The London School of Economics and Political Science

Essays on Development Economics and Chinese Economy

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To Rui Zhang.

Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

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Abstract

This thesis consists of three independent chapters on development economics and Chinese economy.

The first chapter examines how centralization affects regional development. I draw upon plausibly exogenous variations in centralization from a political hierarchy reform in China to investigate it in a novel sub-provincial setting. I show that centralization has positive and significant effects on the overall industrial output and urban population of regions. To understand the mechanism, I propose a theoretical framework, where centralization will help to reduce resource misallocation within a region and improve aggregate productivity. Consistent with it, my analysis of industrial firm-level data reveals a reduction in the dispersion of marginal products after centralization, and I quantify the productivity gains from centralization in a counterfactual analysis. In addition to the positive overall effects on regions, the reform also has distributional effects for the different counties that constitute the region.

The second chapter evaluates a firm-based pollution regulation in China in 2007 to investigate the relationship between political incentives and effects of environmental regulations. I show that when a municipality Party secretary has more incentives to improve the local economy for promotion, measured by his age, adverse impacts in employment and output on regulated firms will be much larger. At the same time, loss in regulated firms will be associated with gains in other unregulated firms in polluting industries, and there is no overall effects in manufacturing activities on polluting industries. I find that emissions of pollutants in municipalities with high-incentive leaders experience a significant reduction.

The third chapter estimates the effects of children genders on parents' time allocation due to the long-existing son preference in developing countries. Using household survey data in China from 1989 to 2009, I show that with more sons instead of daughters, both father's and mother's time on housework will rise. At the same time men will increase their working time on labour markets and women can enjoy more leisure on the contrary. For possible endogeneity in children's gender, I exploit exogenous variations from a law to forbid the use of ultrasound-B to reveal fetus gender.

Contents

Preface

This thesis consists of three self-contained essays that are aimed towards contributing to the understanding of the role of political incentives and public policies in economic development. Specifically, I investigate how changes in the political incentives of local officials and shocks from public policies shape the micro behaviours and finally affect the aggregate economic performance in the context of China.

During the era of reform beginning in 1978, China experienced enormous economic growth. In the market based reform, China has changed from a centrally planned economy to a mixed market economy. Unlike typical market economies, China has a government which is heavily involved in the economic process. As a result, it offers a great opportunity to study the relationship between government behaviours and economic performance.

The first chapter is about the long-standing debate between centralization and decentralization. In the last few decades, the trend all over the world has been to decentralize. But theoretical predictions are ambiguous and current empirical evidence is mixed. The chapter shows a novel benefit from centralization: better allocation of resources. I use a political hierarchy reform in China starting in 1983 as a natural experiment to exploit plausibly exogenous variations in centralization. In the reform, the political decision powers, as well as powers in public finance and administration, are taken out of the hands of local county governments and given to the regional prefecture governments.

First, using a difference-in-differences strategy, I show that centralization has positive causal effects on the overall industrial output and urban population of prefectures. I conduct a series of tests to secure the identification assumptions. Second, to show the mechanism, I propose a theoretical framework with heterogeneous firms. Under decentralization, local county governments subsidize firms using their own fiscal budgets, which produce dispersions in marginal products across counties and then a loss in aggregate productivity. In contrast, under centralization, the prefectural government can manage all the fiscal revenues from its component counties together and reallocate them to equalize marginal products between them. In one word, centralization

can reduce resource misallocation. To empirically identify the mechanism, I use a comprehensive firm-level dataset and show that the dispersion of marginal products, a measurement of resource misallocation, was reduced after the centralization reform. Moreover, I quantify the productivity gains from centralization in a counterfactual analysis.

Meanwhile, besides the overall benefits, the centralization reform also produces distributional effects in the counties within a prefecture. After centralization, the production of specific industrial goods tends to be concentrated in fewer counties than before. Specifically, the capital county of the prefecture will gain a great deal more from this concentration than the others do. I show that this is because capital's comparative advantage is in industrial production, but not the preference or favouritism.

The second chapter evaluates a set of firm-based environmental regulations in China in 2007 to investigate the relationship between the political incentives of local officials and the effects of environmental regulations. The environmental regulations in question are set by the Ministry of Environmental Protection targeting high-polluting firms which account for 65% of the emissions of sulfur dioxide, smoke and industrial dust.

In recent years, policies on the environment and pollution have been among the most controversial topics in both public and academic discussion. Recent research accepts that the regulations are associated with improvements in environmental quality and reductions in the manufacturing activities of regulated firms and industries. In the second chapter, I investigate whether the political incentives of local officials will play a role in the consequences brought by environmental regulations. To show this empirically, I use municipality secretaries' ages as a measurement of political incentives. Due to the retirement age limit and the minimum tenure requirement by the Chinese Communist Party, the possibility of a municipality Party Secretary's promotion decreases sharply after he reaches 57 years old. I link a dataset of municipality Party Secretaries to the nationwide manufacturing enterprise survey, containing information of both regulated and unregulated firms subject to the 2007 firm-based pollution regulations.

Using a triple differences model, I show that when a municipality Party Secretary has more incentives to improve the local economy in order to gain promotion, the firm-based regulations will have larger adverse impacts in employment and output on regulated firms. At the same time, unregulated firms in polluting industries are found to absorb the loss of employment from the regulations, and there is no overall reduction in manufacturing activities on polluting industries. I find that emissions of pollutants in municipalities with high-incentive leaders are significantly reduced, suggesting that the reallocation of economic activities from regulated to unregulated

firms is associated with cleaner production.

To interpret the empirical results better, I present a simple career-concerns model. I find that the political incentives of local officials can explain the responses in different firms and industries. Following Jia (2014), I argue that political incentives act as a complement to economic performance with regard to the promotion rule. I extend the model from Jia (2014) to include the substitution of clean for dirty inputs, and then interpret how regulating the dirty input will induce local officials with different political incentives to choose different levels of input. Highly incentivized officials will enforce the regulations more strictly and reduce output in the regulated firms more drastically. At the same time, their incentives will encourage them to put more effort into reallocating resources from the regulated firms to the unregulated ones.

In the third chapter I focus mainly on a law aiming to forbid the use of ultrasound-B to reveal the gender of fetuses. In developing countries, preference for sons is a long-standing tradition. The existence of such a preference induces pre-birth selection and post-birth discrimination. I exploit exogenous variations from the law to estimate the effects of children's genders on parents' time allocation after births. Using household survey data in China from 1989 to 2009, I show that the forbidding of ultrasound-B reduces the male-biased sex ratio at birth. Using the law as an instrumental variable, I find that with more sons instead of daughters, the time spent on housework by both fathers and mothers rises. At the same time fathers have to increase their work time in the labour market and women can reduce theirs to enjoy more leisure. To secure the identification assumption on exclusion restrictions, I perform a placebo test making use of a sample of adults who are childless. These results are consistent with theoretical predictions founded on a utility-based son preference model.

The implications we get from the analysis in this thesis are not specific to China, a country where government is deeply involved in the economy. It can contribute to the general knowlege in development economics in many ways. For example, my first chapter finds that decentralization can be inefficient due to the misallocation of resources by local governments. In democracies, though classical theories like Tiebout (1956) set the efficiency of decentralization as a benchmark, more recent studies emphasize its shortcomings. For example, Boffa et al. (2015) argues that voters may not monitor decentralized local governments well due to information asymmetry, so decentralization in democracies can produce a similar misallocation pattern as in my first chapter. Chapter 2 of my thesis finds that the promotion incentives of local officials in China will produce heterogeneity in the effects of environmental regulations. This rationale and logic of the political economy of environmental regulations also apply to politicians facing electoral incentives (List and Sturm, 2006). My last chapter seeks to

reveal the essence of son preference, which is common in most developing countries. Rose (2000) investigates the same topic using data from India. These examples and implications make me believe that my thesis can exploit the unique settings from China to shed light on the broader contexts of other countries in the world and contribute to knowledge in general economics.

Chapter 1

The Value of Centralization: Evidence from a Political Hierarchy Reform in China

How does centralization or decentralization at government affect regional development? This paper draws upon a natural experiment in China's political hierarchy from the 1980s to investigate the effects of centralization in a novel sub-provincial setting. Using a difference-in-differences approach, I show that centralization has positive causal effects on the overall industrial output and urban population of regions. To understand the mechanism, I propose a theoretical framework, where centralization will help to reduce resource misallocation within a region and improve aggregate productivity. Consistent with the mechanism of a more efficient allocation of resources, my analysis of industrial firm-level data reveals a reduction after centralization in the dispersion of marginal products. I quantify the loss in productivity under decentralization by hypothetically reallocating inputs to equalize marginal products to the extent observed in conditions of centralization. In addition to the positive overall effects on regions, the reform also has distributional effects for the different counties that constitute the region. After the reform, industrial production tends to concentrate in counties which are the capitals of their regions.

1.1 Introduction

The debate between centralization and decentralization has long been of interest to economists and policy makers. In the last three decades, many countries in the developing world, in particular transition economies in Asia, Eastern Europe and Latin America, have implemented decentralization reforms (World Bank, 2000; Gadenne

and Singhal, 2014). Decentralization in practice consists of the devolution of various decision-making powers, including fiscal, administrative and political, to small-scale entities at the local level. However, while the degree of centralization is considered one of the most important dimensions of policy making, in the economics literature it remains an open question. In theory, although Tiebout (1956) first raised the efficiency of decentralization realized by voting with one's feet, most recent work provides ambiguous predictions on its effects. For example, Besley and Coate (2003) model the debate as a trade-off between conflicts of interests under centralization and externality problems under decentralization; Boffa et al. (2015) argue that a centralized government cannot differentiate policies for voters with heterogeneous tastes but, under a decentralized government, voters with less information may not monitor local governments well. In terms of the related empirical literature, evidence is mixed and most of the existing papers fail to establish a causal relationship (Bardhan, 2002; Mookherjee, 2015). In addition, most of them are based on decentralization reforms and few directly evaluate the effects of centralization. Moreover, most empirical papers do not identify any clear mechanisms that might make either centralization or decentralization beneficial. In general, there is no clear answer, either theoretically or empirically, about the extent to which a government should be centralized.

In the context of China, decentralization from central to local governments is considered one of the main sources of economic growth in the last thirty years (Xu, 2011). Most economists argue that the competition between local governments preserves their incentives to adopt policies promoting growth. However, Young (2000) argues against decentralization. Under the partial reform in China, local governments retain the incentives and powers to distort the economy. On the one hand, faced with rent-seeking opportunities, local governments have the incentive to maintain and even increase distortions for high-margin industries. On the other, decentralization releases powers to local governments and enables them to extract rents. They have various ways of distorting the economy, including subsidies for specific industries, expropriating lands for industrial use or setting trade barriers. As a result, decentralization fragments the domestic market and leads production away from patterns of comparative advantage. Young (2000) observes two simple trends as suggestive evidence of his view. One concerns the convergence in compositions of production across provinces over the last few decades and the other the divergence in prices, labor productivity and factor allocations. The combination of these two trends forms a picture of market fragmentation and production distortion. His paper motivates such research as Poncet (2003) and Cai and Treisman (2006) on the potential caveats to decentralization in China. However, to the best of my knowledge, no single paper offers causal evidence on it.

This paper aims to help fill this gap in the literature by providing a quantitative evaluation of the causal effects of centralization stemming from a political hierarchy reform in China. This hierarchy reform, first launched in 1983 and lasting until 2003, centralized decision powers from a lower "local" county level to a higher "regional" prefecture level. Figure 1.1 provides a simple illustration of the reform. Before it took place, county governments in a prefecture could decide on economic projects, public finance and personnel administration in their own counties. The central government realized the problem of such excessive decentralization: "within a region, too many local governments exist; their works and policies contradict and offset each other, producing fragmentation; it is harmful for social and economic coordination and development" (Central Committee of the Party and the State Council, 1983). In its response, the central government designed the 1983 reform to transform prefectures, which were more decentralized, to more centralized "prefecture-level municipalities". Decision rights on county governance were therefore centralized to the prefecture-level municipalities. I exploit variations in centralization induced by this reform to examine its outcomes for regional development, mainly those in industrialization and urbanization.

To guide the empirical analysis, I present a heterogeneous firm model based on Hopenhayn (2014). Before the centralization reform, county governments could decide on their own subsidies to local industrial firms. After the reform, each prefecture-level government managed the funds from all its component counties and decided how to allocate them. I show that the prefecture as a whole benefited from this centralization reform, while at the same time the component counties experienced differentiated distributional effects. On the one hand, better coordination through the centralized prefecture-level municipality government could help equalize the marginal products; the aggregate total factor productivity and total output in the whole area, would both be improved. On the other hand, since some resources were reallocated from counties with low marginal product firms to those with high marginal product firms, it produced differential outcomes in different counties.

To test the model predictions on the overall and the distributional effects of the centralization reform, I use the variations in the reform timings of different prefectures to conduct a difference-in-differences regression as the baseline empirical model. Using prefecture-level data from 1983 to 2003, I find from the baseline regression that, after the reform, prefectures as a whole experienced increases in industrial output and urban population. Several checks on the identification assumptions are performed to allow a causal interpretation of these effects. At the same time, I find that the reform is associated with improvements in aggregate productivity. To show its mechanism, I

apply the methodology of Hsieh and Klenow (2009) to measure resource misallocation using dispersions of "revenue productivity" (TFPR). With a widely used firm level dataset, I show that the centralization reform could reduce misallocation within prefectures and quantify the loss in aggregate productivity due to decentralization. This suggests that the increase in industrial output was due not only to increasing labour input, but also to the improvement in productivity. This demonstrates the mechanism through which centralization may benefit economic performance.

Second, with regard to the predicted distributional effects, I find from county-level data that the capital counties where prefecture-level governments were located benefited from the centralization reform, while other counties suffered a slight loss from the baseline regression. Meanwhile, production in the industrial sectors became more concentrated in fewer counties. I show that such concentration is not due to preference or favouritism but to advantages in productivity: firms in counties with higher sector-specific productivity produced more after the reform. This offers support to the aggregate implications of the distributional effects.

This paper therefore provides a novel insight into the potential gains from centralization. Although decentralization is well accepted by most policy makers, in this paper I provide an important piece of evidence that excessive decentralization is harmful and clearly identify a novel mechanism: resource misallocation. There is a large theoretical body of literature proposing various theories on the trade-offs between centralized and decentralized institutions. The classical approach formalized by Oates (1972) assumes that centralization can internalize the spillovers across districts, but the accompanying uniformity will produce inefficiency, since preferences are heterogeneous. Recent work has laid greater emphasis on the political process. For example, decentralization can avoid conflicts of interest (Besley and Coate, 2003) and the accountability problem (Seabright, 1996), while it may induce a race-to-the-bottom competition between local governments (Keen and Marchand, 1997) and corrode the state capacity by locally shielding firms from central regulations and tax collectors (Cai and Treisman, 2004). In this paper, I argue that decentralization can bring resource misallocation, a mechanism which has not been formally studied before.

I identify this mechanism by showing that the centralization reform is associated with less resource misallocation and gains in aggregate productivity. In a decentralized institution, county governments collect tax revenues and locally subsidize industrial development. When input factors are imperfectly mobile, as in China, the marginal revenues brought by government spending may be different in different counties, which produces resource misallocation within a region. However, when the power of governance is centralized at a higher, prefecture level, the prefecture-level government can

manage the taxes from counties and allocate them efficiently, resulting in equalized marginal products across counties. In recent years increasing numbers of papers have emphasized the great role of resource misallocation in explaining the disparities in aggregate productivity across countries (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Bartelsman et al., 2013), but few of them quantify the contribution made by different sources of misallocation. The present paper explicitly illustrates how far decentralization causes misallocation and reduces aggregate productivity.

Furthermore, my paper features a sub-provincial centralization experiment to enrich the existing literature. First, due to data availability, previous literature mostly concentrated on "central - provincial" relations, referred to as "fiscal federalism" (Qian and Weingast, 1997). For example, since 1978, China's economic miracle has been widely attributed by numerous papers to decentralization from the central government to provincial governments (see, for example, Maskin et al. (2000) and Jin et al. (2005); see also a survey by Xu (2011)). Unlike them, I focus on the evolution of political powers between two sub-provincial governments. They represent governance entities in China but are left as black boxes in the literature without theoretical or empirical analysis. Among very few "regional - local" papers, Zhuravskaya (2000) studies a similar setting in Russia. Second, given the popularity of decentralization policies, most of the existing literature draws on decentralization reforms. My paper is based on a unique centralization reform in China and can therefore provide direct evidence for the effects of centralization. Third, previous studies focus most on the provision of public goods; however in many developing countries, the function of government is relatively straightforward to foster economic development and growth. Instead of common outcomes in the form of public goods, this paper directly investigates economic performance.

This paper also contributes to the literature on urbanization and regional development. Urban economists have confirmed the importance of political institutions in determining urbanization and urban primacy (Henderson and Becker, 2000). For example, Henderson and Wang (2007) and Ades and Glaeser (1995) have shown that democratization helps to limit the ability of a national ruling class to concentrate resources in the national capital. This paper contributes by offering sub-national causal evidence on these topics.

The remainder of the paper is organized as follows. Section 1.2 documents key features in the context of China relevant to my paper. Section 1.3, as a guide to the empirical analysis, develops a conceptual framework on industrial production in counties under different distortion conditions. Section 1.4 introduces the data. Section 1.5 introduces the basic empirical strategy used in the paper. Section 1.6 documents

how the centralization reform affects industrial developments in overall prefectures and quantifies the improvement in within-prefecture resource allocation. Section 1.7 examines the distributional effects on component counties. Section 1.8 checks the robustness of the presented empirical results. Section 1.9 concludes.

1.2 Background

1.2.1 Political Hierarchy and Jurisdictions in China after 1978

In China, there are four levels in sub-national jurisdictions: province, prefecture, county and township level. Figure 1.2 provides some basic information.[1] The average size of a province in China is about 300,000 km^2, similar to the sizes of Italy or Arizona. As a result, it is difficult for provincial governments to make policies according to local heterogeneity. They have to rely on lower level governments, that is, prefecture- and county-level governments, to implement policies locally. Counties are the basic units in China's local government hierarchy, comparable to counties in the United States. Nevertheless, it is not easy find a comparable counterpart to prefecture-level jurisdictions in the United States. We can use Metropolitan Statistical Areas (MSA) to form a rough analogy, though MSAs are not legal administrative units. Similar to an MSA, in China a prefecture-level jurisdiction combines several counties. Its average size is about 28,000 km^2, similar to the size of the Chicago metropolitan area, a combination of fourteen counties. In terms of population, its average population is about 4.1 million by 2010, similar to the San Francisco metropolitan area, a combination of five counties.

As prefecture- and county-level governments play an essential part in China's political and economic processes, how governance responsibilities and powers are divided is critical in understanding local governments in China. This paper concentrates on the relations between prefecture- and county-level governments. Figure 1.3 sketches the basic political hierarchy in China. A province is composed of several prefecture-level units, and a prefecture-level unit is composed of several county-level units. Prefecture-level governments play an intermediate role between a province and its component counties. There are two types of prefecture-level units: prefecture (*diqu*) or prefecture-level municipality (or prefecture-level city, *dijishi*). It is necessary to emphasize that both prefectures and prefecture-level municipalities are at the same level (prefecture level) in China's political hierarchy. My analysis will base on the transition from prefectures to prefecture-level municipalities at this same level.

[1] Township governments do not possess many decision rights in most economic matters, so will not be discussed in this paper that will analyse the evolution of decision powers between local governments.

A key difference between a prefecture and a prefecture-level municipality lies in their relations with the counties belonging to them: in a prefecture; political, fiscal and administrative powers are decentralized to county-level governments, and in a prefecture-level municipality these powers are centralized to the prefecture-level government.

In a prefecture, all counties are almost autonomous and the county governments have decision rights over the development of their own counties. The prefecture government possesses no power over the counties, since in legal terms it is only a provincial government's "resident agency" (*paichu jigou*) in its prefecture's territory. In other words, it does not constitute an official layer of local administration (Chung and Lam, 2004). Leaders in the prefecture government cannot enact economic policies on their own; instead, they convey decisions by the provincial government to their component counties and oversee them. Moreover, because provinces in China are so large, many policy choices are left to the lower level of the counties. As a result, counties under any prefecture can make independent decisions on their own affairs, including those of an economic and political nature. For example, they can decide where public funds shall be spent and what public goods will be offered; on local taxes and subsidies; on the location of newly opened state-owned enterprises, among other areas. In general, in a prefecture, counties enjoy considerable autonomy.

In contrast, in a prefecture-level municipality, county governments lose their decision rights. The prefecture-level municipality government located in the capital county can administer the development of all of the prefecture-level municipality's counties. The reason for this difference is that the prefecture-level municipality government is not a province government's "resident agency', but an essential intermediate level of government between a province and a county. Legally, its component counties are under the supervision of the prefecture-level municipality government itself, not of the province government. In this situation, county governments are manipulated by the prefecture-level municipality government: their leaders are nominated by the prefecture-level municipality government; their fiscal revenue and expenditure are not only supervised but also managed; they are not allowed to set up new state-owned enterprises freely and the choice of location is coordinated by the prefecture-level municipality government. Counties in prefecture-level municipalities therefore are not as autonomous as in prefectures. The prefecture-level municipality officials can give a range of orders to any given county.

In general, three types of decision rights are transferred to prefecture-level municipality governments (Shi et al., 2009). First come administrative powers in regard to social and economic projects. A prefecture-level municipality government has hundred-

s of administrative rights over developments in its constituent counties. For example, the setting up of a state owned enterprise can no longer be decided by the county in which it is located. Since the reform, this decision must be approved by the corresponding prefecture-level municipality government. Second are powers over public finance revenue and expenditure. Prefecture-level municipalities are a formal independent fiscal regime, while prefectures are not. On the revenue side, prefecture-level municipality governments now take part in sharing counties' tax revenue and the counties have to rely more on transfer payments from upper levels of government. On the expenditure side, prefecture-level municipality governments take more responsibility for public expenditure. Third come the powers to appoint local officers. These powers in three areas of decentralization are similar to those summarized by Bardhan (2002) and World Bank (2000): administrative, fiscal and political. The combination of these powers is important. Blanchard and Shleifer (2001) show political centralization is a key complement to fiscal federalism.

An example documented by a local officer in Wuhu, Anhui Province provides a vivid illustration on the function of prefecture-level municipality governments. In his book, Han (1986), describes how Wuhu prefecture-level municipality government managed its component counties: "Due to huge resource of duck down in Wuhu, every county wanted to build a down coat factory. But the prefecture-level municipality government only approved one factory in the capital county to avoid a waste, which could not happen under previous prefecture setting." We can see from this story that a prefecture-level municipality government can manage public finance funding and decide on several economic projects on component counties, which is impossible for prefecture governments.

It is noteworthy to distinguish two types of component counties in prefectures or prefecture-level municipalities: capital counties (or urban districts, *shiqu*) and peripheral counties (*xian*). A capital county is where the prefecture or prefecture-level municipality government is located. Historically, a capital county was the central and the most developed part in a prefecture or prefecture-level municipality, and its identity as the location of the prefecture-level government is mostly confirmed in history and remains unchanged. The pre-determined differences between capital and peripheral counties allows the present research to explore possible distributional effects on component counties.

1.2.2 The 1983 Reform: "Turning Prefectures into Prefecture-level Municipalities"

The reform starting in 1983 aimed to abolish prefectures and subject counties to the rule of prefecture-level municipalities (Central Committee of the Party and the State Council, 1983). The primary goal of the reform was to accelerate urbanization and industrialization by the better coordination of resources and help the component counties: "The shortcomings of prefecture institution is obvious: within a region, too many local governments exist; their works and policies contradict and offset each other, producing fragmentation; it is harmful for social and economic coordination and development ··· The main solution is to gradually transform prefectures to prefecture-level municipalities and let prefecture-level municipalities lead belonging counties" (Central Committee of the Party and the State Council, 1983). The central government supposed that after a prefecture had been changed into a prefecture-level municipality, the prefecture-level municipality government could better manage and coordinate component counties. The central government expected this prefecture-level municipality setting to increase the efficiency of both capital counties and peripheral counties under the coordination of the prefecture-level municipality governments.

The timings of the reform were mainly made by the central government (Chung and Lam, 2004). The central government also stated some conditions to be met on which kind of prefectures could be reformed. It included requirements on the non-agricultural population (150,000) and industrial output in capital counties (400 million) (Central Committee of the Party and the State Council, 1983). Nevertheless, these conditions were not binding in the actual process. The process of this reform is illustrated in Figure 1.4. Starting from 1983, the number of prefecture-level municipalities increased rapidly and the number of prefectures decreased at a similar rate. Until 2003, except for a few special minority residences, almost all prefectures were turned into prefecture-level municipalities. Moreover, as the pattern of the number of total prefecture level jurisdictions suggests, such a reform was basically a one-on-one transition between a prefecture-level municipality and a prefecture. Figure 1.5 presents an example of the reform.

I will make use of the variations of this reform across space and time to explore the heterogeneous development outcomes of the centralization of governance. As described above, in a prefecture, the powers of governance are devolved to county governments and all counties behave independently; under a prefecture-level municipality, the powers are centralized to prefecture-level municipality government. The "Turning prefectures into prefecture-level municipalities" reform exhibited great many variations in the extent of centralization. Figure 1.6 depicts the variations of the reform across

space. In my empirical analysis, I do not include five provinces: Tibet, Inner Mongolia, Xinjiang, Qinghai and Hainan, since their prefectures did not undergo reform during the time period examined, so there are no within-province variations. The reason for this is low population densities and some special policies such as the ethnic autonomous institution in the first three of them. Furthermore, I exclude those which had already been prefecture-level municipalities before 1983. I also exclude them as they are not a part of the 1983 reform and may not be comparable to those treated during the reform (Chung and Lam, 2004). Most of them are capitals of provinces and much larger than those in the sample. Finally, I exclude four province-level municipalities (Beijing, Shanghai, Tianjin and Chongqing) as well as Hongkong, Macau and Taiwan, whom the reform did not apply to. At last, the policy affected about 60% population in the whole country.

1.3 Conceptual Framework

In this section I develop a simple framework to guide the empirical analysis. The frame is based on the model in Hopenhayn (2014). He adopts a simplified model of firm heterogeneity in perfect competition as in Lucas (1978) and Hopenhayn (1992) to illustrate the relationship between resource misallocation and aggregate productivity. I combine this with the behaviours of local governments in this framework, validating a possible source of distortions faced by firms in developing countries raised by Hsieh and Klenow (2009). In my model, under decentralization, county governments subsidize firms subject to their own budget constraints, producing heterogeneous distortion rates and therefore resource misallocation. Under centralization, the prefecture-level municipality government manage component counties' budgets and reallocate them to reduce misallocation.

There are N counties in a prefecture. I assume that all counties produce a homogeneous output in a perfect competition market and that the production function for county i is given by a Cobb-Douglas function of its representative firm

$$Y_i = A_i L_i^{\eta}, \tag{1.1}$$

where A_i and L_i are total factor productivity and labour in each county i's industrial sector. Production displays decreasing returns in the only input labour ($\eta < 1$).[2]

[2]Here I adopt a diminishing returns to scale production function. In the productivity literature it is a common practice to assume diminishing returns in production, such as Hopenhayn and Rogerson (1993) and Restuccia and Rogerson (2008). In contrast, Hsieh and Klenow (2009) assume diminishing returns are in the demand side where monopolistic competition (Dixit and Stiglitz, 1977) is used, instead of the production side in this paper. Actually the two flavours of modelling are equivalent

Different from Hopenhayn (2014), I assume labour in the industrial sector is supplied with infinite elasticity under an exogenous wage w. It is based on Lewis (1954)'s classical theory where the industrial sector can absorb labour from the agricultural sector with a fixed wage, which is the case throughout the present study period in China. In that sense, w is equalling to the subsistence wage in the agricultural sector.

To promote industrial growth, county government i subsidises output with a distortion rate τ_i. It can be interpreted as support for county governments to offer firms, which are more general than a cash subsidy, such as tax deduction. The revenues of the representative firms are $(1+\tau_i)PY_i$. Then each county's workers and output in the industrial sector is determined as

$$L_i = [\frac{P\eta A_i(1+\tau_i)}{w}]^{\frac{1}{1-\eta}} \tag{1.2}$$

$$Y_i = A_i[\frac{P\eta A_i(1+\tau_i)}{w}]^{\frac{\eta}{1-\eta}} \tag{1.3}$$

The total output Y and workers L in the prefecture is an aggregate of those in the component counties:

$$L = \sum[\frac{P\eta A_i(1+\tau_i)}{w}]^{\frac{1}{1-\eta}} \tag{1.4}$$

$$Y = \sum A_i[\frac{P\eta A_i(1+\tau_i)}{w}]^{\frac{\eta}{1-\eta}} \tag{1.5}$$

We can see from the above equations that the output and the labour depend on the distortion rates they face as well as their productivity. As counties aim to foster as much industrial growth as possible, the distortion rates will depend on their budget constrains. The budget constraints will differ in decentralization and centralization settings.

First, I discuss the decentralization situation, that is, prior to the 1983 reform. Here, each county government faced a constant budget constraint:

$$\tau_i PY_i \leq T_i. \tag{1.6}$$

T_i measures the total resource constraint county i can use to support industrial activities. For simplicity I assume it is exogenous to every county. The assumption is not unrealistic considering the fact that in China amounts of fiscal revenue of local governments largely depend on their endowments of land which can be sold for real estate development.

In this model, I assume that the only preference of local governments is to maxi-

and isomorphic when guiding the counterfactual analysis. I will show this in Section 1.8.7.

mize industrial output by subsidizing subject to their own budget constraint. Under decentralization, each county could make decisions about how to use its own funding to subsidize industry. It is straightforward to see each county will simply run out of its endowment T_i to subsidize industrial production. The distortion rate τ_i, pinned down by i's own budget constraint, differed between counties due to heterogeneous productivity A_i and endowment T_i. It results in differences in the marginal products of labour. There exists potential inefficiency from such resource misallocation. I denote the decentralization equilibrium as $\{\hat{\tau}_i, \hat{L}_i, \hat{Y}_i\}$. $\hat{\tau}_i$ is determined by budget constraint 1.6.

Now I turn to the situation after the reform. In this case, the prefecture have been turned into prefecture-level municipality and its component counties lost their decision rights. The prefecture-level municipality government was now put in charge of all counties. It could determine how much to spend on each county to maximize output in the whole area. The prefecture-level municipality government's budget constraint was:

$$\sum \tau_i P Y_i \leq \sum T_i. \tag{1.7}$$

I denote the equilibrium as $\{\tilde{\tau}_i, \tilde{L}_i, \tilde{Y}_i\}$. Taking the derivative of Equation 1.5 subject to the above budget constraint, we can find that the first order condition is

$$\tilde{\tau}_1 = \tilde{\tau}_2 = \cdots = \tilde{\tau}_N = \tilde{\tau} \tag{1.8}$$

The comparison between two equilibrium outputs can be written as

$$\hat{Y} = \sum A_i [\frac{P\eta A_i(1+\hat{\tau}_i)}{w}]^{\frac{\eta}{1-\eta}} \leq (1+\tilde{\tau})^{\frac{\eta}{1-\eta}} \sum A_i [\frac{P\eta A_i}{w}]^{\frac{\eta}{1-\eta}} = \tilde{Y} \tag{1.9}$$

$$\hat{L} = \sum [\frac{P\eta A_i(1+\hat{\tau}_i)}{w}]^{\frac{1}{1-\eta}} \leq (1+\tilde{\tau})^{\frac{1}{1-\eta}} \sum [\frac{P\eta A_i}{w}]^{\frac{1}{1-\eta}} = \tilde{L} \tag{1.10}$$

Intuitively, before centralization, each county spend its own revenue subject to the budget constraint to subsidize industrial production. Due to heterogeneous productivities, marginal products of their revenue are different across counties. After centralization, the prefecture-level municipality government could take available revenues from all counties together and reallocate across counties to equalize the marginal products. With a more efficient allocation of revenues, total output and workers in the whole area would be improved. From the expressions of $\hat{Y}_i$ and $\tilde{Y}_i$ above, the

predictions for empirical tests can be shown:

$$\hat{Y} \leq \tilde{Y} \tag{1.11}$$

$$\hat{L} \leq \tilde{L} \tag{1.12}$$

This framework is mainly based on Hopenhayn (2014). In Hopenhayn (2014) and other misallocation literature, they often conclude models by showing the dispersion in distortion rates τ_i will bring loss in aggregate productivity. In my model I take a step further, trying to raise a possible source of the dispersion. I introduce local governments who will spend a fix amount of revenues on subsidizing firms. Due to the heterogeneity in productivity across counties, firms in different counties will be subsidized in a different rate. That is the source of the dispersion in τ_i in my model.

1.4 Data

1.4.1 Aggregate Data on Population and Production

The main outcomes of interest relate to the industrial and urban development in China. To this end, I collect and digitize data on output and population both on prefecture level and county level. Output variables can be broken into agricultural and industrial. Population variables can be broken into agricultural and non-agricultural. They are reported by the National Bureau of Statistics in published statistical yearbooks and local gazetteers.

The study period in this paper is from 1983 to 2003; prior to 1983, data is scarce. After 2003, the central government stopped the reform and began to re-decentralize powers to some developed counties under various prefecture-level municipalities, which may contaminate the estimated effects of the centralization reform.

The sample in the empirical part includes all prefectures not transformed in to prefecture-level municipalities by 1983. Although the central government did not encourage it, a small number of prefectures were reformed to prefecture-level municipalities by provincial governments. It would be a concern that those prefectures treated prior to the reform period were not comparable to those whose treatment were required by the central government after it announced the reform. Therefore, I only include those that remained as prefectures until at least 1983.

Panel A of Table 1.1 provides summary statistics of the population and output aggregates. We can see that capital counties were more urbanized and industrialized than peripheral counties, and even showed higher productivity in industrial production. Their output made up the most part in prefectures.

1.4.2 Aggregate Data on Public Finance

Besides the outcomes of population and production, the variables associated with public finance are also significant for confirming that the centralization actually occurred. The public finance data is from "The Prefecture, City and County Public Finance Statistics Yearbook", reported by the Ministry of Finance in China. It documents fiscal revenue and expenditure at both county and prefecture level annually from 1993. I bring in the data for 1993 - 2003.

Summary statistics of the main public finance variables are presented in Panel B of Table 1.1.

1.4.3 Firm-level Statistics

To explore more implications of the effects on industrial sectors and on enterprise, I use firm-level data from Chinas Annual Survey of Industrial Production, a survey also conducted by the National Bureau of Statistics. It is an annual census from 1998 containing all non-state industrial firms with sales more than 5 million Yuan (about 0.6 million dollars in 1998), plus all state-owned firms. It is the most widely used micro data when studying industrial production in China (for example, Hsieh and Klenow (2009) and Song et al. (2011)).

