

## ABSTRACT

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This dissertation contains three essays. In Chapter 2, I investigate the causal factors of the soft budget constraint (SBC) problem. Based on a panel dataset from a survey of Chinese enterprises, the test results support the policy burden hypothesis but not the ownership hypothesis. The findings emphasize the importance of creating a sound social security system in the process of China's enterprise reform.

The other two essays focus on the upgrading of counties to cities in China. Chapter 3 examines its role in providing local governments with incentives on economic growth. Using a large panel data set covering all counties during 1993-2004, I find that the official minimum requirements for upgrading are not enforced in practice. Instead, a county's economic growth rate plays a key role in obtaining city status. Furthermore, I conduct an empirical test to distinguish between a principal-agent incentive mechanism and political bargaining. The findings are consistent with the hypothesis

that the central government uses upgrading to reward local officials for high growth, as well as aligning local interests with those of the center. This essay highlights the importance of both fiscal and political incentives facing the local government.

Chapter 4 examines the consequences of upgrading by looking at various economic, fiscal and public service outcomes. I find that city status increases government size and revenues, and creates more urban employment opportunities. However, there is no significant improvement in local public services after counties were upgraded, and their high growth rates dropped. These results are interpreted by analyzing the incentive structure of local government officials.

THREE ESSAYS ON CHINA'S ECONOMIC REFORM

By

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## Dedication

To my home province, Sichuan,  
and to its people who suffered in the earthquake on the 12th of May, 2008.

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I would like to thank my advisor, Peter Murrell, for his invaluable guidance, patience and encouragement over the years. I feel very fortunate to have such an advisor who always has time to discuss questions with students. His help and support extends far beyond this dissertation.

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# Table of Contents

Dedication.....	ii
Acknowledgements.....	iii
Table of Contents.....	v
List of Tables.....	vii
List of Figures.....	viii
Chapter 1: Introduction.....	1
Chapter 2: Employment Burden, Government Ownership and Soft Budget Constraints: Evidence from a Chinese Enterprise Survey.....	3
2.1 Introduction.....	4
2.2 The policy burden hypothesis and the empirical model.....	7
2.2.1 The employment burden in China's SOEs.....	7
2.2.2 The policy burden hypothesis.....	8
2.2.3 Measurement of soft budgets and the employment burden.....	10
2.2.4 Empirical model.....	11
2.3 Data.....	13
2.4 Econometric problems.....	14
2.5 Empirical results.....	17
2.5.1 First-stage results.....	17
2.5.2 Main results.....	18
2.5.3 Soft taxes and incentives of the local and central governments.....	20
2.5.4 Robustness tests.....	22
2.6 Conclusion.....	23
Chapter 3: The Incentive Role of Creating "Cities" in China.....	29
3.1 Introduction.....	30
3.2 Institutional background: county-to-city upgrading in China.....	33
3.3 Hypotheses and empirical strategy.....	36
3.3.1 Non-enforcement of the formal rules.....	36
3.3.2 Main hypothesis and the empirical model.....	37
3.3.3 Why growth matters? Two alternative hypotheses.....	38
3.3.4 Incentive versus bargaining: empirical strategy.....	41
3.4 Data.....	43
3.5 Empirical results.....	46
3.5.1 The positive effect of growth rate on upgrading.....	47
3.5.2 Testing the non-enforcement of upgrading requirements.....	52
3.5.3 The incentive hypothesis versus the bargaining hypothesis.....	55
3.6 Conclusion.....	58

Chapter 4: The Consequences of Administrative Decentralization: Evidence from China’s County-to-City Upgrading.....	78
4.1 Introduction.....	79
4.2 Data and descriptive analysis.....	82
4.3 Empirical strategies.....	84
4.3.1 Difference-in-differences model.....	84
4.3.2 Propensity score matching.....	85
4.3.3 Difference-in-differences propensity score matching.....	86
4.4 Difference-in-differences estimation results.....	86
4.5 Matching results.....	88
4.5.1 Estimating propensity scores .....	88
4.5.2 Matching results.....	89
4.5.3 Difference-in-differences propensity score matching results .....	90
4.6 Conclusion .....	91
Bibliography .....	100

