Hukou Reform, Labour Reallocation an Firm Growth in China

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Abstract: This paper empirically examines how local manufacturing firms of non-state sector respond to labour reallocation (*Hukou* Reform) in China. A conceptual framework is proposed in which the labour reallocation not only increases labour supply to non-state sector but might also increase non-state sector's access to the local product market. Using Chinese Annual Industrial Firm Survey (2000-2007), we estimate the impact of the labour reallocation on firm employment in a difference-in-differences framework. A propensity score matching approach is used to reduce potential endogeneity issues caused by selective reform adoption. Main findings are as follows. First, the *Hukou* reform increases average firm employment of the non-state sector by 5.1% three years after reform adoption but decreases average firm employment of the state-sector by 5.6% during the same period. Second, the *Hukou* reform increases non-exporters' average employment of the non-state sector by 5.9% but negligible for exporters (0.6%). Third, the *Hukou* reform increases non-exporters' average wage by about 9% three years after reform adoption. Our results provide new insights on the relationship between labour allocation and firm performance in China, in contrast to Imbert et al. (2016).

Keywords: *Hukou* Reform; Labour Reallocation; Non-state Sector; Firm Growth; China

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1 Introduction

A recent World Economic Outlook report (October, 2016, IMF) reveals that a 1 percent increase in the migrant share of the adult population results in 2 percent higher GDP in the long run for receiving developed economies. However, it is uncertain whether this positive relationship also holds in developing countries-such as China-which are witnessing growing internal migration flows. According to the latest two waves of China Population Census data (2000, 2010) released by the National Bureau of Statistics (NBS) the number of internal migrants in China increased from 144 million in 2000 to 261 million in 2010, with an average annual growth rate of 6.1%.[2] During the same period, the average annual growth rate of gross domestic product (GDP) in China reached 10.5%.[3] Is there any relationship between increasing internal migration flows and fast economic growth in China during the 2000s? To uncover this relationship, this paper empirically examines the impact of a reform of the *Hukou* System, a pro-migration reform, on Chinese manufacturing firms' behavior.

An increasing number of empirical studies examining the impact of migration on the local economy have been conducted at the firm-level thanks to the availability of micro data (Ottaviano and Peri 2013). Recent findings on firms shed new light on the impact of migration. For example, Peri (2012) finds that immigrants to the United States promote a more efficient task specialization, thus increasing TFP and, at the same time, promote the adoption of unskilled-biased technology. Also in the United States, Olney (2013) finds that firms respond to immigration by increasing the number of establishments but with little impact on employment within existing establishments. Dustmann and Glitz (2015) find that local firms in Germany adjust to changes in local labour supply through adjustments of the factor price in the

[2] Internal migrants in China are defined as people that have lived away from their registered village (township, street) for more than 6 months. Among them, a large number of internal migrants come from rural areas.

[3] Given the base year in 1978 (100), the gross domestic product increased from 760 in 2000 to 2073 in 2010 (NBS).

non-tradable sector but not in the tradable sector. In the tradable sector, most of the adjustment to changes in relative factor supplies takes place within firms by changing relative factor intensities. A more recent study by Imbert et al. (2016) finds that migration lowers labour costs and increases the profitability of manufacturing firms in China.

Most of the recent firm-level studies on immigration focus on developed countries such as the US (Peri 2012; Olney 2013), Germany (Hornung 2014; Dustmann and Glitz 2015), Israel (Paserman 2013), the UK (Ottaviano, Peri and Wright 2016). Only a few studies are relevant to developing countries such as Mexico (Woodruff and Zenteno 2007), Thailand (Pholphirul 2013) and China (Imbert et al. 2016; Wang et al. 2017). To the best of our knowledge, Imbert et al. (2016) is the first to study the impact of migration on firms in China.[4] Different from Imbert et al. (2016) who use variations in international prices and climatic conditions to build shocks to agricultural income and then use them as instruments for migration flows in China, this study uses the reform of the *Hukou* System, which was adopted by local governments at different points in time, to capture the effects of migration shocks on local firms. Different from Wang et al. (2017) who study how the increasing labour market flexibility, induced by the *Hukou* reform, affects firms' responses to trade openness in employment, this study focuses on whether local manufacturing firms' employment responds to the *Hukou* reform over time. Particularly, this study takes more efforts to address the reform endogeneity issue.

How might the *Hukou* reform affect local manufacturing firms? This study explores the impact of the reform on local manufacturing firms resulting from migration. A recent study in China indicates that rural-urban migration makes local firms more profitable by reducing labour costs (Imbert et al. 2016). Another study by Wang et al. (2017) suggests that the reform causes firms to to respond more strongly

[4] Meng and Zhang (2010) as well as Combes et al. (2105) also investigate the impact of migration in China using micro data but from a worker's perspective.

to trade openness in terms of employment by increasing the degree of labour market flexibility. Meanwhile, other studies suggest that internal migration affects inter-regional trade flows by reducing the border effects within the same country based on evidence from developed countries (Millimet and Osang 2007; Combes, Lafourcade and Mayer 2005). Most importantly, the border effects, pioneered by McCallum (1995) are also significant in China (Poncet 2003; Xing and Li 2011), suggesting that a migration shock not only affect labour supply but also might affect local firms' labour demand. This study starts with examining whether manufacturing firms' employment responds to the *Hukou* reform over time.

This study contributes to the literature in the following two aspects: first, we propose an indicator of the *Hukou* reform to measure the "pull" aspect of migration in China, which differs from that of Imbert et al. (2016) who focus on the "push" aspect of inter-regional rural-urban migration.[5] In particular, given the fact that the *Hukou* System not only restricts migration from rural to urban areas but also restricts migration within the urban areas, our indicator of the *Hukou* reform might capture more than rural-urban migration. Second, we use firm-level data to study the impact of the reform on firm growth of non-state sector in China, adding to the relatively few studies on migration and firm performance in developing countries.

Using data from the Chinese Annual Survey of Industrial Firms (CASIF) data for the period 2000-2007, we estimate the impact of the reform on manufacturing firms' employment in a difference-in-differences framework. We additionally adopt a propensity score matching approach to reduce endogeneity issues caused by the possible selective reform adoption at the city level. We find that the reform increases the average firm employment by 2.3% one year after reform adoption, which further increases to 5.1% three years after reform adoption. The reform impact on firm employment is larger for firms located in labour intensive industries relative to skill

[5] Specifically, the pull aspect of migration refers to factors existing in migration receiving regions that encourage or discourage migration, while the push aspect of migration refers to factors existing in migration sending regions that encourage or discourage migration.

intensive industries, larger for firms located in industries that use migrant workers more intensively, suggesting that the reform mainly affects firms that are more labour intensive and use migrant workers more intensively, which is consistent with previous findings that the reform affected unskilled migrations the most. We do not find evidence that simultaneous policies such as minimum wage reform, restructuring of state sector, reduction in the trade policy uncertainty are contaminating these results, neither do we find new firms appearing in the year of reform adoption and afterwards are responsible for these results. Moreover, we find that the reform also affects state-owned firms. In contrast to the non-state-owned firms that expand after the reform adoption, the state-owned firms downsize because more redundant state-owned workers are absorbed by the expanding non-state-owned sector. Finally, we find that the reform increases average wage, suggesting that the reform not only increases labour supply to the non-state sector but also increases non-state sector's access to local product market. The reform impact on average wage is positive when the market channel offsets the labour supply channel.

The remaining sections are structured as follows: the next section reviews the relevant literature; Section 3 introduces data sources and provides descriptive statistics; Section 4 presents the model specification and the identification strategy; Section 5 discusses the empirical results and the last section concludes.

2 Literature Review

This section mainly reviews three strands of literature: the first strand of literature examines the impact of the *Hukou* System in China; the second strand of literature reviews how local industries and firms respond to a labour supply shock. The third strand of literature reviews studies on migration and border effects.

2.1 Impact of the *Hukou* System on Migrations

The first strand of literature focuses on the impact of the *Hukou* System in China. While a large number of empirical studies have examined the impact of the *Hukou* System on workers (Meng and Zhang 2001; Guo and Iredale 2004; Liu 2005; Whalley and Zhang 2007; Afridi et al. 2015; Song 2016), only a few studies focus on its impact on firms (Knight et al. 1999; Meng 2002; Kamal and Lovely 2013; Imbert et al. 2016; Wang et al. 2017). Firm-level studies provide evidence that urban firms are restricted from recruiting migrant workers to protect local native workers in the late 1990s and early 2000s (Knight et al. 1999; Knight and Song 1999; Meng 2002). For example, in a 1995 firm level survey more than 75 percent of firms report to be restricted from hiring rural migrants, while another firm level survey conducted in the early 2000s reveals that more than 50 percent of firms face restrictions in freely hiring rural migrants (Meng 2002). In addition, Kamal and Lovely (2013) suggest that the *Hukou* System prevents the labour allocation from state-owned firms to non-state-owned firms within the urban sector. They argue that this is because the *Hukou* System exerts an implicit tax on non-state-owned firms but not for state-owned firms. Without the *Hukou* System, non-state-owned firms would be much easier to recruit as many workers as they want. Finally, evidence at the macro level suggests that the *Hukou* System is associated with labour misallocation. For example, Ngai et al. (2016) claim that the *Hukou* System has resulted in over-employment in the agriculture sector of 6.7 points and under-employment in the urban non-agricultural sector of 6.3 points using data in 2013. Au and Henderson (2006a, b) further demonstrate that a majority of Chinese cities are undersized and suffer from productivity losses due to the restrictive *Hukou* System by investigating the relationship between city size and productivity. Several other studies indicate that the labour allocation from the state sector to the non-state sector in China is even more distorted than the labour allocation from agricultural to non-agricultural sector (Brandt, Hsieh and Zhu 2008; Brandt, Tombe and Zhu 2013). Overall, these studies on the *Hukou* System above indicate that the *Hukou* System hinders an efficient

labour allocation from the agricultural to the non-agricultural sector as well as labour allocation from over-employed state-sector to non-state sector. It can be therefore be expected that relaxing the *Hukou* System will affect labour allocation from rural to urban areas as well as labour allocation within urban sector, which, to some extent, are different from inter-regional migration inflows captured by Imbert et al. (2016).

2.2 Migration and Labour Supply

The second strand of literature examines recent empirical studies on how local industries and firms respond to immigration inflows, mostly in the United States and European countries (Hanson and Slaughter 2002; Lewis 2003; Gonzalez and Ortega 2011; Olney 2013; Dustmann and Glitz 2015; Imbert et al. 2016). Hanson and Slaughter (2002) examine the mechanisms through which the U.S. states absorb changes in labour supply between 1980 and 1990, distinguishing explicitly between changes in the output mix and changes in production techniques across industries. Following a similar approach for the same period but with more disaggregated data, Lewis (2003) analyzes the extent to which the industrial mix in U.S metropolitan areas adjusts to changes in local factor supply caused by immigration. Both studies find that most of the adjustment happens through within-industry changes, interpreted as changes in production technology. Gonzalez and Ortega (2011) come to similar conclusions for Spain using the period 2001 to 2006. Instead of carrying out the analysis at the industrial level, Dustmann and Glitz (2015) investigate how changes in the skill mix of local labour supply are absorbed by the economy using firm-level data of Germany. They find that, while factor price adjustments are important in the non-tradable sector, labour supply shocks do not induce factor price changes in the tradable sector. In this sector, most of the adjustments to changes in relative factor supplies take place through changes in relative factor intensities. In addition to changes in relative factor intensities, Olney (2013) examines whether firms respond to immigration by expanding their production activities using data on immigration and the universe of establishments in U.S. cities. The author finds that firms respond to

immigration by increasing the number of establishments, and immigration has little impact on employment within existing establishments. One recent study by Imbert et al. (2016) has examined how manufacturing firms respond to a migration shock in China. Using variations in international agricultural prices and local climatic conditions as "push" factors to predict migration inflows into urban areas, the authors estimate the impact of these migrant inflows on firm behaviour using Chinese manufacturing firm-level data from 1999 to 2005 (the same data we use in this chapter). They find that, by increasing labour supply, migration lowers labour costs and increases the profitability of manufacturing firms. Another work by Wang et al. (2017) studies how the *Hukou* reform affects firms' response to trade liberalization in terms of employment, and they find that the reform makes local firms respond more strongly to trade openness.

2.3 Migration and Border Effects

The third strand of the literature considers how migration may change trade patterns by reducing border effects. The literature on border effects was pioneered by McCallum (1995) to measure the trade-diminishing effects of the Canada-US border. The author found that inter-provincial trade in Canada was 20 times larger than trade between Canadian provinces and US states. It has been adapted to evaluate the degree of integration between and within sovereign countries-within Canada (Helliwell and Verdier 2001), within the US (Wolf, 2000), between OECD countries (Wei 1996), and between EU members (Head and Mayer 2000). These analyses all find large border effects. In addition, evidence indicates that immigration reduces border effects and promotes trade flows (Andrews et al. 2017; Egger et al 2012; Peri and Requena 2010; Wagner et al. 2002; Girma and Yu 2002; Rauch 2001; Head and Ries 1998). One of the main mechanisms behind the trade creation effect of migration is through the reduction in information costs. According to Combes et al. (2005), information barriers make it difficult both for consumers to obtain relevant information on the goods produced in another location and for non-local producers to learn the tastes of

consumers or to be aware of the practices of local retailers. Both effects increase transaction costs and thus perceived prices, which has a negative impact on trade flows. Several other studies find internal migration also significantly reduces border effects within the US (Millimet and Osang 2007) as well as within France (Combes, Lafourcade and Mayer 2005). Finally, using comparable methodologies, researchers have confirmed the existence of border effects in China. Poncet (2003) found that provincial border effects in China increased from 1987 to 1997, and that the trade barriers among provinces was even higher than among members of European Union. Xing and Li (2011) also found that China's internal product trade had a home bias, and inter-provincial trade was significantly affected by provincial borders. Overall, given the significant border effects in China, it seems possible that internal migration, induced by a relaxation of the *Hukou* System, might reduce border effects and might promote domestic trade flows between provinces. If this is the case, local manufacturing firms located in regions with more internal migration may experience increasing domestic trade with the rest of this country.

Given the various institutions and economic development, it is uncertain how firms in developing countries such as China would adjust to increasing migration and whether they would react in a similar way as those in developed countries. Instead of focusing on a specific channel, this study starts with a general evaluation on whether local manufacturing firms respond to the *Hukou* reform or not.

3 Data Sources and Descriptive Statistics

This section introduces data sources and provides some descriptive statistics for the reform and firm-level outcomes.

3.1 Data Sources

We use the Chinese Annual Survey of Industrial Firms (CASIF) data for the period 2000-2007. This data was collected by the National Bureau of Statistics, and covers

all industrial firms in China with annual sales above 5 million RMB.[6] According to the Chinese National Bureau of Statistics (NBS), these firms account for about 80 percent of all industrial value-added activities.