To fit into the study period of the aggregate level analysis, I use firm level data from 1998 to 2003. It consists of over 100,000 firms in 1998 and nearly 200,000 in 2003. I use unique IDs to link firms over years to construct the panel. In some special cases, such as mergers and acquisitions, firms change their IDs. To provide a more precise matching, I follow Brandt et al. (2012) to link firms over time using their Chinese name, address and telephone number in addition to unique IDs.[3]

The information I use from this firm level dataset includes the firm's industry code, location, ownership, outputs, value-added, wage payments and capital stock.

1.5 Empirical Strategy

1.5.1 Baseline Model

To empirically evaluate the impact of the centralization, I use a difference-in-differences method as the baseline model:

$$Y_{jpt} = \beta Treat_{jpt-1} + \phi w_{jpt} + \alpha_j + \delta_{pt} + \epsilon_{jpt} \tag{1.13}$$

[3]The details can be found on the authors' website (www.econ.kuleuven.be/public/n07057/china)

where Y_{jpt} is an outcome variable in prefecture j, province p and year t; $Treat_{jpt-1}$ is the main independent variable, indicating whether prefecture j in province p received treatment by year $t-1$;[4] α_j is a prefecture fixed effect; δ_{pt} is a year×province fixed effect; w_{jpt} are control variables including some simultaneous administrative changes; for example, whether prefecture j becomes a special economic zone, a coastal open city or a deputy-provincial city. I cluster the standard errors at the prefecture level. Thus β provides the overall effects of the centralization reform on outcomes of interest in prefectures as a whole.

The inclusion of prefecture fixed effects α_j captures any time invariant characteristics of prefectures, such as culture and geography. Province-by-year fixed effects δ_{pt} pick up province specific shocks, for example, policies implemented by provinces or the central government, price fluctuations and changes in central-local relations. Controlling for these fixed effects, I identify the effects of the centralization reform using within-province variations.

Variations of the main independent variable $Treat_{jpt-1}$ come from different reform timings in the sample. Thus, a natural challenge to the validity of the baseline empirical strategy is the non-randomness of the reforms across prefectures. If there were some unobservable factors that were simultaneously correlated with timings and outcomes of being reformed, the coefficient of interest β would be biased. For example, it could be the earlier selection of those prefectures with more growth potential as prefecture-level municipalities. More formally, the identification assumption of the baseline regression is a standard parallel trend assumption, as in any difference-in-differences specification: in the absence of reform, the growth in the outcomes of interest would be the same across any prefectures within a province.

1.5.2 Checks on the Identification Assumption

I provide several pieces of evidence in support of the identification assumption. The first one is to show that there are no differential trends prior the reform across prefectures by estimating the baseline difference-in-differences model with flexible coefficients. Then I will use a "de-jure" reform in Zhejiang province as a placebo test. At last I do some robustness checks such as controlling for prefecture specific time trends.

First, I propose a flexible difference-in-differences model to show the trends of the treatment effects before and after the reform year. To be specific, I test the identification assumption by estimating a set of twelve yearly treatment effects beginning five

[4]Most prefectures underwent reform in the middle of the treatment years, so I use a one-year lag to capture the treatment effect more precisely. The results are robust to imposing different lags. I include one of these practices in the robustness section.

years prior to the reform event and continuing for five years thereafter. This enables me to check pre-trends in these yearly treatment effects to secure the rightness of the identification assumption. It is a more flexible form of the baseline regression to allow the effect to vary by year in relation to the reform. The specification can be written as follows:

$$Y_{jpt} = \sum_{\tau=-5}^{5} \beta_\tau I(YearsSinceTreat_{jt} = \tau) + \phi w_{ijpt} + \alpha_j + \delta_{pt} + \epsilon_{jpt} \tag{1.14}$$

where $I(\cdot)$ is an indicator function and $YearsSinceTreat_{jt}$ counts the years at time t since prefecture j was treated. Then $YearsSinceTreat_{jt}$ takes negative values counting the years before the treatment, positive values after the treatment and zero when t is the year it was treated. If the parallel trend assumption holds prior to the reform, $\beta_\tau = 0$ when $\tau < 0$.

Second, I estimate the "placebo" treatment effects in Zhejiang province. Reform did occur in Zhejiang Province, where all prefectures were turned into prefecture-level municipalities during 1983 and 2003. However, the decision rights of county governments in Zhejiang were never actually transferred to prefecture-level municipality governments as a result of agreements between the Zhejiang provincial government and the central government. One main reason is that Zhejiang is a frontier of national defence. The central government was reluctant to make efforts on agglomeration of manufacturing factories in a few places in this province. They preferred a more dispersed economy. Another reason is the small size of the province. The provincial government considered it unnecessary to set the essential intermediate level between the province and counties (Wu, 2004). As a result, the central government and the provincial government agreed on the arrangement that the decision powers would not be taken from county-level governments in Zhejiang, while prefectures should still follow the steps of the nationwide reform to be transformed to prefecture-level municipalities. The county governments could maintain their powers over public finance and implementing economic policy in their own territories. The *de facto* independence of some counties has been confirmed by the province government since the start of the 1983 reform (The People's Government of Zhejiang Province, 1983). If the treatment effects estimated in the baseline model are driven by any unobservable factor correlated with the centralization reform, they would still appear in the placebo test. If no effects were found, it implies that it is centralization but not any other factors that drives my baseline results.

Third, I show that my results are robust to controlling for prefecture specific time trends and many pre-existing geographic and economic conditions. Including

prefecture-specific trends in regressions can control for differential linear trends in the growth rates by prefecture. Though pre-existing geographic and economic conditions are time invariant, controlling for interactions between them and year dummies and can address any concerns that initial conditions may determine developmental paths.

1.6 Overall Impacts on Prefectures

In this section I evaluate the overall impact on economic developments using aggregate and firm level data. To begin with, I confirm that centralization did occur at prefecture level after the 1983 reform with data on public finance, as the premise of my whole story. Then I use prefecture-level data on population and outputs to perform the baseline difference-in-differences regression, with various checks on its causal interpretation. I next show that the centralization reform is associated with geographic concentration in industrial production within prefectures. Finally, I quantify reductions in resource misallocation and resulted gains in productivity as an important channel through which centralization can be beneficial. In general, the results presented in this section reveal that the centralization reform caused increases in the non-agricultural population and the industrial output associated with a better resource allocation within prefectures, together with sizeable gains in productivity. The findings are in accord with Young (2000)'s observation that decentralization in China induces market fragmentation and divergence in prices and productivity, which are signs of distortions in production and resource allocation.

1.6.1 Evidence on Centralization

Before I present my baseline results, I first confirm that the reform since 1983 did bring more centralization at prefecture level. It is necessary to show that after the reform prefecture-level municipality governments manage more powers, funding and responsibilities than before. The data to test this are from the Public Finance Statistics Yearbooks as introduced in Section 1.4.2. I run regressions using the baseline model and outcomes measuring the size and powers of various governments, including the log value of government employment, the log value of government administrative expenditure, the share of fiscal revenue in the whole prefecture, and the share of fiscal expenditure in the whole prefecture, respectively on prefecture level and county level. We expect the 1983 reform to be associated with increases in the size and powers of prefecture level governments, at the expense of county level governments. In addition to those variables measuring centralization, I also check the effects on the transfer payments received from upper-level governments to check if the reform is associated

with more favouritism from the central and provincial governments. The results are listed in Table 1.2.

Looking across column (1) to column (4) in Panel A, when a prefecture government is transformed to a prefecture-level municipality government, it will, as expected, become a "bigger" government. The number of its government employees will increase by 19% and administrative expenditure will increase by 34.2%. Its responsibilities for fiscal revenue and expenditure also rise significantly. At the same time, the decreases in the size of county-level governments are small and not significant, which reflects the difficultly in laying off public-sector employees in China. The share of expenditure and revenue by prefecture-level municipality governments in the whole region also increases significantly by 4.9% and 2.3% respectively, while the corresponding shares by county governments decrease. These results support the centralization process and lay a solid basis of the whole story. In column (5), we find that the prefecture-level municipality governments do not receive more transfer payments than the prefecture governments. It helps to get rid of an alternative story that the treated regions become better just because they get more resources from upper-level governments. It increases our faith that the centralization reform raises the efficiency inside each boundary, and they are not simply getting more help from outside. With these results in hand, I am going to use the reform dummy as the proxy of centralization to analyze whether and how centralization can benefit prefectures' development.

1.6.2 Baseline Results and Identification

As shown above, the 1983 reform centralized the powers of counties into prefecture-level governments. According to the conceptual framework, centralization may increase industrial output and the numbers of employees. In this subsection, I employ the baseline difference-in-differences model (Equation 2.20) to show that this is indeed the case, using prefecture-level data on population and output. Next, I carry out several checks on the identification assumption of the baseline model to ensure that we can interpret the baseline results as causal effects.

The outcomes of interest here are the non-agricultural population.[5] Table 1.3 presents estimates of the baseline model 2.20. I also report changes in the urbanization rate (I define it as the ratio of non-agricultural population to total population) and

[5]The categories of agricultural and non-agricultural population are based on each citizen's registered status in the "Hukou", the system of household registration in China. One may be concerned whether a citizen's registered status as agricultural/non-agricultural actually represents his working in the agricultural/non-agricultural sector. To address the concern, I use records of industrial output and occupational status in China's population census as a robustness check. See Section 1.8.1 for details and results.

industrialization rate (I define it as the ratio of industrial output to total outputs).

The outcome variable in column (1) is the log value of the non-agricultural population. After a prefecture becomes a prefecture-level municipality, its increase in non-agricultural population is 3.9% more than those remaining as prefectures. Column (2) reports the effects on the urbanization rate, a positive effect at 1.0% and significantly different from zero at the 1 per cent level. It reveals that more agricultural population have became non-agricultural in prefecture-level municipalities. As regards to industrial production, the reform is associated with a 6.5% more increase in industrial output. This estimation is significant at the 5 percent level. It can be either due to increases in labour inputs as column (1) shows, or to improvements in efficiency. I will analyze it in Section 1.6.4. The industrialization rate will increase by 1.6% but is with large standard error, which can be a sign of large noise in output data.

These results are reasonable in the light of previous theoretical expectations. When a prefecture is turned into a prefecture-level municipality, its powers of decision are taken into the hands of the prefecture-level municipality government. The centralization of governance helps to improve industrial development within prefectures.

While the above results are in accord with predictions from both theory and intuition, they leave a significant concern unmet: the possible endogeneity of the treatment dummy. The above results can not be interpreted as causal effects if the identification assumption does not hold. For example, if prefectures were selected into the reform by their unobserved characteristics, such as growth potential, the difference-in-differences strategy could be failed. The strategy holds when different prefectures followed the same pre-reform trend. According to the quotation from Central Committee of the Party and the State Council (1983), the central government selected prefectures into the reform according to its capital county's non-agricultural population and output, but not to its overall growing trend. Therefore, such selection rule ensures the rightness of the identification assumption of the difference-in-differences strategy. To test the assumption formally, as introduce in Section 1.5.2, I will use three different ways to validate my identification assumption.

First, by estimating a flexible difference-in-differences model that allows coefficients to vary year by year, we can observe possible differential growth in the outcome variables and check the parallel trend assumption. Figure 1.7 plots these dynamic coefficients along the relative years to the reform event, as well as the associated 95% confidence intervals, estimated from Equation 1.14.

The coefficients plotted in Figure 1.7 are from the flexible difference-in-differences specification on non-agricultural population and industrial output in prefectures re-

spectively, which according to government papers, are most suspect for endogeneity during policy-making. There are no significant pre-existing differential trends in the growth of the non-agricultural population and industrial output: the coefficients before reform are all insignificant and close to zero. Thus, prefectures treated earlier or later in calendar years followed a similar trend in the non-agricultural population and industrial output before the reform. In other words, the absence of evidence on differential pre-trends suggests that the central or provincial level government did not choose the targets of the reform by the growth rate of the non-agricultural population or industrial output. Therefore, the positive effects in the baseline regressions, reported in Table 1.3, can be interpreted causally with confidence.

A placebo test can offer further confirmation of the causal interpretation of the baseline results. As introduced in Section 1.5.2, the reform in Zhejiang was only a *de jure* experiment due to agreements between the provincial government and the central government. The decision rights of the county governments in Zhejiang were never transferred to prefecture-level municipality governments. It provides me an opportunity to run a placebo test to see whether the results found in the baseline regressions reflect the effects through the centralization reform itself. If it was not the centralization that was responsible for baseline results, but instead some other unobservable characteristics or changes in the reform, we could also observe similar effects in the "placebo" reform in Zhejiang. Otherwise, such effects would not appear. For example, if those prefectures with better growth potential were selected into the reform earlier, the positive effects on urban and industrial developments would also appear in Zhejiang Province. To be specific, I estimate the baseline model 2.20 but use only the data in Zhejiang Province. The results are reported in Table 1.4.

From the results it can be found that the "placebo" reform in Zhejiang does not exert any significant effects on non-agricultural population, industrial output, urbanization rate or the industrialization rate. The magnitude and signs of the estimated coefficients are not similar as in the baseline regressions. This makes it unlikely that omitted variables associated with selection into treatments were responsible for the change in urban and industrial developments. It strongly supports the story of centralization as a way of explaining the effects found in previous sections and denies the possibility that prefectures are selected for different treatments on the basis of unobservable characteristics correlated with their outcome variables.

As summarized in Section 1.5.2, besides the checks on the parallel trend and the placebo test, I check the sensitivity of the results to the inclusion of prefecture-specific linear trends and pre-existing economic and geographic characteristics. The inclusion of prefecture-specific time trends can control for the prefectures' linear trends in the

growth rates of the outcomes of interest. The inclusion of interactions between year dummies and initial conditions address the concern that differentiated pre-existing characteristics may have persistent and dynamic impacts on a prefecture's development afterwards. For brevity, the results of these two checks are shown in Appendix 1.8.2 and 1.8.4. None of the coefficients are sensitive to these robustness checks. These increase the faith in the causal interpretation of the baseline results.

The combination of results on the parallel trend, the placebo test and robustness address the concerns about the endogeneity of the treatment variable. After these checks on the identification assumption, it is credible that the treatment effects estimated in the baseline regressions reflect a causal relationship between the centralization reform and urban and industrial development, and not a mere correlation.

1.6.3 Concentration

Young (2000) documents a fact that provinces in China converge to produce similar goods when they got powers decentralized from the central government. Poncet (2003) shows that local protectionism under decentralization slowed down industrial concentration and agglomeration after 1980. A natural implication of centralization is that with a better coordination, a specific industrial sector may become more concentrated geographically. We can expect that after the prefecture-level municipality government took over the power of decision from the county governments local protectionism would be mitigated and it is not necessary for every county to own every sector. Hence, a specific sector will be concentrated in fewer places. To examine the concentration of industrial production, I make use of the γ index developed by Ellison and Glaeser (1997) to measure the geographic concentration of a specific sector within a prefecture. In their paper, the authors construct a model-based index of the geographic concentration of economic activities, the γ index. Lu and Tao (2009) calculate the γ index in a nationwide study and document the general trend of concentration in China. Here I revise the γ index to fit into my county-prefecture scenario as follows:

$$\gamma_{sj} = \frac{G_{sj} - (1 - \sum_{i \in j} x_i^2) H_{sj}}{(1 - \sum_{i \in j} x_i^2)(1 - H_{sj})} \tag{1.15}$$

where γ_{sj} is the Ellison-Glaeser index calculated for each sector-prefecture pair sj; $G_{sj} = \sum_{i \in j}(x_i - s_{si})^2$ is the spatial Gini coefficient, where i is any county belonging to a prefecture j, x_i is i's share of total employment or output of all industries in j, s_{si} is sector s's share of employment or output for region r in county i; H_{sj} is the Herfindahl index of sector s. The greater the EllisonGlaeser index, the higher the geographic concentration. It equals zero if all the firms randomly pick their location. In this part,

I calculate the Ellison-Glaeser index γ_{sjt} for every combination of year, sector and prefecture or prefecture-level municipality, and regress them on the treatment variable $Treat_{jt-1}$ as in the baseline model:

$$\gamma_{sjt} = \beta Treat_{jt-1} + \alpha_j + \delta_{pt} + \theta_{st} + \epsilon_{jst} \tag{1.16}$$

where γ_{sjt} is the Ellison-Glaeser index in prefecture j, sector s and year t; α_j is prefecture fix effects; δ_{pt} is year×province fixed effects; θ_{st} is sector×province fixed effects. Results are given in Table 1.5

The first column shows the result of the Ellison-Glaeser Index measured in outputs and the second shows the result measured in employment. From the results we find that after the centralization reform, the extent of industrial concentration increased significantly. It suggests that when a prefecture is centralized, goods in a given sector tend to be produced in fewer counties than in the past. The findings in this section endorse another well-known observation in Young (2000): decentralization in China induced convergence in the composition of outputs.

1.6.4 Misallocation and Aggregate Productivity

The baseline results provide empirical evidence on the main predictions from the conceptual framework. The centralization reform increases industrial output and population of prefectures as a whole. However, the increases in industrial output can be a result of more inputs. The fact that industrial production becomes more concentrated than dispersed across counties suggests possible gains in aggregate productivity by allocating resource for more efficiency and specification. In Table 1.6, I show how this centralization reform improves productivity in the sense of the aggregate level. I use per capita output as a rough measurement of productivity. Table 1.6 begins by estimating the baseline equations with per capita industrial output and per capita total output as outcome variables. I find that the per capita industrial output increases by 10.5% and the per capita total output increases by 7.2% if a prefecture is turned into a prefecture-level municipality. It also provides the effects of the reform on total population and total output as an addition to the main results in Table 1.3.

The results imply that the increases in industrial output should be associated with increases in efficiency. In the conceptual framework, I argue that centralization can bring better coordination and allocation of resource, therefore aggregate productivity will be improved. The reduced misallocation and rises in aggregate productivity are the main channel how centralization will benefit in this paper. Table 1.3 depicts a rough picture about that. In this subsection, I will provide more precise evidence

using TFP derived from firm-level data to identify the existence of this mechanism.

The problem of resource misallocation in developing countries has attracted growing attention in recent years. Hsieh and Klenow (2009) provides a methodology for evaluating the contribution of misallocation in explaining low aggregate output per worker and total factor productivity (TFP) in developing countries. In the paper, the authors document the dispersion of "revenue productivity" (TFPR) as a proxy of resource misallocation, and then measure how much aggregate manufacturing output in China and India could benefit if the marginal products of labour and capital were equalized to the extent observed in the United States. They also show briefly that the extent of misallocation can be affected by varied policy distortions such as licensing and size restrictions. More recent literature (Bartelsman et al., 2013; Midrigan and Xu, 2014) provides more evidence on the role of misallocation in explaining the TFP gap in a similar framework.

However, when calculating how misallocation can account for the TFP gap, the previous literature has placed less emphasize on its sources and possible remedies, although they all agree that institutions and policies may be responsible for distortions and misallocation (see a survey by Hopenhayn (2014)). In this part, I investigate whether the problem of misallocation is mitigated by more centralized institutions. Intuitively, the decentralized institutions before the reform would seem to encourage county governments to subsidize and protect local enterprises. This would have resulted in distortions in efficient resource allocation within a prefecture. Young (2000) attributed the divergences in price and productivity, a sign of resource misallocation, to the excessive decentralization to local governments in China. If it is the case that decentralization is responsible for misallocation, as my conceptual framework and previous work predicts, we should expect that after a centralizing reform, the prefecture-level municipality government reduces misallocation by reallocating resources and then promoting the efficiency in the whole region. To make a more realistic analysis, I drop the assumption in the conceptual framework that each county has one representative firm and allow for heterogeneous firms in the empirical part.

To measure the misallocation, I borrow the methodology of Hsieh and Klenow (2009) to calculate dispersions of "revenue productivity" (TFPR) as a measurement. To make my calculation close to reality, first I extend my simplified version in the conceptual framework to a multi-input and multi-sector model. I assume each firm u in sector s has a diminishing returns production function:

$$Y_{us} = A_{us}(K_{us}^{\alpha_s} L_{us}^{1-\alpha_s})^{\eta}, \tag{1.17}$$

where A_{us} is the total factor productivity of the firm u, α_s is the share of capital in

sector s and η is the degree of decreasing returns. The final product is a Cobb-Douglas aggregate of products in different sectors. Next I define TFPR of each firm u in sector s as Hsieh and Klenow (2009):

$$TFPR_{us} = \frac{P_s Y_{us}}{K_{us}^{\alpha_s} L_{us}^{1-\alpha_s}} \tag{1.18}$$

The essence of TFPR can be regarded as a measurement of marginal products or distortions faced by firms:

$$TFPR_{us} \propto MRPK_{us}^{\alpha_s} MRPL_{us}^{1-\alpha_s} \propto (1+\tau_{us})^{-1} \tag{1.19}$$

So a high firm TFPR implies that this firm faces a relatively low subsidy or barriers that raise its marginal products, suggesting the firm is smaller than its optimum size. To see how the dispersion of TFPR can proxy for resource misallocation and be responsible for a loss in aggregate TFP, I write the aggregate TFP in prefecture j and sector s as within the monopolistic competition framework, as in my theoretical part:

$$TFP_{js} = [\sum_{u} (A_{us} \frac{\overline{TFPR_{js}}}{TFPR_{ujs}})^{\frac{1}{1-\eta}}]^{1-\eta} / (K^{\alpha_s} L^{1-\alpha_s})^{\eta} \tag{1.20}$$

Following from Jensen's inequality, if and only if marginal products are equalized across firms, or if there is no dispersion in distortion rate or TFPR, aggregate TFP can reach its optimum. That is the same condition under which industrial output is optimized in my conceptual framework.

In practice, I calculate TFPR following Hsieh and Klenow (2009), which also use China's Annual Survey of Industrial Production. The capital share α_s in each sector s is set to be the same in the corresponding industry in the United States, which is taken from the NBER Productivity Database. I trim the 1% tails of distribution of TFPR across industries. The setting of the degree of decreasing returns η needs further consideration. As introduced in the conceptual framework, unlike this paper, Hsieh and Klenow (2009) assumes the diminishing returns occur in demand side with the monopolistic competition model. As a result, in their paper the parameter playing the role of η is the elasticity of substitution σ. These two model are isomorphic and $\frac{1}{1-\eta} = \sigma - 1$. Thus their choice of $\sigma = 3$ implies $\eta = 0.5$. Actually 0.5 is quite a low value for diminishing returns. Hopenhayn (2014) suggests a more common choice of $\eta = 0.85$. I will follow this choice here, but turn back to Hsieh and Klenow (2009)'s monopolistic competition model and set $\sigma = 3$ in Section 1.8.7 as a robustness check.

According to the conceptual framework, under centralization, the prefecture-level

municipality government can reallocate funds across counties and help to reduce the extent of dispersion of distortion rates. From Equation 1.19, we can see when the distortion rates τ_{us} faced by different firms become less dispersed, dispersions of TFPR will also be reduced. Therefore the aggregate TFP within a prefecture will improve according to Equation 1.20.

To illustrate that the centralization can reduce misallocation, I plots distributions of TFPR in Ningde Prefecture before and after the centralization reform in Figure A.1, as an example. We can see that after the centralization reform, the distribution of TFPR in Ningde is clearly less dispersed.

To give a formal analysis, I propose three different methods to show how misallocation and aggregate TFP is associated with the centrlized reform.

To begin with, I use my baseline regression model to check any potential changes in misallocation associated with the centralization reform:

$$Misallocate_{jt} = \beta Treat_{jt-1} + \alpha_j + \delta_{pt} + \epsilon_{jst} \tag{1.21}$$

where $Misallocate_{jst}$ is the dispersion in TFPR within prefecture j and year t; α_j is prefecture fixed effects; δ_{pt} is year×province fixed effects. Here I use standard deviations, ratios of the 75th to 25th percentiles and ratios of the 90th to 10th percentiles in TFPR within any prefecture as measurements of the dispersion in TFPR, as Hsieh and Klenow (2009) do. All of these outcome variables are standardized. Regression results are in Table 1.7.

From the above results we find that all three measurements of the dispersion of TFPR are significantly reduced after the reform. The results suggest that the centralization reform can mitigate resource misallocation within a prefecture, as the theoretical framework suggests.

I repeat the same practice on a county level regression by aggregating dispersions of TFPR within each county instead of prefecture. From both the theoretical model and intuitions, the reform in the prefecture level can improve the allocation across counties but not within counties. So coefficients of treatment variable in county-level regressions are not expected to be significantly negative, as shown in Table 1.8.

Looking across column (1) to column (3), we cannot see any significantly negative effects on the dispersion of TFPR within counties. No stable and robust relationship can be found between the centralization reform and misallocation in the within-county analysis. This regression can be regarded as a "placebo" as opposed to the actual effect on misallocation across counties. Such results tend to confirm that the centralization reform did improve allocation efficiency but that this was not due to other spurious correlations to reduce misallocation measurements.

Second, I conduct a counterfactual analysis similar to Hsieh and Klenow (2009). With Equation 1.20 in mind, I can calculate the efficient aggregate TFP. In that case, marginal products are equalized across firms in a given sector within a prefecture:

$$TFP_{js,EFF} = [\sum_{u}(A_{us})^{\frac{1}{1-\eta}}]^{1-\eta}/(K^{\alpha_s}L^{1-\alpha_s})^{\eta} \tag{1.22}$$

Then I take the ratio of actual TFP to this efficient level of TFP to measure the counterfactual gains from reducing misallocation:

$$\frac{TFP_{js}}{TFP_{js,EFF}} = [\sum_{u}(\frac{A_{us}}{TFP_{js,EFF}}\frac{\overline{TFPR_{js}}}{TFPR_{ujs}})^{\frac{1}{1-\eta}}]^{1-\eta}, \tag{1.23}$$

and aggregate it using a Cobb-Douglas aggregator across sectors within the prefecture:

$$\frac{TFP_{j}}{TFP_{j,EFF}} = \prod_{s}\sum_{u}(\frac{A_{us}}{TFP_{js,EFF}}\frac{\overline{TFPR_{js}}}{TFPR_{ujs}})^{\frac{1}{1-\eta}}]^{\theta_s(1-\eta)}, \tag{1.24}$$

The percent gains in the prefecture are defined as

$$Gains_j = 100(\frac{TFP_{j,EFF}}{TFP_j} - 1) \tag{1.25}$$

Equation 1.25 offers a way to calculate the counterfactual gains when dispersions of TFPR were reduced. Table 1.9 provides the results of two counterfactual practices. Panel A of Table 1.9 assumes that marginal products are equalized across all firms within each prefecture and sector. By this calculation, aggregate TFP in prefectures before they were treated can be raised by 90.6% in 1998 and 97.1% in 2000 without misallocation within each prefecture. This practice is not quite realistic, as it is hard to believe that the centralization reform can get rid of all misallocation within a prefecture. For example, the results in Table 1.8 show that the reform can do nothing about the misallocation within counties. To calculate a more realistic counterfactual gain, I take a conservative stance to hypothetically suppose the dispersions in the decentralized prefectures equal to the dispersion level in centralized prefecture-level municipalities, instead of the fully efficient level as above. These results are listed in Panel B of Table 1.9. Such counterfactual allocation will lead to a gain of 16.1% in TFP in 1998 and 18.1% in 2000.

Collectively, all results in this subsection paint a consistent picture. The centralization reform can reduce resource misallocation due to better coordination and reallocation. This improvement has a sizeable effect on aggregate productivity.

1.7 Distributional Effects on Counties

In the last section, I demonstrate that the centralization reform since 1983 was associated with increases in non-agricultural population and industrial output. Meanwhile, resource misallocation within prefectures was mitigated and aggregate productivity improved, implying better coordination under centralization. Besides these overall effects on prefectures as a whole, we can expect there to be distributional effects on different types of county. From the conceptual framework, when centralized prefecture-level municipality governments reallocate funds, some counties get more than before while others get less. In this section, I explore in turn the heterogeneous responses of different counties to the reform. To avoid endogeneity, I start by examining different response between two types of counties, capital and peripheral. A county's identity as capital or peripheral is determined in history and hardly changed in the last few decades. I will first show capital counties receive more benefits in the centralization reform. Then I will confirm that such distributional effects between capital and peripheral counties are driven by productivity advantages in capital counties, not by preference or favouritism. The organization of this section is similar to that of the last: to begin with, I use data on public finance to show that capital counties actually get more resources from upper-level governments. I then estimate the baseline model on a county level to show that capital counties benefit more than peripheral counties using aggregate data on population and outputs, and justify the corresponding identification assumption for the causal interpretation. Lastly, I make use of the richness of the firm level dataset to reveal that firms in capital counties growing faster is a result from their advantages in productivity.

1.7.1 Transfer Payments and Expenditures in Counties

As in Section 1.6.1, in this subsection I explore the heterogeneous response in government behaviours in capital and peripheral counties. In the first column of Table 1.10, I report the results for the percentage of transfer payments in a county's total revenues, as a measurement of dependency of county's revenues on transfer payments from upper level governments. The results suggest that after the centralization reform, both capital and peripheral counties have to rely more on upper level governments' help on revenue. In the last three columns, I report results on a county's shares of transfer payments, tax revenues and expenditure in the total amounts of those in prefectures. We can observe that after the centralization reform, peripheral counties, unlike capital counties, share much lower transfer payments than before. Similar patterns appear in total expenditure shares. Regarding tax revenue, both capital and peripheral counties'

share remain unchanged after the reform. Those three results imply that after the centralization reform, although shares of counties' tax revenue are similar as before, the transfer payments they get from upper level governments diverge. Capital counties get much more than peripheral counties, which enable them to shave more expenditures. This picture is in line with the conceptual framework that after centralization, prefecture-level municipalities can manage funds from counties together and reallocate them from peripheral counties to capital counties.

1.7.2 Baseline Results at County Level and Identification

I start by estimating the baseline model at county level. Here, to explore the different responses to the reform from capital and peripheral counties, I add an interaction between the treatment variable and a dummy variable $Capital_i$ taking the value of one when i is a capital county (location of a prefecture-level government). To be specific, the baseline specification on county level is:

$$Y_{ijpt} = \beta Treat_{jpt-1} + \gamma Treat_{jpt-1} \times Capital_i + \phi w_{ijpt} + \alpha_i + \delta_{pt} + \delta_{pt} \times Capital_i + \epsilon_{ijpt} \tag{1.26}$$

where Y_{ijpt} is an outcome variable in county i, prefecture j, province p and year t; $Treat_{jpt-1}$ is the main independent variable, indicating whether prefecture j in province p received treatment by year $t-1$; $Capital_i$ is a dummy variable indicating whether this county i is a capital county or not; α_i is a county fixed effect; δ_{pt} is a year×province fixed effect; w_{ijpt} are control variables.

The baseline regression results are presented in Table 1.11. The outcome variables are exactly the same as in the prefecture-level regressions (non-agricultural population, urbanization rate, industrial output and industrialization rate)

The first two columns report the effects of the reform on the outcomes related to urbanization. Column (1) reports the effects of the reform on the non-agricultural population. It suggests that when a prefecture is converted to a prefecture-level municipality, its capital county will experience a 14.6% increase in non-agricultural population relative to those capital counties remaining in prefectures. However, the effect on peripheral counties is negative but not significant at 0.5%. Columns (2) reports the effects on the urbanization rate. The positive effect on the urbanization rate is similar. For capital counties the urbanization rate is 3.2% more in prefecture-level municipalities than in prefectures. For peripheral counties, the impact are small and not significant.

Turning now to industrial production, Columns (3) and (4) show the results for the log value of industrial outputs and industrialization rate. In column (3), we can

see that the reform increases industrial outputs in capital counties by 17.0% while it decreases in peripheral counties by 6.9%. Both effects are significantly different from zero at the 5 per cent level. The industrialization rate does not appear to be correlated with the reform either in the capital or the peripheral counties.

Furthermore, besides the four outcome variables as in the prefecture-level regressions, I construct two more variables on county development to reflect a possible heterogeneous response in different counties: capital counties' share of non-agricultural population in prefecture and capital counties' share of industrial output in prefecture. We can find from Table 1.12 that a capital county shares a larger portion of urban population and indstrial output after the reform. It suggests that the reform produce distributional effects biased in favour of capital counties.

These findings on capital and peripheral counties are in accord with my conceptual framework. The capital county absorbs more resources after the centralization and grows faster since it always accommodates firms with high productivity, as I will show below. At the same time, the peripheral counties may be negatively affected by the outflow of resources, so we observe slightly negative effects on urbanization and industrialization in peripheral counties. Combining these results with those for prefectures as a whole, it can be seen that the magnitude of the coefficients in Table 1.3 is smaller than the estimated positive effects for capital counties. It is another sign that the distributional effects in different counties may offset each other but as a whole the prefectures experience a growth in urbanization and industrialization after the reform.

Since the reform is conducted in prefectures, the county level regressions will not suffer a serious problem of endogeneity. To interpret the gap between the benefits from the centralization in the capital and peripheral counties as a causal effect, the identification assumption to be met is that, in the absence of the reform, two types of county would have evolved in parallel. Following the strategy described earlier, I continue to apply the flexible difference-in-differences, the placebo test and some robustness checks to ensure that the identification assumption holds.

I start by estimating the flexible difference-in-differences model on the non-agricultural population and industrial output in county-level regressions. There are two ways to check the parallel trend assumption. One way is to check that the flexible coefficients of the interaction between the treatment and the capital county dummy are not different from zero in years prior to the reform, as I did in the prefecture level regression. Figure 1.8 plots the coefficients and the confidential intervals estimated from the flexible difference-in-differences regressions on the non-agricultural population and industrial output. We can confirm that the gaps between capital and peripheral counties in the

pre-reform years are all insignificant and close to zero; there is no obvious trend before the reform. Thus, the coefficients of the interaction in the baseline county-level regressions (Table 1.11) can be interpreted as causal effects. The second way to think about this is to individually consider the trends of the capital and peripheral counties. If their pre-existing trends are parallel, then the identification assumption holds. To do this, I plot the two trends in Figure A.2. We can observe that the outcomes in capital counties diverge significantly from their pre-existing trends and that, in contrast, the trends in the peripheral counties are almost flat. These two graphs largely increase our confidence in the identification assumption and validate the causal interpretation of the distributional effects.

As with the prefecture-level regressions, I use here the example of Zhejiang Province to perform a placebo test. As introduced previously, the reform in Zhejiang never actually centralized the decision powers of counties to prefecture-level municipality governments. So the distributional effects across different counties should not be expected to appear in the Zhejiang sample. Following this logic exactly as in the prefecture-level placebo test, Table 1.13 presents the county-level results.

As expected, the "placebo" reform in Zhejiang did not produce any significant distributional effects on counties, on possible outcomes considered in the baseline model. The magnitude and signs of the coefficients with regard to different counties are not similar, as in the baseline county-level regressions. This greatly supports the causal interpretation of the distributional effects found above.