## List of Tables

Chapter 2	
Table 1: Summary statistics .....	25
Table 2: First-stage regression results.....	26
Table 3: Regression analysis - main results.....	27
Table 4: Regression analysis - robustness tests .....	28
Chapter 3	
Table 1. Minimum requirements for county-to-city upgrading.....	65
Table 2. Benefits of being a city: an incomplete list.....	66
Table 3. Construction of the sample for the logit model .....	67
Table 4. Comparison of mean values: upgrading and non-upgrading cases (1993-1997) .....	68
Table 5. Comparison of mean values: counties and county-level cities (1999-2004)	69
Table 6. Positive effect of growth rate on upgrading - baseline results.....	70
Table 7. Positive effect of growth rate on upgrading – robustness checks.....	71
Table 8. Positive effect of growth rate on upgrading – robustness checks (continued) .....	72
Table 9. Correction for sample attrition – probit model with sample selection .....	73
Table 10. Number of counties that satisfied each upgrading requirement in 1993 ....	74
Table 11. Number of county-year observations by upgrading status and requirements satisfied (1994-1997) .....	74
Table 12. Number and percentage of county-year observations by upgrading status and number of requirements satisfied (1994-1997).....	75
Table 13. Number of upgrading cases that satisfied each requirement by region .....	75
Table 14. Non-enforcement of upgrading requirements – allowing flexible intercept and marginal effect .....	76
Table 15. Incentive versus bargaining- results .....	77
Chapter 4	
Table 1: Mean value of variables from public finance dataset .....	96
Table 2: Mean value of variables from population census dataset .....	97
Table 3: Difference-in-differences estimation results.....	98
Table 4: Difference-in-differences estimation results after decomposition.....	98
Table 5: Propensity score matching results .....	99
Table 6: Difference-in-differences propensity score matching estimation.....	99

## List of Figures

### Chapter 3

Figure 1. Number of county-to-city upgrading cases and yearly GDP growth rate ...	61
Figure 2. The spectrum of political models on China's central-local relationship.....	62
Figure 3. Graphical interpretation of results drawn from Table 14 .....	63
Figure 4. Comparison of interest divergence: counties and cities .....	64

### Chapter 4

Figure 1: Comparison of growth rates: upgrading cases and the national average ....	93
Figure 2: Trend of growth rates before and after upgrading.....	94
Figure 3. Histogram of propensity score.....	95
Figure 4. Histogram of log-odds ratio of propensity score.....	95

## Chapter 1: Introduction

This dissertation studies several important issues in China's economic reform that started in 1978.

Chapter 2 studies enterprise reform. There are two competing theories about the causal factors of soft-budget constraint (SBC) problem, namely the ownership hypothesis and the policy burden hypothesis. While the ownership hypothesis attributes the SBC problem to government ownership, the policy burden hypothesis predicts that privatization would not eliminate the SBC problem, as long as the major policy burden – maintaining employment – is not removed from enterprises. My results support the policy burden hypothesis but not the ownership hypothesis. This finding emphasizes the importance of creating a sound social security system in the process of China's enterprise reform.

In Chapter 3 and 4, I turn my focus to reforms in the government sector. The focus is on a distinctive way of creating new urban administrative units in China - upgrading counties to county-level cities. By awarding the title of city to existing counties, upgrading has created more than 400 new cities from 1983 to 1997. During upgrading, local government gets many benefits, including political privileges, administrative independence and revenue collection power.

Chapter 3 studies the determinants of upgrading. The main idea is that upgrading serves as a mechanism of providing incentives to local governments. Using data from 1994 to 2004, I find that a county's economic growth rate plays a key role in obtaining city status. I then conduct an empirical test to distinguish between a principal-agent incentive mechanism and political bargaining. The findings are consistent with the hypothesis that the central government uses upgrading to reward local officials for high growth, as well as aligning local interests with those of the center.

