productive; they are also more likely to be State-owned. However, they do not differ in terms of profitability. Columns (3) and (4) attempt to ensure greater comparability between "exposed" and "non-

[14]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.

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

Table 4: Exposure

Dependent variable	Exposed dummy, 2001 and 2003			
	All firms		Restricted (estimation) sample	
	(1)	(2)	(3)	(4)
Ln Firm average wage		-0.443^a		-0.977^a
		(0.008)		(0.016)
Ln Firm employment	-0.019^a	-0.001	-0.016^a	0.001
	(0.002)	(0.001)	(0.002)	(0.001)
Ln Firm labor productivity	-0.076^a	0.010^a	-0.058^a	-0.001
	(0.003)	(0.002)	(0.004)	(0.001)
Firm profit over output	0.001^a	-0.001	0.001^a	0.001^a
	(0.001)	(0.001)	(0.001)	(0.001)
Ln Capital to labor ratio	-0.011^a	0.001	-0.009^a	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Export dummy	-0.027^a	-0.006^a	-0.027^a	-0.003^c
	(0.004)	(0.002)	(0.005)	(0.002)
State-owned dummy	0.005	0.043^a	0.039^a	0.004
	(0.009)	(0.004)	(0.009)	(0.003)
Foreign dummy	-0.038^a	0.037^a	-0.017^b	-0.001
	(0.006)	(0.006)	(0.007)	(0.003)
City-year fixed effects	Yes	Yes	Yes	Yes
Observations	297,643	297,643	171,593	171,593
R-squared	0.06	0.45	0.03	0.58

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 in t being lower than the local minimum wage in $t+2$, with t being either 2001 or 2003. All the other right-hand side variables are measured at t. The restrict sample contains firms which average wage (normalized by the median observed in the city) is below the median observed among non-exposed (high-wage) firms, and above the first decile of exposed (low-wage) firms

exposed" firms by restricting the sample 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. Interestingly, in column (4), the average wage is virtually the only firm characteristic that determines exposure; the coefficients on the other firm-level characteristics are very close to zero and mostly insignificant. This suggests that the sample restriction is an effective way of tackling selection into exposure based on observable characteristics other than initial average wage (and so on unobservable characteristics that are correlated with these observables). The benchmark estimates in our empirical analysis come from this restricted sample. We do however check that all the results continue to hold when we run the regressions on the entire sample of firms.

Table A-2 in the Appendix shows average firm characteristics in both the whole sample and the restricted sample of firms in 2001 and 2003. Average wages are more than three times higher in non-exposed than exposed firms. Non-exposed firms are also much more productive and larger on average. By way of contrast, in line with Table 4, average productivity and number of employees are fairly comparable across the two groups in the restricted sample, confirming the improved comparability between "exposed" and "non-exposed" firms in the restricted sample. Table A-3 displays the share of exposed firms in 2003 at the 2-digit

industry level. There is substantial heterogeneity across sectors in the proportion of firms that pay in 2003 an average wage that will turn out to be lower than the city-level minimum wage in 2005. Rather intuitively, exposed firms are fewer in sectors characterized by a high-skill intensity such as Manufacture of Telecommunication Equipment & Computers or Manufacture of Instruments. Higher shares of exposed firms are observed in agri-food industries and lower skill intensity manufacturing sectors such as the paper or the textile industries.

4 Results

4.1 Baseline results

Table 5 shows the results from the estimation of Equation (1). The growth rate of firm-level average wages is the dependent variable in column (1). The following columns report the estimates for the growth rate of firm-level employment, labor productivity, TFP and profitability.

The results in column (1) of Table 5 show that the 2004 Reform led to higher average wages in surviving exposed firms. All else equal, the growth rate of average wages in exposed firms rose by 7.8 percentage points as compared to non-exposed firms following the Reform. No such pattern appears before the Reform, with the coefficient on the Exposed dummy being very close to zero and insignificant. As such, the 2004 Reform succeeded in significantly increasing wages for workers in low-wage firms. This is further proof that the 2004 Reform was binding and put more wage pressure on low-wage firms than before.

Column (2) investigates the possible repercussions of this non-negligible cost shock on the number of employees. The negative coefficient on the Exposed dummy alone (this coefficient being null and insignificant for firm-level average wages) suggests that exposed firms structurally grow at a slower pace than non-exposed firms. However, we find no significant job losses after the introduction of the new minimum wage rules in the exposed firms that remain active: there is no change in the employment growth gap between surviving exposed and surviving non-exposed firms following the Reform. Exposed firms do not seem then to react to the Reform by hiring less or firing more workers overall than do other firms.

The following two columns in Table 5 help us understand why the 2004 minimum-wage Reform brought about significantly higher per employee labor costs without reducing employment. Column (3) reports the estimates of Equation (1) for labor-productivity growth (value added per worker). The results suggest that the 2004 minimum-wage Reform was associated with significant productivity growth in exposed firms. While, all else equal, the productivity growth of exposed firms was not different from that of non-exposed firms before

Table 5: Minimum wages and firm outcomes: exposure dummy

Dependent variable	Δ firm outcome, 2001-03 & 2003-05				
	Ln wage (1)	Ln employment (2)	Ln labor productivity (3)	Ln TFP (LP) (4)	Profit over output (5)
Exposed Firm	0.002	-0.033[a]	0.009	-0.012	-0.045
	(0.012)	(0.009)	(0.016)	(0.016)	(0.030)
Exposed Firm × Reform	0.078[a]	0.010	0.031[b]	0.032[b]	0.048
	(0.010)	(0.011)	(0.014)	(0.014)	(0.040)
City-sector fixed effects	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes
Observations	125,927	125,927	125,943	124,254	125,449
R-squared	0.47	0.34	0.40	0.38	0.52

Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 2001 or 2003. Exposed is a dummy for the average wage in the firm in t being lower than the local minimum wage in $t+2$. Reform is a dummy for the 2003-2005 period. Firm-level controls include dummies for firm-level decile in terms of initial employment, wage and labor productivity (normalized by the median observed in the city), as well as dummies for State-owned firms, foreign firms and exporting firms. All of the right-hand side variables are measured at t.

the 2004 Reform, it is 3.1 percentage points higher after the Reform. Column (4) reports the results for firm-level total factor productivity calculated following the procedure suggested by Levinsohn and Petrin (2003). These confirm that firms exposed to higher minimum wages experienced greater productivity gains once the new minimum wage applied. The gap in productivity growth between exposed and non exposed firms is 25 percentage points during the Reform period (see Table A-1 in the Appendix). Hence, the productivity-growth premium attributed to the Reform for exposed firms is 12.5% of the initial productivity-growth gap between exposed and non-exposed firms.[16]

Last, column (5) considers the effect of the tightening of minimum wages on firm profitability growth, finding no significant Reform effect on this dimension either.

Several channels can explain the productivity adjustment to the minimum wage we find. Firms can substitute skilled workers to unskilled workers for example (with total employment unchanged). We do not have information on the skill composition of firms in each year and thus cannot test for this. However, we show in Section 6.1 that other (non exclusive) explanations are also at play, such as capital investment and changes in management practices.

4.2 An alternative definition of exposure

We noted above that the "exposed" dummy is a noisy measure of exposure to the minimum-wage hike, since some fraction of employees will not be exposed to higher minimum wages

[16] $\frac{0.031}{0.46-0.21}$=0.124.

Table 6: Minimum wages and firm outcomes, continuous exposure

Dependent variable	Δ firm outcome, 2001-03 & 2003-05				
	Ln wage (1)	Ln employment (2)	Ln labor productivity (3)	Ln TFP (LP) (4)	Profit over output (5)
Firm exposure	0.717[a]	-0.105[a]	0.058[a]	-0.025	-0.017
	(0.012)	(0.010)	(0.019)	(0.016)	(0.026)
Firm exposure × Reform	0.201[a]	-0.010	0.053[a]	0.041[b]	0.046
	(0.013)	(0.013)	(0.020)	(0.017)	(0.049)
City-sector fixed effects	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes
Observations	125,927	125,927	125,493	124,254	125,449
R-squared	0.49	0.34	0.40	0.38	0.52

Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 2001 or 2003. Firm exposure is measured as (ln Local Minimum Wage$_{t+2}$ - ln Firm-level wage$_t$). Reform is a dummy for the 2003-2005 period. Firm-level controls include dummies for firm-level decile in terms of initial employment and labor productivity (normalized by the median observed in the city), as well as dummies for State-owned firms, foreign firms and exporting firms. All the right-hand side variables are measured at t.

in so-called "exposed" firms and *vice versa* for "non-exposed" firms.

The results in Table 6 show that our findings remain the same when we use a continuous rather than a dichotomous indicator of minimum-wage exposure. The treatment variable is now defined as the interaction between the Reform dummy and the difference (ln Local Minimum Wage$_{t+2}$ - ln Firm-level wage$_t$), i.e. the distance between the current firm-level average wage and the future local minimum wage. We expect that the larger is this difference the more exposed is the firm (due to a greater share of low-wage workers, or lower wages). Our main results are unaffected: the lower is the initial firm-level average wage, as compared to the subsequent local minimum wage, the greater is the post-Reform relative wage rise. The 2004 Reform appears to produce efficiency gains that increase with the exposure to the minimum-wage rise, leaving employment and profits unchanged.[17]

In the same vein, Table 7 investigates what happens for firms whose average wage is above but very close to the future city-level minimum wage, since a significant fraction of their employees is certainly exposed to the minimum-wage rise. We isolate the bottom tier of non-exposed firms, i.e. the firms whose average wage in t is between 1 and 1.3 times the subsequent local minimum wage in $t+2$: we call these "Just above the threshold firms". Our results suggest that these firms are unaffected in all dimensions, confirming that the main repercussions of the 2004 Reform concerned low wage firms, and mainly affected wage growth and productivity growth.

[17]Note that in this empirical model, the variable for firm exposure alone now captures the correlation between firm-level performance growth and firm-level initial wage, since we do not include anymore the deciles for firm-level initial wage, due to multicollinearity issues.

Table 7: Alternative cut-offs for the exposure dummy

Dependent variable	Δ firm outcome, 2001-03 & 2003-05				
	Ln wage	Ln employment	Ln labor productivity	Ln TFP (LP)	Profit over output
	(1)	(2)	(3)	(4)	(5)
Exposed Firm	0.018	-0.043[a]	0.015	-0.013	-0.051
	(0.016)	(0.012)	(0.024)	(0.021)	(0.037)
Exposed Firm × Reform	0.086[a]	0.009	0.038[b]	0.039[a]	0.068
	(0.011)	(0.012)	(0.015)	(0.014)	(0.060)
Just above the threshold firm	0.010	-0.007	0.004	-0.001	-0.018
	(0.009)	(0.009)	(0.015)	(0.015)	(0.020)
Just above the threshold firm × Reform	0.014	-0.004	0.009	0.007	0.040
	(0.009)	(0.010)	(0.014)	(0.013)	(0.040)
City-sector fixed effects	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes
Observations	125,927	125,927	125,493	124,254	125,449
R-squared	0.41	0.27	0.34	0.32	0.49

Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 2001 or 2003. Exposed is a dummy for the average wage in the firm in t being lower than the local minimum wage in $t+2$. Reform is a dummy for the 2003-2005 period. Firm-level controls include dummies for firm-level decile in terms of initial employment, wage and labor productivity (normalized by the median observed in the city), as well as dummies for State-owned firms, foreign firms and exporting firms. All the right-hand side variables are measured at t.