Similarly, I check the robustness of the county-level results to include county-specific trends and counties' pre-existing conditions, besides the checks on the parallel trend and the placebo test. They control for counties' linear trends in the growth rates of outcomes of interest and dynamic impacts of differentiated pre-existing characteristics in counties. The results of these two robustness checks are also to be found in the Appendix 1.8.4 and 1.8.2. Looking across these tables, we find that the coefficients are not sensitive to the inclusion of county-specific trends and pre-existing characteristics. This is enough to validate the causal interpretation of the baseline county-level results.

The same practice in the parallel trend, the placebo test and the robustness as was followed in the prefecture-level regressions, which confirms the validity of the identification assumption. We can be quite sure that the distributional effects reflect a causal relationship between the increasing gap in urban and industrial development between the capital and peripheral counties and the centralization reform.

1.7.3 Pattern of the Distributional Effects

Although we have already observed that industrial sectors become more concentrated within a prefecture using the Ellison-Glaeser Index as a measurement, it is still not clear what the concentration patterns look like, or more specifically, what kinds of county a sector will be concentrated in. If the centralization reallocates resources to more productive counties, then firms will agglomerate in places with an advantage in productivity. More formally, I next test whether firms in a high average sector-specific TFP will produce more under centralization, relative to the decentralization case. I run a firm-level regression:

$$Y_{uijst} = \beta_1 Treat_{jt-1} + \beta_2 Treat_{jt-1} \times TFP_{is} + \eta_u + \delta_{pt} + \theta_{st} + \epsilon_{uijst} \tag{1.27}$$

where Y_{uijst} is log output of firm u in sector s, county i, prefecture j and year t; TFP_{is} is the average TFP in sector s and county i. TFP_{is} is standardized by subtracting the mean and dividing by the standard deviation. To avoid endogeneity, I use the average TFP in the first available year (1998). If a sector agglomerates in a relatively high-productive place, we should expect the coefficient of the interaction between the treatment variable and the average county-sector TFP to be positive. The results are in Table 1.14.

In Column (1) of Table 1.14, we see that firms in counties with higher sector-specific productivity will produce more after the reform. This suggests that the centralization reform will help to reshuffle resources into firms located in places with advantages in productivity. Column (2) shows that such effects do not differ between capital and peripheral counties. The interaction between the treatment dummy, the capital county dummy and the average sector TFP is not significantly different from zero. At the same time, other coefficients and the R-square of the regression almost remain the same. This suggests that when sector productivity is given, firms in capital counties will not gain more than those in peripheral counties. Therefore, the distributional effects from the aggregate data that capital counties benefit more than peripheral counties from the centralization shown in Table 1.11 may be derived from the fact that capital counties own more productive firms and sectors, but not from any special favouritism that allocates too many resources to capital counties. Concerning the magnitudes of the estimated coefficients, results in Column (2) suggest that one standard deviation increase in a county's average TFP will bring 19% more output for its firms after the reform. At the same time, a status of capital counties can give their firms a 13.2% increase in output after the reform, which is not a significant estimation. A case of Ningde Prefecture, which has been introduced in Section 1.6.4, can interpret the

relative size of those two coefficients. Firms in the capital county of Ningde Prefecture on average owned a productivity nearly a half standard deviation higher than firms in peripheral counties. The results in Column (2) predict that the reform can bring a 9.5% increase in output for firms in the capital county of Ningde due to productivity advantage and a 13.2% increase due to the political status as a capital county. Though the size of the latter effect is not ignorable, it is not significantly different from zero and the relative magnitude can not dominate the productivity effect.

Figure 1.9 presents the TFP distribution of firms in peripheral counties relative to firms in capital counties. It is clear that the productivity of the peripheral county firms is much lower than that of capital county firms. The results here suggest that the centralization reform helped to reallocate the resources across counties more efficiently according to the counties' productivity advantages. Capital counties gain more because they have more competent firms. However, the alternative explanation that capital counties benefit from preference or favouritism can be eliminated.

Column (3) to Column (6) repeat the practice separating state-own firms and private-own firms. We can clearly that changes in state-own firms mainly drive previous results, while private-own firms receive no effects. It consistent with the story that government planning on allocating resources play the main role in the centralization reform. It helps to rule out a major alternative explanation that agglomeration is the channel through with centralization benefits. If agglomeration were the main mechanism, both state-own firms and private-own firms should equally get benefits from the reform.

1.8 Robustness

In this subsection, I am going to run various robustness checks towards the baseline model.

1.8.1 Evidence from Census Data

The data that I use in baseline regressions are reported in the National Bureau of Statistics in provincial statistics yearbooks. The population-related variables are based on the Household Registration (*"hukou"*) system. There is some concern about whether the use of such variables measures the economic activity of a resident. It is possible that an agricultural resident and a non-agricultural resident differ only in their literal *hukou* status. Then the increases in non-agricultural population after the reform that we find may be due to changes in the definition of *hukou* and do not reflect any improvements in the urbanization process. To address this concern, I use the

population census data containing information on agricultural and non-agricultural employment in order to carry out a similar analysis to the baseline regressions.

There are three waves in the population census data that are suitable for my analysis: 1982, 1990 and 2000. The aggregate data from the population census reports population employed in different sectors aggregated to county level. It allows me to examine the changes in employment in agricultural and non-agricultural sectors, which is a more precise measurement of population engaged in rural and urban economic activities. The results are presented in Table A.1.

Table A.1 reveals all the evidence involving population variables in the baseline regressions. Both the overall effects on prefectures as a whole and the distributional effects on the component counties can be found even using census data. This suggests that the limitations of using population data based on the "Hukou" system are not of major concern in this research.

1.8.2 Initial Conditions

Pre-existing economic and geographic conditions are believed to have a huge influence on developments. Although in my baseline regressions all the time-invariant impacts brought by heterogeneous geographic conditions are absorbed by the individual fixed effects, it is still possible that they relate to regional developments at different times. Specifically, they may affect the timing of the reform. From Figure 1.6, some obvious patterns of the reform timings are noted. For example, it is easy to see that the coastal areas, which were typically developed before the reform, received the treatment earlier than inland areas. So it is necessary to check the robustness when we control for the impact of initial conditions and make them flexible for different years. Empirically, I include the interactions between economic or geographic conditions and year dummies in the baseline model. For proxies of geographic conditions, I calculate a prefecture or a county's distance from the nearest river and coast line and the corresponding provincial capital city. To take account of the initial economic conditions, I include total population, the urbanization rate and the per capita output from the 1982 census. These variables are exogenous measurements of many important aspects such as initial development, transportation and political connections. Table A.2 lists the prefecture-level regression results and Table A.3 lists the county-level regression results.

We can see that the results remain almost the same as in the baseline regressions. Furthermore, the estimated coefficients of initial conditions are insignificant in most years; for simplicity they are not reported in the table. The above results suggest that the possible caveats accompanying the initial conditions are unnecessary and this helps to strengthen the causal arguments from my results.

The results can also address concerns by Cai and Treisman (2005). They propose a theoretical model to show that units with different initial endowments yields different performance under decentralization. In the setting of this paper, if initial endowments in counties determined their performance and then the entering to centralization, the baseline results would be biased. However, the robust results in this part address this concern.

1.8.3 Using Other Lags of the Main Independent Variable

Another robustness check is related to the definition of our independent variable of interest. In the baseline regression, I use the one-period lagged treatment status $Treat_{jpt-1}$ as the main independent variable. Here I replace it with the treatment status in the current period $Treat_{jpt}$ to check robustness. Since reforms often took effect in the middle of a year, I use the proportion of treated months as the value of $Treat_{jpt}$ if it is the year when the reform took place in county i and prefecture j. For example, if county i in prefecture j was treated in September of year T, then $Treat_{jpT} = 0.75$. The results are shown in Table A.4 and Table A.5.

Almost all the estimated coefficients are similar in magnitude, while some of the significances drop due to an increase standard errors. Furthermore, the results will not change much by choosing other lags. For conciseness, I do not list the results.

1.8.4 Individual-specific Time Trends

In this part, I add individual-specific time trends to the baseline regression. The individual-specific time trends in a fixed-effect model will allow treatment and control groups to follow different trends. If the estimated coefficients do not change much, it will greatly support the identification strategy of the baseline model. The results are presented in Table A.6 and Table A.7. The magnitudes of the coefficients are quite similar while some significances are gone, which may be partly due to the reduction in the degree of freedom. The fact that all coefficients in these regressions lie in the confidence intervals in the baseline estimations is encouraging for the validity of the baseline model.

1.8.5 Possible Heterogeneity Across Different Periods

There are some reasons to believe that the effect of the centralization reform would be heterogeneous in different periods. I divide the examined time period into two: from 1983-1993 and from 1994-2003, and run the baseline regression based on each in turn. One of the main reasons to do this is that in 1994, China initiated a significant fiscal

centralization reform from local governments to the central government. The results can be found in Table A.8 and Table A.9. Although there is no evidence that the reform interacted with the political hierarchy reform between different levels of local governments discussed in the present paper, it is possible that the effect that I found became smaller after 1993 due to the limitations on fiscal capacity in local governments. Meanwhile, changes in population mobility over time may also be responsible for the possible heterogeneity.

1.8.6 Using a Small Sample of the Firm-level Dataset

There is a sampling concern about the empirical results using firm-level data. The observations in those empirical analyses contain firms in prefectures that were not centralized until 1983, which are the same prefectures in baseline regressions using aggregate data; however, the firm dataset became available in 1998. It could be a concern that regressions using the firm-level dataset should adopt the same sampling principle as before. Here, I conduct robustness checks only using prefectures centralized from the first available year (1998). The prefectures in these robustness checks are about one third of the previous. Table A.10 repeats the estimation on effects on dispersions of TFPR within prefectures only using this small sample. We find that the magnitude of coefficients are similar to the full sample, but the standard errors increase; nevertheless, the effects are on the margin of significance. This implies that the only change when using the small sample is a reduction in the preciseness of the estimates as the sample size shrinks. Table A.11 lists the results on dispersions of TFPR within counties. The magnitude is also similar as in the full sample while the standard errors increase. For brevity I do not report results of other checks on regressions using firm-level data. In general, using the small sample will not produce different estimates on coefficients.

1.8.7 Model and Counterfactual Analysis with Constant Returns in Production

As introduced in the conceptual framework, some papers use models with constant returns in production and a monopolistic competition market. Hsieh and Klenow (2009) is the most well-known among them. They assume diminishing returns are in the demand side where monopolistic competition (Dixit and Stiglitz, 1977) is used; these two flavours of modelling are equivalent, indeed, Hopenhayn (2014) and Hsieh and Klenow (2009) show the two models are equivalent. When guiding the counterfactual analysis, they are theoretically isomorphic but with a subtle difference in parameter settings.

Here I briefly show a model with diminishing returns in production and calculate the counterfactual gains with a new choice of parameter.

I assume that each county produces a differentiated good and that the total output Y in the prefecture is a CES aggregate of all the differentiated goods in the component counties:

$$Y = (\sum_{i=1}^{N} Y_i^{\frac{\sigma-1}{\sigma}})^{\frac{\sigma}{\sigma-1}}, \tag{1.28}$$

where Y_i is the industrial output in county i; $\sigma > 1$ is the elasticity of substitution between goods produced in different counties. The production function for county i is given by a Cobb-Douglas function of its representative firm:

$$Y_i = A_i K_i^{\alpha} L_i^{1-\alpha}, \tag{1.29}$$

where A_i, K_i and L_i are total factor productivity, capital and labour in each county i. To promote industrial growth, county government i subsidizes τ_i on output. The revenues of the representative firm are $(1+\tau_i)P_iY_i$, where P_i is derived in Dixit and Stiglitz (1977) to be a fixed markup over the marginal cost from profit maximization of firms in the monopolistic competition market:

$$P_i = \frac{\sigma}{\sigma-1}(\frac{R}{\alpha})^{\alpha}(\frac{w}{1-\alpha})^{1-\alpha}\frac{1}{A_i(1+\tau_i)}. \tag{1.30}$$

With the same definition of TFPR, the aggregate productivity in prefecture j can be written as:

$$TFP_{js} = [\sum_{u}(A_{us}\frac{\overline{TFPR_{js}}}{TFPR_{ujs}})^{\sigma-1}]^{\frac{1}{\sigma-1}} \tag{1.31}$$

We can see the similarity of the above expression to Equation 1.20, the aggregate productivity under constant returns to scale as in my conceptual framework. Here, the elasticity of substitution σ plays the same role as the degree of diminishing returns in production θ as previous. Thus the two models are isomorphic.

The corresponding counterfactual analysis now requires an arbitrary value of σ instead of θ. In Hsieh and Klenow (2009), $\sigma = 3$, implying $\theta = 0.5$. It is lower than the usual value in the literature. I follow their setting. The counterfactual gains in aggregate TFP under $\sigma = 3$ can be found in Table A.12. By this calculation, aggregate TFP in prefectures, before they were treated, can be raised by 45.1% in 1998 and 49.1% in 2000 without misallocation within each prefecture. Results of the more conservative practice to hypothetically move the dispersions in the decentralized prefectures to the centralized level are listed in Panel B of Table A.12. Such counterfactual allocation will lead to a gain of 13.6% in TFP in 1998 and 19.5% in 2000.

1.9 Conclusion

Decentralization has been one of the most popular policy experiments in the recent decades. Particularly in developing countries trying to promote government accountability, policy makers draw on decentralization in various designs. However, there is not a concensus among economists on whether decentralization should be promoted unconditionally. Both classical (Oates, 1972) and the most recent papers (Boffa et al., 2015) emphasize that the theoretical predictions on the trade-off between centralization and decentralization are ambiguous. The empirical evidence on this is also mixed (Bardhan, 2002). For example, literature on fiscal federalism find that decentralization from central to provincial governments can provide incentives to protect markets and such findings have been fruitfully applied to Latin America and transition economies (Qian and Weingast, 1997). Yet, problems such as mismanagement and corruption in local governments are found meanwhile. In the context of China, Young (2000) criticizes decentralization as it induces local protectionism and a distortion of production. Consistent with Young (2000), this paper finds that decentralization distorts production and induces misallocation; centralization at a regional level can in turn reallocate the resource better and increase efficiency.

In this paper, I investigate a political hierarchy reform that changed the relations between sub-provincial governments in China. In this reform starting from 1983, prefectures in China were reformed into prefecture-level municipalities; this made the component counties less autonomous and decision powers at county level were centralized to prefecture level. After the reform, the prefecture-level municipality governments were responsible for economic policies and developments in their component counties. Using a difference-in-differences model, I show that this centralization reform had positive causal effects on non-agricultural population and industrial output within prefectures. It was also associated with gains in productivity through reallocating resource and equalizing the marginal products of inputs across counties. Drawing on the misallocation methodology developed by Hsieh and Klenow (2009), I quantify such gains with a comprehensive firm-level dataset. Except for the positive overall effect on the whole prefectures, the centralization also produced distributional effects on component counties. Counties which are capitals of prefectures got more resources from reallocation by municipality governments, compared with other peripheral counties. I show this distributional effect was mainly due to productivity advantages, and not to favouritism. Patterns in fiscal behaviours of local governments confirm the existence of centralization and the reallocation of resources. These results are in consistent with a heterogeneous firm model emphasizing that centralization helps to mitigate misallocation and improve aggregate productivity.

My findings have important policy implications. From the ambiguous theoretical predictions and mixed empirical evidence, it is accepted that the effects of decentralization differ case by case and through specific contexts, and designs matter a great deal (World Bank, 2000). This paper raises the potential value of centralization when we design policies in developing countries with a federal structure such as China. In a context where local governments have the power and incentives to distort, this paper shows the inefficiencies that may arise from decentralization. It is therefore in line with Young (2000)'s observations. I show that in this setting the centralization to a regional level mitigates misallocation problems with better coordination. In classical works, the most popular reason for preferring decentralization is that it ensures better accountability from local governments since they compete with each other, while centralization may be problematic if central governments act as monopolists and are lack of accountability. However, in the setting of the present paper, centralization affected regions (prefectures), not central government. Prefecture-level municipality government leaders also had to compete for promotion as county leaders. As a result, in this arrangement centralization at a regional level can avoid traditional problems in the political economy while still delivering the benefits from better coordination. Admittedly, this setting would not be a first-best arrangement, compared with the perfect decentralized equilibrium in Tiebout (1956). Nevertheless, in a real world with imperfect markets, distortion powers and incentives of local governments, this paper suggests centralization at a regional level be a second-best choice.

Figures and Tables

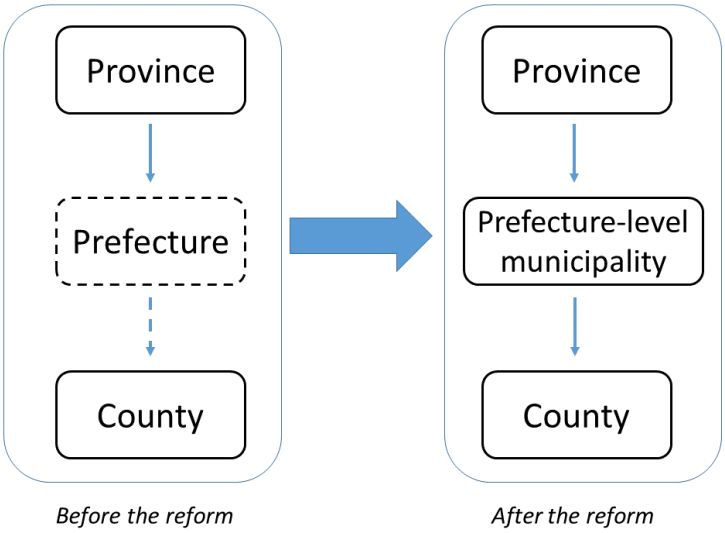


Figure 1.1: Sketch of the centralization reform

Note: This figure plots a simple illustration of the 1983 centralization reform in China. Dashed lines around the "prefecture" before the reform denote prefectures have no powers on component counties and counties are autonomous; Solid lines around the "prefecture-level municipality" after the reform denotes powers of counties are centralized to the prefecture-level municipality.

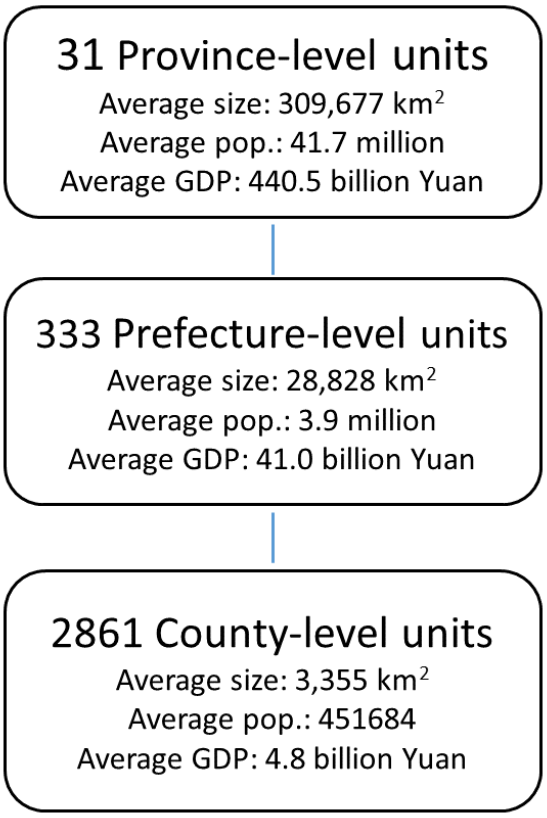


Figure 1.2: Basic information of different levels of jurisdictions in China (2003)
Note: This figure provides some basic information of different levels of jurisdictions in 2003, including numbers of jurisdictions, average sizes, average population and average GDP. The province-level units do not include Hongkong, Macau and Taiwan.

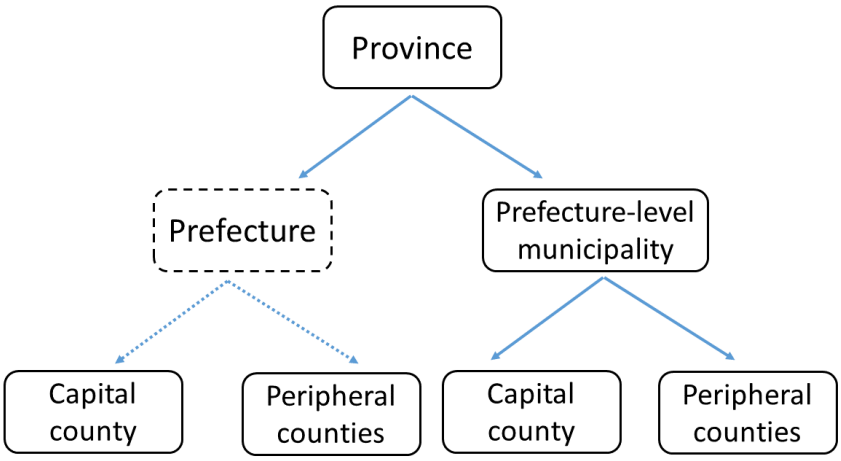


Figure 1.3: Hierarchy of local governments in China
Note: This figure graphs the basic structure of local governance in China. Dashed lines between different levels of local governments suggest that upper level governments do not have administrative powers on lower level governments' jurisdictions. Solid lines suggest that upper level governments have administrative powers on lowers' jurisdictions.

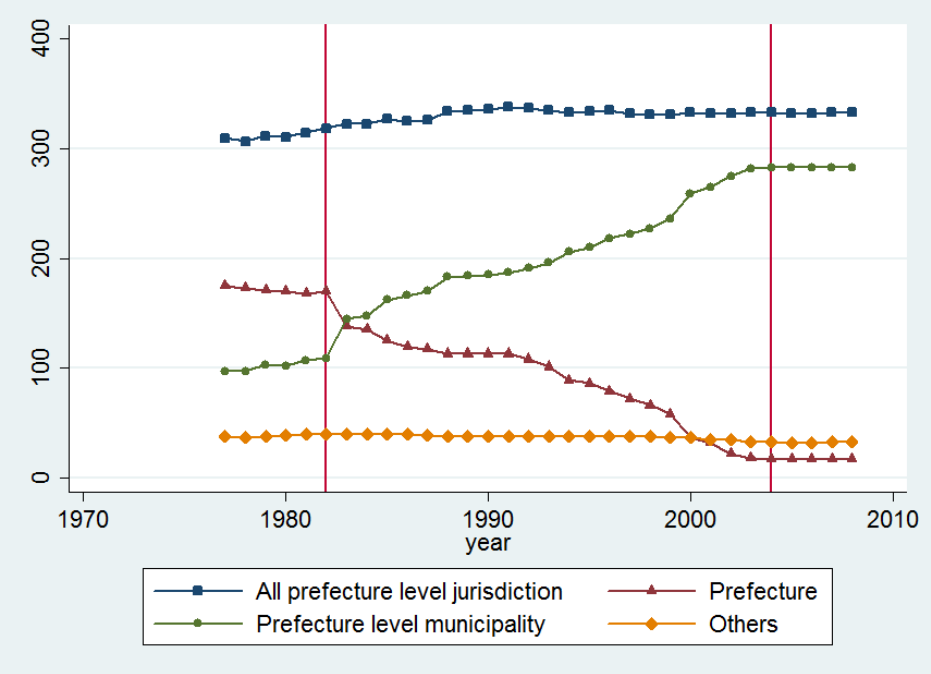


Figure 1.4: Changes in the number of prefecture level jurisdictions
Note: This figure plots the numbers of various types of prefecture-level jurisdictions in China over years.

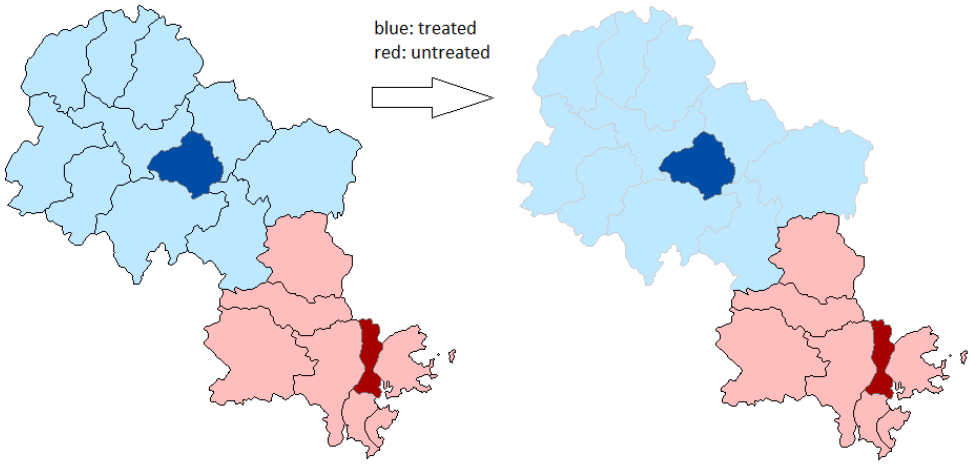


Figure 1.5: An example of the Reform
Note: This figure visualizes an example of the reform. The blue and the red regions are Sanming Prefecture and Quanzhou Prefecutre. Before the reform, they are both prefectures. In the reform the blue treated and turned to a prefecture-level municipality. The red prefecture is not treated and remains prefecture status. The dark-color zones are capital counties and the light-color zones are peripheral counties.

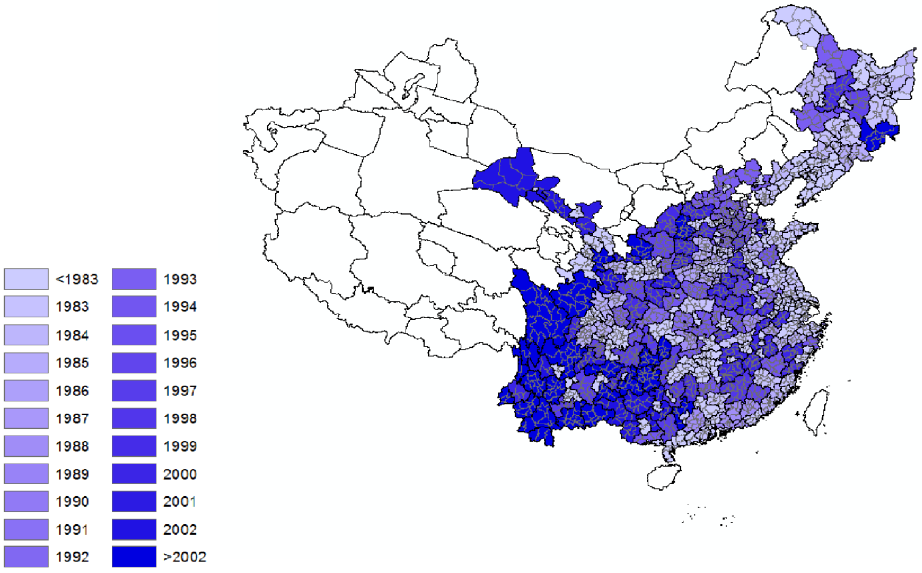
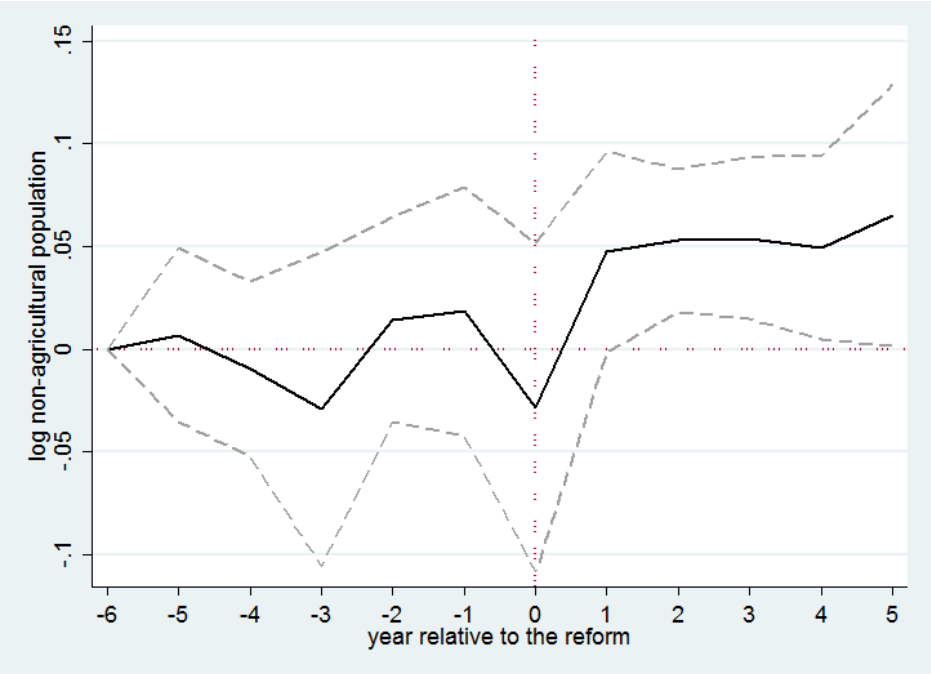
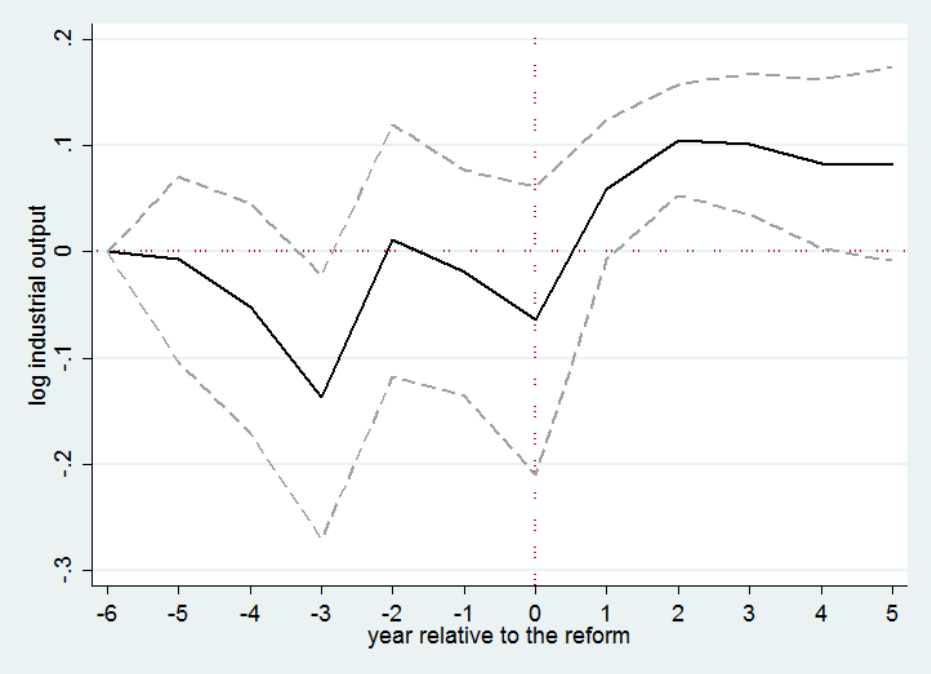


Figure 1.6: Timing of the reform across space
Note: This figure graphs the nationwide reform timing . The darker the colour is, the later the treatment happens. The white zones are those excluded in the sample.



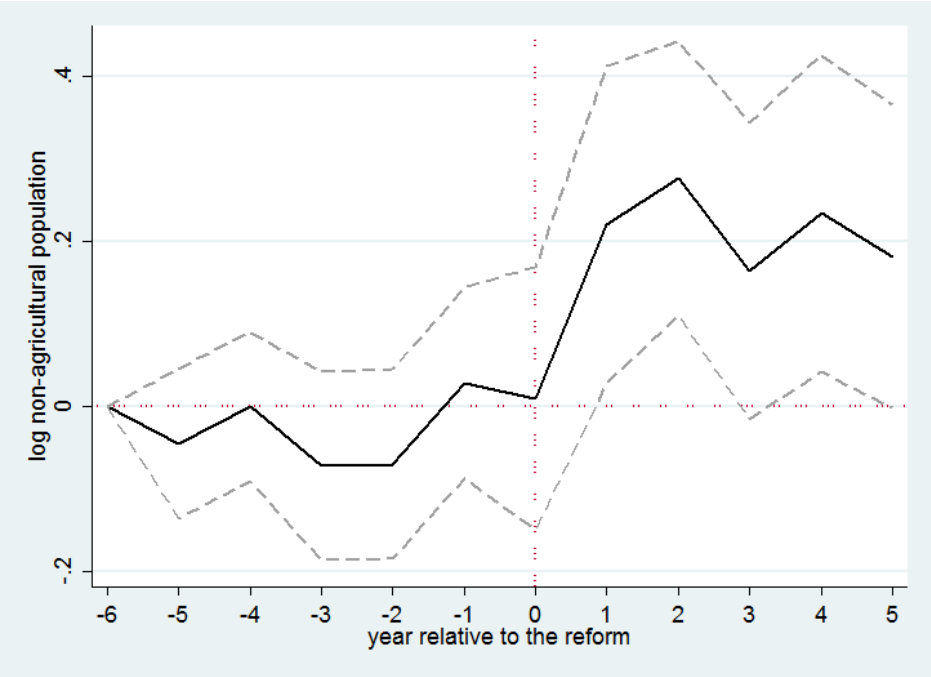
(a) Dynamic treatment effects on log non-agricultural population



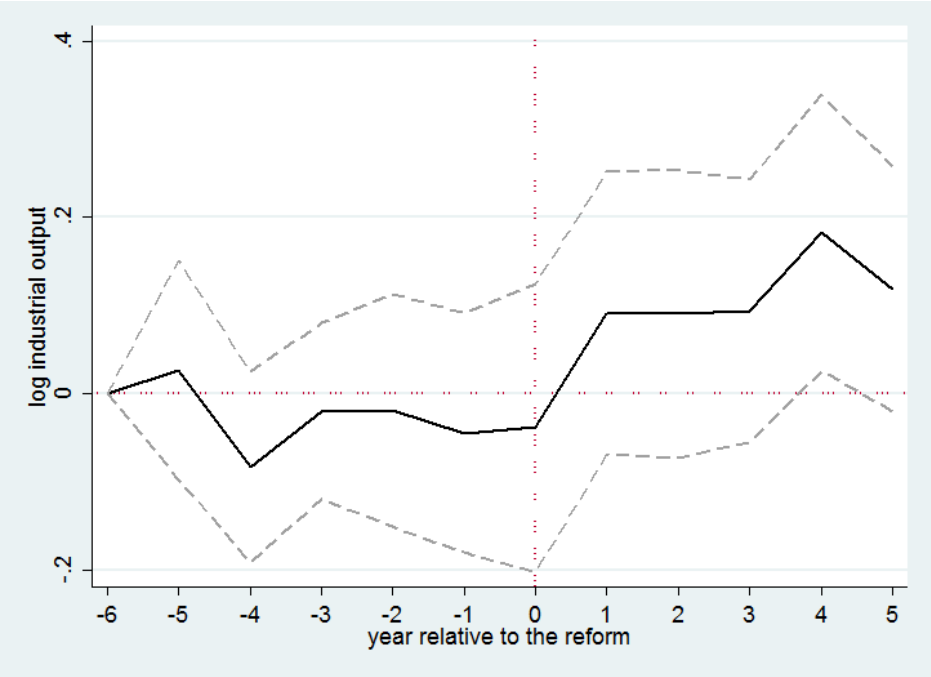
(b) Dynamic treatment effects on log industrial output

Figure 1.7: Dynamic treatment effects on outcomes in prefectures

Note: This figure reports estimates of the dynamic effect of the "Turning prefectures into prefecture-level municipalities" reform on developments in prefectures derived from a flexible difference-in-differences specification. Estimates are constructed by regressing the log of non-agricultural (a) or the log of industrial output (b) on a series of dummy variables indicating whether the year of observation falls in a given relative year as measured from the year of the reform happened. Relative year -6 is the omitted category so that all estimates should be interpreted as relative to the sixth year prior to the reform. All years beyond the relative year 4 are grouped into the effects of relative 5. The solid line plots the estimate and the dashed lines plot the 95 percent confidence interval for the relative year main effects.