To investigate the relationship between the *Hukou* reform and firm behaviour, our sample is restricted to manufacturing firms located in (prefecture-level) cities which could be matched with the *Hukou* reform dataset.[7] Details on the *Hukou* reform can be found in Chapter 2. In China, most provinces are divided into a number of prefecture-level cities. We find that the number of firms located in cities with reform information accounts for 87 percent of all manufacturing firms in the dataset. In addition, we only keep firms that did not change location during our sample period. To be consistent with my previous chapters, we consider firms located in cities that adopted the reform in 2002, 2003 or 2004 as the reform or treatment group, and consider firms located in cities that adopted the reform in 2008 or afterwards as the non-reform or control group.[8] This means that we exclude all firms located in cities that implemented the reform between 2005 and 2007. Finally, we restrict our final sample to non-state-owned firm, including domestic firms and foreign firms. These non-state-owned firms hire the majority of migrant workers as well as absorb the reallocated workers from state-owned firms to non-state-owned firms in China. The number of non-state-owned firms accounts for 91.2% of total manufacturing firms over the whole period. As some state-owned firms were privatized over the sample period, we only keep non-state-owned firms that did not change their ownership. This leaves us with a final sample of 1,262,314 firms of which 568,471 (45%) are located in reform cities and 693,843 (55%) in non-reform cities. The distribution of sample

[6] All state-owned firms will be included even if annual sales do not satisfy this condition. The same dataset has been used by a large number of studies on Chinese firm behaviors (Brandt et al. 2012; Kee and Tang 2016; Huang et al. 2017; Brandt et al. 2017).

[7] The sectoral classification system has changed in 2002. To make the firm-level panel data comparable and consistent over time, we adjust the old industry code in 2002 and before (GB/T 4754-1994) with the new classification system GB/T 4754-2002. Also the administrative divisions of China have changed. We adjust the code of administrative divisions in each year with the same classification System released by the NBS on Dec 31st, 2008 (http://www.stats.gov.cn/tjsj/tjbz/xzqhdm/).

[8] Firm-level data in 2008 and afterwards are not available to us for the time being. We also find that the latest publications on firm behaviours in China only use firm data for the period 1998 to 2007 (Kee and Tang 2016; Huang et al. 2017).

size after each adjustment is presented in Table 3.1.

Table 3.1 Sample construction

Year	Original Sample	Adjust for Classification change & manufacturing firms	Firms located in Prefecture cities	Firms Without Location change	Firms located in treatment and control cities	Non-state-owned firms (private & foreign firms)
2000	162,884	140,862	120,748	120,580	111,897	87,485
2001	171,224	153,665	128,768	128,553	119,422	98,516
2002	181,461	164,667	139,838	139,603	129,137	110,934
2003	196,190	179,250	153,989	153,728	141,607	126,526
2004	276,422	256,346	221,747	221,467	204,644	189,684
2005	271,789	250,746	217,683	217,391	199, 084	188,281
2006	301,902	278,239	244,906	244,605	233, 741	214,024
2007	336,768	312,384	277,313	276,997	254, 012	246,864
Total	1,898,640	1,736,159	1,504,992	1,502,924	1,383, 544	1,262,314

Notes: This table shows the sample cleansing process. Column (2) is the original sample size; Column (3) adjusts the (sectoral/regional) classification change and restricts firms to the manufacturing industry; Column (4) restricts firm to those that locate in the prefecture-level cities and Column (5) keeps firms that does not change location during the sample period; Column (6) keeps firms located in treatment and control groups; Column (7) keeps non-state-owned firms.

An important data issue is that a resampling took place in 2004 after a national firm-level census conducted in that year. Although the Chinese Annual Survey of Industrial Firms (CASIF) aims to cover all industrial firms in China with annual sales above 5 million RMB, smaller firms are not sampled. For example, in a period with rapid firm creation and expansion, information on certain firms may not be reported in a timely manner to the local government. As a result, some firms eligible for the survey were missing in previous survey years but were included since 2004. According to table 3.1, the annual firm sample size increased from 198,190 to 276,422 from 2003 to 2004, but increased at a lower rate from 162,884 in 2000 to 198,190 in 2003.

In practice, given the fact that we have three different reform cohorts, we take the following approach to construct our final samples for estimation: first, we

construct the treatment group and control group for each reform cohort. For example, for reform 2004, the treatment group refers to non-state-owned firms located in cities that adopted the reform in 2004, while the control group is defined as non-state-owned firms located in cities that adopted the reform in 2008 or afterwards. To estimate the reform impact on firm outcomes, we keep firms that exist for at least two years over the whole period: one year in the year of reform adoption and another year in the year after the reform adoption. Therefore, the final sample of reform 2004 is restricted to non-state-owned firms that exist at least for two years: one year in 2004 and another year after 2004. This leaves us with a sub-sample of 567,642 firms of which 122,745 (21.6%) are located in treatment cities and 444,897 (78.3%) in control cities. We take the same approach for the remaining two reform cohorts. For reform 2003, we have a sub-sample of 460,570 firms of which 143,297 (31.1%) are located in treatment cities and 317,273 (68.8%) in control cities. For reform 2002, we have a sub-sample of 333,074 firms of which 31,413 (9.4%) are located in treatment cities and 301,661 (90.5%) in control cities. Our overall estimation is a combination of all sub-samples based on three reform cohorts (2002, 2003 and 2004). This leaves us with a final sample of 1,361,286 firms of which 197,455 (21.8%) are located in treatment cities and 1,063,831 (78.1%) in control cities. Table 3.2 shows the distribution of cohort samples over time.

Table 3.2 Final samples used in the analysis for each reform cohort

Year	Cohort 2002			Cohort 2003			Cohort 2004		
	Treatment	Control	Total	Treatment	Control	Total	Treatment	Control	Total
2000	2,795	26,770	29,565	9,438	20,434	29,872	4,589	19,133	23,722
2001	3,651	38,862	42,513	13,794	29,610	43,404	7,429	27,385	34,814
2002	5,008	51,124	56,132	17,319	38,408	55,727	9,465	35,040	44,505
2003	4,958	49,898	54,856	23,211	51,874	75,085	13,048	46,119	59,167
2004	4,151	38,101	42,252	22,377	50,065	72,442	24,406	87,646	112,052
2005	3,959	35,680	39,639	21,173	46,870	68,043	23,903	85,506	109,409
2006	3,716	33,336	37,052	19,763	43,668	63,431	21,947	79,212	101,159
2007	3,175	27,890	31,065	16,222	36,344	52,566	17,958	64,856	82,814
Total	31,413	301,661	333,074	143,297	317,273	460,570	122,745	444,897	567,642

Notes: the sample is restricted to those firms that exist for at least two years over the whole period: one year in the year of reform adoption, and another year in the year after the reform adoption. Taken reform 2004 for example, we keep firms that exist for at least two years: one in 2004 and another in 2005 or afterwards.

3.2 Descriptive Statistics

1. Pre-existing Differences of Treatment and Control Groups

To provide a more detailed picture about the characteristics of manufacturing firms in the treatment and control groups, Table 3.3 shows summary statistics of firm characteristics by location for the pre-treatment period (2000-2003) using reform cohort 2004 as an example. Results reported in Columns (2) and (3) indicate that firms located in reform prefectures tend to have slightly more output, more sales, higher value added tax, lower wage bills, more employment, more total intermediate inputs for production, less assets, they pay significantly lower wages per worker, have a significantly lower export propensity, are significantly less likely to be foreign firms, and are significantly older. These firms also tend to have slightly larger profits. Column (4) shows the result of a test of the difference between firms in reform and non-reform prefectures using the following simple regression model: $Y_{itc} = \alpha + \beta_1 D_c + \gamma year_t + \varepsilon_{itc}$, where Y_{itc} is the characteristic to be compared, D_c is a dummy variable which equals 1 for the treatment group and 0 otherwise, $year_t$ is the year dummy and ε_{itc} is the error term. Standard errors are clustered at the prefecture level. Estimation results indicate that the differences in firm characteristics between

treatment and control groups are not statistically significant except for variables such as total wage bills, wages per worker, export propensity, percentage of foreign firms, and firm age. To test whether these pre-treatment differences change over time, we further estimate the following model for pre-treatment years: $Y_{itc} = \alpha + \beta_2 \mathrm{D}_c + \gamma \mathrm{year}_t + \sum_{s=2001}^{s=2003} \delta_s \mathrm{D}_c * \mathrm{year}_s + \varepsilon_{itc}$ where $\mathrm{D}_c * \mathrm{year}_t$ is the interaction term between reform dummy and year dummy. Coefficient δ_s captures the pre-treatment differences over time. Estimation results are reported in Columns (5) (6) and (7). We do not find evidence that these pre-treatment differences between treatment and control groups changes significantly over time except for firm age, total employment and total value added tax. Particularly, we find that firms' total employment was actually *decreasing* in treatment group relative to control group over time.

Using the same approach, we compare the pre-treatment differences between treatment and control groups using reform cohort 2003 and reform cohort 2002, respectively. All the results indicate that firms in the treatment group are different from firms in control group to some extent. Particularly, we find the pre-treatment differences between treatment and control groups in terms of firm employment change over time, suggesting that a simple difference-in-differences approach might be problematic in estimating the reform impact on firm employment.

Table 3.3 Firm-level Summary Statistics for treatment and control group for Pre-treatment period (reform 2004)

Variables	Total	Reform	Non-reform	Diff. (β_1)	DiD (δ_{2001})	DiD (δ_{2002})	DiD (δ_{2003})
Total output (in million RMB)	71.7 (479)	73.1 (339)	71.3 (510)	1.72 (10)	-2.00 (2)	-0.50 (4)	-3.59 (6)
Total sales (in million RMB)	69.8 (470)	71.2 (334)	69.4 (501)	1.69 (10)	-1.44 (2)	-0.38 (4)	-2.76 (6)
Total profits (in million RMB)	3.5 (44)	3.4 (23)	3.5 (49)	0.16 (0.6)	-	-0.09 (0.24)	-0.20 (0.31)
Total value added tax (in million RMB)	2.1 (19)	2.1 (12)	2.1 (20)	0.08 (0.2)	0.07 (0.11)	-0.19 (0.12)	-0.26 (0.16)*
Total wage (in 1,000 RMB)	3391.6 (45481)	2749.8 (8131)	3565.3 (51089)	-812 (472)*	-666 (656)	-136 (179)	-247 (183)
Total employment (people)	290.5 (673)	198.5 (663)	288.4 (675)	11.16 (23)	-11.24 (7)	-22.39 (10)**	-34.57 (13)**
Total intermediate inputs (in million RMB)	54.7 (381)	55.1 (272)	54.6 (405)	0.46 (8)	-0.72 (2)	0.76 (3)	-1.40 (4)
Total assets (in million RMB)	61.9 (342)	60.5 (282)	62.3 (356)	-1.61 (7)	0.82 (2)	0.26 (3)	-3.34 (4)
Total fixed assets (in million RMB)	23.3 (165)	23.4 (145)	23.3 (170)	0.12 (2)	-0.32 (1)	0.33 (1)	-0.87(2)
Total liquid assets (in million RMB)	32.7 (191)	31.0 (150)	33.2 (201)	-2.14 (4)	0.82 (1)	-0.24 (1)	-2.40 (2)
Firm age	9.6 (9)	10.3 (10)	9.4 (9)	0.92 (0.5)*	-0.59 (0.47)	-0.91 (0.55)	-1.33 (0.76)*
Wage per worker (in 1,000 RMB)	11.5 (197)	9.2 (8)	12.1 (222)	-2.92 (1)***	-2.41 (2)	-0.25 (0.92)	0.60 (0.44)
Exporters (exporter=1)	0.53 (0.4)	0.38 (0.4)	0.58 (0.4)	-0.19 (0.05)***	-0.00 (0.01)	0.00 (0.01)	0.00 (0.01)
Foreign firms (foreign=1)	0.28 (0.4)	0.21 (0.4)	0.30 (0.4)	-0.09 (0.05)*	0.00 (0.01)	0.02 (0.01)	0.02 (0.02)
N	162,208	34,531	127,677	-	-	-	-

Notes: This table compares the differences in firm characteristics between treatment and control group in the pre-treatment period (2000-2003). Treatment firms are non-state-owned firms located in cities that have adopted the reform in 2004, and the control group consists of non-state-owned firms located in cities that have adopted the reform in 2008 or afterwards. Column (1) shows mean and standard deviation for all firms. Columns (2) and (3) report mean and standard deviation for firms in the treatment group and control group. Column (4) shows the differences in firm characteristics between treatment and control group. Columns (5) (6) (7) report how the differences in firm characteristics between treatment and control group change over time. All price relevant variables are in real value (based on 2000). Standard deviations in parentheses are reported for columns (1) 2) (3). Standard errors in parentheses are reported for columns (4) (5) (6) (7), which are clustered at the city level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.4 Firm-level Summary Statistics for treatment and control group for Pre-treatment period (reform 2003)

Variables	Total	Reform	Non-reform	Diff. (β_1)	DiD (δ_{2001})	DiD (δ_{2002})
Total output (in million RMB)	66.1 (387)	64.7 (281)	66.8 (427)	-2.04 (10)	2.26 (2)	5.67 (2)**
Total sales (in million RMB)	64.4 (384)	63.0 (279)	65.0 (423)	-2.00 (10)	2.14 (2)	5.32 (2)*
Total profits (in million RMB)	3.1 (35)	3.0 (22)	3.1 (4)	-0.07 (0.58)	-	0.35 (0.27)
Total value added tax (in million RMB)	1.9 (16)	1.8 (8)	2.0 (19)	-0.14 (0.25)	-0.05 (0.09)	-0.19 (0.17)
Total wage (in 1,000 RMB)	3256.6 (50005)	2632.4 (7223)	3542.8 (60190)	-911 (515)*	-360 (600)	41 (157)
Total employment (people)	284.8 (667)	265.7 (644)	293.6 (678)	-27.97 (23)	26.92 (7)***	21.17 (6)***
Total intermediate inputs (in million RMB)	51.3 (314)	49.7 (221)	52.0 (348)	-2.27 (7)	1.77 (2)	4.35 (2)*
Total assets (in million RMB)	59.2 (318)	53.7 (265)	61.7 (339)	-7.95 (8)	3.17 (2)	4.21 (2)
Total fixed assets (in million RMB)	22.7 (161)	21.0 (145)	23.4 (168)	-2.42 (3)	1.23 (1.12)	1.44 (1.40)
Total liquid assets (in million RMB)	31.1 (165)	28.2 (120)	32.5 (182)	-4.33 (4)	1.52 (1.23)	2.53 (1.36)*
Firm age	10.2 (9)	11.3 (10)	9.6 (9)	1.71 (0.43)***	0.55 (0.32)*	0.21 (0.40)
Wage per worker (in 1,000 RMB)	11.4 (220)	9.9 (8)	12.1 (266)	-2.21 (1)	-2.50 (2)	-0.69 (0.81)
Exporters (exporter=1)	0.55 (0.49)	0.48 (0.49)	0.58 (0.49)	-0.10 (0.04)**	-0.00 (0.01)	-0.00 (0.01)
Foreign firms (foreign=1)	0.29 (0.45)	0.23 (0.42)	0.31 (0.46)	-0.08 (0.06)	0.02 (001)*	0.03 (0.01)*
N	129,003	40,551	88,452	-	-	