Chapter 4 examines the consequences of upgrading. I merge data from the local public finance statistics with the population census and get a rich set of economic, fiscal and public services outcome variables. My finding confirms that city status

increases government size and revenues, and creates more urban employment opportunities. However, there is no significant improvement in local public services after upgrading, and the high economic growth rates in pre-upgrading period drop to a normal level after upgrading. These results are interpreted by analyzing the incentive structure of local government officials.

The findings of the last two chapters highlight the key role of fiscal and political incentives facing local governments in China. The comparison between incentive and bargaining mechanisms sheds light on an important question about China's politics of governance: where does power lie in China?

## Chapter 2: Employment Burden, Government Ownership and Soft Budget Constraints: Evidence from a Chinese Enterprise Survey

### **Abstract**

There are two competing theories of soft budget constraint (SBC), namely the ownership hypothesis and the policy burden hypothesis. While the ownership hypothesis attributes the SBC problem to government ownership, the policy burden hypothesis predicts that privatization would not eliminate the SBC problem, as long as the major policy burden – maintaining employment – is not removed from enterprises. Using a panel dataset from a survey of Chinese enterprises, I conduct empirical tests on these two competing hypotheses. I explicitly address the endogeneity problems by using instruments, as well as estimating a two-step tobit model to improve the functional form for corner solution outcomes. The test results support the policy burden hypothesis but not the ownership hypothesis. My finding emphasizes the importance of creating a sound social security system in the process of China's enterprise reform.

## 2.1 Introduction

Although the soft budget constraint (SBC) has been discussed widely in the literature since Kornai's seminal work (Kornai, 1980), there exists no consensus on which factors are responsible for the prevalence of soft budgets in the enterprises of transition countries (Anderson et al., 2000; Kornai et al. 2003; Lin and Li, 2006). This paper tests two competing theories of the SBC using firm-level data from China. These two theories are the ownership hypothesis and the policy burden hypothesis.

According to the policy burden hypothesis (Lin et al., 1998), various policy burdens, as legacies of pre-reform policies, are the causal factors of soft budgets in China's State Owned Enterprises (SOEs). Among these policy burdens, the most critical one is the employment burden. Before the economic reform, due to the lack of a social security system, SOEs played the role of providing social welfare to all workers. During the reform era, under pressure from the government, SOEs continue to offer pensions to retired workers and provide jobs for redundant workers. Retired and redundant workers pose a heavy burden and cause losses to enterprises. Informational asymmetries make it hard to distinguish between the losses due to this employment burden and the SOEs' own operational losses. As a result, government at different levels has to take measures to bear the losses, thus softening the budget constraints of those SOEs. Moreover, privatized enterprises may also bear such an employment burden due to government influence, thus they may also have soft budgets.

In contrast, the ownership hypothesis attributes the SBC problem to an enterprise's ownership type, claiming that government ownership is the causal factor of soft budgets (Li, 1992). Since the SBC was first analyzed as a phenomenon of socialist economies, many theoretical models are built with an implicit assumption of state ownership, thus failing to differentiate the policy burden as a separate reason for soft budgets. Since we usually observe state ownership and soft budgets together in many firms, it is no surprise to find a positive correlation between them. However, in order to conclude that there exists a causal relationship, one needs to control for factors that could also affect soft budgets, as well as to deal with possible endogeneity

problems. Otherwise, the correlation may be purely due to omitted variables, such as the unobserved quality of firms.