(a) Dynamic treatment effects on log non-agricultural population



(b) Dynamic treatment effects on log industrial output

Figure 1.8: Dynamic treatment effects on outcomes in counties

Note: This figure reports estimates of the dynamic effect of the "Turning prefectures into prefecture-level municipalities" reform on developments in the gaps between capital and peripheral counties derived from a flexible difference-in-differences specification. Estimates are constructed by regressing the log of non-agricultural (a) or the log of industrial output (b) on interactions between the capital county dummy and a series of dummy variables indicating whether the year of observation falls in a given relative year as measured from the year of the reform happened and their interactions with a dummy variable indicating whether a county is a capital county or not. Relative year -6 is the omitted category so that all estimates should be interpreted as relative to the sixth year prior to the reform. All years beyond the relative year 4 are grouped into the effects of relative 5. The solid line plots the estimate and the dashed lines plot the 95 percent confidence interval for the relative year coefficients of the gap between two types of counties.

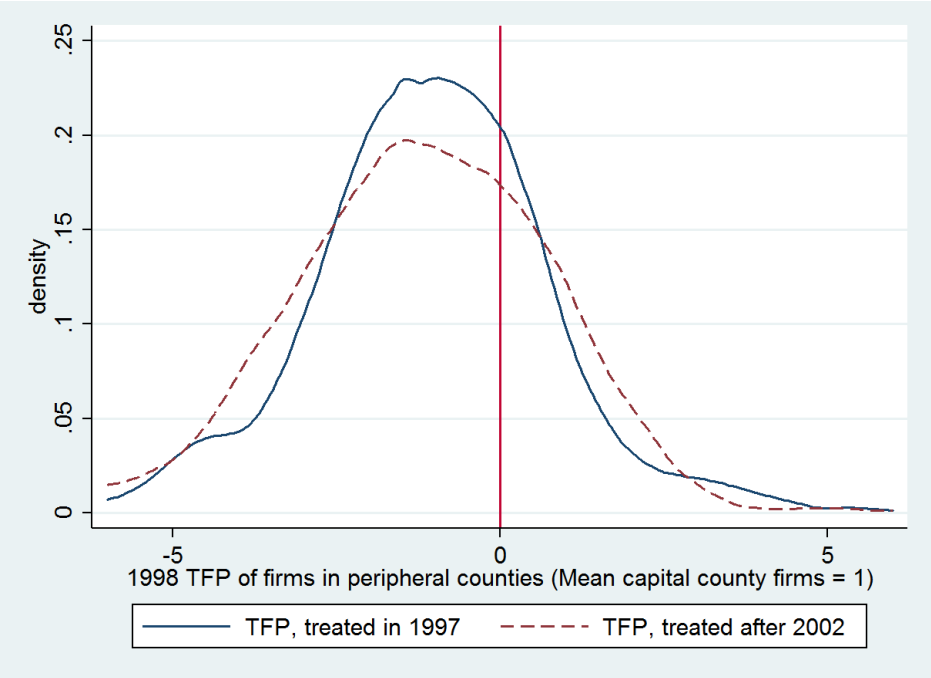


Figure 1.9: TFP distribution in peripheral counties relative to capital counties

Table 1.1: Summary Statistics

	Whole prefecture		Capital county		Peripheral county	
	mean	s.d.	mean	s.d.	mean	s.d.
Panel A: main economic outcomes: 1983 - 2003						
Total population	4075.41	(2186.66)	691.64	(749.00)	486.28	(1427.46)
Non-agricultural pop.	836.80	(643.27)	234.87	(272.68)	63.95	(111.33)
Total output	10284.26	(14057.8)	4953.14	(11545.25)	1956.37	(3257.72)
Industrial output	8684.29	(18347.17)	4234.75	(10488.01)	1246.43	(2707.76)
Panel B: main public finance outcomes: 1993 - 2003						
Gov't employees	2.40	(1.22)	1.41	(1.38)	0.79	(0.56)
Total revenue	1131.43	(1690.25)	598.86	(1538.99)	83.37	(1164.19)
Total expenditure	1756.31	(2162.23)	743.36	(1718.57)	139.42	(148.59)

† Notes: Standard deviation in parentheses. All population variables are measured in 1 thousand people. All output variables are measured in 1 million yuan (1980 constant prices)

Table 1.2: Evidence on Centralization

VARIABLES	(1) log(gov emp)	(2) log(admin exp)	(3) exp share	(4) rev share	(5) log(transfer pay)
Panel A: Prefecture-level Government					
treat	0.190**	0.342***	0.049***	0.023**	-0.062
	(0.089)	(0.046)	(0.017)	(0.011)	(0.227)
Prefecture FE	✓	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓	✓
Observations	1,692	1,714	1,714	1,723	1,582
R-squared	0.462	0.803	0.312	0.518	0.733
# of prefectures	175	175	175	175	175
Panel B: County-level Government					
treat	-0.013	-0.003	-0.003*	-0.000	0.008***
	(0.011)	(0.009)	(0.002)	(0.002)	(0.002)
County FE	✓	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓	✓
Observations	12,972	15,536	15,536	16,012	15,283
R-squared	0.898	0.921	0.120	0.061	0.382
# of counties	1,738	1,738	1,738	1,738	1,738

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(numbers of government employees), log(administrative expenditure), percentage of expenditure in total expenditure of the prefecture, percentage of revenue in total revenue of the prefecture and log(transfer payment). Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Individual fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.3: Baseline Model: Overall Effects on Prefectures

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
treat	0.039***	0.010***	0.065**	0.016
	(0.013)	(0.003)	(0.032)	(0.031)
Prefecture FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	2,564	2,564	2,401	2,245
R-squared	0.726	0.603	0.834	0.307
# of prefectures	178	178	178	178

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.4: Placebo Test Using Sample from Zhejiang Province

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
treat	-0.015	-0.013	0.046	-0.010
	(0.009)	(0.011)	(0.129)	(0.025)
Prefecture FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	156	156	156	156
R-squared	0.969	0.676	0.970	0.582
# of prefectures	6	6	6	6

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects and year fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.5: Geographic Concentration

VARIABLES	(1) EG Index-output	(2) EG Index-employment
treat	0.029***	0.042***
	(0.008)	(0.008)
Prefecture×sector FE	✓	✓
Year×province FE	✓	✓
Year×sector FE	✓	✓
Observations	45,286	45,286
R-squared	0.019	0.337
# of prefecture-sector pairs	9,445	9,445

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are the Ellison-Glaeser Index respectively measured in outputs and employment. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects, year×sector fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.6: Baseline Model: Overall Effects on Aggregate Productivity

VARIABLES	(1) log indus. out. PC	(2) log total output PC	(3) log total pop.	(4) log total out.
treat	0.035**	0.042*	0.028***	0.075**
	(0.016)	(0.025)	(0.010)	(0.034)
Prefecture FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	2,397	2,283	2,621	2,291
R-squared	0.737	0.767	0.473	0.816
# of prefectures	178	178	178	178

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(industrial output per capita), log(total output per capita), log(total population) and log(total output). Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.7: Results on Misallocation at Prefecture Level

VARIABLES	(1) TFPR SD	(2) TFPR 75-25	(3) TFPR 90-10
treat	-0.219**	-0.411**	-0.164**
	(0.111)	(0.207)	(0.083)
Prefecture FE	✓	✓	✓
Year×province FE	✓	✓	✓
Observations	969	969	969
R-squared	0.459	0.345	0.398
# of prefectures	168	168	168

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are measurements on dispersions of TFPR. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.8: Results on Misallocation at County Level

VARIABLES	(1) TFPR SD	(2) TFPR 75-25	(3) TFPR 90-10
treat	-0.004	0.010	-0.011
	(0.009)	(0.009)	(0.018)
County FE	✓	✓	✓
Year×province FE	✓	✓	✓
Observations	6,564	7,254	7,254
R-squared	0.114	0.085	0.098
# of counties	2,764	2,764	2,764

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are measurements on dispersions of TFPR. Main independent variable is whether this county is treated (component county of a prefecture-level municipality). County fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.9: Counterfactual TFP Gains

	(1)	(2)	(3)
Year	1998	1999	2000
Panel A: Equalizing TFPR within prefectures			
%	90.6	97.1	102.0
Panel B: Relative to 1998 prefecture-level municipality gains			
%	16.1	18.1	23.1

† Notes: Panel A reports counterfactual gains from equalizing TFPR within prefectures and sectors. Panel B reports counterfactual gains from moving the dispersions in decentralized prefectures to the relative efficient level in centralized prefecture-level municipalities.

Table 1.10: Results on Fiscal Expenditure Variables

	(1)	(2)	(3)	(4)
VARIABLES	dependency	transfer pay. share	rev. share	exp. share
treat	0.007***	-0.006***	-0.003	-0.003
	(0.002)	(0.001)	(0.003)	(0.003)
treat×capital	0.007	0.011***	0.003	0.009*
	(0.006)	(0.004)	(0.007)	(0.005)
Observations	14,312	15,682	15,665	15,642
R-squared	0.380	0.217	0.063	0.083
# of counties	1,735	1,742	1,738	1,738

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage of transfer payments in county's total revenue, county's share of transfer payments in prefecture or prefecture-level municipality, county's share of tax revenue in prefecture or prefecture-level municipality, county's share of expenditure in prefecture or prefecture-level municipality. The main independent variables are whether this county is treated (component county of a prefecture-level municipality) and the interaction with whether it is a capital county. County fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.11: Baseline Regression Results: Distributional Effects on Counties

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
treat	-0.005	-0.002	-0.069***	-0.002
	(0.006)	(0.004)	(0.029)	(0.006)
treat×capital	0.146***	0.034*	0.170**	0.027
	(0.046)	(0.018)	(0.075)	(0.018)
County FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	21,061	20,968	15,970	14,894
R-squared	0.443	0.073	0.805	0.017
# of counties	1,744	1,745	1,610	1,610

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. The main independent variables are whether this county is treated (component county of a prefecture-level municipality) and the interaction with whether it is a capital county. County fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.12: Capital Counties' Shares in the Whole Prefectures/Municipalities

VARIABLES	(1) non-agri pop share	(2) industrial share
treat	0.042**	0.022*
	(0.016)	(0.012)
County FE	✓	✓
Year×province FE	✓	✓
Observations	1,592	1,214
R-squared	0.385	0.309
# of counties	171	171

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are county's share of non-agricultural population in prefecture or prefecture-level municipality and county's share of industrial outputs in prefecture or prefecture-level municipality. The main independent variables are whether this county is treated (component county of a prefecture-level municipality). County fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.13: Placebo Test Using Sample from Zhejiang Province

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
treat	0.099	0.010	0.193	0.040
	(0.054)	(0.007)	(0.154)	(0.027)
treat×capital	-0.113	0.003	-0.367	-0.030
	(0.068)	(0.015)	(0.204)	(0.033)
County FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	982	982	798	987
R-squared	0.811	0.625	0.970	0.779
# of counties	38	38	38	38

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. County fixed effects and year fixed effects are controlled. Standard errors are clustered on prefecture level.

Table 1.14: Results on Concentration - Firm Level Evidence

	(1)	(2)	(3)	(4)	(5)	(6)
	All firms		State-owned firms		Other firms	
VARIABLES	logoutput	logoutput	logoutput	logoutput	logoutput	logoutput
treat	-0.001	-0.011	-0.000	-0.016	-0.007	-0.009
	(0.013)	(0.014)	(0.017)	(0.018)	(0.022)	(0.024)
treat×tfp	0.011*	0.019**	0.013***	0.023***	-0.001	0.006
	(0.006)	(0.009)	(0.006)	(0.008)	(0.002)	(0.008)
treat×tfp×capital		-0.004		-0.004		-0.005
		(0.003)		(0.003)		(0.004)
treat×capital		0.013		0.025		0.009
		(0.018)		(0.024)		(0.034)
Firm FE	✓	✓	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓	✓	✓
Year×sector FE	✓	✓	✓	✓	✓	✓
Observations	275,580	275,580	143,766	143,766	131,814	131,814
R-squared	0.047	0.047	0.030	0.031	0.086	0.086
# of firms	95,357	95,357	51,520	51,520	43,837	43,837

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variable is log output of a firm. Firm fixed effects, year×province fixed effects and year×sector fixed effects are controlled. Standard errors are clustered on prefecture level.

Appendix Figures and Tables

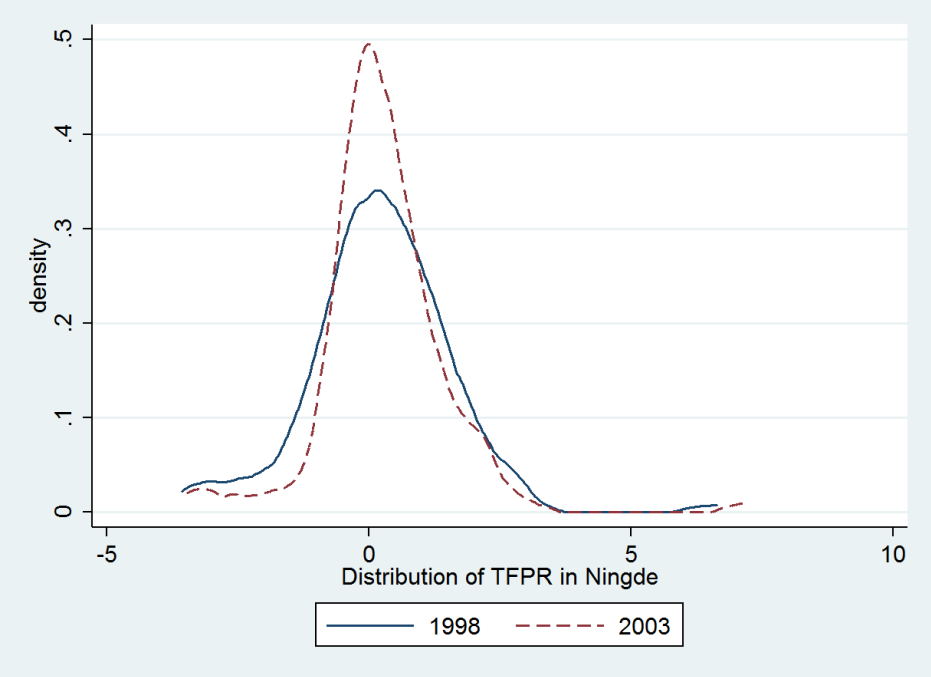
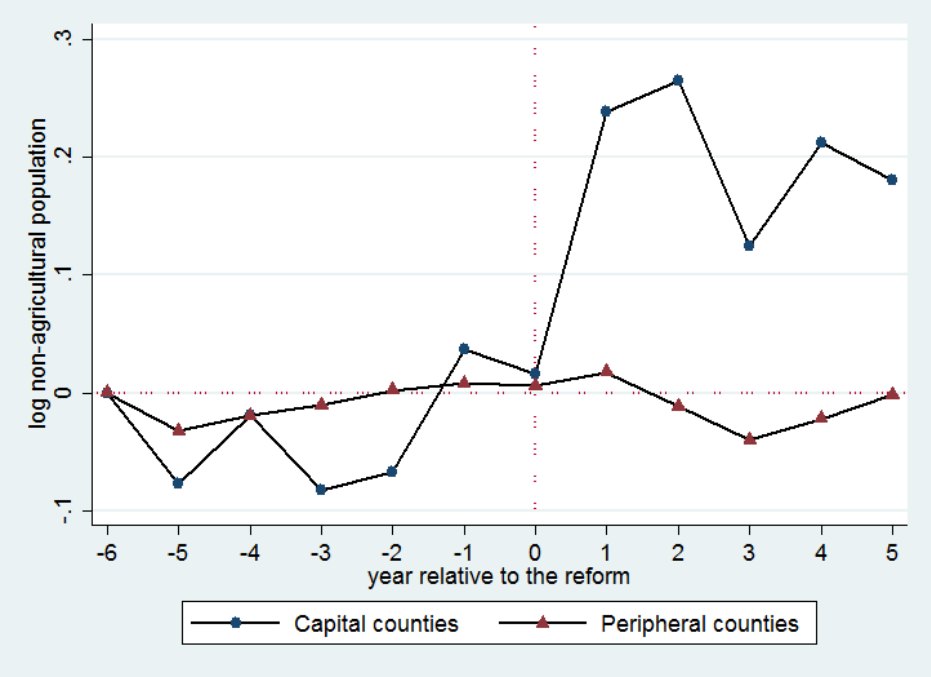
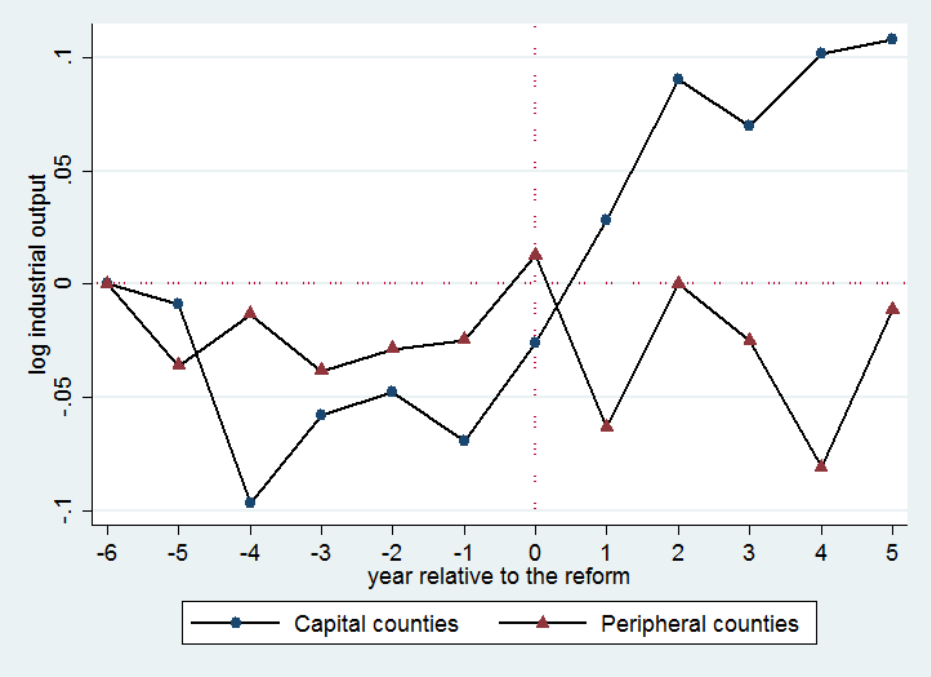


Figure A.1: Distribution of TFPR in Ningde in 1998 and 2003

Note: This figure plots dispersions of TFPR in Ningde, Fujian Province, in 1998 and 2003. Ningde prefecture received the treatment to become a prefecture-level municipality in 1999. Distributions are for deviations of log(TFPR) from sector means. TFPR is calculated as in Hsieh and Klenow (2009).



(a) Dynamic treatment effects on log non-agricultural population



(b) Dynamic treatment effects on log industrial output

Figure A.2: Dynamic treatment effects on outcomes in counties

Note: This figure reports estimates of the dynamic effect of the "Turning prefectures into prefecture-level municipalities" reform on developments of capital and peripheral counties respectively derived from a flexible difference-in-differences specification. Estimates are constructed by regressing the log of non-agricultural (a) or the log of industrial output (b) on a series of dummy variables indicating whether the year of observation falls in a given relative year as measured from the year of the reform happened and their interactions with a dummy variable indicating whether a county is a capital county or not. Relative year -6 is the omitted category so that all estimates should be interpreted as relative to the sixth year prior to the reform. All years beyond the relative year 4 are grouped into the effects of relative 5. The series in red triangles plots the coefficient estimates of the relative year main effects, representing the trend among peripheral counties. The series in blue circle plots the estimate for the sum of the relative year main effects and the interaction with the capital county indicator, representing the trend among capital counties.

Table A.1: Robustness: Regression Using Employment Statistics from Census Data

	(1)	(2)	(3)	(4)
	Prefecture level		County level	
VARIABLES	log(non-agri emp)	urbanization	log(non-agri emp)	urbanization
treat	0.149**	0.045***	-0.005	-0.001
	(0.075)	(0.014)	(0.013)	(0.003)
treat×capital			0.124**	0.043
			(0.059)	(0.027)
Prefecture FE	✓	✓		
County FE			✓	✓
Year×province FE	✓	✓	✓	✓
Observations	491	491	4,259	4,259
R-squared	0.973	0.699	0.996	0.391
# of FE	165	165	1,738	1,738

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural employment) and urbanization rate measured by the share of non-agricultural employment in total employment. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture or county fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table A.2: Robustness: Controlling for Pre-existing Conditions

	(1)	(2)	(3)	(4)
VARIABLES	log(non-agri pop)	urbanization	log(industrial out)	industrialization
treat	0.044***	0.013***	0.071**	0.020
	(0.016)	(0.005)	(0.034)	(0.029)
Prefecture FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	2,564	2,564	2,401	2,245
R-squared	0.692	0.734	0.837	0.204
# of prefectures	178	178	178	178

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects, year×province fixed effects and interactions between pre-existing conditions and year dummies are controlled. Standard errors are clustered on prefecture level.

Table A.3: Robustness: Controlling for Pre-Existing Conditions

VARIABLES	(1) log(nonagri pop)	(2) urbanization	(3) log(indus out)	(4) industrialization
treat	-0.007	-0.001	-0.050*	-0.016***
	(0.013)	(0.003)	(0.027)	(0.006)
treat×capital	0.143***	0.024	0.188***	0.037*
	(0.047)	(0.015)	(0.077)	(0.020)
County FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	21,061	20,968	15,970	14,894
R-squared	0.443	0.235	0.805	0.379
# of counties	1,744	1,744	1,610	1,608

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. The main independent variables are whether this county is treated (component county of a prefecture-level municipality) and the interaction with whether it is a capital county. County fixed effects, year×province fixed effects and interactions between pre-existing conditions and year dummies are controlled. Standard errors are clustered on prefecture level.

Table A.4: Robustness: Prefecture-level Regressions on Treatment in Period t

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
treat	0.030*	0.012***	0.083***	0.004
	(0.016)	(0.004)	(0.034)	(0.023)
Prefecture FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	2,564	2,564	2,401	2,245
R-squared	0.784	0.672	0.823	0.240
# of prefectures	178	178	178	178

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table A.5: Robustness: County-level Regressions on Treatment in Period t

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
treat	-0.006	-0.001	-0.023	-0.004
	(0.012)	(0.003)	(0.027)	(0.006)
treat×capital	0.206***	0.031*	0.081	0.014
	(0.048)	(0.017))	(0.095)	(0.023)
County FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	21,061	20,968	15,970	14,894
R-squared	0.412	0.235	0.805	0.379
# of counties	1,744	1,745	1,610	1,610

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. The main independent variables are whether this county is treated (component county of a prefecture-level municipality) and the interaction with whether it is a capital county. County fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table A.6: Robustness: Controlling for Prefecture-specific Time Trend

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
treat	0.034*	0.002	0.057*	0.035
	(0.020)	(0.004)	(0.033)	(0.049)
Prefecture FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Prefecture trends	✓	✓	✓	✓
Observations	2,564	2,564	2,401	2,245
R-squared	0.736	0.681	0.858	0.315
# of prefectures	178	178	178	178

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects year×province fixed effects, interactions between pre-existing conditions and year dummies and prefecture-specific time trend are controlled. Standard errors are clustered on prefecture level.

Table A.7: Robustness: Controlling for County-specific Time Trend

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
treat	-0.002	0.001	-0.061**	-0.019**
	(0.003)	(0.003)	(0.030)	(0.009)
treat×capital	0.073*	0.006	0.102	0.012
	(0.039)	(0.015)	(0.067)	(0.021)
County FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
County trends	✓	✓	✓	✓
Observations	21,061	20,968	15,970	14,894
R-squared	0.615	0.526	0.861	0.542
# of counties	1,744	1,745	1,610	1,610

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. The main independent variables are whether this county is treated (component county of a prefecture-level municipality) and the interaction with whether it is a capital county. County fixed effects, year×province fixed effects, interactions between pre-existing conditions and year dummies and county-specific time trend are controlled. Standard errors are clustered on prefecture level.

Table A.8: Robustness: Possible Heterogeneity across Different Periods on Prefectures

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
Panel A: 1983-1993				
treat	0.046*	0.016	0.085**	0.020
	(0.027)	(0.039)	(0.041)	(0.025)
Prefecture FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	1034	1034	1121	1052
R-squared	0.769	0.795	0.831	0.745
# of prefectures	178	178	178	178
Panel B: 1994-2003				
treat	0.0035***	0.009*	0.076	0.012
	(0.014)	(0.005)	(0.059)	(0.010)
Prefecture FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	1,530	1,530	1,280	1,193
R-squared	0.490	0.628	0.482	0.255
# of prefectures	178	178	178	178

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table A.9: Robustness: Possible Heterogeneity across Different Periods on Counties

VARIABLES	(1) log(non-agri pop)	(2) urbanization	(3) log(industrial out)	(4) industrialization
Panel A: 1983-1993				
treat	-0.025	-0.003	-0.015	0.009
	(0.021)	(0.004)	(0.037)	(0.011)
treat×capital	0.170***	0.037	0.046	0.009
	(0.063)	(0.026)	(0.151)	(0.045)
County FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	5,948	5,907	7,025	6,468
R-squared	0.363	0.244	0.717	0.380
# of counties	1,249	1,248	1,314	1,290
Panel B: 1994-2003				
treat	0.015	-0.002	-0.004	-0.013*
	(0.013)	(0.003)	(0.035)	(0.007)
treat×capital	0.063	0.010	0.033	0.032
	(0.042)	(0.011)	(0.078)	(0.027)
County FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	15,113	15,061	8,945	8,426
R-squared	0.313	0.223	0.410	0.290
# of counties	1,726	1,726	1,583	1,579

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log(non-agricultural population), urbanization rate, log(industrial output) and industrialization rate. The main independent variables are whether this county is treated and the interaction with whether it is a capital county. County fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level.

Table A.10: Results on Misallocation at Prefecture Level – Small Sample

VARIABLES	(1) TFPR SD	(2) TFPR 75-25	(3) TFPR 90-10
treat	-0.206	-0.385	-0.154
	(0.149)	(0.279)	(0.112)
Prefecture FE	✓	✓	✓
Year×province FE	✓	✓	✓
Observations	315	315	315
R-squared	0.442	0.389	0.402
# of prefectures	59	59	59

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are measurements on dispersions of TFPR. Main independent variable is whether this prefecture is treated to a prefecture-level municipality. Prefecture fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level. The sample only includes prefectures got treated after 1998.

Table A.11: Results on Misallocation at County Level – Small Sample

VARIABLES	(1) TFPR SD	(2) TFPR 75-25	(3) TFPR 90-10
treat	-0.001	0.016	-0.035
	(0.019)	(0.013)	(0.034)
County FE	✓	✓	✓
Year×province FE	✓	✓	✓
Observations	1,357	1,520	1,520
R-squared	0.177	0.133	0.173
# of counties	872	872	872

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are measurements on dispersions of TFPR. Main independent variable is whether this county is treated (component county of a prefecture-level municipality). County fixed effects and year×province fixed effects are controlled. Standard errors are clustered on prefecture level. The sample only includes prefectures got treated after 1998.

Table A.12: Counterfactual TFP Gains – Diminishing Returns in Production

	(1)	(2)	(3)
Year	1998	1999	2000
Panel A: Equalizing TFPR within prefectures			
%	45.1	47.5	49.1
Panel B: Relative to 1998 prefecture-level municipality gains			
%	13.6	16.5	19.5

† Notes: Panel A reports counterfactual gains from equalizing TFPR within prefectures and sectors. Panel B reports counterfactual gains from moving the dispersions in decentralized prefectures to the relative efficient level in centralized prefecture-level municipalities. The results are based on the model with constant returns in production.

Chapter 2

Environmental Regulations, Political Incentives and Local Economic Activities: Evidence from China

How does nationwide environmental regulations produce heterogeneous impacts on economic activities of manufacturing firms located in places with differently incentivized local officials? This paper draws upon a set of firm-based pollution regulations in China from 2007 to investigate the relationship between political incentives and the effects of environmental regulations. I show that when the Party secretary of a municipality has more incentives in terms of his job promotion to improve the local economy, the adverse impacts on employment and the output of regulated firms will increase. At the same time, the loss in the regulated firms will be associated with gains in other unregulated firms in polluting industries, and no overall reduction will be seen in the manufacturing activities of the polluting industries. As for the environmental consequences, I find that the emission of pollutants in municipalities with highly incentivized leaders goes down significantly.

2.1 Introduction

Over the last few decades, policies on the environment and pollution have been among the most frequent and controversial topics in public and academic discussions. Recent research concentrates on the environmental and the economic consequences of regulations, for example, the Clean Air Act Amendments (CAAAs) in the United States, the Clean Air Act in the United Kingdom and the Water Framework Direc-

tive in the European Union. On the one hand, these regulations are found to reduce the emission of pollutants and improve environmental quality. On the other hand, though not all the research papers manage to reach a consensus about these legislations' economic consequences, most of them accept that the regulations are associated with reductions in the manufacturing activities of regulated firms and industries (Jaffe et al., 1995; Henderson, 1996; Levinson, 1996; Becker and Henderson, 2000; Greenstone, 2002, 2003). At the same time, regulations can also have general equilibrium effects on other non-directly regulated firms and industries. Evaluating the cost of unintended consequences on all firms and industries will enable policy makers to consider the adverse effect of well-intentioned regulations and to design welfare programs that protect workers who lose their jobs or part of their income in consequence, by providing job training and temporary compensation. It is also helpful if workers in all firms can learn to expect potential labour market shocks from regulations and make arrangement in advance.

When discussing the unintended consequences brought by environmental regulations, the role played by political factors should not be ignored. For example, in democratic developed countries, electoral incentives may affect the nature of environmental policies (List and Sturm, 2006); in developing countries, incentives for rents or the promotion of local officials may lead to some adverse outcomes for the environment (Burgess et al., 2012; Jia, 2014). It is necessary to explore how different political incentives affect the final consequences of environmental regulation, in particular for policy makers in central government.

This paper uses a set of firm-based environmental regulations in China to comprehensively estimate the economic consequences on different types of firms and industries. The environmental regulations in question were proposed by the Ministry of Environmental Protection in China and took effect at the beginning of 2007. The target firms are called *Key Monitored Enterprises*. These firms accounted for 65% of the emissions of sulphur dioxide, smoke and industrial dust in China in 2006. The Ministry of Environmental Protection set a much stricter quota on these firms and required them to be monitored more carefully and often. They had to investigate the emission of pollutants by the target firms every month, and report the numbers every quarter. In general, it became much more costly for these firms to over-emit pollutants than ever before. According to Lin (2013), more inspections from environmental departments will make it harder for Chinese manufacturing enterprises to hide their pollution and will raise their self-reported emission of pollutants. The Ministry of Environmental Protection hoped it could make the targeted firms obey the stricter regulation and emit less pollutants. I link the list of these primary polluting firms with a variety of

datasets from different sources to evaluate the impact of the firm-based regulations on a series of firms and industries.

In this paper, I take local governments into account and find that the political incentives of local officials can explain the responses in different firms and industries. Jia (2014) investigates a similar question in China. She finds the highly incentivized officials will choose more dirty inputs. Similar Jia (2014), I argue that political incentives act as a complement to economic performance as regards the promotion rule. Highly incentivized officials will enforce the regulations more strictly. As a result, they will cut higher numbers of employees and reduce output more drastically in the regulated firms. At the same time, their incentives will encourage them to put more efforts into reallocating resources from the regulated firms to the unregulated ones. To show this logic formally, I present a simple career-concerns model. I extend her model to include the substitution of clean for dirty inputs, and then interpret how regulating the dirty input will induce local officials with different political incentives to choose different levels of clean and dirty inputs. It will provide more implications on possible regulations to avoid the pollution problem brought by promotion incentives as predicted in Jia (2014).

Empirically, I use the plausibly exogenous variations in local officials' ages to identify official changes in the promotion incentives for officials. The rationale of this lies in the regulations covering the compulsory age of retirement and the minimum tenure required to be promoted. All local officials are obliged to retire at the age of 60. Before being appointed to a new position or retirement, all officials should have completed at least 3 years in their present positions (Chinese Communist Party, 2002). The combination of these two rules determines that any official below the age of 57 years old will have a clear incentive to make more effort to earn promotion, because they will be young enough to complete 3 years in the new position. In contrast, when a local official is older than 56, he cannot reap the advantages of promotion because he is too close to the age of 60 and so his incentive may decline (Xi et al., 2015).

I use a triple differences strategy to identify the effects of the 2007 firm-based regulations. The main empirical finding is that the regulations produce significantly higher adverse effects on regulated firms located in municipalities[1] with highly incentivized local leaders than on those in municipalities with poorly incentivized leaders. At the same time, the highly incentivized local leaders will be inclined to transfer the pressure of unemployment in polluting industries from the regulated firms to the unregulated firms. Overall, the firm-based regulations will not have an adverse impact on the economic activities of industries as a whole. The results suggest that although the

[1]In this paper, I use the word municipality as a short for prefecture-level municipality, for simplicity

regulations will have a negative impact on regulated firms, inputs can be reallocated to unregulated firms to offset the adverse effect of the regulations. I find that overall the air quality will be improved.