Notes: This table compares the differences in firm characteristics between treatment and control group in the pre-treatment period (2000-2002). Treatment firms are non-state-owned firms located in cities that have adopted the reform in 2003, and the control group consists of non-state-owned firms located in cities that have adopted the reform in 2008 or afterwards. Column (1) shows mean and standard deviation for all firms. Columns (2) and (3) report mean and standard deviation for firms in the treatment group and control group. Column (4) shows the differences in firm characteristics between treatment and control group. Columns (5) (6) report how the differences in firm characteristics between treatment and control group change over time. All price relevant variables are in real value (based on 2000). Standard deviations in parentheses are reported for columns (1) 2) (3). Standard errors in parentheses are reported for columns (4) (5) (6), which are clustered at the city level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.5 Firm-level Summary Statistics for treatment and control group for Pre-treatment period (reform 2002)

Variables	Total	Reform	Non-reform	Diff. (β_1)	DiD (δ_{2001})
Total output (in million RMB)	57.4 (335)	42.4 (212)	58.9 (345)	-16.49 (12)	0.12 (2)
Total sales (in million RMB)	55.7 (333)	40.6 (200)	57.1 (343)	-16.61 (11)	0.33 (2)
Total profits (in million RMB)	2.5 (30)	1.6 (12)	2.6 (32)	-0.98 (0.49)**	-0.06 (0.16)
Total value added tax (in million RMB)	1.7 (15)	1.0 (4)	1.7 (16)	-0.70 (0.18)***	-0.05 (0.11)
Total wage (in 1,000 RMB)	3150.9 (64720)	2540.1 (8159)	3211.0 (67778)	-666 (482)	-264 (467)
Total employment (people)	282.6 (641)	259.1 (570)	284.9 (647)	-26.82 (24)	15.9 (8)*
Total intermediate inputs (in million RMB)	44.7 (275)	31.3 (162)	46.1 (284)	-14.84 (9)	0.33 (2)
Total assets (in million RMB)	55.1 (295)	39.7 (158)	56.6 (305)	-16.98 (9)*	2.28 (1)
Total fixed assets (in million RMB)	21.2 (149)	15.8 (74)	21.7 (154)	-5.91 (1)**	0.78 (0.93)
Total liquid assets (in million RMB)	28.7 (151)	20.1 (86)	29.5 (156)	-9.47 (5)	1.01 (0.97)
Firm age	10.0 (10)	7.9 (6)	10.2 (10)	-2.31 (0.45)***	1.34 (0.26)***
Wage per worker (in 1,000 RMB)	11.2 (279)	11.2 (104)	11.2 (291)	0.03 (2)	0.74 (2)
Exporters (exporter=1)	0.53 (0.49)	0.62 (0.48)	0.52 (0.49)	0.09 (0.06)	-0.00 (0.00)
Foreign firms (foreign=1)	0.31 (0.46)	0.52 (0.49)	0.29 (0.45)	0.22 (0.06)***	-0.00 (0.01)
N	72,078	6,446	65,632		

Notes: This table compares the differences in firm characteristics between treatment and control group in the pre-treatment period (2000-2001). Treatment firms are non-state-owned firms located in cities that have adopted the reform in 2002, and the control group consists of non-state-owned firms located in cities that have adopted the reform in 2008 or afterwards. Column (1) shows mean and standard deviation for all firms. Columns (2) and (3) report mean and standard deviation for firms in the treatment group and control group. Column (4) shows the differences in firm characteristics between treatment and control group. Columns (5) report how the differences in firm characteristics between treatment and control group change over time. All price relevant variables are in real value (based on 2000). Standard deviations in parentheses are reported for columns (1) 2) (3). Standard errors in parentheses are reported for columns (4) (5), which are clustered at the city level. * p<0.1, ** p<0.05, *** p<0.01.

2. Firm Employment Before and After the Reform

Figure 3.1 shows average firm employment for treatment and control cities from 2000 to 2007. To draw this figure, we estimate a firm-level fixed effect model for each reform cohort: $Y_{it} = \alpha + \beta_t year_t + \gamma_t D_r * year_t + \lambda_i + \varepsilon_{it}$, where Y_{it} is firm employment in logs, $year_t$ are the year dummies, and $D_r * year_t$ are the interaction terms between a reform cohort r and year dummies. λ_i is the firm-level fixed effect and ε_{it} is the error term. We thus recover the predicted means of the employment, in logs, for the treatment and control groups over time using coefficient estimates α, β_t and γ_t. Compared to providing plots based on means, this approach helps to reduce sample selection issues caused by firm entry and exit during the sample period. According to Table 3.1, the sample size increases significantly over time, particularly from 2003 to 2004. It is very likely that new firms are very different from existing firms. Using plots based on means cannot deal with such cohort effects. In contrast, by controlling firm-level fixed effects, we compare changes in firm employment within the same firm over time.

If reform cohort 2004 is taken as an example, results of Figure 3.1 indicate that non-state-owned firms in both treatment and control regions expanded on average in terms of employment during the sample period except for the period 2003 to 2004. This is because we restrict samples to firms that exist for at least two years: one year in the year of reform adoption, and another year is after the reform adoption. We find that among the 112,052 firms in 2004, 60,972 firms existed for at least one period from 2000 to 2003, and the remaining 51,080 firms are new firms to the sample. Table 3.6 shows a comparison of firm characteristics between these new firms and the remaining firms in 2004. Results indicate that new firms are significantly smaller relative to the other firms. Particularly, the average number of employees was 146 for new firms and 283 for the other firms. Therefore, it is very likely that the dip in 2004 was caused by the large number of new firms that were smaller relative to the other firms. After dropping these new firms, our results are presented in Figure 3.2. We find the dip in 2004 disappears. In addition, we find evidence that the average employment

was increasing faster in reform cities relative to non-reform cities after 2004. Using similar practices for reform cohort 2002 and 2003, we find that non-state-owned firms expanded on average in terms of employment over the same period for both reform cohorts. However, we find that the average employment was increasing faster in reform cities relative to non-reform cities for reform cohort 2002 but not for reform cohort 2003. Given the previous findings that firms in the reform cities are different from their counterparts in the non-reform cities in the pre-treatment period, we cannot draw any conclusion on whether the reform affects firm employment or not before addressing potential endogeneity issues of reform adoption.

Table 3.6 Firm characteristics between new firms and the remaining firms in 2004

Variables	Total firms in 2004	New firm in 2004	other firms in 2004	Difference (New-other)
Total output (in million RMB)	62.8 (543)	32.4 (254)	88.3 (697)	-55.90 (3)***
Total sales (in million RMB)	61.3 (535)	31.5 (252)	86.2 (687)	-54.72 (3)***
Total profits (in million RMB)	3.0 (46)	1.2 (14)	4.4 (61)	-3.25 (0.27)***
Total value added tax (in million RMB)	1.5 (17)	0.78 (6)	2.2 (23)	-1.42 (0.10)***
Total wage (in 1,000 RMB)	2761.4 (13266)	1608.4 (6971)	3727.2 (16753)	-2118 (79)***
Total employment (people)	220.4 (582)	145.7 (363)	283.0 (709)	-137 (3)***
Total intermediate inputs for production (in million RMB)	44.3 (397)	22.9 (170)	62.2 (514)	-39.31 (2)***
Total assets (in million RMB)	-	-	-	-
Total fixed assets (in million RMB)	16.2 (128)	8.8 (56)	22.4 (165)	-13.55 (0.76)***
Total liquid assets (in million RMB)	25.6 (187)	13.3 (88)	36.0 (240)	-22.70 (1)***
Firm age	7.8 (7)	5.4 (5)	9.7 (8)	-4.34 (0.04)***
Wage per worker (in 1,000 RMB)	11.7 (9)	11.0 (8)	12.2 (9)	-1.14 (0.05)***
Exporters or not (exporter=1)	0.46 (0.49)	0.41 (0.49)	0.51 (0.49)	-0.10 (0.00)***
Foreign firms or not (foreign=0)	0.23 (0.42)	0.20 (0.40)	0.26 (0.44)	-0.06 (0.00)***
N	112,052	51,080	60,972	-

Notes: A new firm refers to firms that do not exist from 2000 to 2003 until 2004. Other firms refer to firms that exist for at least one period from 2000 to 2003. The variable of total asset is not available in 2004. All price relevant variables are in real value. Column (1) is the mean value and standard deviation for firm characteristics using all samples in 2004. Column (2) is the mean value and standard deviation for firm characteristics using new firms in 2004. Column (3) is the mean value and standard deviation for firm characteristics using old firms in 2004. Column (4) estimates a simple regression model: $y_i = \alpha + \beta New_i + \varepsilon_i$, where New_i is a dummy variable, which equals to 1 for new firms in 2004 and 0 otherwise, ε_i is the error term. The coefficients β and corresponding standard errors are reported in column (4). Standard deviations in parentheses are reported for columns (1) - (3). Standard errors in parentheses are reported for columns (4). * p<0.1, ** p<0.05, *** p<0.01.

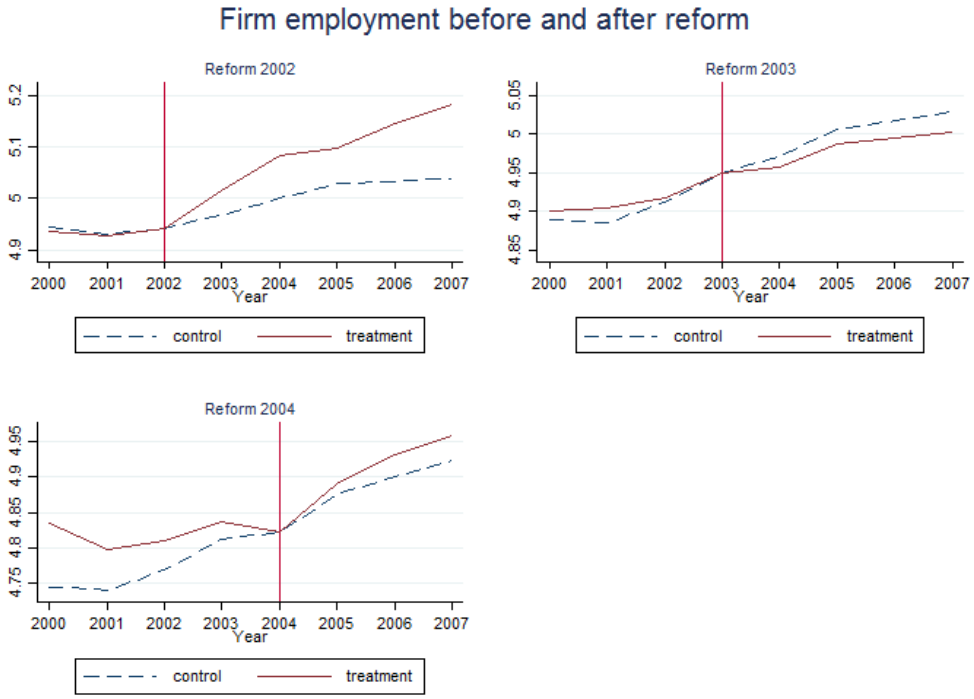

Figure 3.1 Firms' employment in log in treatment and control groups from 2000 to 2007 recovered from a firm-level fixed effect model

Figure 3.2 Firms' employment in log in treatment and control groups from 2000 to 2007 recovered from a firm-level fixed effect model (without new firms appearing in the year of reform adoption and afterwards)

4 Model Specification and Identification Strategy

This section describes the econometric modelling approach to estimate the causal impact of the reform on firm employment.

4.1 DiD With Multiple Time Periods

To estimate the causal impact of the *Hukou* reform on firm employment, we use a similar multi-period DiD model as used in Chapter 3:

$$Y_{itc} = \alpha + \beta Reform_c + \sum_{s=-4}^{s=5} \lambda_s \, 1(r = s) + \sum_{s=-4}^{s=-1} \gamma_s (Reform_c \cdot 1(r = s)) + \sum_{s=1}^{s=5} \delta_s (Reform_c \cdot 1(r = s)) + Cohort_c + \tau_i + \varepsilon_{itc}, \; s \neq 0 \quad (4.1)$$

where Y_{itc} is the employment in log for firm i in city c and time t; $Reform_c$ is a reform dummy taking the value 1 if a city adopted the reform and 0 for cities that have not adopted the reform before 2008. The exact timing of the reform year is given by the indicator function $1(\cdot)$. We define a variable r which measures time (in years) relative to reform adoption. $r > 0$ indicates that the reform was adopted, and $r \leq 0$ indicates time until reform adoption. We use $r = 0$ as base year by taking into account of the fact that a firm might take some time to respond to the reform. $Reform_c \cdot 1(r = s)$ is the interaction term between the reform dummy and relative time. Our main coefficient of interest is δ_s $(s > 0)$ which captures the average reform impact on employment in the treatment group over time. β captures average difference between firms in reform and non-reform cities. This term is dropped when we include firm-level fixed effects. γ_s captures pre-treatment trend differences. $Cohort_c$ denotes the reform cohort, namely whether the reform was adopted in 2002, 2003 or 2004. τ_i denotes firm-level fixed effects, and ε_{itc} is the error term.

4.2 Identification Strategy

To identify the effect of the reform, we use a difference-in-differences model with firm-level fixed effects. First, this specification can accommodate to a

difference-in-differences model with multiple time periods. In order to generalize the idea of "before" and "after" we define a variable r which measures time relative to the reform adoption. Second, this specification can accommodate time-invariant unobserved factors that would affect reform adoption. For example, some firms are located in coastal regions with more access to international trade than other firms located in inland regions. In other words, the geographical location would affect firms' labour demand. If a city's labour demand is associated with the probability of reform adoption, suggested by the results in Chapter 2, controlling these firm-level fixed effects helps to reduce the estimation bias caused by omitted variables. Since we control for firm-level fixed effects, we are estimating the within-firm impact of the reform rather than the between-firm impact of the reform. Third, this specification allows us to examine whether there are pre-treatment differences in trend between treatment and control groups. We use the year of reform adoption as base year, and include interaction terms for periods before the year of reform adoption.