These results overall confirm that the main repercussions of the 2004 Reform concerned low and very low wage firms, and mainly affected wage growth and productivity growth. There is no evidence of significant effects on the evolution of employment or profitability for surviving firms.

5 Robustness checks

Our results are robust to a number of checks regarding the validity of our identification assumptions and the definition of our estimation sample.

5.1 Pre-trends

Our difference-in-difference approach assumes that, all else equal, in the absence of the 2004 minimum wage Reform, the gap between exposed and non-exposed firms in terms of performance growth would have remained the same (a common-trend assumption). We cannot directly test this, but can check whether exposed and non-exposed firms had already started to diverge in some dimensions pre-Reform. We thus add another 1999-2001 wave of data to our estimation sample. The results in Table 8 suggest that the relative change between 2001-03 and 2003-05 does not reflect a pre-existing trend.

Table 8: Accounting for pre-trends, exposure dummy

Dependent variable	Δ firm outcome, 1999-01, 2001-03 & 2003-05				
	Ln wage (1)	Ln employment (2)	Ln labor productivity (3)	Ln TFP (LP) (4)	Profit over output (5)
Exposed Firm	-0.009	-0.020[b]	-0.011	-0.021	0.579
	(0.013)	(0.009)	(0.015)	(0.015)	(0.616)
Exposed Firm × 2001-2003 period	0.046[a]	-0.009	0.012	0.008	-0.419
	(0.013)	(0.011)	(0.018)	(0.016)	(0.409)
Exposed Firm × Reform	0.088[a]	0.012	0.035[b]	0.037[b]	-0.390
	(0.012)	(0.010)	(0.016)	(0.015)	(0.433)
City-sector fixed effects	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes
Observations	170,980	170,980	170,300	168,465	170,229
R-squared	0.42	0.31	0.37	0.36	0.46

Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 1999, 2001 or 2003. Exposed is a dummy for the average wage in the firm in t being lower than the local minimum wage in $t+2$. Reform is a dummy for the 2003-2005 period. Firm-level controls include dummies for firm-level decile in terms of initial employment, wages and labor productivity (normalized by the median observed in the city), as well as dummies for State-owned firms, foreign firms and exporting firms. All the right-hand side variables are measured at t.

The difference between exposed and non-exposed firms remains stable between 2001-2003 as compared to 1999-2001 for all outcome variables. The exception is the growth rate of firm-level average wages, which is slightly higher for exposed firms in the pre-Reform 2001-03 period (but much less than post-Reform). Overall, the estimated coefficients on our key variable, Exposed Firm × Reform, are virtually unchanged (or are if anything slightly larger) confirming that the effects captured by our double differences reflect the 2004 Reform.

5.1.1 IV strategy

Even though the use of double differences is validated by the above pre-trend analysis, 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 very low-wage firms (the city-year fixed effects already control for local shocks that are common to all firms in a given city). We thus consider IV estimation to complement the difference-in-difference. 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. Our instrument relies on the "40% rule" that minimum wages should be at least 40% of local average wages.

One predictor of the local minimum wage could indeed be this lower bound. 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 $t+2$ (in turn 2003 and 2005 in our analysis):

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

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

$\widetilde{\text{Wage}}_{t+2}^{c^p}$ is thus the weighted sum of local sectoral wages in t that are assumed to grow between t and $t+2$ at the same rate as wages in the rest of China between $t-2$ and t. The local industry employment shares in $t-2$ are used as weights. $\widetilde{\text{Wage}}_{t+2}^{c^p}$ is assumed to be unrelated to any shocks affecting local labor supply and demand, and thus local wages between t and $t+2$. Our instrument for "Exposed" is then a dummy for firm average wages in year t being lower than $0.4 \times \widetilde{\text{Wage}}_{t+2}^{c^p}$. The Exposed×Reform dummy is instrumented by the interaction between this IV and the dummy for the Reform period.

Note that this IV specification uses the initial level of employment, average wages and productivity as firm-level controls, instead of dummies for firm-level deciles in terms of these initial characteristics. If we use the deciles, the instrument is weaker. This is because part of the potential specific shocks that affect firms with different initial wages, size or productivity are already picked up in the deciles, leaving little additional variation for the instrument.[18] We have however estimated the IV using the deciles as controls, and the coefficients on our variable of interest do remain very similar.[19]

The first stage of the IV results are shown in Table A-4. The first two columns correspond to the first-stage results of the exposed dummy and the exposed×Reform interaction that are common to all outcomes. The exposed dummy and its prediction described above are positively correlated. The size of the coefficient (0.224) is reassuring, showing that the IV is not completely collinear with the exposed dummy. We check that our instrumental variables are not weak. The partial R-squared and F-tests associated with our instruments appear at the bottom of Table A-4. We also report the underidentification test at the bottom of Table 9. All of these tests pass.

The two-stage least squares estimates in Table 9 confirm the previous findings for all the firm-level outcomes. The coefficients are broadly unaffected. If anything they are somewhat

[18] The levels impose a log-linear relationship between the initial characteristics and subsequent performance growth, while the deciles fit the data better

[19] These results are available upon request.