This paper aims to contribute to several areas of the literature. First, many people have written about the direct impact of environmental regulations on industries. For example, Henderson (1996), Becker and Henderson (2000), Greenstone (2002), Greenstone et al. (2012), Davis et al. (2006) and Walker et al. (2013) all find an adverse effect of the CAAA on employment, wages, and total factor productivity in the targeted industries of the United States. Berman and Bui (2001), on the contrary, argue that a regulation on air quality in Los Angeles has not produced any negative effects on the regulated industries and firms. However, the previous literature mostly evaluates the impact on directly targeted firms or industries. Few papers provide insight into the unintended impact of environmental regulations on non-directly targeted firms and industries. In general equilibrium, environmental regulations will have an impact on the whole input and output market. There are no reasons to believe that other non-targeted firms are independent of these general equilibrium effects.

Moreover, few previous papers consider the potential heterogeneous effects in different political settings. The significant role of political factors in environmental policies and pollution levels in developing countries is confirmed by many papers. For example, Burgess et al. (2012) find that increases in the number of political jurisdictions in Indonesia had led to increased deforestation and lower timber prices, due to more intensive competition between local officials. Jia (2014) uses a career-concerns model to explain why local officials more closely connected to the central government in China produce more pollution. Cai et al. (2015), Kahn et al. (2015) and Lipscomb and Mobarak (2015) are all concerned about the incentives of local officials to pollute their neighbours by spreading pollutants down rivers. However, as far as I know, no recent papers discuss whether different political incentives will induce a uniform environmental regulation to have heterogeneous effects in different locations.

Third, this paper brings together a variety of comprehensive datasets, contributing to improve the quality of present studies on environmental issues in China. I digitize the list of *Key Monitored Enterprises* in 2007 and link them to the Annual Survey of Industrial Production, a widely used firm-level dataset in China. This provides me micro-level variations on regulation status within each industry. Compared to previous papers on environmental issues in China, all of which can only document regulations at industry-level or region-level, it can precisely measure each firm's status in a regulation. For proxies of political incentives, I use a dataset digitized by Chen (2015), including backgrounds and some demographic characteristics of every Party

secretary at municipalities in China from 2000 to 2010. I also digitize their connections with higher-level officials, including provincial leaders and members of Politburo, as a complement to the work of Chen (2015). As for the pollution outcomes, I collect two municipality-level measurements. One is an official record of sulfur dioxide emissions from the China City Statistics Yearbooks of 1993. Because of the notorious misreporting of official pollution data, I also use the Aerosol Optical Thickness (AOT) data provided by the National Aeronautics and Space Administration (NASA), based on observations from the Moderate Resolution Imaging Spectroradiometer (MODIS). It measures the degree to which aerosols prevent the transmission of light by absorption or scattering of light, so it is closely related to air quality and becomes a possible way of measuring it.

The paper is organized as follows. Section 2.2 presents a career-concerns model to pin down the relationship between political incentives and the effects of environmental regulation. Section 2.3 provides an institutional background to the empirical studies. Section 2.4 introduces the datasets that I will use in this paper. Section 2.5 introduces the empirical strategies. Section 2.6 and Section 2.7 present main and robust results respectively. Section 2.8 sums up the themes in the paper.

2.2 Conceptual Framework

To illustrate how promotion incentives will affect the economic activities in regulated and unregulated firms, this section presents a model based on the theory of career concerns (Holmstrom, 1982). Jia (2014) uses an application of Holmstrom (1982) to model how local officials in China are motivated by career concerns to choose different levels of pollution. The basic structure of my model draws heavily on Jia (2014). In this section, I follow her to assume that a local official responds to career concerns by making two decisions: one is to do with his effort to increase the total resources or budget he can devote to production and the other is to do with the way that this can be allocated among inputs. I extend her model to include substitution of clean for dirty inputs, and then interpret how a regulation on dirty input will affect local officials with different political incentives to choose different inputs.

2.2.1 Production

To begin with, I introduce as the basis of the performance and promotion of local officials the technology used to produce final output. The local official can produce final output Y with a constant elasticity of substitution (CES) production function,

aggregating differentiated intermediate input:

$$Y = (\sum_{i=1}^{m} Y_i^{\rho})^{1/\rho}, \tag{2.1}$$

where $0 < \rho < 1$. Each input is produced by one single firm. The budget constraint of the local official is given by:

$$\sum_{i=1}^{m} p_i Y_i = e, \tag{2.2}$$

where p_i is a given set of prices exogenously determined by the national market. The firms producing input are price takers. e is the total available budget or resource. The local official can increase the total resource e by exerting more costly efforts. For example, the local official can make efforts to bargain for more transfer payments from the central government, and then the available budget will be increased by these additional revenues. As a result, he has more available budget to allocate among firms but also suffer some cost from the efforts spent on bargaining.

For simplicity, I rewrite the production function in logs:

$$y = \frac{1}{\rho} \ln(\sum_{i=1}^{m} Y_i^{\rho}). \tag{2.3}$$

Among these firms producing various types of input, I assume for simplicity that only the first firm emits pollutants. It will create an additional non-monetary cost $b(Y_1)$ to the local official associated with the products besides its monetary cost of p_1. One can imagine the non-monetary cost as physical disgust at a dirty environment or additional efforts to subject the pollutants to environmental regulations.

Following standard assumptions in the theory of career concerns, I assume that the final observed output of the local official also depends on his competence θ:

$$\tilde{y} = \theta + \frac{1}{\rho} \ln(\sum_{i=1}^{m} Y_i^{\rho}), \tag{2.4}$$

where $\theta \backsim N(\bar{\theta}, \sigma_{\theta}^2)$. θ can be interpreted as his personal ability to amplify production, which is his most important characteristic considered by the central government in the process of deciding promotions. θ cannot be observed directly either by the central government or by the local official himself.

2.2.2 Political Incentives

I next describe the promotion rule and associated political incentives of the local official. The central government considers two factors in determining whether to promote local officials. One is the unobserved competence θ introduced above. The other is an observed personal characteristic C, which I name the "political incentive". C measures the probability of being promoted, given one's competence. Promotion, therefore, is written as:

$$E(C\theta) \geqslant \bar{U}, \tag{2.5}$$

where $\bar{U}$ is a constant measuring a standard for promotion.

The central government can observe C but not θ. It has to infer θ from the final observed output $\tilde{y}$. The rule can then be rewritten as:

$$CE(\theta \mid \tilde{y}) \geqslant \bar{U}. \tag{2.6}$$

Rewrite the expectation term as:

$$E(\theta \mid \tilde{y}) = E(\tilde{y} - y \mid \tilde{y}). \tag{2.7}$$

The probability of promotion is:

$$P = Pr[\theta \geqslant E(y) - y + \bar{U}/C] = 1 - \Phi(E(y) - y + \bar{U}/C). \tag{2.8}$$

The local official is going to maximize the expected benefits from promotion minus the costs:

$$\max f(P) - a(e) - b(Y_1), \tag{2.9}$$

where $f(\cdot)$ and $a(\cdot)$ are utility functions involving the gains from promotion and costs from efforts. To keep the model simple, I assume $f(P) = P$, $a(e) = Ae$ and $b(Y_1) = BY_1$.

2.2.3 Solution and Comparative Statics

The problem of the local official can be written as:

$$\max P - Ae - BY_1, \tag{2.10}$$

s.t.

$$\sum_{i=1}^{m} p_i Y_i = e \tag{2.11}$$

$$P = 1 - \Phi(E(y) - y + \bar{U}/C). \tag{2.12}$$

The first order conditions are:

$$\phi Y_1^{\rho-1} \frac{1}{\sum_{i=1}^{m} Y_i^{\rho}} = Ap_1 + B \tag{2.13}$$

$$\phi Y_i^{\rho-1} \frac{1}{\sum_{i=1}^{m} Y_i^{\rho}} = Ap_i, \forall i \neq 1. \tag{2.14}$$

The solution yields:

$$\frac{Y_1^{\rho-1}}{Y_i^{\rho-1}} = \frac{Ap_1 + B}{Ap_i}, \forall i \neq 1 \tag{2.15}$$

$$Y_1 = \frac{\phi(B + Ap_1)^{1/\rho-1}}{(B + Ap_1)^{\rho/\rho-1} + A\sum_{i=2}^{m} p_i^{\rho/\rho-1}}, \tag{2.16}$$

$$Y_i = \frac{\phi A p_i^{1/\rho-1}}{(B + Ap_1)^{\rho/\rho-1} + A\sum_{i=2}^{m} p_i^{\rho/\rho-1}}, \forall i \neq 1 \tag{2.17}$$

Next I calculate the comparative statics in the equilibrium conditions for each Y_i. I model the regulations targeted at firm 1 as an increase in B, the cost of emitting pollutants. One possible explanation is that, since the regulations raise the intensity of monitoring the emission of pollutants in firm 1, the local official has to spend more efforts on cutting by-products when he wants to increase the product of a firm. Another explanation is that the regulations raise difficulties for those who want pollution permits and emissions quotas.

The comparative static results yield:

$$\frac{\partial^2 Y_1}{\partial C \partial B} < 0, \tag{2.18}$$

$$\frac{\partial^2 Y_j}{\partial C \partial B} > 0, \forall i \neq 1 \tag{2.19}$$

They suggest that the local official with a higher political incentive is more responsive to the regulations. When the cost of the dirty input rises, the highly incentivized official will reduce more dirty input and increase more clean input. In other words, an environmental regulations aiming to increase costs of pollution will be more effective in a place with a highly incentivized official. I will use a set of firm-based regulations in 2007 and local officials' incentives for promotion in China to test these predictions empirically.

2.3 Background

2.3.1 Pollution in China and the 2007 Firm-Based Regulation

In the last three decades, China has experienced a rapid growth in its economy. At the same time, pollution is the most notorious by-product of the economic miracle to have emerged in recent years. For example, according to a report by the World Bank (2007), in 2003 only one percent of urban population in China lived in cities with annual average PM10 levels below 40 $\mu g/m^3$, a safety standard raised by the European Union[2]; about 25,000 km of Chinese rivers failed to meet the water quality standards for aquatic life and about 90 percent of the sections of rivers around urban areas were seriously polluted. The health costs associated with such severe pollution are high. Chen et al. (2013) estimate that long-term exposure to an additional 100 $\mu g/m^3$ of total suspended particles (TSP) reduces life expectancy by about 3 years, using an arbitrary Chinese heating policy as a quasi experiment. The rapid industrialization is blamed most often for the pollution. Ebenstein (2012) uses regional variation in the effective levy rate to show the causal effects of industrial activities and water pollution on the death rates from cancer. Not surprisingly, the demand for better accountability on the environment and less pollution for citizens in China is growing. Since 2007, several demonstrations to protest against paraxylene (PX) plants have been held in large cities in China, which have greatly worried the government[3].

Realizing the enormous costs of pollution and the growing demand for a clean environment, the Chinese government has put more and more effort into solving these problems. "Protecting the environment" was listed as one of the five Basic State Policies[4] in China as long ago as the 1990s. Since the start of the Ninth Five-Year Plan (1996), the environmental consequences of economic development have been taken into account in the government's goals. In 1996, the central government identified three rivers, three lake basins, two control zones, one city and one sea as key regions for limiting the emission of major pollutants.

However, many studies find that these environmental policies have always failed to reach their goals. For example, Gao et al. (2009) document many difficulties in implementing emission policies in the two control zones. Schreifels et al. (2012) find the national emission of sulfur dioxide was not reduced by 10% as required by the Tenth Five-Year Plan. Stoerk (2015) finds that, though the political attention to air

[2]http://ec.europa.eu/environment/air/quality/standards.htm

[3]See http://www.theguardian.com/environment/2011/aug/14/china-protest-against-px-chemical-plant for a report.

[4]The other four are: family planning, reform and opening up, protecting farmland, and rejuvenation through science and education.

pollution has increased, the 10% reduction in the emission of sulfur dioxide failed again during the Eleventh Five-Year Plan. The role of local governments in these failures draws much comment. Chen et al. (2012) and Ghanem and Zhang (2014) find apparent cheating and manipulation of the aggregate environment-related statistics by local governments. Jia (2014) argues that the promotion incentives of local officials will cause more pollution. Kahn et al. (2015) confirm the incentives among local officials to pollute their neighbours.

At the end of 2006, the Ministry of Environmental Protection announced a list of firms, named *Key Monitored Enterprises* across the whole country [5]. They were selected in the following way. The Ministry of Environmental Protection listed all the firms in the country emitting sulfur dioxide, smoke and industrial dust, and ranked them from highest emission to lowest. The Key Monitored Enterprises were the firms which accounted for 65% of the total amount of emissions. Following this rule, the Ministry of Environmental Protection pinned down 3592 firms as the Key Monitored Enterprises producing air pollution. For simplicity, I focus in this paper only on the 3592 firms emitting air pollutants.

The Ministry of Environmental Protection required its local branches to monitor these key firms carefully. In the regulations, it raised several main points. The local branches of the Ministry of Environmental Protection had to investigate the emission of pollutants by these firms every month, and report the numbers every quarter. The numbers would be compared with those reported automatically by monitoring instruments, which would perhaps reduce the possibility of misreporting. Local governments were required to take steps to reduce their emissions, including certificates of their emission quotas, more environmental expenditure on them and more clean technology to use. In general, the cost to these firms of emitting pollutants would become much higher than before.

2.3.2 Political Incentives of Local Officials in China

A fundamental institution in China is its decentralized economic governance and centralized political governance (Xu, 2011). On the one hand, sub-national governments have taken control of local economic developments since 1978. In spite of this, on the other hand, political authority is still centralized. Sub-national officials are appointed by the upper-level government and not elected by citizens. In other words, their career paths are totally determined by upper-level officials while they must comply with controls on resources and on the freedom of local policies to increase the likelihood of being promoted. As a result, how the Chinese Communist Party (CCP) appoints

[5] http://gcs.mep.gov.cn/zhxx/200801/t20080115_116297.htm

and promotes officials has become one of the most basic questions in the research on Chinese politics.

The CCP has built a system, similar to the "nomenklatura" in the Soviet Union, to manage all its cadres. In this system, the appointment, promotion, transfer and dismissal of central and local officials are all evaluated and put into action by the CCP. During and after the reform era that began in 1978, the cadre management system became less subjective and several procedures and standards were revealed (Chinese Communist Party, 2002). Before a decision (for example, promotion or appointment to a new position) about an official is made, some upper-level government officials will rate his performance, talk with him in person and collect comments from his colleagues. Nevertheless, the specific rules upon which the evaluations are made remain unclear. Many papers try to deduce the rules for promoting local officials in China. A widely accepted determinant in promotion decisions on local officials is economic performance within their region (see Xu (2011) for a survey). For example, Li and Zhou (2005) find that better economic performance raises the likelihood of promotion for provincial leaders. Competition in economic performance between local officials resulting from such a promotion mechanism is then regarded as the key explanatory point for China's economic miracle.

While economic performance plays a vital role in promotion, it is not the only factor. Jia et al. (2015) show that connections act as a complement to economic performance. After collecting comprehensive data on the backgrounds of local officials from county to provincial level, Landry et al. (2015) find that economic performance plays a great role at the county and the prefectural level but no role at all at the provincial level. Zheng et al. (2014) show that better environmental performance is associated with an increased probability that a governor will be promoted. Zuo (2015) finds the municipal governors are often promoted when they give a good performance in social policies and public welfare.

Besides these performance-based scores, there are some mandatory rules in promotion, such as age, tenure and the political cycles of local officials. In this paper, I mainly focus on the political incentives brought by age. According to the Regulations for the Selection and Appointment of Party Leaders and Officials (Chinese Communist Party, 2002), an official is required to hold the same office for at least three years before he gets his next assignment or retires, which he must do at the age of 60. As a result, a local official older than 57 is very unlikely to be promoted, since if he were he would reach 60 without the full preceding tenure. An official younger than 57 does not have to worry about this age limit and the effort he puts in to various types of performance is beyond question useful in evaluating his record for possible promotion

(Xi et al., 2015).

2.4 Data

To empirically evaluate the impact of the firm-based regulations, I employ data from several different sources. In this section, I introduce them with some basic information. I combine these datasets and finally construct a municipality-level panel of data from 2004 to 2009.

2.4.1 Firm-level Statistics

I exploit rich firm-level information from Chinas Annual Survey of Industrial Production, a survey conducted by the National Bureau of Statistics. It has been an annual census since 1998 containing all state-owned firms and the non-state industrial firms with sales of more than 5 million Yuan. It has been used widely to study industrial production in China (for example, Hsieh and Klenow (2009) and Song et al. (2011)).

To fit into the period of the environmental regulations, I use a firm level data set from 2004 to 2009. It consists of over 270,000 firms in 2004 and nearly 320,000 in 2009. I use unique firm IDs to link them over years. In some cases like acquisitions and mergers, firms IDs may change. I use methods and codes provided by Brandt et al. (2012) to provide a more precise matching. They link firms over time using their Chinese name, address and telephone number in addition to their unique IDs[6].

To link the 3592 Key Monitored Enterprises to observations in the Annual Survey of Industrial Production, I first match the Key Monitored Enterprises' Chinese names provided by the Ministry of Environmental Protection. Regarding the firms unmatched by name, I search their addresses manually on the Internet and match them with observations in the Annual Survey of Industrial Production on addresses. Finally, it was found that 2816 out of the 3592 Key Monitored Enterprises could be matched to the sample in the Annual Survey of Industrial Production. These 2816 matched Key Monitored Enterprises account for 7.8% of the total employment of industrial firms in 2006. The remaining unmatched firms are mostly small privately-owned plants judging by their names and other publicly available information, which is not included in the Annual Survey of Industrial Production, because the survey investigated only non-state firms with sales of more than 5 million Yuan and all state-owned firms.

[6] www.econ.kuleuven.be/public/n07057/china

2.4.2 Aggregate-level Pollution Outcomes

I collected aggregate level data on pollution from two different sources. The first is an official data set from the China City Statistics Yearbooks. These Yearbooks provide a series of environmental variables in every municipality in China from 1993, including investment in pollution abatement covering industrial emissions of pollutants in water, sulfur dioxide and industrial dust.

To avoid manually misreported pollution outcomes (Chen et al., 2012; Ghanem and Zhang, 2014), I turn to satellite data for robustness checks. The National Aeronautics and Space Administration (NASA) has provided data on Aerosol Optical Thickness (AOT) from 2000, based on observations from the Moderate Resolution Imaging Spectroradiometer (MODIS). AOT is the degree to which aerosols prevent the transmission of light by absorption or scattering of light. It is closely related to air quality. For example, Wang and Christopher (2003) calculate the correlation between AOT and PM 2.5/Air Quality Index to be higher than 0.7. Jia (2014) uses these data as proxies for air quality in China.

2.4.3 Data on Local officials

The information on local officials is extracted from Chen (2015). In his paper, he digitizes resumes of municipality Party secretaries, from 2000 to 2010, which mention 989 individual leaders, from 333 prefectural-level cities (municipalities) in 27 provinces. The distribution of the leaders ages is plotted in Figure 2.4.

It should be noted that any jurisdiction in China has two local leaders: a local governor and a local Party secretary. The governor is the head of the local government and the Party secretary is the head of the local branch of the Communist Party. In the present paper I focus on the latter. Although governors are executive officers in local governments, Party secretaries rank by law higher than governors and governors should act under the guidance of Party secretaries. Therefore in practice Party secretaries hold the power of final decision in local economic development.

Figure 2.1 presents different levels of jurisdiction in China. Municipalities are major types of prefectural-level jurisdiction. According to Bo (2016), in the "turning prefectures to prefecture-level municipalities" reform, decision rights in local governance, including fiscal, political and administrative power, were centralized from counties to municipalities. After the reform, instead of Party secretaries in component counties, municipal Party secretaries became the most important local leaders in their municipality. The reform completed in 2003, so in this paper I start examining the period from 2004 to ensure that municipal Party secretaries were the ones responsible for the

economy in their own jurisdiction.

2.5 Empirical Strategy

The empirical model follows the strategy in Greenstone (2002, 2003). The main empirical specification in this paper is the following triple differences equation:

$$\%\Delta Y_{it} = \beta_1 Regulate_i \times Incentive_{mt} \times Post07_t + \beta_2 Regulate_i \times Post07_t$$
$$+\beta_3 Regulate_i \times Incentive_{mt} + \sum_{\tau=04}^{09} \beta_{4\tau} I(t=\tau) Incentive_{mt} + X_{it} + \delta_{pt} + \theta_{st} + \epsilon_{it} \tag{2.20}$$

Here i indexes a plant, belonging to municipality m and province p, $I(\cdot)$ is an indicator function. The term $\%\Delta Y_{it}$ is the outcome variable (employees and outputs in most cases) measured as the percentage change between year t and year $t-1$. It is defined as:

$$\%\Delta Y_{it} = \frac{Y_{i,t+1} - Y_{it}}{(Y_{i,t+1} + Y_{it})/2} \tag{2.21}$$

This is an alternative to the difference between the logarithms of year $t+1$ and those of year t. It is a second-order approximation of the log difference, ranging from -2 to +2. Expansions and contractions are portrayed symmetrically. The largest benefit of taking this measurement is that it can allow the entry and exit of firms. For example, the value of $\%\Delta Y_{it}$ of an exiter i operating in year t but not in year $t+1$ is equal to -2.

The key independent variable is the triple interaction $Regulate_i \times Incentive_{mt} \times Post07_t$, whose effect on outcome variables is captured by the coefficient β_1. $Regulate_i$ denotes the regulatory status of firm i at year t. Its definition changes in different regressions, depending on context. It is explained in detail in the following sections. $Incentive_{mt}$ denotes whether promotion is a powerful incentive for the local leader in municipality m in year t. In the baseline model, I use $Young_{mt}$, whether the Party secretary's age is less than 57, as the proxy for the incentive of promotion. As noted above, due to the strict requirement of retiring at the age of 60, municipal Party secretaries older than 57 are almost impossible to promote. As a result, they have low promotion incentives compared to those younger than 57, who may still be able to complete a 3-year tenure in a new position. I will also use the connectivity of local officials as a robustness check.

For a formal triple differences model, besides the interaction $Regulate_i \times Incentive_{mt} \times Post07_t$, we still need to add $Regulate_i \times Post07_t$, $Regulate_i \times Incentive_{mt}$, $Incentive_{mt} \times$

$Post07_t$, $Incentive_{mt}$, $Post07_t$ and $Regulate_i$ in the model. I control for $Regulate_i \times Post07_t$ and $Regulate_i \times Incentive_{mt}$ in the regression. $Incentive_{mt} \times Post07_t$ and $Incentive_{mt}$ are contained in a more flexible specification $\sum_{\tau=04}^{09} \beta_{4\tau} I(t=\tau) Incentive_{mt}$, but the results do not change if use $Incentive_{mt} \times Post07_t$ and $Incentive_{mt}$ instead. $Post07_t$ is absorbed by province by year fixed effects δ_{pt}. $Regulate_i$ is eliminated in the first difference transformation.

In the regression, I control for officials' tenure and age, year by province fixed effects and year by sector fixed effects. As an official can be the leaders of different municipality leaders, I can also control for officials' individual fixed effects. I also control for the interactions between firms' ownership and year dummies. Time-invariant firm characteristics will be eliminated in the percentage changes setting. Thus, the coefficient β_2 alone captures the effect on the regulated firms relative to unregulated, in municipalities with poorly incentivized leaders; β_3 captures the pre-existing gap between regulated and unregulated firms in municipalities with highly incentivized leaders, relative to municipalities with poorly incentivized leaders.

β_3 gives a level difference between regulated and unregulated firms under highly incentivized leaders relative to poorly incentivized leaders before the regulations were put into effect. We still need an extension of the baseline model to estimate the relative trend of the above difference for identification, as well as the level difference captured by β_3. The identification assumption requires that the relative trend between regulated and unregulated firms would be the same in a municipality with a highly incentivized local leader or a poorly incentivized one, in the absence of these regulations. I check this by estimating a flexible triple difference model:

$$\begin{aligned}\%\Delta Y_{it} = \sum_{\tau=04}^{09} \beta_{1\tau} I(t=\tau) Regulate_i \times Incentive_{mt} + \sum_{\tau=04}^{09} \beta_{2\tau} I(t=\tau) Regulate_i \\ + \sum_{\tau=04}^{09} \beta_{4\tau} I(t=\tau) Incentive_{mt} + X_{it} + \delta_{pt} + \theta_{st} + \epsilon_{it}.\end{aligned} \tag{2.22}$$

This regression model allows the treatment effect to vary by calendar years. The key interested coefficients are $\beta_{1\tau}$. I expect $\beta_{1\tau}$ have the same sign as β_1 in the baseline model 2.20 when $\tau > 2007$, while take the opposite sign or equal to zero when $\tau \leq 2007$. If it is the case, then the results of the baseline model 2.20 are not due to the pre-existing trends but can be interpreted as casual effects from the 2007 regulation.

2.6 Main Results

2.6.1 Impact on Regulated Firms

First, I evaluate the direct effects of the regulations on the targeted firms. The regression model is:

$$\%\Delta Y_{it} = \beta_1 Regulated_i \times Young_{mt} \times Post07_t + \beta_2 Regulated_i \times Post07_t$$
$$+\beta_3 Regulated_i \times Young_{mt} + \sum_{\tau=04}^{09} \beta_{4\tau} I(t=\tau) Young_{mt} + X_{it} + \delta_{pt} + \theta_{st} + \epsilon_{it} \quad (2.23)$$

where $\%\Delta Y_{it}$ is the employees or output of firm i measured as the percentage changes between year t and year $t-1$, $Regulated_i$ denotes whether firm i is listed as one of the *Key Monitored Enterprises* and is subject to the strict regulations, $Young_{mt}$ denotes whether the Party secretary is young (aged below 57) and promotion is a powerful incentive, $Post07_t$ is a dummy variable taking the value of 1 since 2007. The coefficient we are interested in is β_1, the coefficient of the triple interaction $Regulated_i \times Young_{mt} \times Post07_t$.

The results are presented in Table 2.2. Column (1) presents the coefficients on percentage changes in employees. We can see that the regulations produce significantly higher adverse effects on regulated firms located in municipalities with highly incentivized local leaders, compared with those in municipalities with poorly incentivized leaders. To be specific, the effects on regulated firms with poorly incentivized municipality Party secretaries are statistically no different from zero. In contrast, the regulation will reduce 34.4% of the employees in regulated firms in municipalities with highly incentivized leaders. Similar results appear in Column (2), showing the regression of firms' outputs. The negative impact on the outputs of regulated firms is 31.6% larger in municipalities with highly incentivized leaders.

It is not surprising that the stricter environmental regulations have adverse effects on regulated firms and industries. However, it is interesting to find that a higher promotion incentive will amplify such negative effects. Jia (2014) shows that highly incentivized officials will resort to more polluted inputs. In a similar framework, I find in this section that when we put strict regulation on polluted firms, highly incentivized officials will also cut their economic activities more. We see that the firm-based regulations can cut more employees and output in those places where the local leaders have a greater possibility of being promoted. Combined with the results in Jia (2014), the present results suggest that local officials with more political incentive are more responsive to the changes in relative cost of polluting inputs, whether positive or

negative as price shocks. This confirms that political incentives serve to complement economic performance in the promotion of local officials as suggested by the theoretical model.

The significantly positive β_3, the coefficient of $Regulated_i \times Young_{mt}$, indicates that before the regulations, young leaders tended more than old leaders to favour regulated firms over unregulated firms. This is reasonable because the first order conditions in the theoretical model predict that highly incentivized leaders will use more polluted inputs than poorly incentivized leaders, which is also the main conclusion in Jia (2014). She argues that higher promotion incentives will induce officials to choose a higher level of pollution. A positive β_3 in this paper suggests the similar conclusion: without the existence of the environment regulations, in equilibrium young leaders prefer those dirty firms.

To extend the estimation of the pre-existing difference to the relative trend, I estimate the dynamic impact of the regulations:

$$\%\Delta Y_{it} = \sum_{\tau=04}^{09} \beta_{1\tau} I(t=\tau) Regulated_i \times Young_{mt} + \sum_{\tau=04}^{09} \beta_{2\tau} I(t=\tau) Regulated_i + \sum_{\tau=04}^{09} \beta_{4\tau} I(t=\tau) Young_{mt} + X_{it} + \delta_{pt} + \theta_{st} + \epsilon_{it}. \quad (2.24)$$

The results are shown as a graph in Figure 2.5. We can see that without the 2007 regulations, the gap between regulated and unregulated firms becomes larger in highly incentivized municipalities than in poorly incentivized municipalities. The regulations significantly reverse the enlarging gap. This suggests that the estimated coefficient $\beta_{1\tau}$ is not naturally inherited from some negative pre-existing trend, but induced by the 2007 regulations.

2.6.2 Impact on Unregulated Firms in Polluting Industries

Next, I evaluate the indirect effects of the regulations on firms without regulation in polluting industries. I define the polluting industries based on Ministry of Environmental Protection (2010) as the following two-digit industries: production and supply of electric power and hot power, non-metal minerals products, smelting and pressing of ferrous metals, raw chemical materials and chemical products, smelting and pressing of non-ferrous metals, petroleum, coking and nuclear fuel processing, papermaking and paper products agricultural and sideline products, and timber processing, bamboo,

cane, palm fiber and straw products. The regression model is:

$$\%\Delta Y_{it} = \beta_1 RegExpose_m \times Young_{mt} \times Post07_t + \beta_2 Regulate_i \times Post07_t$$
$$+\beta_3 RegExpose_m \times Young_{mt} + \sum_{\tau=04}^{09} \beta_{4\tau} I(t=\tau) Young_{mt} + X_{it} + \delta_{pt} + \theta_{st} + \epsilon_{it} \tag{2.25}$$

where $Young_{mt}$ denotes whether the Party secretary is young (age is smaller than 57) and promotion is a powerful incentive for him, $Post07_t$ is a dummy variable taking value of 1 since 2007. Since the sample is limited to those firms which were not regulated, we need to extend the regulatory status $Regulated_i$ to some other variables measuring the shock of the regulations on unregulated firms. Therefore, I use $RegExpose_m$, which denotes municipality m's exposure to the regulations at year t. I calculate the number of employees in regulated firms in each municipality or the number of regulated firms in each municipality as proxies for the intensity of the regulation an unregulated firm is exposed to. The intuition related to the "exposure" measurement is that, when more firms in a region receive regulation, the input markets and the output market will be affected more, then in the general equilibrium the unregulated firms will get more affected by changes in equilibrium price and quantities of inputs and output.

The DDD estimator β_1 is still the interested coefficient. We can see from Table 2.3 that when a local leader with more incentives to be promote, he will incline to transfer the pressure of unemployment from regulated firms to other unregulated firms in polluting industries. No matter measured in the number of employees in regulated firms or the number of regulated firms in the same municipality, increases in municipalities' exposure to the regulation will induce a more positive impact in those with highly incentivized Party secretaries. A 1% increase in the number of employees in regulated firms has no significant effects on unregulated firms in a municipality with low promotion incentives, while it will increase 0.5% in employees and 0.3% in outputs on unregulated firms if this municipality has a highly incentivized leader. The corresponding effects brought by one more regulated firms are 0.4% in employees and 0.3% in outputs in a municipality with high promotion incentives. In this regression, the sample is all unregulated firms in polluting industries, since they may be more likely to receive the spillover effects from the regulation. I also perform the same regression using the sample of all unregulated firms, both in polluting industries and clean industries as a robustness check. The dynamic impact by allowing flexible coefficients are presented in Figure 2.6. There does not exist any upward trend before 2007 which can threaten the validity of the identification assumption.

2.6.3 Overall Impact on Aggregate Economic Activities

Previous firm-level results suggest that environmental regulation will produce a heterogeneous impact on places with local leaders who have different levels of political incentives. When the municipality Party secretary is young, i.e. more incentives to improve the local economy for promotion, the loss in employment and output in regulated firms will be associated with gains in other unregulated firms. The prediction about the impact of the firm-based regulations on the whole polluting industries is ambiguous, since the negative effects on the regulated firms may be offset by the increases brought by those unregulated ones. In this subsection, I am going to estimate the overall impact on the polluting industries overall at aggregated municipality level using the following specification:

$$Y_{mt} = \beta_1 RegExpose_m \times Young_{mt} \times Post07_t + \beta_2 RegExpose_m \times Post07_t$$
$$+\beta_3 RegExpose_m \times Young_{mt} + \sum_{\tau=04}^{09} \beta_{4\tau} I(t=\tau) Young_{mt} + X_{it} + \delta_{pt} + \alpha_m + \epsilon_{it} \tag{2.26}$$

From Table 2.4 we cannot find any robust evidence about a clear positive or negative effects on the whole polluting industries. This suggests that the adverse shock on regulated firms brought by the regulations will be totally offset. The firm-based regulations do not harm the economic activities of polluting industries as a whole. Inputs and outputs from the regulated firms are reallocated to other unregulated firms.

2.6.4 Impact on Air Pollution

Another interesting outcome to measure is air quality. I have shown that, in polluting industries, the economic activities of regulated firms are almost fully reallocated to unregulated firms in polluting industries. It is interesting to check whether such a reallocation can bring a better environment. There are two possibilities. If the unregulated firms adopt cleaner technology, the reallocation will improve the air quality. However, if the Key Monitored Firms are regulated only because they are large firms and emit more sulfur dioxide and dust with the same or even cleaner technology than unregulated firms do, the environmental quality will not be improved. To check this,

I evaluate the following specification:

$$Y_{mt} = \beta_1 RegExpose_m \times Young_{mt} \times Post07_t + \beta_2 RegExpose_m \times Post07_t$$
$$+\beta_3 RegExpose_m \times Young_{mt} + \sum_{\tau=04}^{09} \beta_{4\tau} I(t=\tau) Young_{mt} + X_{it} + \delta_{pt} + \alpha_m + \epsilon_{it} \tag{2.27}$$

where Y_{mt} is a certain air quality measurement. Here I use the municipality-level emission of sulfur dioxide and industrial dust as the outcome variable, digitized from China City Statistics Yearbook. Table 2.5 presents the results. We can see that, after the regulations, municipalities with highly incentivized officials emit less sulfur dioxide and industrial dust than those with poorly incentivized officials. Combined with the results from the previous sections that municipalities with highly incentivized officials reallocate more inputs in polluting industries from regulated firms to unregulated firms, Table 2.5 suggests that unregulated firms have cleaner technology.

2.7 Robustness

2.7.1 Measurement of Local officials' Political Incentives

There are several concerns about using the age of 57 years as a threshold of high or low political incentives. First, given the rule about tenure and retirement, age 57 is not an arbitrary number, but one may still worry about how robust it would be to use a different number as a threshold. To check the robustness, I repeat here the baseline regression using age 55 as the threshold. Intuition would suggest that, as age increases, the political incentive of an official to be promoted will decline. The results are presented in Table 2.6. We can see that results are similar but slightly smaller than the baseline estimates.