As shown in Section 4.3.2, we find significant pre-treatment differences between firms located in treatment and control cities in terms of employment. It is possible that reform adoption might be endogenous at the city level caused by other time-variant factors. Evidence in the Chapter 2 confirms that the reform is more likely to be adopted in cities with a higher demand for labour. If firms located in reform cities were growing more slowly relative to firms located in non-reform cities prior to the reform adoption, our estimates will be biased downward. If firms located in reform cities were growing faster relative to firms located in non-reform cities prior to the reform adoption, our estimates will be biased upward. To account for this endogeneity issue, we use propensity score matching to find more comparable non-reform cities for reform cities. The purpose of matching is to construct the counterfactual on the treated outcomes had they not been treated by pairing cities in the treatment region with cities in the control region whose observable characteristics X match those of the treated cities up to some selected degree of closeness (Caliendo and Kopeinig 2008).

4.3 Propensity Score Matching

In implementing propensity score matching one has to decide (i) whether to match with or without replacement (ii) the number of units to use in the comparison group, and (iii) the choice of the matching methods. In this study, we use matching with replacement, use all control group observations and each treated city will have at least two nearest neighbours. We use a Probit model to estimate the propensity score. The propensity score refers to the conditional probability that a city *c* adopts the reform given observable city-level characteristics. In addition, we use city-level data in the last year prior to reform adoption for the estimation. We assume that city-level data prior to the reform adoption is not affected by the reform. We select the last year prior to the reform because city-level characteristics in that year capture the current city conditions more precisely than earlier years and might predict the probability of reform adoption better than earlier years. We conduct the same approach for each reform year. Most of the information here comes from a city-level dataset. City-level data are mainly taken from the China City Statistical Yearbooks for the years 2001-2003. Given the fact that the China City Statistical Yearbooks do not provide important information such as migration and over-education, we complement the city-level data with micro census data in 2000 to calculate construct these variables.

Another important issue for the validity of propensity score matching is the selection of variables. According to the previous findings (Jin et al. 2017), the incentives to adopt the reform are stronger the bigger the expected gains to aggregate income brought about by improving labour market efficiency as well as by employment. Following the similar empirical framework, we select our main variables that potentially determine the probability of reform adoption. To increase the matching quality, we introduce a number of new variables with the remaining variables are the same as those adopted b Jin et al. (2017). Our model specification is as follows:

$$\Pr(Reform_{ct} = 1) = \alpha + \beta_1 OverEdu_{ct-1} + \beta_2 Rprod_{ct-1} + \beta_3 logpaymentpc_{ct-1} + \gamma \mathrm{X}_{ct-1} + \delta Z_{ct-1} + \varepsilon_{ct}, \quad (4.2)$$

where $Reform_{ct}$ is a dummy variable with 1 indicating a city c adopted the reform in year $\mathrm{t}, \mathrm{t} \in \{2002, 2003, 2004\}$, and 0 for cities that did not adopt the reform until 2008; $OverEdu_{it-1}$ is the migrants' incidence of over-education in city c in year $\mathrm{t}-1$, a higher value indicates that a larger number of migrant workers are over-educated and the efficiency of local labour market is lower; $Rprod_{it-1}$ is the relative labour productivity between rural and urban area of the prefecture in city c in year $\mathrm{t}-1$, a higher value indicates that labour productivity in the rural areas is closer to that in the urban areas and the degree of labour misallocation between rural and urban area is lower. Both variables reflect the degree of labour misallocation. $logpaymentpc_{ct-1}$ is the per capita fiscal expenditure on education in log in city c in year $\mathrm{t}-1$; X_{ct-1} include variables such as composition of GDP, unemployment rate, net population growth rate to capture the potential increase in the population and employment. Z_{ct-1} controls for other city characteristics such as the city population, urban population ratio, agricultural population ratio, migration ratio, log per capita fiscal revenues, log per capita GDP. ε_{ct} is the error term.

We use reform cohort 2004 to demonstrate the results of propensity score matching, leaving results of other reform cohorts in the Appendix. Results are presented in Table 4.1. We find that cities with higher per capita spending on education are associated with a lower probability of reform adoption but do not find a significant association for per capita fiscal revenues and per capita GDP. In addition, we find that a less industrialised economic structure (lower ratio of secondary industry) is associated with a higher probability of reform adoption. Moreover, a higher incidence of over-education and lower relative labour productivity of rural to urban area, which suggest lower efficiency of labour allocation, are associated with a higher probability of reform adoption. Finally, we find that a higher urban unemployment rate and a higher net population growth rate are less likely to be associated with the

reform adoption. Therefore, we find evidence that both efficiency of labour allocation and potential demand for labour would significantly affect the probability of reform adoption. In other words, firms located in these cities with a lower efficiency of labour allocation and a higher potential demand for labour are more likely to be firms located in treatment regions. Our estimation results will be biased without coping with this reform endogeneity at the city level.

Figure 4.1 compares the distribution of the estimated propensity score between treatment and control cities. Unsurprisingly, the treatment group has higher predicted values of propensity score on average, and most observations are on the common support. After matching, we expect the distribution of covariates between the treatment and control group to be very similar, or balanced. Table 4.2 tests whether covariates are balanced. Before matching, we find that cities in the treatment group tend to have significantly lower per capita fiscal revenues, lower per capita spending on education, lower unemployment rate, a lower net population growth rate, and a higher incidence of over-education as well as a higher agricultural ratio. After matching, the differences in the means of these covariates are significantly reduced and are not statistically significant at the conventional level. In addition to the distribution of covariates, we also find that the R-squared for the matched samples fell dramatically from 0.260 to 0.034, which suggests that reform cities and non-reform cities are more similar after matching. All the evidence above indicates that the propensity score matching is successful in eliminating the differences in city-level characteristics between treatment and control groups. We conduct the same approach for the other two reform cohorts.

Tables 4.3 show firm-level characteristics between reform and non-reform cities after matching for the pre-treatment period. Results indicate that there is no significant pre-treatment difference between firms in the reform cities and firms in the non-reform cities except for a few variables. For a few variables that witness significant pre-treatment difference between treatment and control groups, we do not

find significant pre-treatment trend differences except for firm age variable. Particularly, we find the pre-treatment difference between reform and non-reform cities decreases dramatically in terms of employment variable after the matching. Figure 4.2 further shows the firm employment between reform and non-reform cities over time using matched samples. Compared to Figure 3.1, we find the pre-treatment difference in the pattern of employment is greatly reduced.

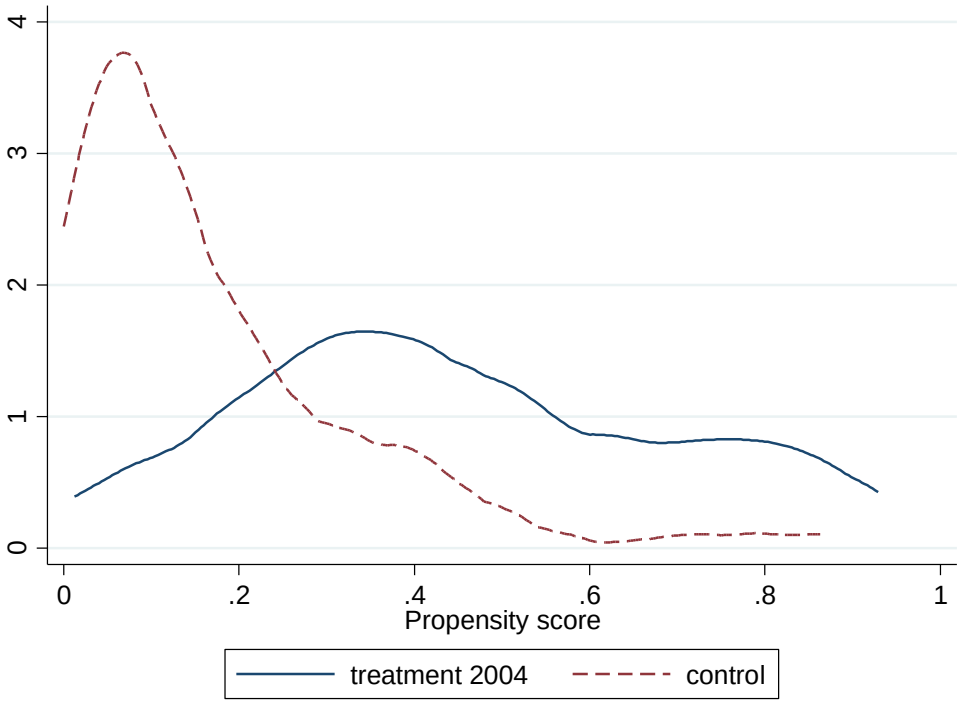

Figure 4.1 Propensity score between treatment and control groups (reform 2004)

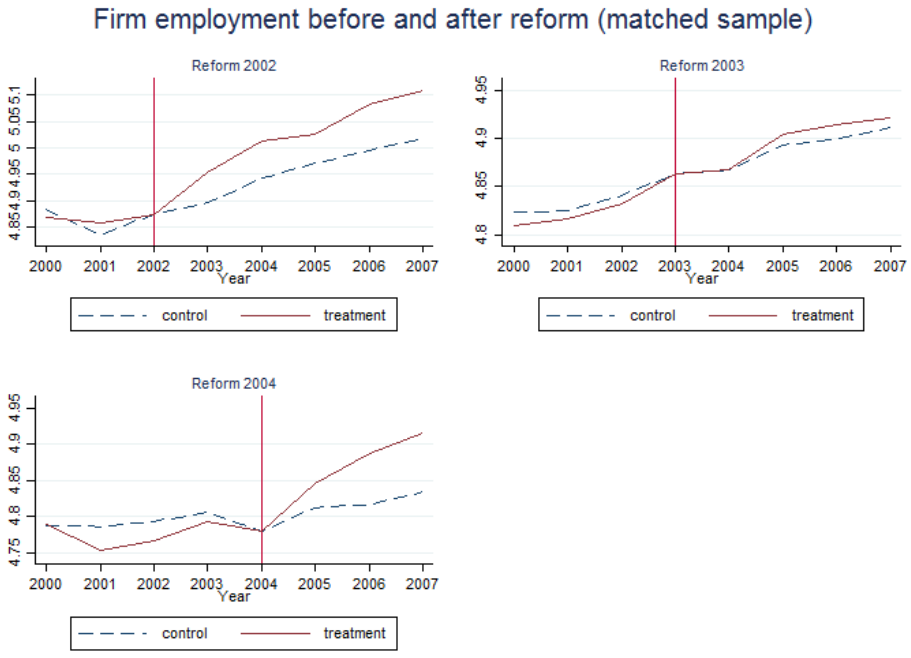

Figure 4.2 Firms' employment in log in treatment and control groups from 2000 to 2007 recovered from a firm-level fixed effect model using matched samples

Table 4.1 Determinants of reform adoption (reform 2004)

	(1) Probit	(2) Logit	(3) OLS
Per capita payments on education(log)	-0.184**	-0.186**	-0.198**
	(0.075)	(0.083)	(0.082)
Per capita fiscal revenue(log)	-0.023	-0.017	-0.019
	(0.061)	(0.062)	(0.067)
Per capita GDP(log)	0.054	0.048	0.064
	(0.089)	(0.087)	(0.098)
Secondary industry(GDP)	-0.981*	-0.984*	-1.369***
	(0.515)	(0.519)	(0.519)
Tertiary industry(GDP)	-0.930*	-1.027*	-1.361**
	(0.552)	(0.562)	(0.568)
Urban unemployment	-3.166***	-3.012***	-3.123***
	(0.756)	(0.776)	(0.757)
Net population growth rate	-0.049***	-0.050***	-0.048***
	(0.010)	(0.010)	(0.010)
Incidence of over-education	1.111***	1.092***	1.117***
	(0.362)	(0.355)	(0.373)
Relative labour productivity	-0.268*	-0.266*	-0.257*
	(0.139)	(0.137)	(0.142)
Urban ratio	0.100	0.070	0.016
	(0.193)	(0.209)	(0.203)
Agricultural ratio	-0.182	-0.138	-0.288
	(0.279)	(0.301)	(0.282)
migration ratio	0.235	0.236	0.288
	(0.498)	(0.509)	(0.450)
City population	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Observations	197	197	197
Adjusted R^2	-	-	0.194

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 4.2 Differences in city-level characteristics between reform and non-reform cities before and after matching (reform 2004)

Variable	Unmatched Matched	Mean Treated	Control	%bias	%reduce |bias|	t-test t	p>|t|	V(T)/ V(C)
Per capita payments	U	5.03	5.28	-50.3		-2.85	0.005	0.70
on education(log)	M	5.05	5.01	8.7	82.8	0.38	0.704	0.55
Per capita fiscal	U	6.32	6.58	-28.1		-1.69	0.094	1.07
revenue(log)	M	6.36	6.27	10.3	63.4	0.49	0.624	1.14
Per capita GDP(log)	U	9.39	9.55	-23.2		-1.40	0.164	1.12
	M	9.42	9.37	7.4	68.0	0.35	0.729	1.09
Secondary	U	0.48	0.51	-15.3		-0.93	0.351	1.23
industry(GDP)	M	0.49	0.48	5.5	63.9	0.26	0.796	1.35
Tertiary	U	0.39	0.40	-16.4		-1.00	0.321	1.19
industry(GDP)	M	0.39	0.40	-12.0	26.7	-0.60	0.552	1.71
Urban unemployment	U	0.07	0.10	-57.8		-3.07	0.002	0.36*
	M	0.07	0.08	-12.1	79.1	-0.63	0.533	0.49*
Net population	U	3.25	4.85	-58.7		-3.26	0.001	0.59
growth rate	M	3.30	3.26	1.6	97.2	0.09	0.931	0.96
Incidence of	U	0.26	0.24	35.3		1.95	0.052	0.56
over-education	M	0.26	0.27	-9.7	72.6	-0.47	0.643	0.61
Relative labour	U	0.74	0 .74	-2.4		-0.14	0.886	1.02
productivity	M	0.74	0.74	1.5	37.6	0.08	0.936	2.15*
Urban ratio	U	0.31	0.35	-16.4		-0.96	0.339	0.89
	M	0.30	0.35	-17.2	-5.0	-0.89	0.377	1.33
Agricultural ratio	U	0.71	0.63	41.9		2.45	0.015	0.88
	M	0.71	0.70	3.2	92.4	0.16	0.872	1.29
migration ratio	U	0.18	0.20	-24.7		-1.31	0.192	0.36*
	M	0.18	0.18	1.2	95.0	0.08	0.940	0.84
City population	U	118.38	108.61	9.7		0.56	0.578	0.77
	M	117.93	119.27	-1.3	86.2	-0.06	0.950	0.80