Table 9: Minimum wages and firm outcomes: exposure dummy, IV

Dependent variable	Δ firm outcome, 2001-03 & 2003-05				
	Ln wage	Ln employment	Ln labor productivity	Ln TFP (LP)	Profit over output
	(1)	(2)	(3)	(4)	(5)
Exposed Firm	-0.058	-0.024	0.093	0.039	-0.060[c]
	(0.079)	(0.038)	(0.058)	(0.075)	(0.031)
Exposed Firm × Reform	0.191[a]	-0.015	0.059[b]	0.042	0.065
	(0.021)	(0.017)	(0.027)	(0.027)	(0.064)
City-sector fixed effects	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes
Underidentification test	11.99[a]	11.99[a]	12.27[a]	12.42[a]	12.36[a]
Observations	125,927	125,927	125,493	124,254	125,449
R-squared	0.49	0.34	0.43	0.39	0.52

Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 2001 or 2003. Exposed is a dummy for the average wage in the firm in t being lower than the local minimum wage in $t+2$. Reform is a dummy for the 2003-2005 period. Firm-level controls include firm-level initial employment, wages and labor productivity, as well as dummies for State-owned firms, foreign firms and exporting firms. All the right-hand side variables are measured at t. The instrument used for the "Exposed" dummy in the IV procedure is a dummy for firm average wages in t being below the predicted minimum wage in $t+2$ 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.

larger for firm-level average wages and, to a lesser extent, labor-productivity growth. For the LP TFP growth rate, the coefficient is also slightly higher and the p-value is equal to 0.12 due to larger standard errors. Overall, taking the standard errors into account, the IV estimates are not significantly different from the benchmark results, apart from those on firm-level average wage growth.

5.2 Alternative samples

Our two final robustness checks consist in changing the sample. We first carry out the benchmark analysis on the whole sample of firms (i.e. without trimming very low wage and high-wage firms). The results appear in Table A-5 in the Appendix and are very similar to the benchmark results.

We also propose a specification with firm fixed effects. By construction, the sample is now restricted to firms that appear in both the 2001-2003 and 2003-2005 periods. Again, the estimated coefficients on the "Exposed×Reform" interaction remain very similar (see Table A-6 in the Appendix).

We thus conclude from these robustness checks that our benchmark results provide reliable estimates of the effects of the 2004 minimum-wage Reform on firm-level outcomes in China.

6 The productivity channel and alternative explanations

In this section we dig deeper into the mechanisms behind our measured productivity effect.

6.1 The channels underlying productivity gains

We here propose two types of results that further confirm productivity as surviving firms' main margin of adjustment following the 2004 Reform. The first extends the analysis to firms that do not survive. The productivity channel implies that firms that were unable to obtain the required productivity improvements would exit the market, producing a negative effect of the 2004 Reform on firm survival probability.

The first two columns in Table 10 report the results from the estimation of Equation (1) with the probability that the firm fails to survive between t and $t+2$ as the dependent variable. Column (1) relies on the dichotomic exposure measure and column (2) the continuous measure. Our results confirm that the lower was the initial firm-level average wage compared to the subsequent minimum wage in the locality the greater the relative rise in the exit probability following the 2004 Reform. The difference in difference coefficient is positive, but significant in column (2) only. This confirms that the higher minimum wages and greater enforcement imposed by the 2004 Reform reduced firm survival, especially for the firms that initially paid very low wages. As exposed firms are on average smaller and less productive than the others (see Section 3.2) the greater exit probability for exposed firms after 2004 suggests a cleansing effect of the 2004 Reform.

A number of potential channels can explain surviving firms' productivity gains. In line with efficiency wages, higher wages could increase workers' job satisfaction and effort, and reduce labor-force turnover within firms, increasing overall productivity.[20] The cost shock could also trigger the adoption of better management or organizational practices. Note that the adoption costs related to management or organizational practices may not be monetary. Recent work on South Asia has identified changes in management and production practices that can trigger substantial productivity gains without major investments in specific machines or any form of physical capital (Bloom et al., 2013; Atkin et al., 2015). Unfortunately, the firm-level data we use here do not include any proxies for 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 cost reduction and productivity enhancement. The positive effect of inventory reduction on productivity has been shown

[20]The considerable rate of worker turnover in Chinese firms has been the subject of growing concern in China over the past fifteen years (Bloom et al., 2015).

Table 10: Minimum wage repercussion channels

Dependent variable	Δ firm outcome, 2001-03 & 2003-05							
	Exit		Inventory over output		Capital over labor		Cash over revenue	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposed Firm	-0.003		0.200		-0.037		0.269^{b}	
	(0.008)		(0.132)		(0.042)		(0.133)	
Exposed Firm × Reform	0.011		-0.254^{c}		0.094^{c}		-0.527^{b}	
	(0.010)		(0.143)		(0.052)		(0.259)	
Exposure		0.079^{a}		0.352^{c}		0.116^{a}		0.460
		(0.009)		(0.205)		(0.038)		(0.370)
Exposure × Reform		0.032^{a}		-0.312^{c}		0.160^{b}		-0.777^{c}
		(0.012)		(0.164)		(0.071)		(0.405)
City-sector 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
Observations	161,575	161,575	125,451	125,451	125,927	125,927	125,455	125,455
R-squared	0.25	0.25	0.29	0.29	0.26	0.26	0.19	0.19

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 in t being lower than the local minimum wage in $t+2$. Firm exposure is measured as (ln Local Minimum Wage$_{t+2}$ - ln Firm-level wage$_{t}$). Reform is a dummy for the 2003-2005 period. Firm-level controls in the odd columns include dummies for the firm-level decile in terms of initial employment, wages and labor productivity (normalized by the median observed in the city), as well as dummies identifying State-owned firms, foreign firms and exporting firms. Firm-level controls in the even columns include dummies for the firm-level decile in terms of initial employment and labor productivity, as well as dummies for State-owned firms.

for example in Japanese (Lieberman and Demeester, 1999) and Indian (Bloom et al., 2013) firms. Columns (3) and (4) of Table 10 show the relationship between the 2004 minimum wage Reform and the firm-level inventory to output ratio. The negative and significant coefficients on the interaction terms between our proxies for exposure and the Reform dummy suggest that inventory management practices improved after the Reform. The average inventory to output ratio was 0.73 for exposed firms in 2003, so that the point estimate of -0.25 on the exposed dummy interaction implies a 34% lower inventory over output ratio following the reform.