Second, it is possible that after 2007 the upper-level government appointed some old local officials to posts in highly-polluted areas to ensure that the environmental regulations would be implemented well. For this reason, I limit the sample in the baseline regression to those municipalities which had no new appointments around 2007. Two-thirds of all municipalities did not change their Party secretaries in 2006 and 2007. I extend the sample to years with the same Party secretaries as in 2006 and 2007 in these municipalities. This yields a sub-sample with about 30% of the number of observations in the whole sample. Here I use as an indicator of political incentives whether the Party secretary was younger than 54 on his appointment. The rationale is that he had to finish a 3-year tenure before he could be considered for

promotion. If he was older than 54 upon appointment, then, given the 60-year-old retirement restriction, he would have been older than 57 after a 3-year tenure and thus hard to consider for promotion and a fresh 3-year tenure in the new position. As the individual's age on his appointment is time-invariant, the regression can be written as:

$$\%\Delta Y_{it} = \beta_1 Regulated_i \times Young_m \times Post07_t + \beta_2 Regulated_i \times Post07_t$$
$$+\beta_3 Regulated_i \times Young_m + \sum_{\tau=04}^{09} \beta_{4\tau} I(t=\tau) Young_m + X_{it} + \delta_{pt} + \theta_{st} + \epsilon_{it} \quad (2.28)$$

The results are presented in Table 2.7. We can see that the magnitudes and the significance of most coefficients are not changed much when using the time-invariant appointment age as the proxy for political incentives. The regulated firms under highly incentivized local officials have to reduce their employees and output more than those under poor incentivized ones. It is worth noting that β_3, the coefficient of $Regulated_i \times Young_m$ is significantly negative in Column (1) and (2), while in the baseline results it is positive. β_3 measures the gap in economic activities of regulated firms between municipalities with highly and poor incentivized local officials prior to 2007. The significantly positive estimate in the baseline results is consistent with theoretical predictions and previous literature (Jia, 2014): local officials with higher promotion incentives will choose more dirty input without regulation. However, in this robustness checks we find that local officials appointed before 54 years old tend to choose a lower level of the dirty input. An explanation for this is that incentives may vary with time, so the time-invariant appointment age may capture officials' incentives incompletely. For example, for a Party secretary who got his position at 55 years old in 2005, though by rule he had to stay at least three years in the position and then became too old for promotion, practically he still had some chances to be promoted before finishing the 3-year tenure if he had some extraordinary performance. As a result he may work hard (for example increase investments in polluting firms) in 2005 and 2006 trying to "take the last chance" to be promoted before he reaches 57 years old. Nearly 20% officials once experienced a tenure less than three years in my sample. If these officials are excluded in this robustness check, the magnitude of β_3 will be not significantly different from zero. For conciseness I do not report this result here. It suggests that the negative β_3 in Column (1) and (2) of Table 2.7 can be explained by the additional incentives by those appointed after 54 years old but still younger than 57.

In the baseline part I use age to proxy for local officials' incentives for promotion. The promotion rule in China is more an empirical than a theoretical question. As

introduced in the background section, above, connections are regarded as a significant complement to economic performance in the way that people are promoted (Jia, 2014). For this reason I digitize information on the connectivity of municipal Party secretaries as other proxies of political incentive, to check the robustness of the use of age in the baseline results. Connectivity is a highly complicated question since there are many possible ways to connect two officials. Normally researchers identify officials as connected if they are from the same hometown, the same school or have ever worked in the same place. It is relatively easy to document the connectivity between province-level and central officials, due to detailed resume information and the limited number of officials. In my paper, the difficulty of identifying the connectivity between municipal-level and province-level officials is high. In practice, I identify a municipal leader and a province or central leader who are from the same hometown or have worked in the same municipality as connected officials. I use the same empirical strategy as in the baseline regression. The results are listed in Table 2.8. It can be found that the results are quite similar to the baseline results. Because my measurement may incompletely capture the true extent of connectivity, I use the baseline regression in preference, using age as the proxy for political incentives.

2.7.2 Impact on Unregulated Firms in All Industries

In the baseline results I evaluate the unintended effects of the regulations using the sample of firms without regulation in polluting industries. Here, I also estimate the same model using the sample of all unregulated firms, in both polluting industries and clean industries, as a robustness check. The results are listed in Table 2.9. We find that the response of unregulated firms in clean industries is similar to that from unregulated firms in polluting industries.

2.7.3 Measurement of Regulation Exposure of Unregulated Firms

In the baseline regressions, I use two definitions of regulation exposure on unregulated firms. One is the number of employees in regulated firms in each municipality and the other is the number of regulated firms in each municipality, yielding regression results in Table 2.3 for unregulated firms in polluting industries and Table 2.9 for unregulated firms in all industries. Here, I check the robustness of alternative measurements of regulation expose. In Table 2.10, I use the percentage of employment of regulated firms in the whole municipality as the measurement. The results predict similar patterns for unregulated firms. When the share of regulated firms' employment in the whole

municipality is 1% larger, an unregulated firm in the same municipality can have a 0.143% positive spillover effects on its employment and 0.255% on its output.

2.7.4 Measurement of Pollution

In Table 2.11 I estimate the impact of the firm-based regulations using Aerosol Optical Thickness (AOT) as the proxy for air quality to avoid the risk of misreporting in China's official air quality data (Chen et al., 2012). We see that the results are similar to those using officially-reported emissions of pollutants.

2.7.5 County Level Exposure

In the baseline regression I control for year by province fixed effects and use the number of employees in regulated firms in the same municipality or the number of regulated firms in the same municipality as proxies for the intensity of regulation that an unregulated firm is exposed to. Here as a robustness check I control for year by municipality fixed effects. In this case, I have to find variations in exposure to the regulations at a sub-municipal level. For this reason, I calculate the number of employees in regulated firms in the same county or the number of regulated firms in the same county. To be specific, the regression equation is:

$$\begin{aligned}\%\Delta Y_{it} = \beta_1 RegExpose_c \times Young_{mt} \times Post07_t + \beta_2 RegExpose_c \times Post07_t \\ + \beta_3 RegExpose_c \times Young_{mt} + X_{it} + \delta_{mt} + \theta_{st} + \epsilon_{it} \quad (2.29)\end{aligned}$$

where $RegExpose_c$ is exposure to the regulations at a county level, measured by the number of employees in regulated firms in the same county or the number of regulated firms in the same county. The results are listed in Table 2.12. We find a similar pattern to that in the baseline regression. In general, I prefer the baseline specification because municipalities after 2003 took the place of counties as basic units of local governance. Migration across counties but within a municipality is relatively easy. The specification controlling for municipality fixed effects but using county-level exposure may ignore possible spillovers across counties. Nevertheless, *ex post* the two specifications yield similar results.

2.8 Conclusion

Economic consequences are considered to be one of the most significant things to evaluate before policy makers make decisions to institute environmental regulations.

Recent studies normally conclude that the regulations are associated with reductions in manufacturing activities on regulated firms and industries (Henderson, 1996; Greenstone, 2002). However, the previous literature rarely evaluates the unintended impact of environmental regulations on non-directly targeted firms and industries; therefore there is little evidence of the general equilibrium results on the whole economy.

At the same time, in developing countries, political factors play a significant role in environmental issues (Burgess et al., 2012; Jia, 2014). It remains unclear whether different politicians will induce the effects of national environmental regulations to be heterogeneous in practice.

This paper uses a set of firm-based environmental regulations in China to estimate the economic consequences on different types of firms and industries. The environmental regulations by the Ministry of Environmental Protection in China, starting from the beginning of 2007, targeted firms which accounted for 65% of the emission of sulfur dioxide and industrial dust. I take local governments into account and try to use the political incentives of local officials to explain the responses in different firms and industries.

I link the list of these regulated firms with a variety of datasets from different sources to evaluate the impact of the regulations on different firms and industries. I use the plausibly exogenous variations in local officials' ages to identify within-official changes in promotion as an incentive. The promotion and retirement rules of the CCP determine that officials younger than 57 years old have a clear incentive to put more effort into earning promotion. I use a triple differences strategy to identify the effects of the 2007 firm-based regulations.

The triple differences specification finds that the regulations produce significantly higher adverse effects on regulated firms located in municipalities with highly incentivized local leaders than in those with poorly incentivized leaders. At the same time, the highly incentivized local leaders will be inclined to transfer the pressure of unemployment created thereby from regulated firms to other unregulated firms in polluting industries. Overall, the firm-based regulations will not have an adverse impact on the economic activities of the industries as a whole. The results suggest that although regulation will have a negative impact on regulated firms, inputs can be reallocated to unregulated firms to offset the adverse effects of the regulations. I find that the overall air quality will be improved. In general, when promotion is a powerful incentive for local officials, the firm-based regulations can improve environment quality without a significant cost in economic activities.

Evaluating the cost of unintended consequences on all industries will inform the policy makers about ways to design employment transition assistance programs that

provide job training and temporary compensation for the job losses that result from unintended adverse effect of the regulations. This paper suggests that making use of the political incentives of local officials to enforce environmental regulations on targeted firms using dirty technology is a good strategy for improving environmental quality without sacrificing economic development.

Figures and Tables

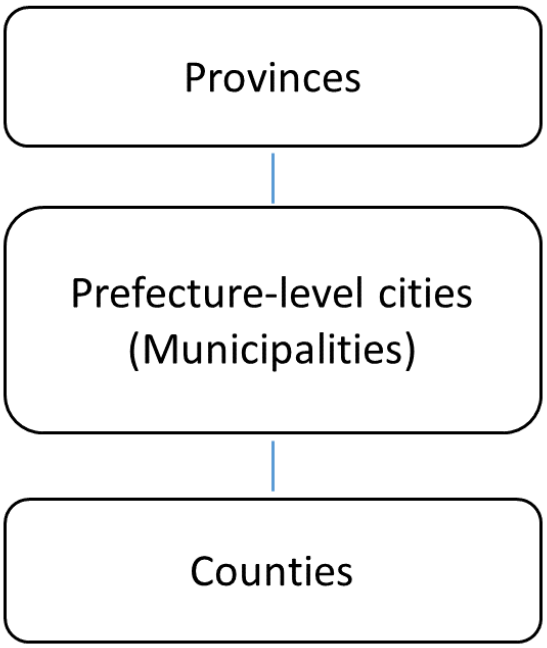


Figure 2.1: Hierarchy of jurisdictions in China

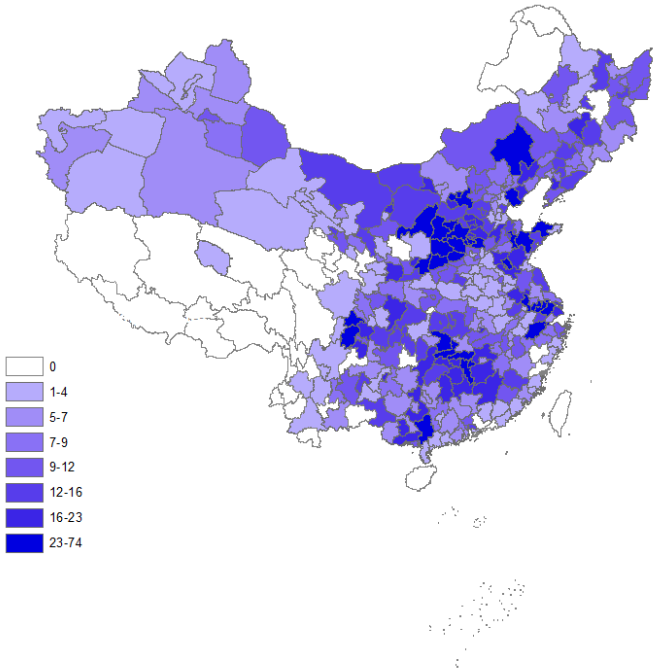


Figure 2.2: Number of regulated firms by municipality

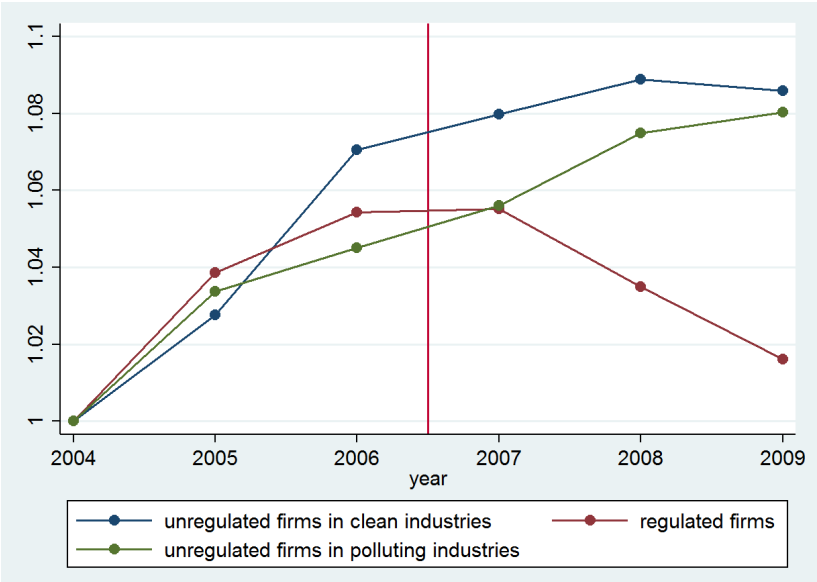


Figure 2.3: Trends of employees of firms in different industries
Note: Normalized to 2004 level.

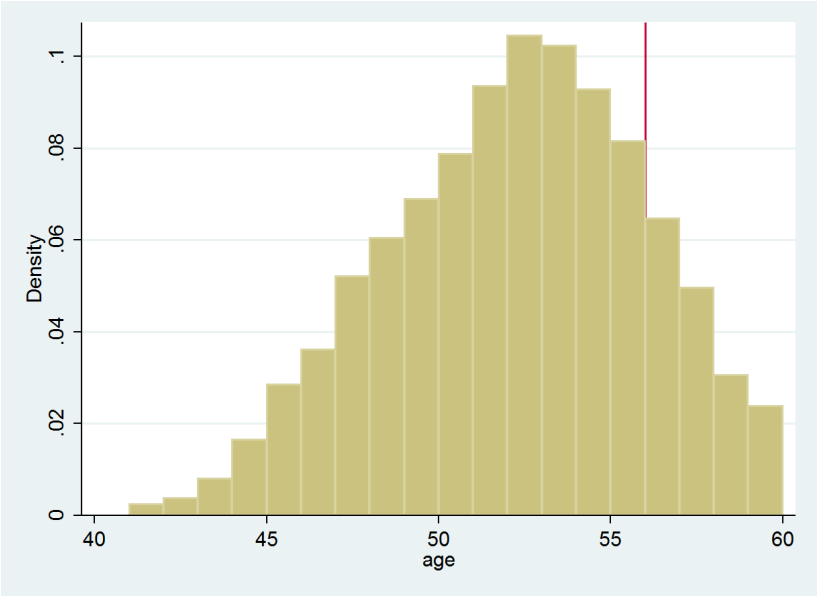


Figure 2.4: Distribution of ages of Party secretaries

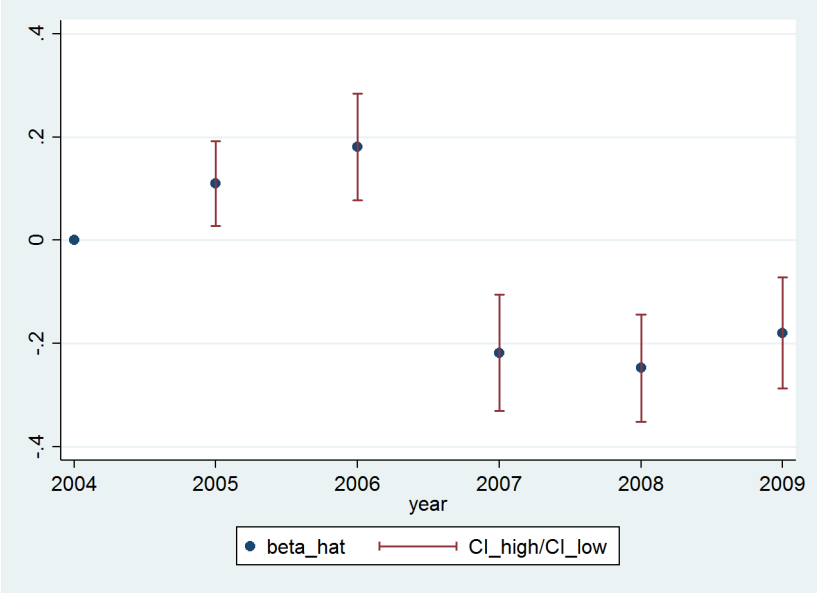


Figure 2.5: Dynamic impacts on employees in regulated firms
Note: coefficients normalized to 2004 level.

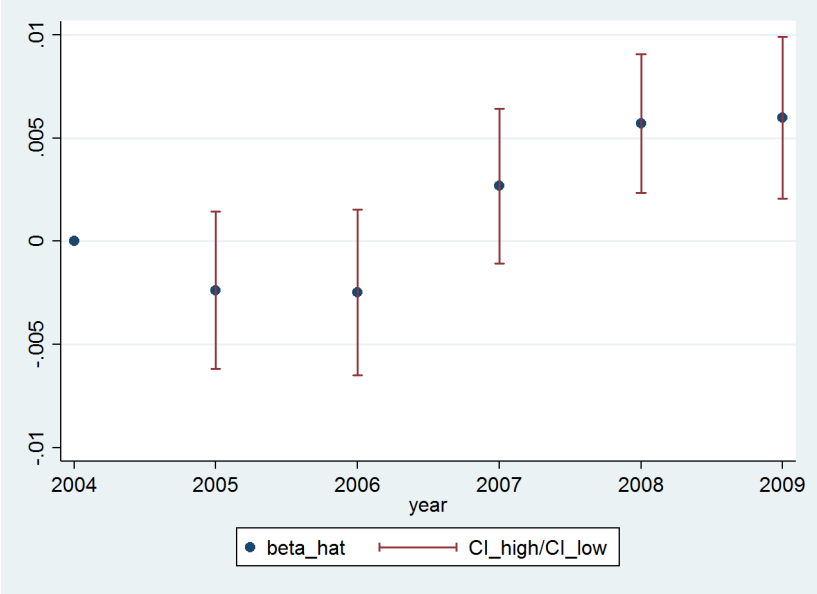


Figure 2.6: Dynamic impacts on employees in unregulated firms in polluting industries
Note: coefficients normalized to 2004 level.

Table 2.1: Summary Statistics

	(1)	(2)	(3)	(4)
Year	2005	2006	2007	2008
Panel A: Regulated firms				
Employees	1975.43	2010.80	2026.05	2007.08
	(7042.78)	(7307.01)	(7270.71)	(7352.99)
Outputs	1168.71	1776.42	1797.47	2150.60
	(4554.34)	(10440.15)	(6852.12)	(8338.32)
Panel B: Unregulated firms				
Employees	205.44	220.29	226.60	236.65
	(952.33)	(940.17)	(957.33)	(899.88)
Outputs	636.09	798.71	1074.41	1346.76
	(622.04)	(778.19)	(2614.45)	(4176.77)

† Notes: Standard deviation in parentheses. Output is measured in 1 million Yuan.

Table 2.2: Impacts on Regulated Firms

VARIABLES	(1) Δ employees	(2) Δ output
young×regulated×post07	-0.344***	-0.316***
	(0.025)	(0.021)
regulated×post07	0.002	-0.005
	(0.002)	(0.015)
young×regulated	0.302***	0.290***
	(0.010)	(0.011)
Year×sector FE	✓	✓
Year×province FE	✓	✓
Observations	444,529	442,722
R-squared	0.090	0.421

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage changes in employees and outputs. Main independent variable is the interaction between whether the firm is regulated, whether the municipality Party secretary is younger than 57 years old and the post-2007 dummy. Year×province fixed effects and year×sector fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.3: Impacts on Unregulated Firms in Polluting Industries

VARIABLES	(1) Δ employees	(2) Δ output	(3) Δ employees	(4) Δ output
young×regulated emp×post07	0.005***	0.003***		
	(0.001)	(0.001)		
regulated emp×post07	0.002	0.003		
	(0.003)	(0.002)		
young×regulated emp	0.001	-0.000		
	(0.002)	(0.002)		
young×# of regulated firms×post07			0.004*	0.003*
			(0.002)	(0.002)
# of regulated firms ×post07			-0.002	-0.001
			(0.002)	(0.002)
young×# of regulated firms			0.001	-0.001
			(0.001)	(0.001)
Year×sector FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	431,431	429,520	431,431	429,520
R-squared	0.262	0.542	0.261	0.526

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage changes in employees and outputs. Main independent variable is the interaction between the intensity of regulation in the same municipality (the number of employees in regulated firms in the municipality or the number of regulated firms in the municipality), whether the municipality Party secretary is younger than 57 years old and the post-2007 dummy. Year×province fixed effects and year×sector fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.4: Overall Impacts on Pollution Industries – Municipality Level

VARIABLES	(1) Δ employees	(2) Δ output	(3) Δ employees	(4) Δ output
young×regulated emp×post07	-0.019	0.031		
	(0.015)	(0.025)		
regulated emp×post07	0.003	-0.057**		
	(0.015)	(0.022)		
young×regulated emp	0.003	0.022		
	(0.016)	(0.032)		
young×# of regulated firms×post07			-0.024**	0.025
			(0.011)	(0.027)
# of regulated firms ×post07			0.021**	-0.007
			(0.010)	(0.022)
young×# of regulated firms			-0.003	0.008
			(0.004)	(0.015)
Municipality FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	1,672	1,672	1,672	1,672
R-squared	0.628	0.997	0.632	0.997

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage changes in employees and outputs. Main independent variable is the interaction between the intensity of regulation in the same municipality (the number of employees in regulated firms in the municipality or the number of regulated firms in the municipality), whether the municipality Party secretary is younger than 57 years old and the post-2007 dummy. Municipality fixed effects and year×province fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.5: Overall Impacts on Air Quality

VARIABLES	(1) so2	(2) dust	(3) so2	(4) dust
young×regulated emp×post07	-0.063***	0.055		
	(0.022)	(0.046)		
regulated emp×post07	0.001	-0.117**		
	(0.023)	(0.046)		
young×regulated emp	-0.003	-0.020		
	(0.018)	(0.039)		
young×# of regulated firms×post07			-0.025***	-0.004***
			(0.008)	(0.001)
# of regulated firms×post07			-0.003	-0.007
			(0.005)	(0.008)
young×# of regulated firms			0.004	0.000
			(0.011)	(0.008)
Municipality FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	1,596	1,656	1,596	1,656
R-squared	0.366	0.311	0.364	0.270

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are log value of sulfur dioxide and industrial dust emitted at municipality level. Main independent variable is the interaction between the intensity of regulation in the same county (the number of employees in regulated firms in the county or the number of regulated firms in the county), whether the municipality Party secretary is younger than 57 years old and the post-2007 dummy. Municipality fixed effects and year×province fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.6: Robustness: Using 55 as the Threshold Age

	(1)	(2)	(3)	(4)
	regulated vs unregulated		unregulated firms	
VARIABLES	Δ employees	Δ output	Δ employees	Δ output
young×regulated×post07	-0.306***	-0.297***		
	(0.021)	(0.014)		
regulated×post07	0.003	0.000		
	(0.012)	(0.001)		
young×regulated	0.284***	0.276***		
	(0.008)	(0.005)		
young×# of regulated firms×post07			0.003***	0.003*
			(0.001)	(0.002)
# of regulated firms ×post07			-0.002	0.000
			(0.002)	(0.001)
young×# of regulated firms			0.001	-0.001
			(0.001)	(0.001)
Year×sector FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	444,529	442,722	431,431	429,520
R-squared	0.102	0.575	0.278	0.560

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage changes in employees and outputs. Main independent variable is the interaction between the intensity of regulation in the same municipality (the number of employees in regulated firms in the municipality or the number of regulated firms in the municipality), whether the municipality Party secretary is younger than 55 years old and the post-2007 dummy. Year×province fixed effects and year×sector fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.7: Robustness: Age at Appointment

	(1)	(2)	(3)	(4)
	regulated vs unregulated		unregulated firms	
VARIABLES	Δ employees	Δ output	Δ employees	Δ output
young×regulated×post07	-0.279***	-0.251***		
	(0.039)	(0.031)		
regulated×post07	0.005	0.000		
	(0.010)	(0.001)		
young×regulated	-0.213***	-0.178***		
	(0.005)	(0.004)		
young×# of regulated firms×post07			0.005**	0.004*
			(0.003)	(0.003)
# of regulated firms ×post07			0.003*	0.002
			(0.002)	(0.002)
young×# of regulated firms			-0.000	0.001
			(0.001)	(0.001)
Year×sector FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	140,615	139,941	136,910	136,127
R-squared	0.091	0.426	0.103	0.401

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage changes in employees and outputs. Main independent variable is the interaction between the intensity of regulation in the same municipality (the number of employees in regulated firms in the municipality or the number of regulated firms in the municipality), whether the municipality Party secretary is younger than 54 years old at appointment and the post-2007 dummy. Year×province fixed effects and year×sector fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.8: Robustness: Using Connection as Political Incentives

	(1)	(2)	(3)	(4)
	regulated vs unregulated		unregulated firms	
VARIABLES	Δ employees	Δ output	Δ employees	Δ output
connect×regulated×post07	-0.415***	-0.307***		
	(0.045)	(0.034)		
regulated×post07	-0.012	0.001		
	(0.010)	(0.001)		
connect×regulated	-0.006***	-0.006***		
	(0.002)	(0.002)		
connect×# of regulated firms×post07			0.007***	0.005**
			(0.003)	(0.003)
# of regulated firms ×post07			0.006***	0.004**
			(0.002)	(0.002)
connect×# of regulated firms			0.001	0.001
			(0.001)	(0.001)
Year×sector FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	444,529	442,722	431,431	429,520
R-squared	0.101	0.538	0.159	0.667

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage changes in employees and outputs. Main independent variable is the interaction between the intensity of regulation in the same municipality (the number of employees in regulated firms in the municipality or the number of regulated firms in the municipality), whether the municipality Party secretary has connection with higher level officials and the post-2007 dummy. Year×province fixed effects and year×sector fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.9: Robustness: Impacts on Unregulated Firms in All Industries

VARIABLES	(1) Δ employees	(2) Δ output	(3) Δ employees	(4) Δ output
young×regulated emp×post07	0.007***	0.008***		
	(0.002)	(0.002)		
regulated emp×post07	0.005***	0.002***		
	(0.002)	(0.001)		
young×regulated emp	-0.005***	-0.006***		
	(0.002)	(0.002)		
young×# of regulated firms×post07			0.006***	0.004***
			(0.001)	(0.001)
# of regulated firms ×post07			0.003***	0.000
			(0.001)	(0.001)
young×# of regulated firms			0.000	-0.003***
			(0.000)	(0.000)
Year×sector FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	1,735,061	1,729,470	1,735,061	1,729,470
R-squared	0.141	0.538	0.137	0.512

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage changes in employees and outputs. Main independent variable is the interaction between the intensity of regulation in the same municipality (the number of employees in regulated firms in the municipality or the number of regulated firms in the municipality), whether the municipality Party secretary is younger than 57 years old and the post-2007 dummy. Year×province fixed effects and year×sector fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.10: Robustness: Alternative Regulation Exposure Measurement of Unregulated Firms

	(1)	(2)	(3)	(4)
	unreg firms in polluting indus		unreg firms in all indus	
VARIABLES	Δ employees	Δ output	Δ employees	Δ output
young×% regulated emp×post07	0.143*	0.255***	0.138***	0.213***
	(0.081)	(0.074)	(0.059)	(0.054)
% regulated emp×post07	-0.046	-0.135	-0.052	-0.109
	(0.066)	(0.121)	(0.049)	(0.095)
young×% regulated emp	-0.161***	-0.138***	-0.074**	-0.060*
	(0.046)	(0.045)	(0.033)	(0.033)
Year×sector FE	✓	✓	✓	✓
Year×province FE	✓	✓	✓	✓
Observations	431,431	429,520	1,693,271	1,680,894
R-squared	0.206	0.446	0.217	0.503

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage changes in employees and outputs. Main independent variable is the interaction between the intensity of regulation in the same municipality (the percentage of employment in regulated firms in the municipality), whether the municipality Party secretary is younger than 57 years old and the post-2007 dummy. Year×province fixed effects and year×sector fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.11: Rubustness: Overall Impacts on Air Quality – AOT

VARIABLES	(1) log aot	(2) log aot
young×regulated emp×post07	-0.034*	
	(0.019)	
regulated emp×post07	-0.009	
	(0.025)	
young×regulated emp	-0.003	
	(0.018)	
young×# of regulated firms×post07		-0.002**
		(0.001)
# of regulated firms×post07		-0.001
		(0.001)
young×# of regulated firms		-0.000
		(0.004)
Municipality FE	✓	✓
Year×province FE	✓	✓
Observations	1,401	1,401
R-squared	0.560	0.559

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are AOT at municipality level. Main independent variable is the interaction between the intensity of regulation in the same county (the number of employees in regulated firms in the county or the number of regulated firms in the county), whether the municipality Party secretary is younger than 57 years old and the post-2007 dummy. Municipality fixed effects and year×province fixed effects are controlled. Standard errors are clustered on municipality level.

Table 2.12: Robustness: County-level Exposure, Controlling for Year×Municipality FE

VARIABLES	(1) Δ employees	(2) Δ output	(3) Δ employees	(4) Δ output
young×regulated emp×post07	0.004**	0.004***		
	(0.002)	(0.001)		
regulated emp×post07	0.005*	0.002		
	(0.003)	(0.002)		
young×regulated emp	-0.002	-0.003*		
	(0.002)	(0.002)		
young×# of regulated firms×post07			0.004*	0.002
			(0.002)	(0.002)
# of regulated firms ×post07			-0.002**	0.003
			(0.001)	(0.004)
young×# of regulated firms			0.003***	0.000
			(0.000)	(0.001)
Year×sector FE	✓	✓	✓	✓
Year×municipality FE	✓	✓	✓	✓
Observations	431,431	429,520	431,431	429,520
R-squared	0.268	0.549	0.267	0.535

† Notes: *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Outcomes variables are percentage changes in employees and outputs. Main independent variable is the interaction between the intensity of regulation in the same county (the number of employees in regulated firms in the county or the number of regulated firms in the county), whether the municipality Party secretary is younger than 57 years old and the post-2007 dummy. Year×municipality fixed effects and year×sector fixed effects are controlled. Standard errors are clustered on municipality level.

Chapter 3

Son Preference, Children's Gender and Parents' Time Allocation: Evidence from China

This paper investigates the effects of children's gender on parents' time allocation due to the long-existing son preference in developing countries. A collective model generates predictions about the impact of the birth of more sons on family behaviours when son preference is treated as a premium in the father's utility function. Using data from China, I show that, with more sons instead of daughters, the time spent by both men and women on housework rises, while men have to increase their work time in the labour market and women can reduce theirs. The results are consistent with theoretical predictions and robust for further tests. For the possible endogeneity of children's gender, I treat the law forbidding the use of ultrasound-B to reveal the gender of a fetus as a natural experiment and use instrumental variables as the identification strategy.

3.1 Introduction

Sex imbalance is well documented by many demographers and economists, in particular in the context of developing countries, as the well-known phrase "missing women" by Sen (1990) encapsulates. The problem is critical for developing countries, not only because of its immediate ethically negative implications for women, but also for its many associated economic outcomes and social consequences, which together impede development. For example, Edlund et al. (2013) attribute a drastic increase in crime in China to the "surplus" men; Wei and Zhang (2011) argue that the rising sex ratio should be responsible for the country's recent housing price bubbles. Behind the

severely male-biased sex imbalance, the persistent preference for sons in the developing world plays a key role. Son preference not only motivates parents' desire for a male-biased family (Edlund, 1999), but also affects their behaviours after the birth of children. For example, one of the most immediate concerns is discrimination against girls, since their parents may prefer boys and allocate more resources to them.

In this paper I investigate how the time allocation of parents is influenced by the gender composition of their children, as a result of preference for sons. This paper seeks to shed light on supply of working adults and allocation of resources among children. To guide my empirical analysis, I propose a collective household model, involving an intra-household decision-making process and resource allocation. In the model, son preference is modelled as a premium in the father's utility function. The model generates predictions for empirical testing about the impact of the birth of sons instead of daughters on a family's behaviours.

In the empirical part, I use individual-level household survey data from China to test the theoretical predictions generated by the collective framework. I divide the available time allocation of an adult into three parts: time spent on the labour market, time spent on housework and time for leisure. I exploit plausibly exogenous variations in children's gender from a law forbidding the use of Ultrasound-B to test the gender of a fetus. The main finding is that, with more sons instead of daughters, the time spent on housework by both men and women rises, while men have to increase their work time on the labour market and women can reduce theirs. These results are consistent with theoretical predictions founded on a utility-based son preference. In order to check the robustness of these results and supply more evidence on the channels involved, I conduct further empirical tests. To ensure the validity of identification, I perform a placebo test making use of a sample of adults who are childless.

This paper contributes to the existing literature in several ways. First, most of the empirical literature related to the son preference confines the outcome variables to those of fertility, the sex ratio at birth, and the sex-differential survival of children. This paper contributes by painting a complete picture of the effects of gender on family life. I mainly consider how the preference for sons affects a couple's work inside and outside the family through their different attitudes to boys and girls. To the best of my knowledge, the only paper with a similar topic is by Rose (2000). She investigates the effect of an additional child's gender on the time allocation of rural Indian households.

Second, in studies of the relationship between children's gender and other household outcomes, the endogeneity problem arises. As introduced above, the preference for sons determines the imbalance in children's gender and also affects parents' behaviours themselves, which implies that using the gender of children as independent

variable will bias the ordinary least squares (OLS) regressions. Few researchers take this problem into account. In fact, many papers treat the gender of a baby as randomly assigned and even take it as an instrumental variable for fertility or other endogenous variables (Angrist and Evans, 1998). However, in the context of developing countries the existence of this preference for sons and the emergence of prenatal sex selection techniques make gender selection possible and even prevalent, which makes the exogeneity of children's genders suspicious. For example, in Rose (2000), if households can select the specific gender they want Anderson and Ray (2010), her estimates may be inconsistent. Li and Wu (2011) examine the effects of the gender of the first child on the mother's bargaining power, nutrition and health. They argue that the gender of the first child is exogenous, since Ebenstein (2011) showed that the sex ratio of first-order births during the 1980s was close to the natural rate. The present paper addresses this problem by exploiting the exogenous variation brought by a policy in China to identify the effects of the sex ratio at birth on the mother's nutrition and health. In addition, I use the distance between a household's home and the nearest facility offering family planning services to construct a multiplier measuring the plausibly heterogeneous impact of this policy on different families. I instrument the children's gender composition with the interaction between the time trend before/after the policy and the distance and estimate it using a two-stage least squares (2SLS) regression.