Table 4.3 Firm-level Summary Statistics for treatment and control group for Pre-treatment period using matched samples (reform 2004)

Variables	Total	Reform	Non-reform	Diff. (β_1)	DiD (δ_{2001})	DiD (δ_{2002})	DiD (δ_{2003})
Total output (in million RMB)	70.7 (459)	73.4 (340)	67.9 (556)	5.49 (10)	-1.85 (3)	-0.68 (5)	0.66 (7)
Total sales (in million RMB)	68.9 (451)	71.5 (335)	66.1 (546)	5.34 (10)	-1.08 (3)	-0.65 (5)	1.27 (7)
Total profits (in million RMB)	3.5 (47)	3.4 (23)	3.7 (63)	-0.33 (0.86)	-	0.37 (0.32)	0.31 (0.34)
Total value added tax (in million RMB)	2.2 (19)	2.1 (12)	2.3 (25)	-0.19 (0.40)	0.19 (0.16)	-0.04 (0.21)	0.14 (0.29)
Total wage (in 1,000 RMB)	3028.9 (66581)	2756.6 (8157)	3311.8 (94703)	-561 (640)	-2668 (2651)	-57 (153)	-103 (204)
Total employment (people)	280.7 (635)	298.7 (665)	262.0 (601)	37.33 (24)	-11 (13)	-19 (15)	-15 (16)
Total intermediate inputs (in million RMB)	53.9 (360)	55.3 (273)	52.5 (433)	2.84 (8)	-1.07 (3)	0.20 (4)	0.86 (5)
Total assets (in million RMB)	62.1 (340)	60.7 (283)	63.5 (391)	-2.69 (8)	0.98 (3)	-0.64 (5)	-1.52 (5)
Total fixed assets (in million RMB)	24.2 (169)	23.4 (146)	25.0 (190)	-1.55 (4)	0.56 (1)	-0.20 (3)	0.06 (3)
Total liquid assets (in million RMB)	31.5 (177)	31.1 (151)	32.0 (200)	-0.86 (4)	0.19 (1)	-0.73 (1)	-1.50 (2)
Firm age	10.1 (10)	10.3 (10)	9.9 (10)	0.36 (0.68)	0.11 (0.61)	-0.02 (0.62)	-0.11 (0.76)
Wage per worker (in 1,000 RMB)	10.9 (287)	9.2 (8)	12.7 (410)	-3.49 (2)	-10.90 (11)	0.29 (0.29)	0.15 (0.39)
Exporters (exporter=1)	0.44 (0.49)	0.38 (0.48)	0.51 (0.49)	-0.13 (0.05)**	-0.00 (0.01)	0.00 (0.01)	-0.00 (0.01)
Foreign firms (foreign=1)	0.21 (0.40)	0.21 (0.40)	0.21 (0.40)	0.00 (0.04)	-0.00 (0.01)	-0.00 (0.01)	-0.01 (0.02)
N	67,282	34,280	33,002				

Notes: This table compares the differences in firm characteristics between treatment and control group in the pre-treatment period (2000-2003) using matched samples. Treatment firms are non-state-owned firms located in cities that have adopted the reform in 2004, and the control group consists of non-state-owned firms located in cities that have adopted the reform in 2008 or afterwards. Column (1) shows mean and standard deviation for all firms. Columns (2) and (3) report mean and standard deviation for firms in the treatment group and control group. Column (4) shows the differences in firm characteristics between treatment and control group. Columns (5) (6) (7) report how the differences in firm characteristics between treatment and control group change over time. All price relevant variables are in real value (based on 2000). Standard deviations in parentheses are reported for columns (1) 2) (3). Standard errors in parentheses are reported for columns (4) (5) (6) (7), which are clustered at the city level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

5 Empirical Results

This section estimates the reform impact on non-state-owned firms' employment. First, we provide some benchmark results using different model specifications. Second, we conduct some robustness checks by considering new firms as well as simultaneous policies such as minimum wage policy and restructuring of state sector. Third, we conduct some heterogeneous analysis on the reform impact.

5.1 Reform Impact on Firm Employment

Table 5.1 reports the reform impact on firms' employment using different model specifications. We introduce several different model specifications to justify our preferred model specification described earlier in the chapter. Column (1) uses a difference-in-differences model for all (non-state-owned) firms. We find that the reform impact on firm employment is close to zero within three years after reform adoption. Meanwhile, the coefficients of pre-treatment difference in trend suggest that firms located in reform cities were expanding more slowly relative to firms located in non-reform cities in the pre-treatment period. As a result, the insignificant reform impact on employment might be driven by pre-treatment difference in trend. Using the propensity score matching approach, we find more comparable non-reform cities for reform cities based on a number of observable city-level variables. Column (2) uses a difference-in-differences model for matched firms. Different from previous results using all firms, it turns out that the average firm employment increases by 2.2% one year after reform adoption, which increases to 6.3% three years after reform adoption. In addition, we find a smaller pre-treatment difference in trend on average.

To further reduce the pre-treatment difference in trend, we control for city-level fixed effects. This is because some city-level time-invariant factors such as geographical location, which has not been captured by the propensity score matching, might affect firms' demand for labour. Results are reported in Columns (3) and (4).

Using all firms, we find that the reform impact on firm employment is negligible within three years of reform adoption. By contrast, we find a larger reform impact on firm employment using matched firms. We find that the average firm employment increases by 2.1% one year after reform adoption, which increases to 5.9% three years after reform adoption. The reform impact on employment decreases slightly. In addition, we still find some pre-treatment difference in trend.

Furthermore, we control for firm-level fixed effects. This is because some firm-level time-invariant factors such as production technology, which is not captured by city-level fixed effects, might affect firms' demand for migrant workers. Results are reported in Columns (5) and (6). Using all firms, we do not find obvious reform impact on firm employment within three years after reform adoption as before. Using matched firms, we find that the average firm employment increases by 2.4% one year after reform adoption, which increases to 5.1% three years after reform adoption. Meanwhile, the pre-treatment difference in trend is much smaller than before and is not statistically significant at the conventional level, indicating that the estimates are not driven by pre-treatment difference in trend.

Using the firm-level fixed effects model for matched firms, we further explore the reform impact on firm employment for each reform year. Results are reported in Table 5.2. We find that the reform impact on employment varies across reform years. Particularly, we find a larger reform impact on employment for reform 2004 but a smaller reform impact on employment for reform 2003. To account for the heterogeneous reform effects across reform cohorts, we control for the cohort dummies in our preferred model specification.

Overall, we find evidence that the average firm employment increases faster after the reform adoption, which is not explainable by pre-treatment difference in trend. However, to establish a causal impact of the reform on firm employment, it is necessary to make sure that the reform impact on firm employment still holds after

controlling for other simultaneous policies that might be associated with both firm employment and reform adoption.

Table 5.1 Reform impact on firms' employment in log

	(1)	(2)	(3)	(4)	(5)	(6)
	DiD		City-level FE		Firm-level FE	
	All firms	Matched firms	All firms	Matched firms	All firms	Matched firms
Reform*(t-4)	0.137***	0.015	0.071	-0.022	0.074*	0.001
	(0.044)	(0.049)	(0.056)	(0.062)	(0.041)	(0.043)
Reform*(t-3)	0.012	-0.032	0.013	-0.045	0.038	-0.014
	(0.040)	(0.042)	(0.040)	(0.046)	(0.026)	(0.031)
Reform*(t-2)	0.017	-0.003	0.020	-0.009	0.015	-0.010
	(0.031)	(0.041)	(0.031)	(0.040)	(0.019)	(0.025)
Reform*(t-1)	0.008	-0.001	0.008	0.004	0.008	0.000
	(0.024)	(0.035)	(0.024)	(0.033)	(0.011)	(0.016)
Reform*(t+1)	-0.000	0.022	-0.001	0.021	0.004	0.024
	(0.011)	(0.014)	(0.012)	(0.014)	(0.012)	(0.015)
Reform*(t+2)	0.002	0.054**	0.004	0.051**	0.011	0.046**
	(0.021)	(0.023)	(0.020)	(0.022)	(0.019)	(0.020)
Reform*(t+3)	-0.001	0.063**	0.001	0.059**	0.007	0.051**
	(0.027)	(0.028)	(0.026)	(0.028)	(0.023)	(0.025)
Reform*(t+4)	-0.041	0.045	0.003	0.063*	0.006	0.037
	(0.047)	(0.053)	(0.032)	(0.033)	(0.025)	(0.029)
Reform*(t+5)	0.083	0.051	0.103***	0.082**	0.099***	0.071*
	(0.105)	(0.137)	(0.029)	(0.041)	(0.028)	(0.037)
Observations	1359590	581289	1359590	581289	1359590	581289
Adjusted R^2	0.010	0.014	0.073	0.055	0.872	0.862
N_clust	235	142	235	142	235	142

Notes: This table estimates the reform impact on firm employment in log. Columns (1) (2) use a difference-in-differences model. Columns (3) and (4) use a difference-in-differences model with city-level fixed effects. Columns (5) and (6) use a difference-in-differences model with firm-level fixed effects. Columns (1) (3) (5) use all firms, and Columns (2) (4) (6) use matched firms using propensity score matching. All standard errors are clusters at the city level. Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

Table 5.2 Reform impact on firms' employment for each reform year

	(1)	(2)	(3)	(4)	(5)	(6)
	Reform 2002		Reform 2003		Reform 2004	
	All firms	Matched firms	All firms	Matched firms	All firms	Matched firms
Reform*(t-4)	.	.	.	.	0.090*	0.001
	.	.	.	.	(0.048)	(0.048)
Reform*(t-3)	.	.	0.011	-0.013	0.058	-0.032
	.	.	(0.028)	(0.039)	(0.042)	(0.040)
Reform*(t-2)	-0.007	-0.016	0.020	-0.009	0.042	-0.025
	(0.025)	(0.051)	(0.023)	(0.032)	(0.032)	(0.031)
Reform*(t-1)	-0.005	0.024	0.007	-0.008	0.023	-0.013
	(0.018)	(0.025)	(0.013)	(0.019)	(0.018)	(0.017)
Reform*(t+1)	0.047***	0.057	-0.015	0.000	0.014	0.033*
	(0.016)	(0.037)	(0.015)	(0.018)	(0.016)	(0.017)
Reform*(t+2)	0.082***	0.072	-0.020	0.011	0.029	0.070***
	(0.026)	(0.052)	(0.020)	(0.025)	(0.025)	(0.025)
Reform*(t+3)	0.066***	0.055	-0.024	0.015	0.035	0.081**
	(0.025)	(0.048)	(0.026)	(0.032)	(0.034)	(0.035)
Reform*(t+4)	0.110***	0.087*	-0.027	0.010	.	.
	(0.033)	(0.044)	(0.026)	(0.032)	.	.
Reform*(t+5)	0.141***	0.091**	.	.	.	.
	(0.034)	(0.040)	.	.	.	.
Observations	332563	73614	460067	268244	566960	239431
Adjusted R^2	0.857	0.833	0.859	0.857	0.867	0.866
N_clust	157	16	180	63	196	85

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

5.2 Robustness Checks

To establish a causal impact of the reform on firm employment, we conduct a number of robustness checks to make sure that the reform impact on employment is not caused by other factors irrelevant to the reform adoption. First, we control for the minimum wage policy. Second, we control for the restructuring of state sector. Third, we control for the reduction in the trade uncertainty. Finally, we test whether our results are mainly driven by new firms appearing in the year of reform adoption and afterwards.

- Minimum Wage Policy

The minimum wage law in China was enacted in 1994 and the enforcement tightened since 2004. In March 2004, the Ministry of Labour issued a new directive which established even more comprehensive minimum standards and threatened tougher punishment for lax enforcement of labour law. This new minimum wage policy emphasized the following major changes: extension of coverage to town-village enterprises and self-employed business; creation of new standard for hourly minimum wages; increases in the penalty for violators: from 20%-100% to 100%-500% of the wage; higher frequency of minimum-wage adjustment: once at least every two years. According to Huang et al. (2014), local governments faced a trade-off in making minimum wage policies. On the one hand, local governments have incentives to slow the rising minimum wage in order to attract firm investment and reduce the cost of labour. On the other hand, local governments are forced to compete with other regions to increase the minimum wage in order to attract sufficient labour supply and avoid massive labour outflows. To some extent, increasing the minimum wage is similar to the *Hukou* reform in terms of attracting labour supply from other cities.

Also, a number of studies have found that the minimum wage policy in China significantly affects firm employment (Wang and Gunderson 2011; Huang et al. 2014; Fang and Lin 2015). For example, Wang and Gunderson (2011) show that minimum wage has negative employment effects in slower growing regions, with even greater

negative effects in non-state-owned organizations. Fang and Lin (2013) find that minimum wage changes have significant effects on employment in the more prosperous Eastern part of China, resulting in employment reduction for females, young adults, and less-skilled workers. By contrast, Huang et al. (2014) find that firms have heterogeneous responses to minimum wage changes: firms with high wages or large profit margin increase employment, while those with low wages or small profit margin downsize. Particularly, Huang et al. (2014) also find that the increase in enforcement of China's minimum wage in 2004 has since amplified this heterogeneity.

Using the same dataset on the monthly minimum wage, Figure 5.1 shows the trend of minimum wage between reform and non-reform cities over time for each reform year. We find that the monthly minimum wage increased steadily over time for both reform and non-reform cities. For reform years 2003 and 2004, we find the trend of monthly minimum wage was different between reform and non-reform cities from 2001 to 2003. We find some evidence that the monthly minimum wage was increasing faster for reform cities relative to non-reform cities during this period. For reform year 2002, we find a faster increase in the monthly minimum wage for reform cities relative to non-reform cities from 2004 to 2006. Figure 5.2 depicts the trend of minimum wage between reform and non-reform cities using matched cities, and the patterns above still hold.

To test whether there is significant difference in trend between reform and non-reform cities in the pre-treatment period, we use our preferred model specification to look at whether there is a significant relationship between the reform and the monthly minimum wage in log. Table 5.3 shows the estimation results. Column (1) uses all cities, and we find an obvious pre-treatment difference in trend. Column (2) uses matched cities, and we find a smaller but also non-trivial pre-treatment difference in trend. Therefore, this evidence above suggests that reform cities witnessed a faster increase in the monthly minimum wage in the pre-treatment

period. If the minimum wage is negatively associated with firm employment, omitting the minimum wage variable would underestimate the *Hukou* reform impact on firm employment.