Part of the adjustment may also operate through 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 to labor ratio (fixed assets for production purposes over employment). The difference in difference coefficients in columns (5) and (6) of Table 10 are positive and significant at the 10% confidence level. The point estimates in column (5) suggest that the capital to labor ratio growth rate is nearly 10 percentage points higher in exposed firms following the Reform.

Our findings thus suggest that the 2004 Reform reduced inventory and boosted the capital to 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 important feature of the Chinese economy is its malfunctioning 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 (7) and (8) of Table 10 show the difference in difference estimation of the cash to sales ratio.[21] We find that firm-level cash falls post-Reform for exposed compared to non-exposed firms. The average 2003 exposed-firm cash to sales ratio is 1.28, so that our estimates imply a 41% fall in the cash balance for exposed firms. The firm-level improvements made by exposed firms post-Reform hence come at the cost of a reduction in cash balances.

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) and Atkin et al. (2015) describe multiple barriers to technology adoption in developing countries: utility cost, incomplete information, present bias, time constraints, 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 2004 minimum-wage Reform may have increased the value of adopting new and better technologies or management practices.

Another question relates to the reason why the low-productivity firms that improve after the minimum wage 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 a 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 partly explain the situation, 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' reaction to the 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.

[21] Cash is defined as the average current asset balance deducting net account receivables and inventory.

6.2 Alternative explanations

We now ask whether there are alternative explanations of the zero employment but positive firm-level productivity effects we attribute to minimum wages.

6.2.1 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). 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 workers in the National Business Surveys, the substitution should produce a negative effect of minimum wages on firm-level employment. This is inconsistent with what we find. 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, total hours in exposed firms should rise relative to non-exposed firms, which could explain the rise in labor productivity.

We test for this scenario 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 in 2000.[22] The number of migrants is calculated as the number of people without a local residence permit (*hukou*),[23] which city-level information comes from the population census of 2000. The results in Table A-7 in the Appendix reveal no difference in the relative change in employment and productivity between exposed and non-exposed firms following the Reform according to local migration intensity.[24]

In the same vein, we check that the relative wage growth after the 2004 Reform does not mask a compensating fall in fringe benefits. This pattern would be compatible with the substitution of migrants for regular workers, as migrants receive fewer fringe benefits. The first three columns of Table A-8 show the results from the estimation of Equation (1) with the change in total wages per employee, including fringe benefits, between t and $t+2$ as the dependent variable. In columns 4 to 6 the explained variable is the change in the fringe benefits (or welfare pay) that firms provide to their employees between t and $t+2$.

The results confirm larger relative growth in average total wages in exposed firms. The

[22] In unreported robustness checks (available upon request) we find similar results when using the ratio of migrants to residents in the working-age population.

[23] 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.

[24] The split is based on how the city migration intensity compares to the national average.

size of these estimated coefficients is comparable to those for average labor pay. As further confirmed by the positive difference in difference coefficients, fringe benefits were then not adjusted downwards to compensate for higher wages.

Finally, 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 earn less than the hourly minimum wage. However, this low-wage gap between migrants and local workers was lower in 2005 than in 2001: the "cost advantage" of migrant workers fell after the 2004 Reform. This is in line with one of the Reform's objectives which was to improve migrant coverage in terms of labor standards.

Overall, these results do not lend support to the hypothesis that exposed firms substituted migrants for local workers in response to the higher and tighter minimum wages imposed by the 2004 Reform.

6.2.2 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, and especially those that were the most exposed to the minimum-wage hike, might require both their local and migrant workers to increase their work hours. As we observe the number of employees, but not hours worked, the lack of any employment effects and the rise in productivity post-2004 could show longer work hours in exposed firms. We cannot directly test for this mechanism. However, Du and Pan (2009) show that work hours for both migrants and resident workers between 2001 and 2005 actually fell in China. 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. In spite of falling work hours, the average value of 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 reduce the number of hours worked, in line with the previous UK results of Machin et al. (2003).

Given the above, the "number of hours" mechanism does not seem a likely explanation of our results.

6.2.3 Pricing strategy

A last potential explanation is that firms adjusted to the 2004 Reform by increasing output prices. If the price-elasticity of demand is sufficiently low, firms can then raise wages and

not change their employment (see Aaronson, 2001, in the US and Canada and Harasztosi and Lindner, 2015, in Hungary for example). With rising prices, firm labor productivity and TFP measured in value would also increase. However profitability should also decrease (at least slightly) due to incomplete pass-through of the cost shock into prices (the rest being absorbed by firms through lower markups). Conforming to this logic, Harasztosi and Lindner (2015) find a slightly negative effect of minimum wage on profitability for example.

We do not directly observe prices. However, if these effects are at work, they should be heterogeneous across sectors. Under perfect competition, price equals marginal cost: additional labor costs such as those from minimum-wage rises should then be passed on to consumers in the form of higher prices (Draca et al., 2011). On the contrary, in less-competitive industries firms can bear part of the wage increase through lower mark-ups. Draca et al. (2011) find UK evidence of greater falls in profitability in less-competitive industries following the introduction of the UK minimum wage at the end of the 1990s.