Third, this paper can also offer solid evidence on the essence of the son preference. Few documents identify how the preference for sons takes effect in the lives of the household, as a result limiting the responses to the question why sons are preferred to daughters. Theoretically, there are two leading explanations for it. First, sons may produce more returns or require less bearing cost than daughters do, a consequence which prompts what I call a *constraint-based* preference, since it will affect family budget constraints. Second, sons and daughters may bring only a systematic bias in parents' utility, fathers' in particular, due to some existing social norms. I call this consequence a *utility-based* preference, since it does not change family budget constraints but only the utility functions themselves. Ben-Porath and Welch (1976) provide an early discussion of those two possible essences in explanation of the fertility pattern in the US, which they refer to respectively as the "sex-concern effect" and the "price effect". In this paper I want to justify those two explanations in the context of developing countries and construct a theoretical framework based on a collective model, which will generate distinct predictions about family outcomes under different features of the preference for sons. My empirical analysis helps to separate these two different mechanisms. It will help us to understand what role the preference for sons play in this area of household decision making.

This paper is organized as follows. Section 3.2 lays out a theoretical framework that generates predictions for the gender effects on time allocation. Section 3.3 introduces the background. Section 3.4 presents the data and strategy used in the empirical analysis. Section 3.5 includes the baseline results, as well as further discussions about the robustness, identification and implications of the empirical results. Section 3.6 concludes.

3.2 Conceptual Framework

Most economic analysis concerning household behaviours in early stage follow the unitary framework developed by Becker (1973). They view the household as a collection of individuals who has one set of preferences and behave as if they were in agreement over the best way to allocate their time and consumption. However, the unitary framework faces empirical challenges about the welfare of persons *within* the family (Haddad et al., 1997) and the model's failure is mainly due to its assumption that family members act as a unity. The second generation of household frameworks features an intra-household decision-making process and allocation of resources (Manser and Brown, 1980; Chiappori, 1988). Some empirical works offer credible evidence for such a collective framework, for example, Thomas (1990), Strauss and Thomas (1995) and Quisumbing and Maluccio (2003). In the context of China, Chau et al. (2007) show that the collective model performs better in predicting household labour supply. I will also justify the collective model using empirical results.

I first consider a collective framework that contains two persons in the family, husband and wife, who enjoy consumption and leisure. Here I take the son preference as a kind of utility premium for fathers, considering the patriarchal norms from an anthropological standpoint. This preference is easy to understand, bearing in mind that the function of male offspring is to continue the family line, genetically carry the unique Y chromosome and culturally carry the family name[1]. To be specific, the utility functions are:

$$U = U(C_1, L_1, \pi(\beta)g(R_2)) \tag{3.1}$$

$$V = V(C_2, L_2) \tag{3.2}$$

where U and V are husband's and wife's utility functions respectively, C_1 and C_2 are their consumption, L_1 and L_2 are their leisure. $\pi(\beta)g(R_2)$ is a term representing the offspring's effect on the husband's utility, where β is the gender indicator of the

[1]I discuss the constraint-based case after empirically testing the predictions from the constraint-based model.

children, which is greater for sons, and R_2 is the wife's household labour time. To simplify, I exclude the husband's household labour time from the model. As seen in the descriptive statistics, the husband's household labour takes much less time than the wife's and is relatively ignorable compared to his work time, which can be explained by Becker (1985)'s sexual division of the household due to a kind of specialization effect. Nevertheless, I still test its response in the empirical analysis. $\pi(\cdot)$ and $g(\cdot)$ are both increasing functions, which implies that the husband's utility increases when he has a son and not a daughter or when his wife devotes more time to housework, for example, parenting children. The household chooses consumption and leisure to maximize a weighted objective function:

$$\max U(C_1, L_1, \pi(\beta)g(R_2)) + \mu V(C_2, L_2) \tag{3.3}$$

where the sharing rule μ is a function of the husband's and wife's bargaining power a_1 and a_2, decreasing in a_1 and increasing in a_2:

$$\mu = \mu(a_1, a_2(\beta)) \tag{3.4}$$

where the wife's bargaining power a_2 is an increasing and concave function of β, i.e. when the sex ratio of the children is more male-biased, the wife's bargaining power a_2 becomes greater. This is a key assumption in the model and is supported by many empirical works in developing countries. For example, Li and Wu (2011) find that a woman with a first-born son has a 3.9 percentage greater role in household decision-making than a woman with a first-born daughter. In McElroy (1990)'s analysis, the exogenous changes in children's gender composition can be regarded as a shock to the extra-household environment parameters (EEPs), which induce an impact on the threat of divorce and thus on the couples' bargaining power. Suppose a couple with more sons divorce. The wife will be labelled as "able to give birth to more sons", which in the context of China will enable her to get a wealthier husband than another woman with less sons might get, since the preference for sons prevails there.

The budget constraint for the family is:

$$C_1 + C_2 = W_1(T - L_1) + W_2(T - L_2 - R_2) \tag{3.5}$$

where W_1 and W_2 are husband's and wife's wage rates respectively, T is the total time one has, R_2 is the wife's time spent on household labour. The labour supply from each of them in the labour market are $H_1 = T - L_1$ and $H_2 = T - L_2 - R_2$. After solving the above maximization problem, we can derive the optimal level of variables about

household decisions such as consumption and leisure.

To examine the response of the endogenous outcomes corresponding to the children's gender shock, we consider comparative statics towards β. The results are as follows:

$$\frac{\partial L_1^*}{\partial \beta} < 0 \tag{3.6}$$

$$\frac{\partial L_2^*}{\partial \beta} \lesseqgtr 0 \tag{3.7}$$

$$\frac{\partial R_2^*}{\partial \beta} \lesseqgtr 0 \tag{3.8}$$

so that

$$\frac{\partial H_1^*}{\partial \beta} > 0 \tag{3.9}$$

$$\frac{\partial H_2^*}{\partial \beta} \lesseqgtr 0 \tag{3.10}$$

The detailed proof of the results above is given in Appendix 3.7.1. The results suggest that the husband's leisure will decrease and his labour supply will increase when he has more sons instead of daughters. The effects on the wife's time allocation are ambiguous. I show the conditions to ensure $\partial H_2^*/\partial \beta < 0$ and $\partial R_2^*/\partial \beta > 0$ in the appendix. Intuitively, a male-biased gender shock will produce a positive effect on the wife's leisure, due to the increase in her bargaining power and also a positive effect on the time she spends on household labour, because the husband values her parenting behaviours on boys more than on girls. If the latter dominate (for example, when the marginal effect of children's gender on the wife's bargaining power $\mu_2(a_1, a_2(\beta))$ is small), she also will have to increase her household labour time. I test these predictions in the empirical part.

3.3 Background

The son preference has a long history in China. The concept of the dominance of the husband in a family is embedded in traditional Chinese culture. On the one hand, the male can carry on the names of family lines (the surname). On the other, in most cases, the female will move out from her parents family into that of her husband, no longer providing income or labour for her birth family. Such a preference for sons in the culture was regarded as harmful feudal tradition, to be brought to an end by the Chinese Communist Party. As a result, after the founding of the People's Republic

of China in 1949, the government started to advocate respect for women, and the sex ratio at birth was almost balanced until the 1980s (Zeng et al., 1993). However, due to the national family planning program in 1980, known as the One Child Policy, since the 1980s sex imbalance has emerged. Since parents under the One Child Policy cannot ensure the desired number of sons, they have to rely on sex selection, either prenatally or postnatally, to achieve family continuity (Ebenstein, 2010).

Of the two methods of sex selection, the postnatal one is more traditional; it takes the form of infanticide or the abandoning of baby girls. However, Coale and Banister (1994) and Zeng et al. (1993) show that the prenatal method but not the postnatal one is more probably responsible for the sex imbalance in and after the 1980s. The technology to identify the gender of a fetus and that to abort are both necessary for this form of selection. First, abortion is never regarded as illegal or immoral behaviour in China, partly due to the country's non-religious culture. Moreover, in order to facilitate the One Child Policy, abortion equipment which was necessary for controlling the number of births was provided in hospitals, clinics and the so-called family planning service stations in communities and villages after 1979. Second, ultrasound-B examination, a convenient and affordable method of revealing the gender of the fetus, began to spread across China at much the same time. By observing the external genitalia of the developing fetus, ultrasound-B examination is much more accurate than such traditional Chinese methods as feeling the pulse of a pregnant woman. At the same time, it is more affordable and accessible than other modern methods such as amniocentesis. In 1979, the year when the One Child Policy was implemented, China manufactured its first ultrasound-B machine. In 1987, over 13,000 machines were already in use in hospitals. At the same time, imports of foreign-made ultrasound-B machines also reached their peak. By the beginning of the 1990s, almost all county and township hospitals and family planning service stations owned ultrasound-B machines, and such equipment was also available in many private clinics (Zeng et al., 1993).

With the implementing of the One Child Policy and the combined technology of ultrasound-B and methods of abortion, the sex ratio for boys at birth in China experienced an abnormal increase in the 1980s and 1990s. According to the estimates of Chen et al. (2013), nearly half of the increase in male bias at birth can be attributed to local access to ultrasound-B examinations. In addition, gaps in mortality and health investment between boys and girls also enlarged. Many works have documented this outcome (Arnold and Liu, 1986; Zeng et al., 1993; Chu, 2001; Ebenstein, 2010). Realizing the possible consequences of prenatal sex selection, the government put some limits on the use of ultrasound-B. In 1986 and 1989, the Ministry of Health and the

State Family Planning Commission handed down two notices forbidding prenatal sex selection except for the purpose of diagnosing hereditary diseases. However, the effects of the notices were trivial: they lacked the power of laws and gave too few detailed administrative rules about the use of ultrasound-B. For example, none of them specifies the penalties for possible violations. During the 1990s, the imbalance became more serious.

In 2002, the "Law of Population and Family Planning" was enacted. Not only was the use of ultrasound-B to reveal fetal gender and of abortion for selection purposes forbidden by law for the first time, but the penalties, including the fines and sentences for doctors and clinics that violated these rules, were also spelled out. The law came into effect in September of 2002. Next, the State Family Planning Commission, the Ministry of Health and the State Food and Drug Administration jointly enacted the "Regulation on Forbidding Fetal Sex Determination for Non-medical Purposes and on Abortion for Sex Selection" in November 2002. The regulation gave more details about implementing the prohibition of ultrasound-B. For example, doctors who helped to test the gender of a fetus would be fined and their license to practice would be revoked.

3.4 Data and Empirical Strategy

In this section I present my data and empirical strategy for formally testing the effect of children's gender on parents' time allocation, which is predicted by the theoretical model. The main data source that I use is the China Health and Nutrition Survey. I start with Ordinary Least Squares estimates on this effect. Due to the possible endogeneity of children's genders because of prenatal selection, I propose an instrumental variable method using the Law of Population and Family Planning as the main source of exogenous variation. A formal description of the data and the method is given below.

3.4.1 Data

The data used in this paper come from the China Health and Nutrition Survey, which is led by the Population Center at the University of North Carolina. The survey covers nine provinces (Guangxi, Guizhou, Heilongjiang, Henan, Hubei, Hunan, Jiangsu, Liaoning, and Shandong) in eight waves (1989, 1991, 1993, 1997, 2000, 2004, 2006 and 2009). These provinces vary in geographic conditions, social norms and development levels. Figure 3.1 maps the survey regions. There are about 4400 households in the survey overall, including over 19,000 individuals. Alongside the ordinary demographic and economic variables, the survey has well-recorded information about pregnant

women. It records every pregnancy of each woman, including the results of the pregnancy, the date that the pregnancy ended and the gender of the child(ren), etc. Most variables about pregnancy are not included in the 1989 survey. Therefore, in the empirical part I use the data from 1991 to 2009, seven waves in total. The sample in this paper includes all families with at least one newly born baby during the period under scrutiny. The descriptive statistics of the main variables in seven waves are listed in Table 3.1 for every pregnant woman and Table 3.2 for husbands if identified.

The major dependent variables are the fathers' and mothers' work time at home (the variable *housetime*) and in the labour market (the variable *worktime*), with both variables measured in hours per week. Parents' time at home and in the labour market may vary with gender and numbers of children. Figure 3.2 plots parents' time allocation before and after the birth of their sons or daughters. It only includes parents with only one child for simplicity. We can see from Figure 3.2 that a father's working time in the labour market increase more after he gives birth to a boy relative to a girl. A mother's working time decrease more and her time on housework increase more facing with a boy instead of a girl. These arguments will be tested formally in following sections with the whole sample. In addition, from Table 3.2, we may note that the mean values of the husbands' household labour time in each wave are much less than those of wives, which coincides with Becker's sexual division theory and the social norms in developing countries. It can justify my simplification in the theoretical part, which does not incorporate the husbands' household labour time into the model.

To explore variations in the children's gender, I construct two measurements as the key independent variables. The first is the children's sex ratio in the household (the variable *Ratio*), which is defined as (Number of boys)/(Total Number of children). The other one is the gender-biased score (the variable *Score*) defined as (Number of boys)-(Number of girls). The impact of one more child is linear in this measurement, but non-linear in the previous measurement. The samples of men and women are not totally matched as husbands and wives, since some married women did not report their husbands' line number. In addition to the key characteristics of an individual, including whether the family includes parents from the older generation (*Old*), whether the individual reaches retirement age (*Retire*), and the total number of children (*Acctotal*), etc.

There are some concerns about the CHNS data that it may form a unbalanced panel data. In my sample, only less than 30% individuals appear in all seven waves. If individuals drop out of the sample non-randomly, it may produce caveats for the empirical analysis. Here I provide two arguments which prevent the unbalanced sample in this paper from the caveats. First, though the number of households and individuals

missing at some waves is large, most of the reasons are plausible. One of the reasons is due to changes in sampled provinces and communities. Some new provinces and communities were added during the survey to replace those unable to participate. Then all individuals in those provinces or communities will miss at least one wave of the survey. However, given the fact that the sample process is random, those who do not drop out during all available waves in their provinces or communities should be considered to be parts of the "balance" panel, though they do not appear at all seven waves. Another reason is that my sample only consists of married adults. Some individuals were still teenager or unmarried at the first available wave, but became eligible for my sample during the survey. It is a natural process to grow up for any individual, so I also take it as a plausible reason to be missing at some waves. Taking those together with the balance panel, I find they compose nearly 70% of my sample. Second, I test whether dropping out of the survey is systematically related to my key outcome variables. T-tests fail to reject the hypothesis that men and women who are going to be missing at next wave have a different work time in the labour market for each wave. Both of the arguments add to our confidence that the unbalanced characteristic will not become a possible caveat for the empirical analysis.

3.4.2 OLS Specifications

I estimate the basic regression model with OLS:

$$Y_{it} = \alpha_i + \beta_{pt} + \gamma Gender_{it} + \theta X_{it} + \zeta_t Z_i + \mu_{it} \tag{3.11}$$

where Y_{it} is the time allocated to some specific activity by a parent i. Possible activities include market labour, household labour and leisure. $Gender_{it}$ is the measurement of the children's gender, including $Ratio_{it}$ and $Score_{it}$ which were defined in Section 3.4.1. The gender effect we care about is captured by the coefficient γ. X_{it} is a series of the time-varying control variables such as whether there are older parents in the family, whether the individual reaches retirement age, the total number of children, the age of the youngest child, etc. Z_i is a series of such time-invariant control variables as education, whether the individual is from a rural area, etc. α_i and β_{pt} are individual and province by year fixed effects; including the province by year fixed effects not only controls for the variations in the macro economy, but also for the transitions in institutions and social norms at province level, which may affect the outcomes of individual time allocations.

As described in Section 3.3, the gender composition can be selected rather than randomly assigned by the nature of developing countries with the diffusion of prenatal

selection technology, which may result in inconsistent estimates. Take the example of a regression with women's housework time as the dependent variable. On the one hand, a family with a strong preference for sons may select a corresponding children's sex ratio and require the wife to take on more household responsibility, which will induce an upward bias for the OLS estimation; on the other, since a family with a wife who works less in the labour market and contributes more in household work may be more bound to traditional production and may be less accessible to modern medical technology, such correlations will bias the OLS estimation towards zero. If such unobserved factors which affect both the gender results and the time allocation are time invariant, controlling for individual fixed effects can solve the problem. However, if they are time variant, other strategies are needed for a consistent estimate.

3.4.3 Intrumental Variables

Due to such potential endogeneity, I propose here an identification strategy using instrumental variables. Through the rigorous rules on the use of ultrasound-B, a change may have ensued in the trend of the sex ratio at birth. To specify, before September 2002, like the diffusion of ultrasound-B, the sex ratio at birth would have presented an increasing trend. However, after this time, the trend may have been reversed by legally forbidding such technology. Figure 3.3 draws the gender composition over time in the sample data.

Furthermore, the treatment effects of the policy may be heterogeneous on different households. I construct a variable $proximity_i$ for household i, which is defined as $[max(distance_{i,1989}) - distance_{i,1989}]/max(distance_{i,1989})$, where $distance_{i,1989}$ is the distance between the home and the nearest facility offering family planning services, $max(distance_{i,1989})$ is the greatest distance between any household within the same community and the nearest family planning facility. Figure 3.4 depicts the distribution of the distance. The variable ranges from 0 to 1. The way I construct it makes sure that it is a linear decreasing function of the distance. The main objective is to measure accessibility to the technology of gender selection. The *proximity* variable is about how close an individual's house is to the nearest family planning facility. From intuition, those in closer proximity to a family planning facility will receive greater treatment effects during the diffusion of its technology and will also be affected more after the law. In Figure 3.5, I draw the average sex ratio at birth over time separately for the sample which is further away than 1 kilometer (median of the whole sample) and for the sample which is not so far. It can be seen that the trend in the longer distance sample is relatively plain, which offers an intuition about the existence of heterogeneous trends among different households.

Alongside the above two graphs, results of the first stage regression can offer a formal justification for the construction of instrumental variables. But there is some concern about the exogeneity of our instruments. I address this concern in Section 3.5.2.

To make use of such exogenous variation, a Two Stage Least Squares analysis is needed formally. Specifically, I will estimate a model for the first stage regression:

$$\begin{aligned} Gender_{it} = \alpha_i + \beta_t + \theta X_{it} + \zeta_t Z_i + \delta_1 proximity_i * (birthyear_{it} - 1991) \\ + \delta_2 proximity_i * (birthyear_{it} - 1991) * P_{it}^{2002} + \delta_3 proximity_i * P_{it}^{2002} + \epsilon_{it} \end{aligned} \tag{3.12}$$

where P_{it}^{2002} is a dummy variable indicating whether this parent's child was born after September 2002. $(birthyear_{it} - 1991)$ is a linear exogenous time trend. Using the interaction between heterogeneous proximity and the time trends as instruments, I expect to identify the effect of the sex ratio at birth.

3.5 Results

In this section, I present the results of using empirical strategies, as described above, to test the collective household framework.

3.5.1 Baseline Results

Table 3.3 reports OLS and 2SLS estimations of model 3.11 on three main outcome variables: men's and women's labour supply in the labour market and women's housework time. The odd numbered columns report the results using the OLS model and the even numbered columns report the 2SLS model. As the key independent variable I use two different constructions of gender composition, children's sex ratio (*Ratio*) and the gender-biased score (*Score*), reported respectively in Panel A and Panel B.

The OLS specification, controlling for variables, individual and province by year fixed effects, shows that there are no significant effects of the children's gender composition on a husband's work time in the labour market. The effects on the wife's market and household labour time are small but significantly different from zero. One more son instead of a daughter will reduce the wife's labour supply by 0.569 hours a week and increase her housework time by 0.318 hours.

The difference between the OLS and 2SLS models is huge. The 2SLS results in Panel A suggest that a boy instead of a girl will induce an 6.777-hour increase in a man's labour supply per week and a 7.419-hour decrease in a woman's; at the same

time, a woman's weekly housework time will increase by 5.494 hours, which implies a four hours' increase in her leisure. The results in Panel B use the sex ratio as independent variable. If the sex ratio of the children increases by 0.1, men's labour supply will increase by 2.482 hours per week and women's will decrease by 3.214. Changes in women's housework time are similar. The final rows in each panel report the F-stats of the excluded instruments. The F-stats in all the 2SLS specifications are larger than 10, so the instruments are strong enough. At the same time, all these 2SLS regressions pass the over-identification test.

Such a huge difference suggests the existence of bias in the OLS estimates. On the one hand, omitted variable bias may exist in the OLS estimates. For example, families in less developed areas are always bound by traditional culture, preferring husbands to work more than wives in the labour market and in the grip of traditional medicine instead of modern medical technology such as ultrasound-B. This can bias the OLS estimation to zero. On the other hand, the OLS estimates can be biased towards zero because of the measurement error in reported gender composition. Due to the family planning policy, a couple in China sometimes have to hide some of their children from census and survey, which produces a classical measurement error on the gender ratio of their children. The 2SLS estimates can remedy this problem.

I next consider the first stage regression in the IV strategy. The results of the first stage model 3.12 are listed in Table 3.4. Columns 1 and 2 present the first stage results for the husbands' market labour outcomes, using gender-biased scoring ($Score$) and the sex ratio at birth ($Ratio$) as distinct independent variables. Columns 3 to 6 present the corresponding first stage results for the time spent by wives on market labour and household labour. From the results we can find that the instruments predict the children's gender composition quite well, both the gender-biased score and the sex ratio at birth. δ_1, the coefficients of the interaction term between the time trends and the proximity factor $birthyear_{it} * proximity_i$ are significantly positive, implying that with the diffusion of the related technology (in particular the ultrasound-B machines) the gender bias would grow larger. Such effects were larger in those households which lived nearer to the family planning facilities. What we care more about are the coefficients of the triple interaction term $P_{it}^{2002} * (birthyear_{it} - 1991) * proximity_i$. The negative significance suggests that the implementation of the 2002 Law did stop the increasing trend towards gender imbalance.

3.5.2 Exogeneity of the Instruments

The previous 2SLS results are based on the instruments that involves the 2002 law banning the use of ultrasound-B and the distance between the household and the near-

est family planning service facility. There are some concerns about their exogeneity, which may affect the identification. In this section I present some evidence to confirm that my instruments are exogenous.

The major concern is the exogeneity of the distance to family planning service facilities. In his famous paper using college proximity as an instrument for education, Card (1993) raises some possible caveats in assuming such geographic variation to be endogenous, which may also apply to the context in the present paper. In my setting, for instance, one may think of the possibility that a less-constrained family would choose a house which was convenient for receiving family planning services. China's family planning policy began in 1980. In the 1989 data, over 97% households had lived in the same place for over nine years, which means that most households had moved into their houses before the start of family planning services. So it is unlikely that endogeneity would come from selecting the households' distance from family planning services for unobserved reasons. In the first four columns of Table 3.5, I report the results from the sample of those who in 1989 had lived in the same place for more than 9 years. Not surprisingly, the estimation results are similar.

Even if the possibility can be ruled out that a family selected house on the basis of its distance from family planning facilities, there are still certain other concerns. One of them is that the location of one's house will reflect one's preferences, which may correlate with labour outcomes. For example, the households where career success is emphasized tend to live in a crowded area, which has better access to many facilities, including family planning services. But since almost all families live in the same place for many years, it is natural to think that the effects brought by their preferences should be persistent. In a fixed effect regression, such persistent effects will be absorbed in household or individual fixed effects.

An alternative strategy for avoiding possible endogeneity from information about distance is to use the time trends alone as the instrument, instead of the interactions between proximity and time trends. From the last four columns of Table 3.5, it can be seen that it makes little difference even if we do use the time trends as instruments. The estimates are statistically significant and similar in magnitude to previous results. For simplicity, I do not report the first stage estimates, which are also similar.

Another consideration about identification is the exogeneity of the trend before 2002. The time invariant characteristics are absorbed in the individual fixed effects. Moreover, after controlling for the Province×year fixed effects, the province-level time varying shocks will not be a problem. Nevertheless, since the trend before 2002 is not produced from an exogenous policy shock but from a technology diffusion process, we should still take a cautious attitude to it. To address this problem, I use the

variation from the 2002 law as the sole instrument and put the other two in the control variables. To be specific, the variable $proximity_i * (birthyear_{it} - 1991) * P_{it}^{2002}$ is the only instrument now. Since the first stage equation is to regress the gender composition to all instruments and controls, the results are totally the same as in Table 3.4. The results in the second stage regression can be checked in Table 3.6. Since the variation in the sole instrument arises from the 2002 law, it is certain to be exogenous.

We find that if we use only the variation from the policy shock, most of the results will not change significantly. Only the outcomes of women in response to the sex ratio of children are insignificant, while the signs and the magnitudes are similar. In general, when using the interaction between the proximity multiplier, the 2002 dummy and the time trend as the only instrument which is free from an endogeneity concern, the results are similar to those of our main regression, implying that the initial specification will not lead to an endogeneity caveat.

3.5.3 Measurement of the Proximity

In the baseline regression, I define a variable $proximity_i$ as $[max(distance_{i,1989}) - distance_{i,1989}]/max(distance_{i,1989})$, where $distance_{i,1989}$ is the distance between the home and the nearest facility offering family planning services, $max(distance_{i,1989})$ is the greatest distance between any household within the same community and the nearest family planning facility. I construct this variable to measure accessibility to the technology of gender selection. I use its interaction with variables about the 2002 law as the instruments to do the 2SLS estimation. In this subsection, I test the robustness of using a different definition of the proximity.

Instead of using $proximity_i$, a linear function of the distance between the home and the nearest facility offering family planning services, I define an alternative measurement $proximity2_i$ as a dummy variable, taking the value of 1 if the distance to the nearest family planning facility is less than the average distance among the whole community. I repeat the same practice as in the baseline regression and present the 2SLS results in Table 3.7. We can find from it that the magnitudes and the significances of estimated coefficients are not affected much by this alternative measurement. It adds to our confidence that the baseline results are not driven by the specific definition of the measurement of the accessibility to the technology of gender selection. For simplicity I do not report the first stage and the reduced form results, but they are both similar to those in the baseline regressions.

3.5.4 Reduced Form Evidence

In this part I present the reduced form results of the 2SLS strategy. The reduced form evidence describes the relationship between the parents' time allocation and the time trend. The style of the reduced form regression is similar to a differences-in-differences (DID) strategy. For a formal reduced form regression, I estimate the model:

$$Y_{it} = \pi_1 proximity_i * (birthyear_{it} - 1991) + \pi_2 proximity_i * (birthyear_{it} - 1991) * P_{it}^{2002} + \pi_3 proximity_i * P_{it}^{2002} + \alpha_i + \beta_t + \theta X_{it} + \zeta_t Z_i + \nu_{it} \quad (3.13)$$

The results are presented in Table 3.8.

We see from the results that the directions and magnitudes fit with the corresponding 2SLS results, as predicted. Before 2002, the time spent on housework by parents increased with time, while the market labour time for mothers largely went down and the market labour time for fathers increased non-significantly. After 2002, such trends all shifted in the reverse direction. Combining with the trend of sex ratio, the reduced form evidence are consistent with the 2SLS results. It adds to the confidence of the IV strategy.

3.5.5 Placebo Test

To explore the possible violation of the exclusion restriction, I estimate the effect of the time trend on individuals' time allocation with a "placebo" group. In this group, the sample is consist of married adults who had no children. If the children's genders are not the only channels through which the instruments affect individuals' time allocations in above the 2SLS model, but any other unobserved factors in the time trend, then the placebo group would also exhibit spurious effects on their allocation of time. Specifically, I estimate the same equation in the reduced form specification 3.13 on the placebo sample. The results are presented in Table 3.9.

We see that almost all the coefficients of time trends are never statistically significantly different from zero, except for the one with a pre-2002 time trend on the time spent on housework for the women's placebo sample. Even the coefficient is in a reverse direction to that of the main regressions, which in some ways strengthens the conclusions. These findings can exclude the possibility that some unobserved factors act as alternative channels to influence the time allocation outcomes.

3.5.6 Length of Working Hours and Labour Force Participation

In the baseline results, I combine individuals working zero hours with those working positive hours to do the estimation. In reality, the decisions about working in the labour market can be separated to two parts. Individuals will decide whether to participate in the labour force at first, and then decide how long they work in the labour market. For simplicity, in the baseline estimations I do not take this difference into account. In this subsection, I estimate the impacts at the intensive and extensive margins. The results are listed at Table 3.10.

The first column estimates the impacts of children gender on working hours for men being in work (with positive working hours). The second column estimates the probability of participating in work for all men. We can see that the estimated coefficients in the first column are similar to the baseline results, and the probability of being in work for a man is not affected by whether the child is a boy or a girl. On the contrary, for women, both the length of working hours conditional on being in work and the probability of participating in work are both significantly reduced by an increasing gender ratio at birth. The results are not surprising. Compared with men, women are easier to quit their jobs and become housewives when needed. Having sons instead of daughters will affect women's decision on working both at the intensive and extensive margins.

3.5.7 Men's Housework Time

In the theoretical model, I assume that husbands are specialized in their labour market work, so I did not test the response about men's housework time to their children's gender composition in the baseline results. However, it is intuitive to think about one case where it may be relevant. On the one hand, the time that men spend on housework can make a similar contribution to that of women. On the other, the time that they spend on their sons will provide intrinsic utility in itself. So it is reasonable to regard it as playing a similar role to that of women's housework, i.e. presented as part of the husband's utility function. The predictions from theory should also be the same.

In this part I give the results of testing the theory in Table 3.11. The OLS and 2SLS results are quite similar to those for women, indicating that men will also take more responsibility in households when they have sons and not daughters. The results imply that sons will get more care than their sisters will, from both their father and their mother.

Although the results are significant and similar to those for women, they will not undermine the conclusions drawn in the theoretical and empirical parts, and will even strengthen some of the implications of these. On one hand, it is shown in the descriptive statistics that the mean values of men's housework time are much less than those of women, suggesting that the role of wives in families is much more important than the role of husbands. From this perspective, it is not unreasonable to exclude husbands' housework in the theoretical model for the sake of simplicity. On the other hand, the speculation that an increase in men's housework time when they have more sons means a reduction in the time available for their leisure, as suggested in the theoretical model, is more plausible, which indicates that their bargaining power is further somewhat weakened.

3.5.8 Dividing Household Labour Time

It is easy to conceive that parents would directly devote most of the increased housework time to child care when facing shocks from children. But there can also be some spill-over effects on the time devoted to other housework not directly related to child care, in particular those which can produce positive externality to children. In order to be sure of the main channel, here I divide it into two types of dependent variable, child-care labour and non child-care labour. Table 3.12 presents the results. The table shows that only the time directly devoted to child care by both husbands and wives is affected significantly by the children's gender composition. As for the non child-care housework time, the coefficients are also positive, which is consistent with intuition, but not significant.

3.5.9 Implications on Alternative Models

Above analysis is based on the bargaining model that treats the preference for sons as some kind of premium in the father's utility function, which I call the utility-based preference. The results in the empirical part are consistent with the model, and may shed light on other theories about family and son preference.

In a unitary model, husband and wife are treated as a unity with the same utility function. But the model faces similar empirical difficulties as Haddad et al. (1997) raised. It cannot explain the opposite direction of the outcomes to do with husbands' and wives' leisure. For example, Rose (2000) models the family behaviour by means of a unitary model and a constraint-based preference assumption. It predicts that under credit constraint both parents' leisure time will shrink in response to a son instead of a daughter, but when there is no credit constraint it will expand instead. However,

as I have shown in the empirical part, the positive outcomes for women's leisure and the negative outcomes for men's suggest that theoretical predictions from the unitary model may not have good explanatory power for the difference in behavioral response among family members.

This paper can also shed light on the essence of son preference. As discussed above, there are two explanations for this. One is the utility-based preference I have modelled in the theoretical part; the other is the constraint-based preference. It has been shown that the empirical results are consistent with the model with the former explanation. Now assuming that the preference term $\beta g(R_2)$ appears only in the parents' budget constraints, but not in the utility functions. One reason for doing this is the wage gap between men and women. In developing countries: male members of the workforce can play more roles in production than can females in most families that rely on agriculture. Even in a non-agricultural labour market, women receive relatively low wages with the same human capital. Even if men and women could earn at the same level of income in the future, their duties to parents in the latters retirement are always imbalanced. Sons and their wives are often required to take the responsibility for looking after parents, mainly in the form of income transfer. Now we return to the theoretical framework. Instead of appearing in parents' utility functions, here I assume that the children are a source of future income:

$$\max U(C_1, C_2, L_1) + \mu V(C_3, C_4, L_2) \tag{3.14}$$

where C_1 and C_3 are their first period consumption, C_2 and C_4 are the second period consumption, and L_1 and L_2 are their leisure in the first period. The inter-temporal budget constraint for the family is:

$$C_1 + \rho C_2 + C_3 + \rho C_4 = W_1(T - L_1) + W_2(T - L_2 - R_2) + \rho\beta g(R_2) \tag{3.15}$$

The corresponding comparative static results are as follows:

$$\frac{\partial L_1^*}{\partial \beta} > 0 \tag{3.16}$$

$$\frac{\partial L_2^*}{\partial \beta} > 0 \tag{3.17}$$

$$\frac{\partial R_2^*}{\partial \beta} > 0 \tag{3.18}$$

We can see from the comparative static results that both women's and men's leisure will increase after a boy is born. As a boy will create a larger future income than a girl, his parents can both enjoy more leisure. Such prediction is not consistent with

the empirical results. In the empirical part it has been shown that women's leisure will increase and men's leisure will decrease facing a son instead of a girl. Though we cannot exclude this channel, it cannot explain the whole empirical results alone. So in general, it is incautious to accept the constraint-based explanation as the only source of the son preference. It suggests that the utility-based explanation, a more cultural reason, must play a key role in the formation of the son preference.

3.6 Conclusion

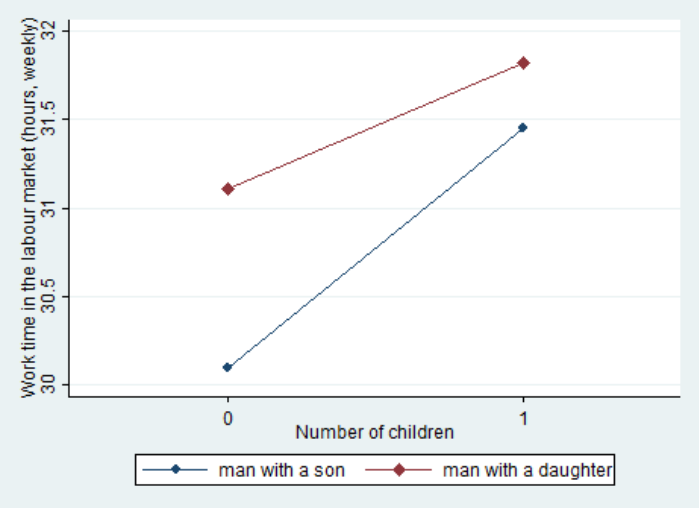
In this paper, I investigate the relationship between the gender composition of the children in family's and the allocation of their parents' time. In the theoretical part, I take the utility-based explanation for the preference for sons that sons will bring a premium to the fathers' utility function and incorporate it with a bargaining model. Through an analysis of the comparative statics, I show that men's time in market labour will rise. The impact on the time spent by women earning outside the home and on housework are ambiguous, depending on the relative marginal effect of a male-biased shock on the wife's bargaining power compared to that on the husband's utility. To empirically test the model, I used data from the China Health and Nutrition Survey in the period 1991 to 2009. What troubles the previous empirical papers is the possible endogeneity of children's gender due to the diffusion of technology for ultrasound-B and abortion. I draw upon the "Law of Population and Family Planning", which forbids the use of ultrasound-B, as the source of a natural experiment, to explore the exogenous variation in children's gender. The estimations on men's responses to the larger proportion of boys in their children are totally consistent with the theoretical model. As regards women, the results show that they will spend more time on housework and less on the labour market, which suggests that their increase in bargaining power after the birth of a son may be dominated by the effects on the husbands' utility. The results are robust to additional empirical tests. Moreover, the same reduced-form specification on the placebo group with no children yields small and insignificant results.