We propose two different approaches to measure the minimum wage policy from different perspectives. The first approach is to use city-level monthly minimum wage in log for each year as well as interaction terms between monthly minimum wage in log and year dummies. The second approach uses the change rate of city-level monthly minimum wage from 2001 to 2005. To be exact, the change rate is calculated as $\mathrm{MW}rate_i = \frac{MW_{i,2005} - MW_{i,2001}}{MW_{i,2001}}$. Given the fact the minimum wage tightened enforcement in 2004, a higher value of $\mathrm{MW}rate_i$ implies that a city i witnessed a larger relative change in the monthly minimum wage from 2001 to 2005 during which the *Hukou* reform was adopted. We control for the change rate as well as interaction terms between change rate and year dummies.

Table 5.4 shows the reform impact on firm employment by controlling variables relevant to minimum wage. Columns (1) and (2) use the minimum wage in log for each year as well as interactions terms with year dummies as control variables. Using all firms, we find that the average firm employment increases by 0.5% one year after reform adoption, which increases to 1.1% three years after reform adoption. We also find an obvious pre-treatment difference in trend. Using matched firms, we find that the average firm employment increases by 2.3% one year after reform adoption, which increases to 5.4% three years after reform adoption. And we find a smaller pre-treatment difference in trend. Columns (3) and (4) use the change rate of minimum wage as well as interaction terms with year dummies as control variables. We find very similar results. For matched firms, we find the average firm employment increases by 2.6% one year after reform adoption, which increases to 5.6% three years after reform adoption.

Overall, we find that the reform impact on firm employment would be underestimated slightly without controlling for variables relevant to minimum wage policy. To be exact, after controlling for minimum wage variables, the reform impact on firm employment increases from 5.1% to 5.6% three years after reform adoption.

Table 5.3 The *Hukou* Reform and monthly minimum wage in log

	(1)	(2)
	All cities	Matched cities
Reform*(t-4)	-0.093***	-0.060
	(0.018)	(0.038)
Reform*(t-3)	-0.102***	-0.066**
	(0.015)	(0.033)
Reform*(t-2)	-0.027*	-0.001
	(0.014)	(0.027)
Reform*(t-1)	-0.002	0.025
	(0.012)	(0.026)
Reform*(t+1)	0.028**	0.014
	(0.014)	(0.020)
Reform*(t+2)	0.001	-0.013
	(0.013)	(0.021)
Reform*(t+3)	-0.024*	-0.034
	(0.013)	(0.022)
Reform*(t+4)	-0.014	0.008
	(0.015)	(0.023)
Reform*(t+5)	-0.007	0.079*
	(0.014)	(0.041)
Observations	4057	1302
Adjusted R^2	0.891	0.903
N_clust	227	141

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

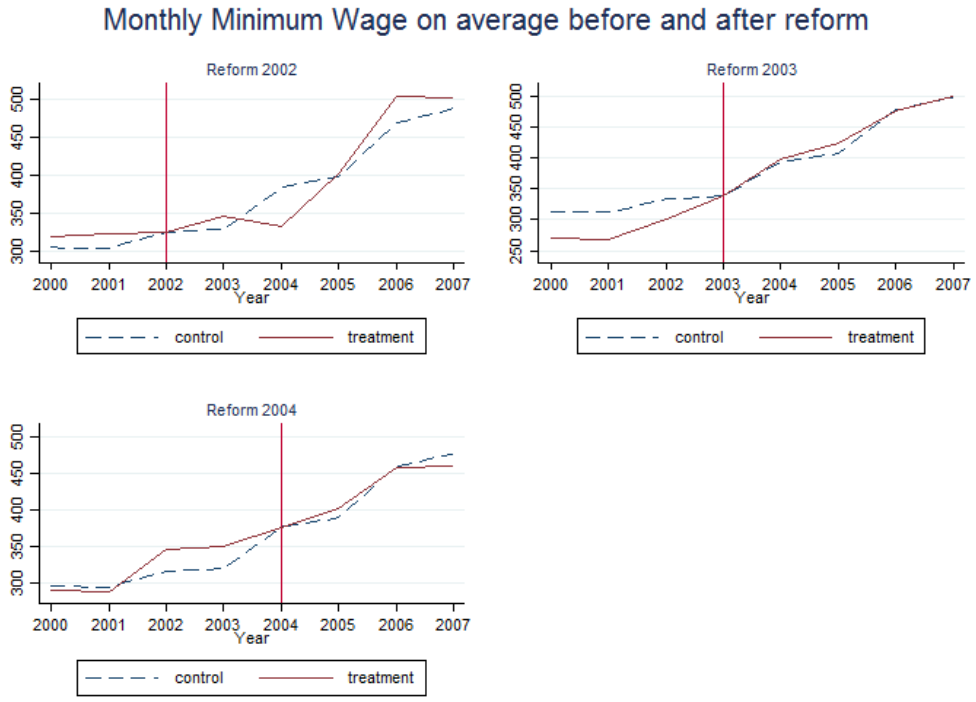

Figure 5.1 Monthly Minimum wage between reform and non-reform cities over time

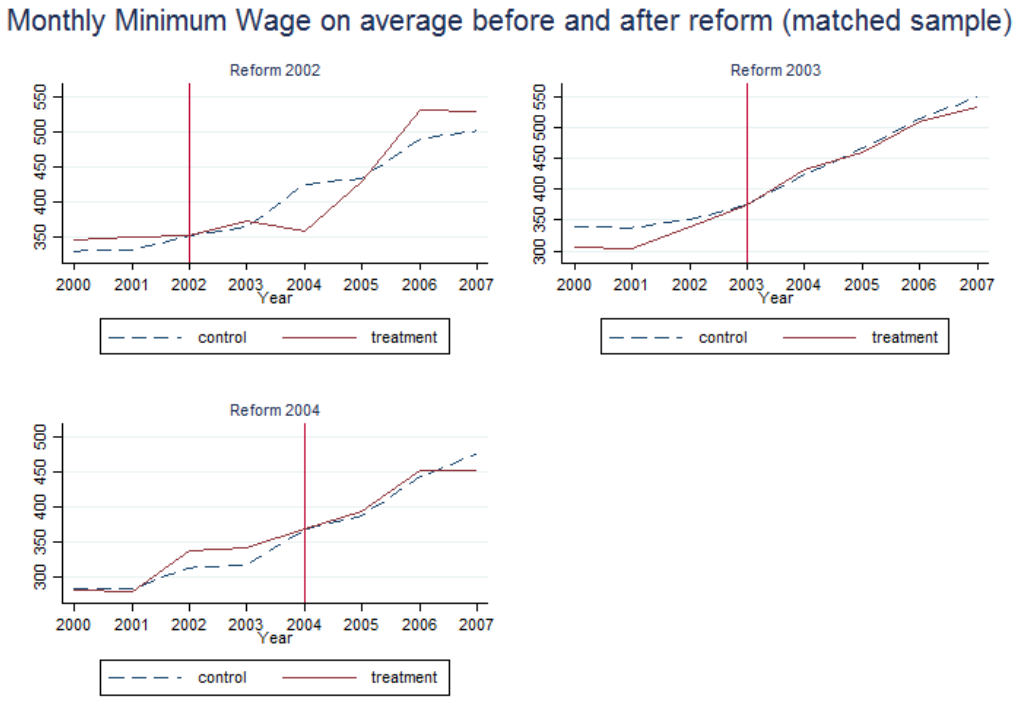

Figure 5.2 Monthly Minimum wage between reform and non-reform cities over time (using matched samples)

Table 5.4 Reform impact on firms' employment by controlling for minimum wage

	(1)	(2)	(3)	(4)
	Minimum wage		Changes in the Minimum wage	
	All firms	Matched firms	All firms	Matched firms
Reform*(t-4)	0.043	-0.004	0.068	0.001
	(0.039)	(0.045)	(0.043)	(0.039)
Reform*(t-3)	0.017	-0.023	0.026	-0.023
	(0.023)	(0.032)	(0.032)	(0.031)
Reform*(t-2)	0.004	-0.018	0.012	-0.012
	(0.016)	(0.025)	(0.022)	(0.025)
Reform*(t-1)	0.005	-0.001	0.003	-0.003
	(0.011)	(0.016)	(0.012)	(0.016)
Reform*(t+1)	0.005	0.023	0.008	0.026*
	(0.012)	(0.015)	(0.012)	(0.014)
Reform*(t+2)	0.015	0.048**	0.019	0.049**
	(0.018)	(0.021)	(0.019)	(0.019)
Reform*(t+3)	0.011	0.054**	0.018	0.056**
	(0.023)	(0.027)	(0.024)	(0.024)
Reform*(t+4)	0.005	0.037	0.018	0.046
	(0.026)	(0.028)	(0.025)	(0.028)
Reform*(t+5)	0.107***	0.060	0.096***	0.037
	(0.028)	(0.041)	(0.029)	(0.036)
Observations	1347328	580840	1346572	580840
Adjusted R^2	0.872	0.862	0.872	0.862
N_clust	227	141	226	141

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

- Size of the State Sector

Another potential policy shock is the restructuring of the state sector in China (Appleton et al. 2002; Dong and Xu 2009; Huang et al. 2017). Previous studies suggest that a large state sector is associated with a more fragmented product market in China (Poncet 2005). In other words, non-state-owned firms located in cities with a faster decrease in the size of state sector would witness a faster growth in labour demand through a more integrated product market. Figure 5.3 shows the ratio of the state sector in terms of output between reform and non-reform cities over time. We find that the relative size of state sector was decreasing over time for both reform and non-reform cities. In addition, the relative size of state sector was decreasing slowly in reform cities relative to non-reform cities after reform adoption, suggesting that the restructuring of the state sector was faster in non-reform cities after reform adoption. One explanation is that the *Hukou* reform reduced the degree of labour redundancy in the state sector through labour reallocation from the state sector to the non-state sector and in turn improved competitiveness. Figure 5.4 further depicts the ratio of state sector between reform and non-reform cities over time using matched cities. We find similar patterns for reform 2002 and reform 2004 but not for reform 2003.

To test whether the reform is associated with the size of state sector, we estimate the relationship between the *Hukou* reform and the relative size of state sector using our preferred model specification. Results are reported in Table 5.5. Column (1) uses all cities and we find that the relative size of state sector is decreasing more slowly in reform cities relative to non-reform cities. Column (2) uses matched cities and we find a much smaller difference between reform and non-reform cities with respect to the relative size of state sector after reform adoption

Table 5.6 shows the reform impact on firm employment by controlling the relative size of state sector as well as interaction terms with year dummies. Columns (1) and (2) use the ratio of state sector's output to proxy for the relative size of state sector. Using matched firms, we find the average firm employment increases by 2.4%

one year after reform adoption, which further increases to 4.9% three years after reform adoption. Columns (3) and (4) use the ratio of state sector's employment to proxy for the relative size of state sector. Using matched firms, we find the average firm employment increases by 2.4% one year after reform adoption, which further increases to 5.1% three years after reform adoption.

Therefore, we do not find obvious evidence that the reform impact on firm employment is mainly driven by the restructuring of state sector.

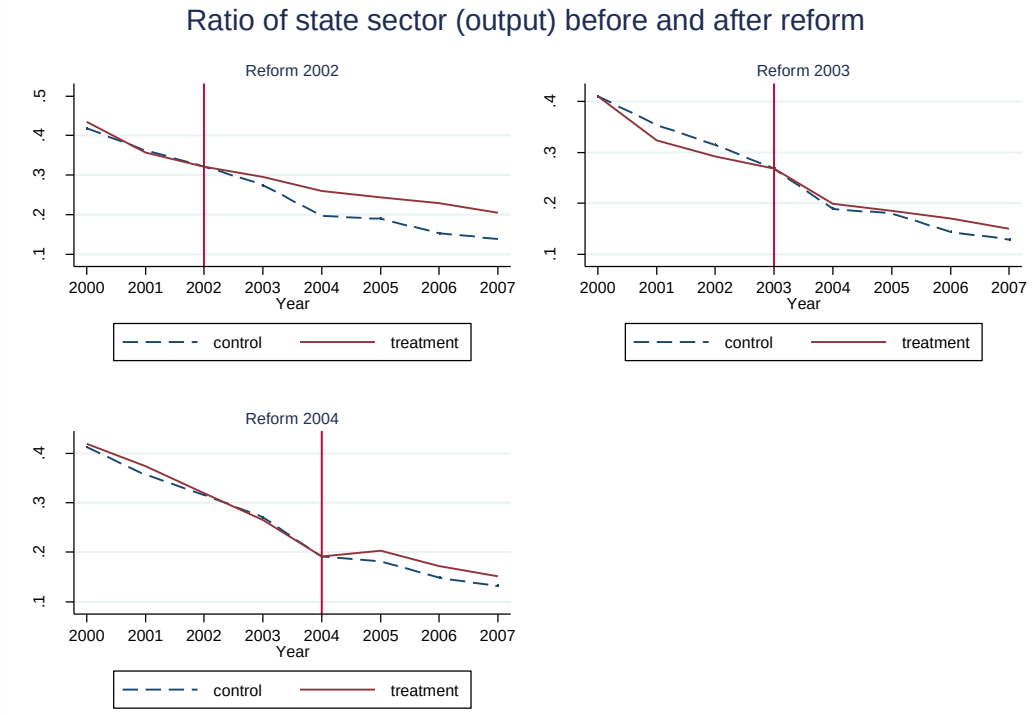

Figure 5.3 Ratio of state sector in terms of output between reform and non-reform cities over time

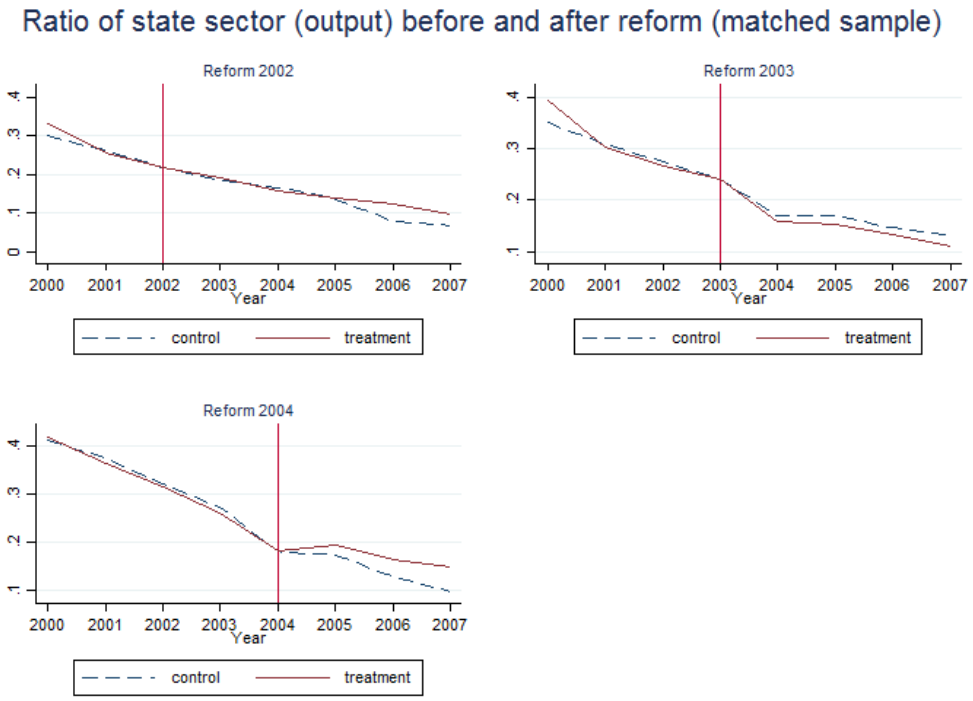

Figure 5.4 Ratio of state sector in terms of output between reform and non-reform cities over time (using matched samples)