We thus look at sectoral variation according to the degree of competitive pressure. Aghion et al. (2015) argue that competition is a key determinant of how industrial policy affects firm performance. Looking at competition will thus also allow us to check for possible firm-level price reactions to the 2004 minimum-wage Reform. If this mechanism is at play, we should see larger productivity increases for exposed firms in more competitive industries.

As in Draca et al. (2011) we split industries into "high" and "low" competition according to how their competition indicator compares to the national median. We follow Aghion et al. (2015) and measure competition using the Lerner Index, which assesses the size of markups relative to the firms' total value added.[25] Table A-9 shows no difference in the impact of the 2004 Reform between high- and low-competition industries. There continue to be no negative repercussions on firm employment or profitability. That the measured efficiency gains do not differ by sectoral competition, together with the absence of effect on profitability, suggests that omitted output prices are not likely to account for the productivity effect we measure.

7 Conclusion

We here considered the repercussions of the 2004 minimum-wage shock in China 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-trend analysis and IV. We find that 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 employment and profits. These

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

productivity gains at least partly reflect better inventory management and greater capital investment, at the cost of lower cash flow. Overall, the minimum-wage Reform in China has produced a cleansing effect. Given the multiple channels of firm-level reaction to minimum wages (employment, price, productivity etc.), the effects of minimum wages likely greatly depend 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 relevant.

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Appendix

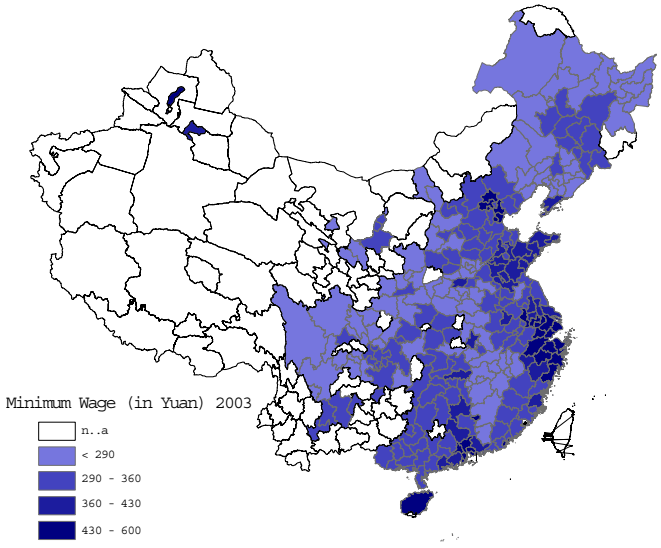

Figure 1: Monthly minimum wages in 2003 (Yuan)

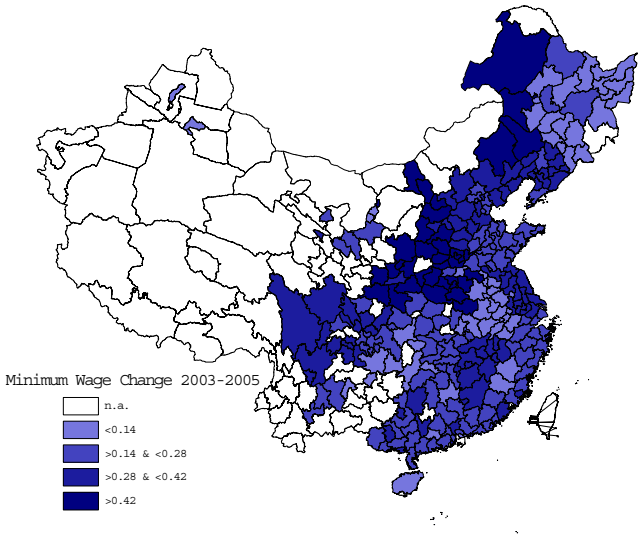

Figure 2: Δ Monthly minimum wages 2003-05

Table A-1: Summary statistics on exposure and wage evolution - Whole sample

Firm type	Pre-Reform period 2001-03 Exposed	Pre-Reform period 2001-03 Non-exposed	Reform period 2003-2005 Exposed	Reform period 2003-2005 Non-exposed
Number present in our sample at t	22,210	112,194	34,282	129,526
of which alive at t+2	14,818	91,026	22,424	101,773
Survival rate	0.67	0.81	0.65	0.79
	Surviving firms			
Mean Δ ln wage	0.58	0.06	0.79	0.16
Median Δ ln wage	0.54	0.09	0.68	0.13
s.d. Δ ln wage	0.71	0.47	0.62	0.45
Mean Δ ln labor productivity	0.38	0.16	0.46	0.21
Median Δ ln labor productivity	0.36	0.17	0.43	0.21
s.d. Δ ln labor productivity	1.07	0.86	1.01	0.87
Mean Δ ln employment	-0.15	0.04	-0.05	0.06
Median Δ ln employment	-0.04	0	0	0.01
s.d. Δ ln employment	0.65	0.51	0.60	0.51
	All firms			
Mean Δ ln Minimum wage	0.12	0.12	0.28	0.25
Median Δ ln Minimum wage	0.11	0.12	0.26	0.24
s.d. Δ ln Minimum wage	0.09	0.08	0.11	0.10

Authors' calculations from the 2001, 2003 and 2005 NBS annual surveys. Δ indicates the change between t and t+2, with t being either 2001 or 2003. See the main text for details.