This paper can contribute to literature in many ways and its implications are important for policy makers in developing countries. The above findings highlight the significance of the intra-household decision-making process. The empirical results support the utility-based explanation as the source of the son preference. Moreover, the findings imply clear harm for women in developing countries, due to the long-standing preference for sons. Since the theoretical and empirical results suggest that parents both devote less time to daughters due to the father's preference for sons, it is reasonable to attribute the relatively low achievements of women in careers to

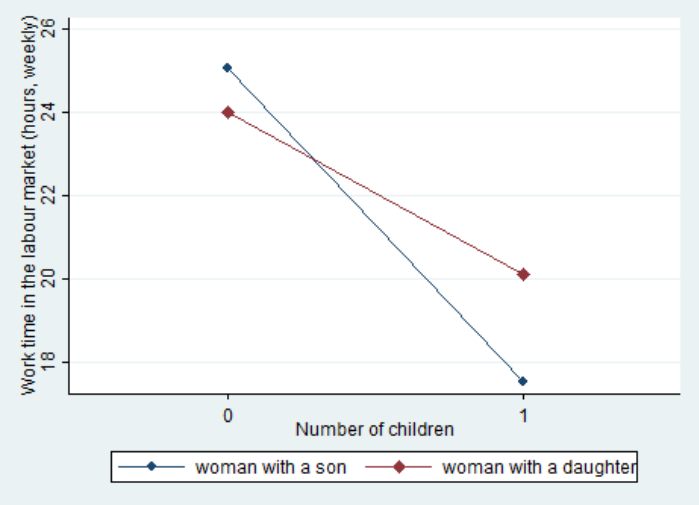
some extent to the poor care they received in their childhood. The results can address some puzzles about the ways in which culture affects the economic outcomes. Such findings can also reveal many policy implications for developing countries with a strong preference for sons, such as China and India. For example, economists have long argued that an adult's performance in the labour market is related to the resources received in childhood. Therefore, gender discrimination in adulthood can be partly attributed to the preference for sons. As a result, women's lifelong value is yet further reduced and produces worse discrimination and preference for sons, forming a miserable trap for females. Such an imbalance can be alleviated more effectively not merely through interventions on fairness in the adult labour market, but by advocating better care for young girls and even transferring more resources to them.

Figures and Tables

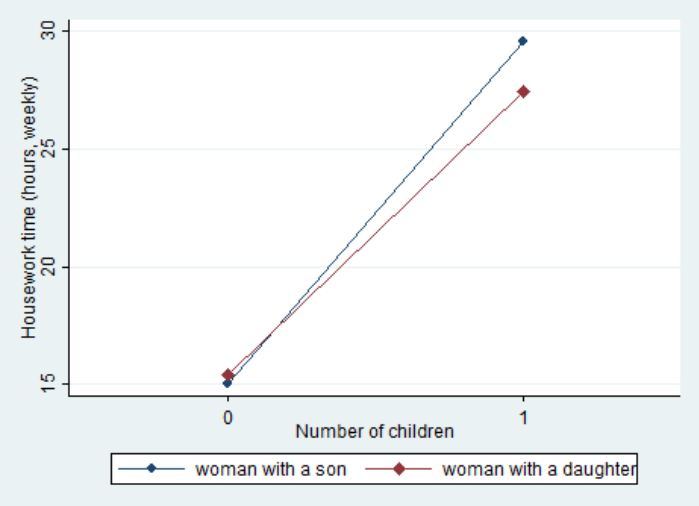
Figure 3.1: Map of Survey Regions



(a) Average Work Time in the Labour Market of Men with One Child



(b) Average Work Time in the Labour Market of Women with One Child



(c) Average Housework Time of Women with One Child

Figure 3.2: Time Allocation of Parents with One Child

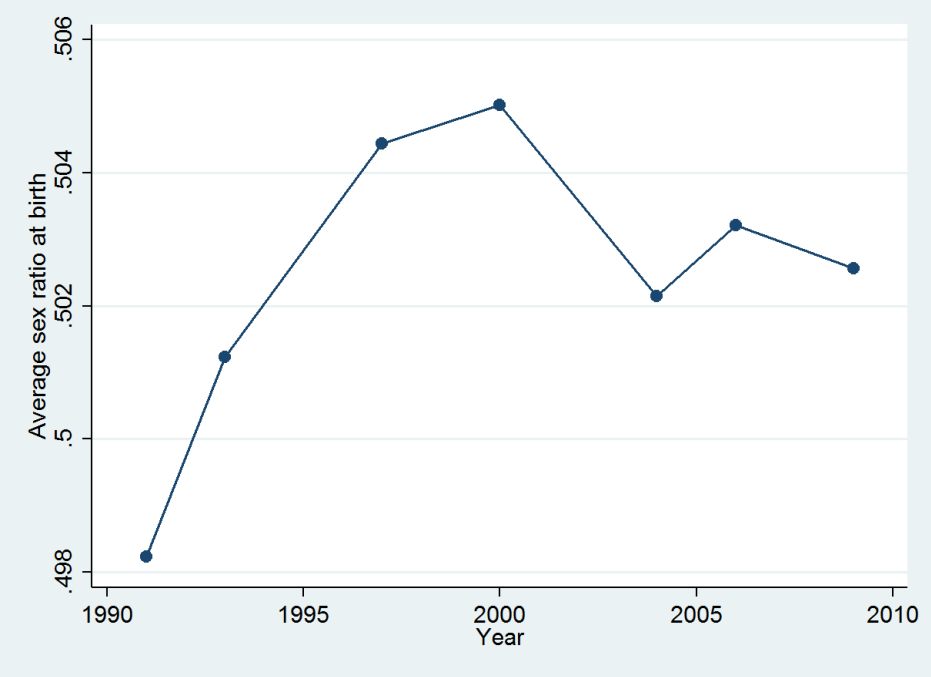


Figure 3.3: Time Trend of Average Sex Ratio at Birth

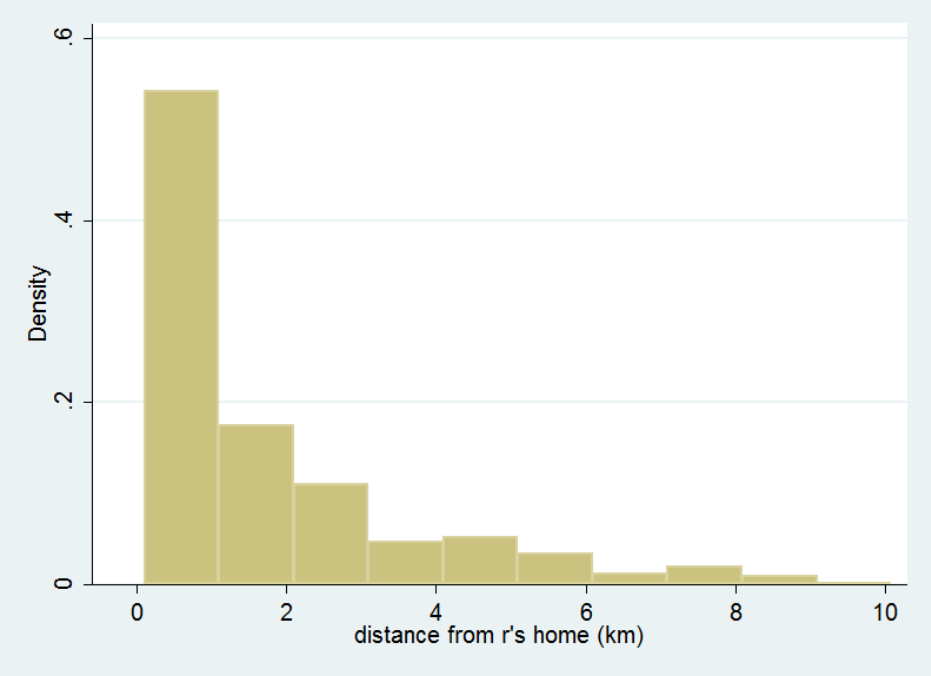


Figure 3.4: Distribution of Distance to Nearest Family Planning Facilities

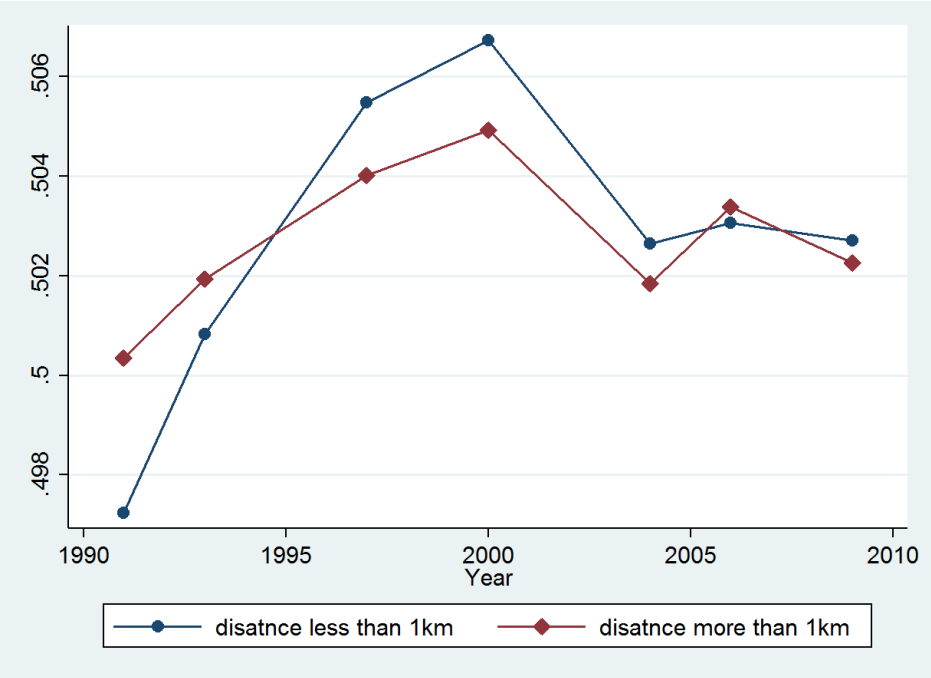


Figure 3.5: Time Trend of Average Sex Ratio at Birth for Two Subsamples

Table 3.1: Descriptive Statistics for Sample of Women

VARIABLES Wave	1991	1993	1997	2000	2004	2006	2009
worktime	48.680	48.171	46.724	46.149	46.230	47.450	45.328
	(13.444)	(13.349)	(16.330)	(15.657)	(27.929)	(25.993)	(25.183)
housetime	8.732	8.989	14.145	14.343	17.015	15.235	12.213
	(7.451)	(8.827)	(16.332)	(16.563)	(19.213)	(19.412)	(13.512)
ratio	0.501	0.501	0.504	0.503	0.502	0.503	0.502
	(0.129)	(0.104)	(0.131)	(0.105)	(0.072)	(0.107)	(0.081)
score	0.002	0.002	0.007	0.007	0.004	0.005	0.003
	(0.421)	(0.212)	(0.242)	(0.265)	(0.146)	(0.201)	(0.169)
acctotal	1.684	1.666	1.635	1.617	1.602	1.597	1.589
	(0.871)	(0.865)	(0.855)	(0.847)	(0.840)	(0.836)	(0.830)
numtotal	1.315	0.048	0.074	0.048	0.023	0.05	0.03
	(1.038)	(0.226)	(0.277)	(0.226)	(0.156)	(0.228)	(0.183)
retire	0.619	0.634	0.640	0.601	0.761	0.766	0.710
	(0.486)	(0.482)	(0.480)	(0.490)	(0.426)	(0.424)	(0.454)
old	0.006	0.011	0.017	0.019	0.02	0.024	0.018
	(0.079)	(0.106)	(0.129)	(0.138)	(0.141)	(0.154)	(0.133)
eduyear	6.412	7.087	7.072	7.956	8.312	8.572	8.442
	(3.641)	(3.257)	(3.431)	(3.199)	(3.131)	(3.532)	(3.546)

† Notes: Values are sample means over seven waves in CHNS, with standard deviations in parentheses.

Table 3.2: Descriptive Statistics for Sample of Men

VARIABLES							
Wave	1991	1993	1997	2000	2004	2006	2009
worktime	48.392	47.209	46.343	45.636	47.144	46.216	46.219
	(14.547)	(14.679)	(16.879)	(17.220)	(27.706)	(27.223)	(26.725)
housetime	2.514	2.890	3.401	5.883	7.233	7.952	6.241
	(3.541)	(3.836)	(4.326)	(5.134)	(8.421)	(10.189)	(10.512)
ratio	0.494	0.501	0.504	0.505	0.502	0.503	0.502
	(0.344)	(0.120)	(0.124)	(0.142)	(0.084)	(0.121)	(0.094)
score	-0.074	0.003	0.007	0.008	0.005	0.006	0.003
	(1.133)	(0.247)	(0.219)	(0.242)	(0.176)	(0.194)	(0.198)
acctotal	1.817	1.803	1.758	1.727	1.698	1.688	1.671
	(0.916)	(0.908)	(0.901)	(0.891)	(0.877)	(0.872)	(0.864)
numtotal	1.309	0.064	0.097	0.062	0.032	0.063	0.04
	(1.127)	(0.260)	(0.311)	(0.254)	(0.189)	(0.253)	(0.211)
retire	0.312	0.359	0.449	0.430	0.736	0.766	0.659
	(0.463)	(0.480)	(0.497)	(0.495)	(0.441)	(0.424)	(0.474)
old	0.056	0.006	0.014	0.008	0.013	0.018	0.007
	(0.23)	(0.079)	(0.118)	(0.09)	(0.113)	(0.132)	(0.084)
eduyear	6.958	7.263	7.323	7.780	8.213	8.094	7.656
	(3.673)	(3.635)	(3.522)	(3.468)	(3.236)	(3.831)	(3.628)

† Notes: Values are sample means over seven waves in CHNS, with standard deviations in parentheses.

Table 3.3: Results of OLS and the 2nd Stage Regression of 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)
	Men		Women			
METHOD	OLS	2SLS	OLS	2SLS	OLS	2SLS
VARIABLES	worktime	worktime	worktime	worktime	housetime	housetime
Panel A: Score as Independent Variable						
score	0.056	6.777*	-0.569**	-7.419*	0.318*	5.494*
	(0.299)	(4.060)	(0.244)	(4.117)	(0.192)	(2.885)
Province×year FE	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Observations	17,780	12,847	19,199	11,897	17,803	10,734
F-stats of excluded instruments		24.41		18.42		17.45
Panel B: Ratio as Independent Variable						
ratio	-0.602	24.816*	-1.351**	-32.137**	0.696	15.086*
	(0.872)	(15.079)	(0.679)	(17.238)	(0.517)	(8.390)
Province×year FE	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Observations	17,780	12,847	19,199	11,897	17,803	10,734
F-stats of excluded instruments		20.76		21.39		19.28

† Notes: Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. The first stage results are listed in Table 3.4. Dependent variables are work time and housework time in women sample, while in men sample is only work time as the theoretical part models. Key independent variable is children's gender composition, including the gender-biased score (Panel A) and the sex ratio at birth (Panel B). The instruments are Proximity×(birthyear-1991), Proximity×(birthyear-1991)×Post-2002 dummy and Proximity×Post-2002 dummy. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc.

Table 3.4: Results of The 1st Stage Regression

	(1)	(2)	(3)	(4)	(5)	(6)
	Men		Women			
VARIABLES	score	ratio	score	ratio	score	ratio
Proximity×(birthyear-1991)	0.014***	0.003***	0.015***	0.002***	0.015***	0.002***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Proximity×(birthyear-1991)	-0.012***	-0.003**	-0.010***	-0.001*	-0.010***	-0.002**
×Post-2002 dummy	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.001)
Proximity×Post-2002 dummy	-0.082***	0.004	-0.073*	-0.004	-0.069*	-0.005*
	(0.038)	(0.011)	(0.041)	(0.003)	(0.038)	(0.003)
Province×year FE	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Observations	12,847	12,847	11,897	11,897	10,734	10,734
R-squared	0.012	0.008	0.001	0.007	0.017	0.009

† Notes: Columns in this table are results of the first stage regressions corresponding to all the 2SLS columns in Table 3.3. Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc.

Table 3.5: Further evidence on identification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	The sample living here over 9 years				Only using time trend as IV			
	Men		Women		Men		Women	
VARIABLES	worktime	housetime	worktime	housetime	worktime	housetime	worktime	housetime
Panel A: Score as Independent Variable								
score	6.264***	4.939**	-7.030*	5.828**	6.261***	5.013***	-7.608***	5.956***
	(2.034)	(1.980)	(3.800)	(2.550)	(2.561)	(1.207)	(2.292)	(1.751)
Province×year FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	12,431	12,137	11,561	10,470	17,232	16,713	17,503	16,088
Panel B: Ratio as Independent Variable								
ratio	21.671*	16.018**	-24.220*	17.572**	25.145***	14.446***	-34.164***	14.685**
	(13.132)	(7.166)	(14.638)	(7.941)	(8.455)	(3.977)	(4.559)	(7.153)
Province×year FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	12,431	12,137	11,561	10,470	17,232	16,713	17,503	16,088

† Notes: Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Dependent variables are work time and housework time in women and men sample. Key independent variable is children's gender composition, including the gender-biased score (Panel A) and the sex ratio at birth (Panel B). The first four columns only use sample living in a place over 9 years, where the instruments are still Proximity×(birthyear-1991), Proximity×(birthyear-1991)×Post-2002 dummy and Proximity×Post-2002 dummy. The latter four columns use (birthyear-1991), (birthyear-1991)×Post-2002 dummy and Post-2002 dummy as instruments. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc.

Table 3.6: The 2nd Stage Results Using Variation from the 2002 Law as Instrument

	(1)	(2)	(3)	(4)
	Men		Women	
VARIABLES	worktime	housetime	worktime	housetime
Panel A: Score as Independent Variable				
score	7.011*	6.856***	-4.712**	5.893*
	(4.185)	(3.044)	(2.579)	(3.047)
Province×year FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Observations	12,847	12,552	11,897	10,734
Panel B: Ratio as Independent Variable				
ratio	38.814*	41.300***	-23.776	56.486
	(22.668)	(20.430)	(45.521)	(66.944)
Province×year FE	Y	Y	Y	Y
Individual and Year FE	Y	Y	Y	Y
Observations	12,847	12,552	11,897	10,734

† Notes: Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Dependent variables are work time and housework time in women and men sample. Key independent variable is children's gender composition, including the gender-biased score (Panel A) and the sex ratio at birth (Panel B). The instruments are still Proximity×(birthyear-1991), Proximity×(birthyear-1991)×Post-2002 dummy and Proximity×Post-2002 dummy.. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc.

Table 3.7: The 2nd Stage Results Using Alternative Definition of Proximity

	(1)	(2)	(3)	(4)
	Men		Women	
VARIABLES	worktime	housetime	worktime	housetime
Panel A: Score as Independent Variable				
score	12.468***	13.743*	-7.694**	7.796*
	(5.349)	(7.044)	(3.845)	(4.132)
Province×year FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Observations	12,847	12,552	11,897	10,734
Panel B: Ratio as Independent Variable				
ratio	50.164***	53.167	-39.115*	61.438*
	(20.219)	(39.793)	(21.696)	(32.948)
Province×year FE	Y	Y	Y	Y
Individual and Year FE	Y	Y	Y	Y
Observations	12,847	12,552	11,897	10,734

† Notes: Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Dependent variables are work time and housework time in women and men sample. Key independent variable is children's gender composition, including the gender-biased score (Panel A) and the sex ratio at birth (Panel B). The instrument is Proximity×(birthyear-1991)×Post-2002 dummy only. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc. Proximity×(birthyear-1991) and Proximity×Post-2002 dummy are also included as controls.

Table 3.8: Reduced Form Results

	(1)	(2)	(3)	(4)
	Men		Women	
VARIABLES	worktime	housetime	worktime	housetime
Proximity×(birthyear-1991)	0.147	0.251**	-0.881*	0.221**
	(0.551)	(0.122)	(0.492)	(0.111)
Proximity×(birthyear-1991)	-1.921*	-1.429***	2.083*	-6.958***
×Post-2002 dummy	(1.071)	(0.484)	(1.233)	(0.769)
Proximity×Post-2002 dummy	6.004	17.259**	9.747	90.270***
	(14.279)	(6.813)	(16.933)	(10.552)
Province×year FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Observations	13,130	12,714	12,546	11,419
R-squared	0.084	0.062	0.191	0.066

† Notes: Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Dependent variable are work time and housework time in both men and women sample. Key independent variables are Proximity×(birthyear-1991), Proximity×(birthyear-1991)×Post-2002 dummy and Proximity×Post-2002 dummy. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc.

Table 3.9: Placebo Test Results

	(1)	(2)	(3)	(4)
	Men		Women	
VARIABLES	worktime	housetime	worktime	housetime
Proximity×(birthyear-1991)	-0.470	-1.333	-0.802	-3.745*
	(3.615)	(1.438)	(2.908)	(2.269)
Proximity×(birthyear-1991)	-2.737	7.041	-9.363	-2.196
×Post-2002 dummy	(15.310)	(6.083)	(7.080)	(5.519)
Proximity×Post-2002 dummy	-35.212	-87.177	146.093	71.484
	(256.168)	(101.848)	(100.186)	(78.088)
Province×year FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Observations	3,131	3,108	3,453	3,432
R-squared	0.086	0.067	0.129	0.129

† Notes: Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Dependent variable are work time and housework time in both men and women sample without any children. Key independent variables are Proximity×(birthyear-1991), Proximity×(birthyear-1991)×Post-2002 dummy and Proximity×Post-2002 dummy. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc.

Table 3.10: Impacts on Working Hours and Decision to Work

	(1)	(2)	(3)	(4)
	Men		Women	
VARIABLES	work hours	1(work hour>0)	work hour	1(work hour>0)
Panel A: Score as Independent Variable				
score	6.011***	0.002	-4.168*	-0.086***
	(2.166)	(0.059)	(2.189)	(0.034)
Province×year FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Observations	9,194	12,847	6,619	11,897
Panel B: Ratio as Independent Variable				
ratio	20.916*	0.019	-27.776*	-0.301**
	(11.565)	(0.415)	(15.164)	(0.144)
Province×year FE	Y	Y	Y	Y
Individual and Year FE	Y	Y	Y	Y
Observations	9,194	12,847	6,619	11,897

† Notes: Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Dependent variable are length of work hours for all economic active individuals and whether to participate in the labour force. Key independent variable is children's gender composition, including the gender-biased score (Panel A) and the sex ratio at birth (Panel B). The instrument is Proximity×(birthyear-1991)×Post-2002 dummy, Proximity×(birthyear-1991) and Proximity×Post-2002 dummy. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc.

Table 3.11: Impacts On Men's Household labour Time

	(1)	(2)	(3)	(4)
METHOD	OLS	2SLS	OLS	2SLS
VARIABLES	housetime	housetime	housetime	housetime
score	-0.015	5.449***		
	(0.123)	(2.068)		
ratio			-0.208	18.467**
			(0.365)	(7.942)
Province×year FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Observations	17,033	12,552	17,033	12,552

† Notes: Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Key independent variable is children's gender composition, including the gender-biased score and the sex ratio at birth. The instruments are Proximity×(birthyear-1991), Proximity×(birthyear-1991)×Post-2002 dummy and Proximity×Post-2002 dummy. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc.

Table 3.12: Impacts On Different types of Household labour Time

	(1)	(2)	(3)	(4)
	Men		Women	
VARIABLES	non child-care	child-care	non child-care	child-care
Panel A: Score as Independent Variable				
score	1.341	4.310***	0.601	3.745**
	(1.378)	(1.175)	(1.677)	(1.897)
Province×year FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Observations	12,552	12,852	10,734	11,898
Panel B: Ratio as Independent Variable				
ratio	5.266	16.265***	0.978	13.137*
	(5.242)	(4.728)	(4.889)	(7.767)
Province×year FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Observations	12,552	12,852	10,734	11,898

† Notes: Standard errors are clustered on community level. *** denotes significance at 0.01, ** at 0.05, and * at 0.1. Dependent variables are non child-care and child-care housework time respectively. Key independent variable is children's gender composition, including the gender-biased score (Panel A) and the sex ratio at birth (Panel B). The instruments are Proximity×(birthyear-1991), Proximity×(birthyear-1991)×Post-2002 dummy and Proximity×Post-2002 dummy. Control variables include whether there are old parents in the family, whether the individual reaches retiring age, the total number of children, province×year fixed effects, interactive between time dummies and education, urban/rural etc.

3.7 Appendix

3.7.1 Comparative Static Results of the Utility-based Model

To simplify, assume that both utility functions are additively separable and increasing and concave in each argument ($U_i > 0$; $U_{ii} < 0, \forall i$; $U_{ij} < 0, \forall i \neq j$; similar for V), and that an interior solution exists. The first order conditions can easily be obtained. Take the partial derivative with respect to β, and the formulae, including 6 comparative static variables, can be written as follows:

$$\begin{pmatrix} U_{11} & 0 & 0 & 0 & 0 & -1 \\ 0 & \mu V_{11} & 0 & 0 & 0 & -1 \\ 0 & 0 & U_{22} & 0 & 0 & -W_1 \\ 0 & 0 & 0 & \mu V_{22} & 0 & -W_2 \\ 0 & 0 & 0 & 0 & \pi(\beta)g'' & -W_2 \\ 1 & 1 & W_1 & W_2 & W_2 & 0 \end{pmatrix} \begin{pmatrix} \frac{\partial C_1^*}{\partial \beta} \\ \frac{\partial C_2^*}{\partial \beta} \\ \frac{\partial L_1^*}{\partial \beta} \\ \frac{\partial L_2^*}{\partial \beta} \\ \frac{\partial R_2^*}{\partial \beta} \\ \frac{\partial \lambda^*}{\partial \beta} \end{pmatrix} = \begin{pmatrix} 0 \\ -\mu_2 a_2' V_1 \\ 0 \\ -\mu_2 a_2' V_2 \\ -\pi'(\beta) g' \\ 0 \end{pmatrix} \tag{3.19}$$

Applying Cramer's Rule, the following comparative statics results can be obtained:

$$\begin{aligned} \Delta = {} & \mu^2 V_{11} V_{22} (\pi g'' U_{22} + W_1^2 \pi g'' U_{11} + W_2^2 U_{11} U_{22}) \\ & + \mu \pi g'' U_{11} U_{22} (V_{22} + W_2^2 V_{11}) > 0 \end{aligned} \tag{3.20}$$

$$\Delta_3 = W_1 \mu U_{11} (\mu_2 a_2' \pi V_1 g'' V_{22} + W_2 \mu_2 a_2' V_2 \pi g'' V_{11} + W_1 W_2 \mu \pi' g' V_{11} V_{22}) > 0 \tag{3.21}$$

$$\Delta_4 = -\mu W_2^2 V_{11} U_{11} U_{22} (\mu_2 a_2' V_2 - \pi' g') - \mu \mu_2 a_2' \pi g'' V_{11} (V_2 U_{22} + W_1^2 U_{11}) \lesseqgtr 0 \tag{3.22}$$

$$\begin{aligned} \Delta_5 = {} & -\mu \pi' g' W_2 V_{11} U_{11} U_{22} (1 - \mu_2 a_2') - \mu V_{22} U_{11} U_{22} (\pi' g' - \mu_2 a_2' V_2) \\ & - \mu^2 \pi' g' V_{11} V_{22} (U_{22} + W_1 U_{11}) \lesseqgtr 0 \end{aligned} \tag{3.23}$$

$$\begin{aligned} -(\Delta_4 + \Delta_5) = {} & \mu V_{22} U_{11} U_{22} (\pi' g' - \mu_2 a_2' V_2) + \mu \mu_2 a_2' W_2^2 V_{11} U_{11} U_{22} (V_2 - \pi' g') \\ & + \mu^2 \pi' g' V_{11} V_{22} (U_{22} + W_1 U_{11}) + \mu \mu_2 a_2' \pi g'' V_{11} (V_2 U_{22} + W_1^2 U_{11}) \lesseqgtr 0 \end{aligned} \tag{3.24}$$

So that

$$\frac{\partial L_1^*}{\partial \beta} = \frac{\Delta_3}{\Delta} < 0 \tag{3.25}$$

$$\frac{\partial L_2^*}{\partial \beta} = \frac{\Delta_4}{\Delta} \lesseqgtr 0 \tag{3.26}$$

$$\frac{\partial R_2^*}{\partial \beta} = \frac{\Delta_5}{\Delta} \lesseqgtr 0 \tag{3.27}$$

which implies

$$\frac{\partial H_1^*}{\partial \beta} = -\frac{\Delta_3}{\Delta} > 0 \tag{3.28}$$

$$\frac{\partial H_2^*}{\partial \beta} = -\frac{(\Delta_4 + \Delta_5)}{\Delta} \lesseqgtr 0 \tag{3.29}$$

Now let us check the ambiguous effects on L_2, R_2 and H_2 in detail. From the three expressions above, it can be found that most of the terms in them take one direction: most terms in $\partial L_2^*/\partial\beta$ and $\partial R_2^*/\partial\beta$ are positive, while for $\partial H_2^*/\partial\beta$ they are negative So it is straightforward to find conditions that will guarantee unambiguous results. The key is the relative marginal effect on the wife's bargaining power compared to that on the husband's utility. For example, one of the conditions which can cancel out all the negative terms in $\partial R_2^*/\partial\beta$ is $\mu_2 a_2' < 1$ and $\mu_2 a_2' V_2 < \pi' g'$, which intuitively means that the increase in the wife's bargaining power when she has more sons instead of daughters is smaller than the increase in the husband's utility due to his preference for sons. Even if the conditions are not met, it is totally possible that the negative terms can also be dominated by other positive terms. The same arguments apply to $\partial L_2^*/\partial\beta$ and $\partial H_2^*/\partial\beta$, too.

3.7.2 Comparative Static Results of the Constraint-based Model

The basic assumptions for utility functions are similar. After obtaining the first order conditions, take the partial derivative with respect to β; then the formulae including eight comparative static variables can be written as follows:

$$\begin{pmatrix} U_{11} & 0 & 0 & 0 & 0 & 0 & 0 & -1 \\ 0 & U_{22} & 0 & 0 & 0 & 0 & 0 & -\rho \\ 0 & 0 & U_{33} & 0 & 0 & 0 & 0 & -W_1 \\ 0 & 0 & 0 & \mu V_{11} & 0 & 0 & 0 & -1 \\ 0 & 0 & 0 & 0 & \mu V_{22} & 0 & 0 & -\rho \\ 0 & 0 & 0 & 0 & 0 & \mu V_{33} & 0 & -W_2 \\ 0 & 0 & 0 & 0 & 0 & 0 & \rho\beta g'' & 0 \\ 1 & \rho & W_1 & 1 & \rho & W_2 & 0 & 0 \end{pmatrix} \begin{pmatrix} \frac{\partial C_1^*}{\partial \beta} \\ \frac{\partial C_2^*}{\partial \beta} \\ \frac{\partial L_1^*}{\partial \beta} \\ \frac{\partial C_3^*}{\partial \beta} \\ \frac{\partial C_4^*}{\partial \beta} \\ \frac{\partial L_2^*}{\partial \beta} \\ \frac{\partial R_2^*}{\partial \beta} \\ \frac{\partial \lambda^*}{\partial \beta} \end{pmatrix} = \begin{pmatrix} 0 \\ 0 \\ 0 \\ 0 \\ -\mu_2 a_2' V_1 \\ -\mu_2 a_2' V_2 \\ -\mu_2 a_2' V_3 \\ -\rho g' \end{pmatrix} \tag{3.30}$$

Applying Cramer's Rule, the following comparative statics results can be obtained:

$$\begin{aligned} \Delta = {} & \mu^3 \rho\beta g'' V_{11} V_{22} V_{33} (U_{22} U_{33} + \rho^2 U_{11} U_{33} + W_1^2 U_{11} U_{22}) \\ & + \mu^2 \rho\beta g'' U_{11} U_{22} U_{33} (V_{22} V_{33} + \rho^2 V_{11} V_{33} + W_2^2 V_{11} V_{22}) > 0 \end{aligned} \tag{3.31}$$

$$\begin{aligned} \Delta_3 = {} & \mu^2 \rho\beta g'' U_{11} U_{22} U_{33} (\mu\rho g W_1 V_1 V_{22} V_{33} + \mu_2 \rho W_1 a_2' V_2 V_{11} V_{33} \\ & + g' V_3 V_{11} V_{22}) > 0 \end{aligned} \tag{3.32}$$

$$\begin{aligned} \Delta_6 = {} & -\mu_2 \mu^2 a_2' \rho\beta g'' V_3 V_{11} V_{22} (U_{22} U_{33} + \rho^2 U_{11} U_{33} + W_1^2 U_{11} U_{22}) \\ & + \mu^2 \rho^2 \beta g'' g W_2^2 U_{11} U_{22} U_{33} V_{11} V_{22} > 0 \end{aligned} \tag{3.33}$$

$$\begin{aligned} \Delta_7 = {} & -\mu^2 \rho g' (W_2^2 V_{11} V_{22} U_{11} U_{22} U_{33} + \rho^2 V_{11} V_{33} U_{11} U_{22} U_{33} + W_1^2 V_{11} V_{22} V_{33} U_{11} U_{22} \\ & + \mu V_{11} V_{22} V_{33} U_{22} U_{33} + \mu\rho^2 V_{11} V_{22} V_{33} U_{11} U_{33} + V_{22} V_{33} U_{11} U_{22} U_{33}) > 0 \end{aligned} \tag{3.34}$$

$$\frac{\partial L_1^*}{\partial \beta} = \frac{\Delta_3}{\Delta} > 0 \tag{3.35}$$

$$\frac{\partial L_2^*}{\partial \beta} = \frac{\Delta_6}{\Delta} > 0 \tag{3.36}$$

$$\frac{\partial R_2^*}{\partial \beta} = \frac{\Delta_7}{\Delta} > 0 \tag{3.37}$$

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