Table 5.5 The *Hukou* reform and the ratio of state sector at the prefecture level

	(1)	(2)
	All cities	Matched cities
Reform*(t-4)	0.010	-0.003
	(0.030)	(0.038)
Reform*(t-3)	0.017	0.010
	(0.022)	(0.030)
Reform*(t-2)	-0.005	0.002
	(0.019)	(0.025)
Reform*(t-1)	-0.004	-0.002
	(0.017)	(0.021)
Reform*(t+1)	0.024	0.010
	(0.015)	(0.019)
Reform*(t+2)	0.040**	0.014
	(0.017)	(0.021)
Reform*(t+3)	0.043**	0.025
	(0.019)	(0.023)
Reform*(t+4)	0.056***	0.001
	(0.021)	(0.028)
Reform*(t+5)	0.061**	0.027
	(0.028)	(0.033)
Observations	4245	1304
Adjusted R^2	0.785	0.823
N_clust	235	142

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

Table 5.6 Reform impact on firms' employment by controlling for restructuring of state sector

	(1)	(2)	(3)	(4)
	State sector in terms of output		State sector in terms of employment	
	All firms	Matched firms	All firms	Matched firms
Reform*(t-4)	0.072**	0.007	0.059*	0.003
	(0.036)	(0.042)	(0.034)	(0.043)
Reform*(t-3)	0.032	-0.019	0.023	-0.025
	(0.024)	(0.031)	(0.022)	(0.031)
Reform*(t-2)	0.013	-0.017	0.011	-0.022
	(0.016)	(0.023)	(0.015)	(0.023)
Reform*(t-1)	0.006	-0.004	0.007	-0.005
	(0.010)	(0.015)	(0.010)	(0.015)
Reform*(t+1)	0.004	0.024	0.003	0.024
	(0.011)	(0.015)	(0.011)	(0.015)
Reform*(t+2)	0.011	0.044**	0.011	0.046**
	(0.017)	(0.019)	(0.017)	(0.020)
Reform*(t+3)	0.006	0.049**	0.006	0.051*
	(0.022)	(0.025)	(0.022)	(0.026)
Reform*(t+4)	0.001	0.039	-0.004	0.040
	(0.025)	(0.027)	(0.024)	(0.027)
Reform*(t+5)	0.096***	0.095***	0.086***	0.092**
	(0.025)	(0.035)	(0.025)	(0.038)
Observations	1359044	580905	1359044	580905
Adjusted R^2	0.872	0.862	0.872	0.862
N_clust	235	142	235	142

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

- Reduction in Trade Policy Uncertainty

This section studies whether the U.S. granting of Permanent Normal Trade Relations (PNTR) to China would contaminate the reform impact on firm employment. U.S. imports from China had been subject to the relatively low NTR tariff rates reserved for WTO members since the 1980s. But for China, these low rates required annual renewals that were uncertain and politically contentious. Without renewal, U.S. import tariffs on Chinese goods would have jumped to the higher non-NTR tariff rates assigned to non-market economies and originally established under the Smoot-Hawley Tariff Act of 1930. PNTR-and the subsequent China's accession to WTO-eliminated the uncertainty associated with these annual renewals by permanently setting U.S. duties on Chinese imports to NTR levels. Pierce and Schott (2016) argue that the reduction in trade policy uncertainty faced by Chinese exporters to the U.S., as measured by the normal-trade-relations (NTR) gap, contributes to the sharp drop in U.S. manufacturing employment beginning in 2001. Handley and Limao (2013) argue that the removal of trade policy uncertainty toward China can explain between 22-30% of Chinese exports to the US after WTO accession.

Also, China's accession to WTO in 2001 has been thought to reduce the trade policy uncertainty differently across Chinese prefectures. Facchini et al. (2017) find that Chinese prefectures facing a larger decline in their average NTR-gap experience a greater increase in internal migration. Since the *Hukou* reform is after China's accession to WTO, it is also likely that Chinese prefectures facing a larger decline in their average NTR-gap are also more likely to adopt the *Hukou* reform to increase labour supply. If this is the case, the reform impact on firm employment would be over-estimated. To reduce the concern that our estimated reform impact on firm employment is contaminated by the reduction in trade policy uncertainty, we control the average NTR-gap at the prefecture level as well as the interaction term between the average NTR-gap and year dummies. Results are reported in Table 5.7. Using matched firms, we find that the average firm employment increases by 1.9% one year

after reform adoption, which further increases to 4.9% three years after reform adoption.

Therefore, although the reform impact on firm employment reduces slightly, we do not find obvious evidence that the reform impact on firm employment is driven by the reduction in trade policy uncertainty.

Table 5.7 Reform impact on firms' employment in log by controlling for reduction in trade policy uncertainty

	(1)	(2)
	All firms	Matched firms
Reform*(t-4)	0.073*	-0.000
	(0.041)	(0.044)
Reform*(t-3)	0.037	-0.016
	(0.027)	(0.031)
Reform*(t-2)	0.015	-0.013
	(0.019)	(0.026)
Reform*(t-1)	0.007	-0.000
	(0.011)	(0.016)
Reform*(t+1)	0.004	0.019
	(0.012)	(0.016)
Reform*(t+2)	0.012	0.045**
	(0.019)	(0.022)
Reform*(t+3)	0.009	0.049*
	(0.024)	(0.028)
Reform*(t+4)	0.006	0.032
	(0.026)	(0.033)
Reform*(t+5)	0.104***	0.076**
	(0.026)	(0.034)
Observations	1338440	576289
Adjusted R^2	0.872	0.862
N_clust	229	139

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

- Exclusion of New Firms

Finally, we test whether the reform impact on firm employment is mainly driven by new firms. After dropping new firms appearing in the year of reform adoption and afterwards, we estimate the reform impact on employment using our preferred model

specification. Table 5.8 shows the estimation results. We find that the average firm employment increases by 1.9% one year after reform adoption, which increases to 4.1% three years after reform adoption. In other words, we do not find obvious evidence that the reform impact on employment is mainly driven by new firms. Furthermore, we keep firms that exist for every year from 2000 to 2007. We find that the sample size decreases dramatically from 581289 to 169986. It turns out the reform impact on firm employment is getting smaller but still non-trivial.

Table 5.9 estimates the reform impact on firm employment after controlling for variables relevant to the minimum wage reform, the relative size of state sector and the reduction in the trade policy uncertainty. Columns (1) and (2) keep firms that exist for at least twice. Using matched firms, we find that the average firm employment increases by 1.9% one year after reform adoption, which further increases to 4.6% three years after reform adoption. Columns (3) and (4) further restrict to firms that exist before the reform adoption. Using matched firms, we find that the average firm employment increases by 1.4% one year after reform adoption, which further increases to 3.3% three years after reform adoption. Columns (5) and (6) further restrict to firms that exist for every year from 2000 to 2007. Using matched firms, we find that the reform increases the average firm employment by 1.3% one year after reform adoption, which further increases to 2.3% three years after reform adoption.

Overall, we do not find obvious evidence that the reform impact on firm employment is mainly driven by new firms. In the extreme case in which samples are restricted to firms that exist for every year, the reform impact on firm employment is still non-trivial.

Table 5.8 Reform impact on firms' employment by keeping certain firms

	(1)	(2)	(3)	(4)
	Keep firms exist before and after reform adoption		Keep firms exist for every year	
	All samples	Matched samples	All samples	Matched samples
Reform*(t-4)	0.069	-0.005	0.068*	-0.012
	(0.042)	(0.044)	(0.040)	(0.040)
Reform*(t-3)	0.033	-0.020	0.023	-0.015
	(0.027)	(0.030)	(0.028)	(0.032)
Reform*(t-2)	0.010	-0.016	0.012	-0.024
	(0.019)	(0.024)	(0.018)	(0.025)
Reform*(t-1)	0.003	-0.005	0.004	-0.011
	(0.011)	(0.015)	(0.011)	(0.015)
Reform*(t+1)	-0.002	0.019	-0.014	0.018
	(0.011)	(0.014)	(0.013)	(0.017)
Reform*(t+2)	0.004	0.038*	0.001	0.035*
	(0.016)	(0.019)	(0.017)	(0.021)
Reform*(t+3)	-0.000	0.041*	-0.009	0.033
	(0.020)	(0.024)	(0.020)	(0.025)
Reform*(t+4)	0.001	0.030	-0.010	0.026
	(0.024)	(0.030)	(0.028)	(0.036)
Reform*(t+5)	0.099***	0.066*	0.081***	0.053*
	(0.028)	(0.037)	(0.028)	(0.031)
Observations	1037068	444437	412222	169986
Adjusted R^2	0.868	0.858	0.878	0.869
N_clust	234	142	231	140

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

Table 5.9 Reform impact on firms' employment by controlling minimum wage reform, restructuring of state sector and reduction in the trade policy uncertainty

	(1)	(2)	(3)	(4)	(5)	(6)
	Restrict to firms exist in the year of reform adoption and afterwards		Further restrict to firms exist before reform adoption		Further restrict to firms exist for every year	
	All samples	Matched samples	All samples	Matched samples	All samples	Matched samples
Reform*(t-4)	0.068*	0.007	0.064*	0.002	0.064*	-0.003
	(0.039)	(0.039)	(0.039)	(0.039)	(0.037)	(0.033)
Reform*(t-3)	0.024	-0.021	0.020	-0.026	0.016	-0.017
	(0.029)	(0.032)	(0.029)	(0.030)	(0.032)	(0.035)
Reform*(t-2)	0.009	-0.016	0.005	-0.022	0.005	-0.029
	(0.019)	(0.024)	(0.019)	(0.023)	(0.019)	(0.025)
Reform*(t-1)	0.001	-0.004	-0.003	-0.009	-0.001	-0.013
	(0.011)	(0.015)	(0.010)	(0.013)	(0.011)	(0.014)
Reform*(t+1)	0.006	0.019	0.001	0.014	-0.010	0.013
	(0.011)	(0.014)	(0.011)	(0.014)	(0.013)	(0.016)
Reform*(t+2)	0.017	0.042**	0.011	0.033*	0.005	0.030
	(0.017)	(0.019)	(0.016)	(0.018)	(0.018)	(0.019)
Reform*(t+3)	0.015	0.046*	0.008	0.033	-0.007	0.023
	(0.022)	(0.024)	(0.019)	(0.022)	(0.022)	(0.021)
Reform*(t+4)	0.013	0.036	0.007	0.028	-0.006	0.026
	(0.026)	(0.031)	(0.025)	(0.032)	(0.030)	(0.033)
Reform*(t+5)	0.094***	0.060	0.096***	0.059*	0.079***	0.058***
	(0.026)	(0.036)	(0.026)	(0.035)	(0.026)	(0.022)
MW	YES	YES	YES	YES	YES	YES
State sector	YES	YES	YES	YES	YES	YES
Trade policy	YES	YES	YES	YES	YES	YES
Observations	1325386	575840	1010740	440323	403693	169098
Adjusted R^2	0.873	0.862	0.869	0.859	0.879	0.869
N_clust	220	138	220	138	217	136

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

5.3 Heterogeneous Reform Impacts

In this sub-section, we further explore whether different firms respond to the reform differently. According to the previous findings of Jin et al. (2017), the reform increased migration inflows, particularly for unskilled migration inflows. Therefore, it is predictable that firms that use more migrant workers prior to the reform adoption are more likely to be affected by the reform relative to other firms that use less migrant workers. Also, if these unskilled migrants were largely absorbed by unskilled firms, the reform would affect unskilled firms the most relative to skilled firms. To test these assumptions, we conduct the following analysis. First, we examine whether the reform impact on firm employment is larger for firms that use migrant workers more intensively. Second, we examine whether the reform impact on firm employment is larger for unskilled firms.

- Migration Intensity at the industrial level

Since the firm-level data does not report information on migrant workers, we measure the migrant use intensity at the industrial level and assume that the relative migrant use intensity at the industrial level would not change significantly in the short term. We use micro census 2000 to calculate the share of migrant workers at the industrial level. We define migrant workers as those workers that have lived away from their registered place for more than 6 months. We further define inter-prefecture migrant workers as those workers that have lived away from their registered prefecture for more than 6 months. We then calculate the share of migrant workers for each industry. Based on the distribution of migrant share across manufacturing industries, we define industries with lowest 50 percentile in terms of migrant use intensity as industries that use migrant workers less intensively, and define industries with highest 50 percentile in terms of migrant use intensity as industries that use migrant workers more intensively.

Table 5.10 shows the reform impact on firm employment for firms located in

industries with different migrant use intensity. Columns (1) and (2) use firms that use inter-prefecture migrant workers more intensively. Using matched firms, we find that the average firm employment increases by 3.0% one year after reform adoption, which further increases to 6.0% three years after reform adoption. Columns (3) and (4) use firms that use all migrant workers more intensively. Using matched firms, we find that the average firm employment increases by 2.7% one year after reform adoption, which further increases to 4.6% three years after reform adoption. By contrast, we find a smaller reform impact on firm employment for firms located in industries that use migrant workers less intensively. Columns (5) and (6) use firms located in industries that use inter-prefecture migrant workers less intensively. Using matched firms, we find that the average firm employment increases by 0.5% one year after reform adoption, which further increases to 2.8% three years after reform adoption. Columns (7) and (8) use firms located in industries that use all migrant workers more intensively. Using matched firms, we find that the average firm employment increases by 1.2% one year after reform adoption, which further increases to 4.3% three years after reform adoption. Overall, we find a larger reform impact on firms that use migrant workers more intensively, which is consistent with the findings of Chapter 3 that the reform increased migration inflows. Assuming that a firm's preference for migrant workers is relatively stable in the short term, those firms that use migrant workers more intensively prior to the reform adoption are benefiting from the reform adoption the most.