Table A-2: Summary statistics on firm characteristics

Firm type	Year 2001 Exposed	Year 2001 Non-exposed	Year 2003 Exposed	Year 2003 Non-exposed
	Whole sample			
Mean wage	266	876	358	1020
Mean labor productivity	37	61	56	78
Mean employment	246	304	221	285
Share of exporting firms	0.19	0.29	0.21	0.31
	Restricted sample			
Mean wage	287	607	385	732
Mean labor productivity	38	46	58	59
Mean employment	239	239	217	226
Share of exporting firms	0.20	0.28	0.21	0.31

Authors' calculations from the 2001 and 2003 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 in 2003 by sector

Industry (2 digit)	Share of exposed firms in 2003
Processing of Food from Agricultural Products	0.34
Manufacture of Beverages	0.31
Manufacture of Food	0.30
Processing of Timber, Manufacture of Wood	0.29
Manufacture of Rubber Products	0.28
Manufacture of Paper and Paper Products	0.26
Manufacture of Textile	0.25
Manufacture of Non-metallic Mineral Products	0.25
Manufacture of Artwork and Other Manufacturing	0.24
Manufacture of Leather, Fur etc.	0.24
Manufacture of Cultural & educational Products	0.23
Manufacture of Wearing Apparel & Footwear	0.22
Manufacture of Furniture	0.21
Processing of Petroleum & Coking	0.20
Manufacture of Raw Chemical Materials & Chemical Products	0.20
Smelting and Pressing of Ferrous Metals	0.20
Manufacture of Metal Products	0.19
Manufacture of Plastic Products	0.19
Printing, Reproduction of Recording Media	0.19
Smelting and Pressing of Non-ferrous Metals	0.18
Manufacture of Medicines	0.17
Manufacture of Chemical Fibers	0.17
Manufacture of Special Purpose Machinery	0.17
Manufacture of Electrical Machinery and Equipment	0.16
Manufacture of General Purpose Machinery	0.16
Recycling and Disposal of Waste	0.16
Manufacture of Instruments and Appliances & Office Machinery	0.15
Manufacture of Transport Equipment	0.15
Manufacture of Telecommunication Equipment & Computers	0.12
Manufacture of Tobacco	0.07

Authors' calculations from the 2003 NBS annual survey and Minimum wage data. Exposed firms are firms which wage in 2003 is below the city-level minimum wage in 2005. See the main text for details.

Table A-4: First stage: Instrumental-variable approach with exposed dummy

Dependent variable	Exposed dummy (1)	Exposed dummy× Reform (2)
Predicted Firm exposed dummy	0.224[a]	-0.323[a]
	(0.057)	(0.022)
Predicted Firm exposed dummy × Reform	-0.049[c]	0.679[a]
	(0.025)	(0.033)
Ln Firm employment	-0.0001	-0.003[a]
	(0.001)	(0.001)
Ln Firm Wage	-0.801[a]	-0.503[a]
	(0.050)	(0.031)
Ln Firm Labor productivity	-0.001	-0.0002
	(0.003)	(0.001)
Exporting Firm	-0.005	-0.005[b]
	(0.003)	(0.002)
State-owned Firm	0.003	-0.006
	(0.005)	(0.004)
Foreign Firm	-0.0004	0.0002
	(0.004)	(0.003)
Observations	125,927	125,927
R-squared	0.75	0.73
Shea partial R-squared	0.06	0.41
Test of excluded instruments: F(2,260)	7.84[a]	404.66[a]
Prob > F	0.001	0.001

Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 2001 or 2003. Exposed is a dummy for the average wage in the firm in t being lower than the local minimum wage in $t+2$. Reform is a dummy for the 2003-2005 period.

Table A-5: Minimum wages and firm outcomes: exposure dummy, whole sample

Dependent variable	Δ firm outcome, 2001-03 & 2003-05				
	Ln wage (1)	Ln employment (2)	Ln labor productivity (3)	Ln TFP (LP) (4)	Profit over output (5)
Exposed Firm	-0.038[b]	-0.034[a]	0.022	0.001	-0.037[b]
	(0.016)	(0.009)	(0.017)	(0.016)	(0.018)
Exposed Firm × Reform	0.159[a]	0.012	0.043[a]	0.043[a]	0.032
	(0.015)	(0.011)	(0.015)	(0.016)	(0.023)
City-sector fixed effects	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes
Observations	224,788	224,788	224,108	222,011	224,005
R-squared	0.44	0.28	0.34	0.32	0.27

Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 2001 or 2003. Exposed is a dummy for the average wage in the firm at t being lower than the local minimum wage at $t+2$. Reform is a dummy for the 2003-2005 period. Firm-level controls include dummies for firm-level decile in terms of initial employment, wage and labor productivity (normalized by the median observed in the city), as well as dummies for State-owned firms, foreign firms and exporting firms. All the right-hand side variables are measured at t.

Table A-6: Minimum wages and firm outcomes: exposure dummy, firm fixed effects

Dependent variable	Δ firm outcome, 2001-03 & 2003-05				
	Ln wage	Ln employment	Ln labor productivity	Ln TFP (LP)	Profit over output
	(1)	(2)	(3)	(4)	(5)
Exposed Firm × Reform	0.282[a]	0.014	0.051[c]	0.060[b]	0.099
	(0.012)	(0.014)	(0.027)	(0.026)	(0.107)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes
Observations	97,616	97,616	97,063	96,135	97,057
R-squared	0.83	0.75	0.68	0.70	0.55

Standard errors in parentheses are clustered at the firm level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 2001 or 2003. Exposed is a dummy for the average wage in the firm at t being lower than the local minimum wage at $t+2$. Reform is a dummy for the 2003-2005 period. Firm-level controls include a dummy for exposed firms. All the right-hand side variables are measured at t.