Table 5.10 Reform impact on firms' employment across migrant use intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Use inter-prefecture migrants more		Use all migrants more		Use inter-prefecture migrants less		Use all migrants less	
	All samples	Matched samples	All samples	Matched samples	All samples	Matched samples	All samples	Matched samples
Reform*(t-4)	0.099***	0.023	0.081**	-0.003	0.034	-0.009	0.054	0.011
	(0.037)	(0.038)	(0.039)	(0.042)	(0.044)	(0.043)	(0.040)	(0.039)
Reform*(t-3)	0.025	-0.030	0.021	-0.041	0.019	-0.012	0.025	-0.005
	(0.033)	(0.034)	(0.035)	(0.037)	(0.028)	(0.032)	(0.028)	(0.032)
Reform*(t-2)	0.010	-0.023	0.005	-0.031	0.011	-0.005	0.016	-0.001
	(0.022)	(0.028)	(0.023)	(0.030)	(0.019)	(0.023)	(0.018)	(0.024)
Reform*(t-1)	0.002	-0.000	-0.003	-0.004	0.007	0.001	0.009	0.004
	(0.012)	(0.018)	(0.013)	(0.019)	(0.011)	(0.013)	(0.011)	(0.015)
Reform*(t+1)	0.017	0.030*	0.019	0.027	-0.007	0.005	-0.003	0.012
	(0.011)	(0.015)	(0.012)	(0.017)	(0.013)	(0.015)	(0.013)	(0.016)
Reform*(t+2)	0.029	0.054***	0.029	0.048**	0.003	0.025	0.007	0.035*
	(0.019)	(0.020)	(0.020)	(0.023)	(0.017)	(0.019)	(0.017)	(0.020)
Reform*(t+3)	0.031	0.060**	0.027	0.046	-0.004	0.028	0.003	0.043
	(0.024)	(0.027)	(0.024)	(0.028)	(0.022)	(0.025)	(0.024)	(0.027)
Reform*(t+4)	0.031	0.051	0.019	0.027	-0.012	0.010	0.001	0.033
	(0.031)	(0.037)	(0.029)	(0.033)	(0.023)	(0.029)	(0.027)	(0.034)
Reform*(t+5)	0.114***	0.066*	0.074***	-0.002	0.066**	0.028	0.098***	0.099**
	(0.029)	(0.037)	(0.025)	(0.027)	(0.029)	(0.053)	(0.036)	(0.049)
Observations	732524	313054	606479	251860	592859	262785	718904	323979
Adjusted R^2	0.871	0.857	0.874	0.860	0.880	0.874	0.876	0.869
N_clust	220	137	219	136	220	138	220	138

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

- Skill intensity at the industrial level

Since the firm-level data only report the skill information for the year 2004, we cannot take into account of the changing skill intensity over time. Also, skill information is not available for firms that do not exist in 2004. Therefore, we measure the skill intensity at the industrial level using the firm data in 2004, and assume that the skill intensity at the industrial level is relatively stable over time. We define a skill intensive firm as one in which the number of workers with skilled certificates accounts for more than 25 percent of total workforce. We then calculate the share of skill intensive firms in each industry. Based on the distribution of skill intensity share across manufacturing industries, we define industries with lowest 50 percentile in terms of skill intensity share as labour intensive industries, and define industries with highest 50 percentile in terms of skill intensity share as skill intensive industries. We estimate the reform impact on firm employment for firms located in labour and skill intensive industries, respectively. Results are reported in Table 5.11. Columns (1) and (2) estimate the reform impact on labour intensive firms' employment. Using matched firms, we find that the average firm employment increases by 1.1% one year after reform adoption, which further increases to 3.0% three years after reform adoption. Columns (3) and (4) estimate the reform impact on skill intensive firms' employment. Using matched firms, we find that the average firm employment increases by 2.4% one year after reform adoption, which further increases to 5.6% three years after reform adoption. Therefore, we find a larger reform impact on firm employment for firms located in labour intensive industries relative to firms located in skill intensive industries, which is consistent with the findings of Chapter 3 that the reform increased unskilled migration the most.

Table 5.11 Reform impact on firms' employment across skill intensity at the industrial level

	(1)	(2)	(3)	(4)
	Skilled industry		Unskilled industry	
	All firms	Matched firms	All firms	Matched firms
Reform*(t-4)	0.045	-0.025	0.084**	0.031
	(0.046)	(0.045)	(0.037)	(0.040)
Reform*(t-3)	0.019	-0.030	0.024	-0.018
	(0.031)	(0.035)	(0.031)	(0.032)
Reform*(t-2)	0.011	-0.015	0.008	-0.017
	(0.021)	(0.026)	(0.020)	(0.025)
Reform*(t-1)	0.005	-0.006	-0.000	0.001
	(0.012)	(0.015)	(0.012)	(0.017)
Reform*(t+1)	-0.002	0.011	0.012	0.024
	(0.011)	(0.015)	(0.013)	(0.016)
Reform*(t+2)	0.005	0.028	0.025	0.049**
	(0.016)	(0.019)	(0.020)	(0.021)
Reform*(t+3)	0.002	0.030	0.024	0.056**
	(0.021)	(0.025)	(0.025)	(0.027)
Reform*(t+4)	0.002	0.026	0.021	0.041
	(0.025)	(0.031)	(0.031)	(0.036)
Reform*(t+5)	0.086***	0.081*	0.111***	0.056
	(0.030)	(0.045)	(0.031)	(0.037)
MW	YES	YES	YES	YES
State sector	YES	YES	YES	YES
Trade policy	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES
Observations	598112	251884	727271	323955
Adjusted R^2	0.883	0.873	0.869	0.858
N_clust	220	138	220	137

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

5.4 The *Hukou* Reform and Labour Reallocation

- Firm Ownership and Heterogeneous Reform Impact on Employment

In the previous analysis, we found that the reform increased non-state-owned firms' employment, particularly for those firms located in labour intensive industries as well as those firms located in industries that use migrant workers more intensively prior to the reform. In this section, we further provide some evidence on the potential mechanism behind. Particularly, we examine whether the reform facilitates the labour reallocation from the state sector to the non-state sector.

We use both non-state-owned and state-owned firms to conduct this analysis. We construct an ownership dummy variable, which equals to 1 for state-owned firms and 0 otherwise. We use the triple-differences approach based on the reform dummy, year dummy and ownership dummy variables. To be exact, we add variables such as interaction terms between reform dummy, year dummy and ownership dummy, interaction terms between year dummy and ownership dummy, interaction term between reform dummy and ownership dummy and the ownership dummy to the model specification (4.1). We use the coefficients of interaction terms between reform dummy, year dummy and ownership dummy to capture whether the reform impact on state-owned firms is different from that of non-state-owned firms. Results are reported in Table 5.12.

Using the matched samples, we find that the coefficient is -0.106 three years after reform adoption, suggesting that the reform impact on firm employment is much smaller for state-owned firms relative to non-state-owned firms. We also find that the coefficient on the reform impact is 0.05 for non-state-owned firms three years after the reform adoption, which is consistent with previous results. Therefore, we can calculate that the coefficient on the reform impact is -0.056 for state-owned firms, suggesting that the reform decreased state-owned firms' average employment by 5.6% three years after reform adoption. Column (2) further controls for simultaneous

policies and we find very similar results. One explanation for these results is that the reform facilitates the labour reallocation from state-owned firms to non-state-owned firms as non-state-owned firms expand.

Overall, we find evidence that the reform also affects state-owned firms. In contrast to the non-state-owned firms that expand after the reform adoption, the state-owned firms downsize after the reform adoption when more redundant workers of state-owned firms are absorbed by the expanding non-state-owned sector.

Table 5.12 A Comparison of reform impact between state-owned and non-state-owned firms

	(1)	(2)
	Matched samples	Matched samples
Reform*(t-4)*State-owned firm	-0.027	-0.027
	(0.067)	(0.066)
Reform*(t-3) *State-owned firm	0.007	0.014
	(0.050)	(0.046)
Reform*(t-2) *State-owned firm	0.016	0.027
	(0.039)	(0.037)
Reform*(t-1) *State-owned firm	-0.036	-0.029
	(0.033)	(0.032)
Reform*(t+1) *State-owned firm	-0.029	-0.025
	(0.036)	(0.034)
Reform*(t+2) *State-owned firm	-0.062	-0.069*
	(0.042)	(0.040)
Reform*(t+3) *State-owned firm	-0.106*	-0.108**
	(0.055)	(0.051)
Reform*(t+4) *State-owned firm	-0.131	-0.145*
	(0.080)	(0.080)
Reform*(t+5) *State-owned firm	-0.061	-0.038
	(0.083)	(0.091)
MW	NO	YES
State sector	NO	YES
Trade policy	NO	YES
Firm FE	NO	YES
Observations	627197	621508
Adjusted R^2	0.873	0.873
N_clust	142	138

Notes: Due to space limitation, we do not report coefficients such as reform impact on non-state-owned firms as well as relevant variables in the table. Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

- Export Status and Heterogeneous Reform Impact on Employment

We have found that the reform facilitates labour reallocation from the state sector to the non-state sector. This labour reallocation not only increases labour supply to the firms of the non-state sector, but might also increase non-state sector's access to the local product market. To find out the most likely mechanism behind, this section further compares reform impact on firm employment between exporters and non-exporters. An exporter is defined as those firms with at least one year of positive exports prior to the reform adoption. Similar as before, we add variables such as interaction terms between reform dummy, year dummy and export dummy, interaction terms between year dummy and export dummy, interaction term between reform dummy and export dummy and the export dummy to the model specification (4.1). Results are reported in Table 5.13. We find that the reform increases non-exporters' average employment by 5.9% three years after reform adoption but only increases exporters' average employment by 0.6% during the same period. Therefore, the reform impact is much smaller for exporters relative to non-exporters.

Table 5.13 A comparison of reform impact on firm employment in log between exporters and non-exporters

	(1) Matched samples	(2) Matched samples
Reform*(t-4)*Exporter	0.003	0.017
	(0.040)	(0.039)
Reform*(t-3) * Exporter	0.007	0.015
	(0.019)	(0.018)
Reform*(t-2) * Exporter	-0.010	-0.005
	(0.016)	(0.016)
Reform*(t-1) * Exporter	-0.005	-0.002
	(0.014)	(0.013)
Reform*(t+1) * Exporter	-0.000	-0.000
	(0.014)	(0.014)
Reform*(t+2) * Exporter	-0.024	-0.025
	(0.018)	(0.017)
Reform*(t+3) * Exporter	-0.052**	-0.053**
	(0.025)	(0.025)
Reform*(t+4) * Exporter	-0.037	-0.034
	(0.028)	(0.027)
Reform*(t+5) * Exporter	-0.047*	-0.045
	(0.027)	(0.029)
MW	NO	YES
State sector	NO	YES
Trade policy	NO	YES
Firm FE	NO	YES
Observations	581289	575840
Adjusted R^2	0.902	0.902
N_clust	142	138

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

- Reform Impact on Non-exporters' Average Wage

We have found that the reform impact on firm employment is much larger for non-exporters relative to exporters. One explanation is that the labour reallocation from the state-sector to the non-state sector, driven by the reform, increases non-state sector's access to the local product market, which is more significant for non-exporters than exporters if exporters are less likely to be affected by the change in domestic market access. In this section, we further examine the reform impact on non-exporters' average wage. If the market channel does exist and even dominate the labour supply channel, it is possible that the average wage increases rather than decreases after the reform adoption. Table 5.14rm adoption. In other words, labour supply channel is not the only channel that the labour reallocation affects firms' expansion of the non-state sector. The labour reallocation from the state sector to the non-state sector also increases non-state sector's access to the local product market through reducing market disintegration caused by the state sector. The average wage increases when the market channel offsets the labour supply channel.

Overall, this section shows an alternative channel through which labour reallocation from the state sector to the non-state sector affects the expansion of the non-state sector in China. This channel differs from the previous one held by Imbert et al. (2016) that labour reallocation affects firm expansion through reducing the average labour cost. Nevertheless, our results confirm previous arguments that the state sector in China is accountable for product market disintegration through local protectionism (Young 2000; Poncet 2005). Given the fact that labour reallocation increases local firms' market access, our results are consistent with existing studies that factor reallocation within manufacturing firms in China would significantly improve firm productivity ((Hsieh and Klenow 2009; Song et al. 2011).

Table 5.14 Reform impact on non-exporters' average wage in log

	(1)	(2)
	Matched samples	Matched samples
Reform*(t-4)	-0.029	-0.033
	(0.023)	(0.022)
Reform*(t-3)	-0.014	-0.024
	(0.023)	(0.025)
Reform*(t-2)	-0.023	-0.016
	(0.018)	(0.018)
Reform*(t-1)	-0.037**	-0.026
	(0.019)	(0.019)
Reform*(t+1)	0.075**	0.066**
	(0.030)	(0.025)
Reform*(t+2)	0.104***	0.095***
	(0.035)	(0.035)
Reform*(t+3)	0.102**	0.085**
	(0.041)	(0.041)
Reform*(t+4)	0.103***	0.098***
	(0.030)	(0.030)
Reform*(t+5)	0.149*	0.029
	(0.086)	(0.072)
MW	NO	YES
State sector	NO	YES
Trade policy	NO	YES
Firm FE	NO	YES
Observations	348014	344835
Adjusted R^2	0.525	0.528
N_clust	142	138

Notes: Standard errors in parentheses;* p<0.1, ** p<0.05, *** p<0.01.

6 Conclusion

This paper studies whether manufacturing firms' employment responds to the *Hukou* reform. Using the Chinese Annual Industrial Firm Survey data (CAIFS) for the period 2000 to 2007, we estimate the reform impact on firm employment with a difference-in-differences model. To reduce the reform endogeneity issue, we use a propensity score matching approach to restrict the comparison to observable similar firms. Our estimates suggest that firm employment increases by 2.3% one year after reform adoption, which further increases to 5.1% three years after reform adoption. Several robustness checks indicate that the reform impact on firm employment is not driven by minimum wage reform, restructuring of state sector, reduction in trade policy uncertain as well as new firms. Moreover, we find that the reform impact on firm employment is larger for firms located in labour intensive industries relative to skill intensive industries, larger for firms located in industries that use migrant workers more intensively, suggesting that the reform mainly affects manufacturing firms that are labour intensive and use migrant workers more intensively, which is consistent with the previous findings that the reform affects the unskilled migrations the most. Furthermore, we find that the reform also affects state-owned firms. In contrast to the non-state-owned firms that expand after the reform adoption, the state-owned firms downsize because more redundant state-owned workers are absorbed by the expanding non-state-owned sector. Finally, we find evidence consistent with a conceptual framework in which the reform not only increases labour supply to the non-state sector but only increases the non-state sector's access to the local product market which used to be segmented by the state sector. To some extent, this study demonstrates that local governments in China have successfully used the *Hukou* reform to activate the local economy since the early 2000s.

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