Table A-7: Heterogeneity depending on migration intensity

Dependent variable	Δ firm outcome, 2001-03 & 2003-05				
	Ln wage	Ln employment	Ln labor productivity	Ln TFP (LP)	Profit over output
	(1)	(2)	(3)	(4)	(5)
Exposed Firm	-0.020	-0.031[b]	-0.006	-0.035[c]	-0.020[c]
	(0.015)	(0.012)	(0.021)	(0.021)	(0.011)
Exposed Firm × Reform	0.104[a]	0.014	0.042[c]	0.056[b]	0.007
	(0.015)	(0.014)	(0.024)	(0.023)	(0.007)
Exposed × High-migration city	0.041[b]	-0.004	0.027	0.044[c]	-0.045
	(0.017)	(0.015)	(0.029)	(0.023)	(0.037)
Exposed × High-migration city × Reform	-0.047[b]	-0.006	-0.020	-0.043	0.073
	(0.019)	(0.021)	(0.030)	(0.027)	(0.066)
City-sector fixed effects	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes
Observations	125,927	125,927	125,493	124,254	125,449
R-squared	0.47	0.34	0.40	0.38	0.52

Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 2001 or 2003. Exposed is a dummy for the average wage in the firm at t being lower than the local minimum wage at $t+2$. Reform is a dummy for the 2003-2005 period. "High-migration city" is a dummy for the share of migrants in the city population being above the average observed across cities. Firm-level controls include dummies for firm-level decile in terms of initial employment, wages and labor productivity (normalized by the median observed in the city), as well as dummies for State-owned firms, foreign firms and exporting firms. All the right-hand side variables are measured at t.

Table A-8: Minimum wages and Fringe benefits

Dependent variable	Δ Ln firm average total wage 2001-03 & 2003-05		Δ Ln firm fringe benefits 2001-03 & 2003-05	
	(1)	(2)	(3)	(4)
Exposed Firm	0.007		0.033	
	(0.012)		(0.024)	
Exposed Firm × Reform	0.072[a]		0.052[b]	
	(0.011)		(0.023)	
Exposure		0.793[a]		0.622[a]
		(0.020)		(0.057)
Exposure × Reform		0.201[a]		0.110[a]
		(0.014)		(0.034)
City-sector fixed effects	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes
Observations	125,924	125,924	82,284	82,284
R-squared	0.46	0.48	0.21	0.21

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 being lower than the local minimum wage at $t+2$. Firm exposure is measured as (ln Local Minimum Wage$_{t+2}$ - ln Firm-level wage$_t$). Reform is a dummy for the 2003-2005 period. Firm-level controls in columns (1) and (4) include dummies for firm-level decile in terms of initial employment, wages and labor productivity (normalized by the median observed in the city), as well as dummies for State-owned firms, foreign firms and exporting firms. Firm-level controls in columns (2) and (5) include dummies for firm-level decile in terms of initial employment and labor productivity (normalized by the median observed in the city), as well as dummies for State-owned firms, foreign firms and exporting firms. Firm-level controls in columns (3) and (6) include firm-level initial employment, wages and labor productivity, as well as dummies for State-owned firms, foreign firms and exporting firms. All the right-hand side variables are measured at t. The instrument for the "Exposed" dummy in the IV procedure is a dummy for the firm average wage at t being below the predicted minimum wage at $t+2$ 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-9: Heterogeneity depending on sectoral competition intensity

Dependent variable	Δ firm outcome, 2001-03 & 2003-05				
	Ln wage	Ln employment	Ln labor productivity	Ln TFP (LP)	Profit over output
	(1)	(2)	(3)	(4)	(5)
Exposed Firm	-0.009	-0.032[a]	0.008	-0.009	-0.045
	(0.013)	(0.010)	(0.020)	(0.020)	(0.035)
Exposed Firm × Reform	0.082[a]	0.016	0.038[b]	0.041[b]	0.057
	(0.011)	(0.011)	(0.018)	(0.017)	(0.053)
Exposed × Low competition sector	0.036[b]	-0.002	-0.002	-0.009	0.001
	(0.015)	(0.014)	(0.027)	(0.029)	(0.027)
Low competition sector × Reform	-0.027[a]	0.006	-0.002	0.002	0.022
	(0.010)	(0.014)	(0.022)	(0.023)	(0.031)
Exposed × Low competition sector × Reform	-0.014	-0.017	-0.019	-0.027	-0.027
	(0.015)	(0.015)	(0.027)	(0.031)	(0.044)
City-sector fixed effects	Yes	Yes	Yes	Yes	Yes
City-year fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes
Observations	125,927	125,927	125,493	124,254	125,449
R-squared	0.47	0.34	0.40	0.38	0.52

Standard errors in parentheses are clustered at the city level. [a], [b] and [c] indicate significance at the 1%, 5% and 10% confidence levels. Δ indicates the change between t and $t+2$, with t being either 2001 or 2003. Exposed is a dummy for the average wage in the firm at t being lower than the local minimum wage at $t+2$. Reform is a dummy for the 2003-2005 period. "Low competition sector" is a dummy denoting sectors with an above median Lerner index (measuring the importance of markups: Aghion et al., 2015). Firm-level controls include dummies for firm-level decile in terms of initial employment, wages and labor productivity (normalized by the city median), as well as dummies for State-owned firms, foreign firms and exporting firms. All the right-hand side variables are measured at t.

Institut de Recherches Économiques et Sociales
Université catholique de Louvain

Place Montesquieu, 3
1348 Louvain-la-Neuve, Belgique

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