In order to conduct tests on these two theories, variation of ownership and the employment burden is needed. Before 1995, there was little change to SOEs' ownership. Moreover, the reforms relevant to the employment decisions in SOEs remained very limited in scope (Bodmer, 2002). It was not until 1995 that radical forms of enterprise restructuring took place under the policy of “grasping the large and letting go the small” (*zhuada fangxiao*). Since then, many enterprises introduced private shares and started to lay off large numbers of workers. The Chinese word “*gaizhi*” is used to denote such radical enterprise reforms.<sup>1</sup> Many SOEs went through *gaizhi* during the sample period of 1995-2001, thus creating sufficient variation in ownership and employment burden to conduct meaningful tests.

In my regressions, I include measures of both the employment burden and ownership on the right-hand side to estimate their effects on the soft budgets. This allows me to identify which one is the causal factor of soft budgets and to distinguish one hypothesis from the other. To handle the endogeneity problem of ownership and employment burden, I use a set of instruments that reflect the exogenous variation of privatization and employment policies. Since the dependent variable (measure of the soft budgets) equals zero in many cases, a two-step tobit model is adopted as improving functional form for corner solution outcomes. My test results generally support the policy burden hypothesis but not the ownership hypothesis. I also find that different measures of soft budgets respond in different ways to changes in the employment burden. These results are explained by analyzing the different incentives faced by the local and central government. Finally, I conduct several robustness tests.

The relationship among the employment burden, privatization and the SBC in China has attracted a lot of attention in recent years. Some researchers look at the determinants of privatization, such as Guo and Yao (2005), Bai et al. (2005) and Brandt et al. (2005); others investigate the causes of employment adjustment in SOEs (Dong and Putterman, 2003) or in the public sector (Dong and Xu, 2006). However,

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<sup>1</sup> Literally, *gaizhi* means transformation of (ownership) system; practically, *gaizhi* includes many types of reform, not all of which contain privatization. For detailed description of *gaizhi* in China, see Garnaut et al. (2004).

these papers do not answer the question “what is the causal factor of the soft budgets”. For example, Dong and Putterman (2003) summarize that “overstaffing of SOEs has been commonly understood as a by-product of soft budget constraints”. In other words, they assume the soft budgets to be the reason for employment redundancy. Thus, they neglect the fact that the existence of any soft budgets cannot be taken as exogenous and needs to be explained. Bai et al. (2005) raise the question of whether stability concerns are at the root of the soft budgets, but do not provide an answer. Thus, the determinants of soft budgets in Chinese enterprises remain an unsolved question.

Among studies that investigate the causal factors of soft budgets, Anderson et al. (2000) survey Mongolian enterprises and show that government ownership, especially central government ownership, leads to soft budgets. Frydman et al. (2000) look at central and eastern European firms and conclude that privatization is necessary to harden budget constraints. These two papers, like most others, do not examine the policy burden issue.<sup>2</sup> Although Li and Liang (1998) estimate the effect of redundant employment on soft budgets, they do not answer whether it is the ultimate causal factor of the SBC problem. Luo (2005) examines the effect of privatization on the hardening of budget constraints but does not analyze the policy burden hypothesis as a competing theory. In fact, studies discussing the policy burden hypothesis are rare. The two exceptions are Lin and Li (2006) and Gu and Zhang (2006), who use theoretical models to demonstrate that policy burdens lead to or intensifies SBC problem. To the best of my knowledge, the only paper that directly tests the policy burden hypothesis is Lin et al. (2003). They use industry-level data and get results that are consistent with the policy burden hypothesis. Compared to these studies, this paper contributes to the empirical SBC literature by explicitly testing the policy burden hypothesis against the ownership hypotheses using firm-level data that covers a period when Chinese firms experienced great changes in both ownership and employment.

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<sup>2</sup> See Djankov and Murrell (2002) for a broad survey of the empirical SBC literature.

The rest of the paper is organized as follows. Section 2.2 discusses the hypothesis and measurement issues, and then lays out the empirical model. Section 2.3 introduces the survey and describes the data. Section 2.4 discusses econometric problems. Section 2.5 presents the main results, followed by interpretation and robustness tests. Section 2.6 briefly concludes the paper.

## **2.2 The policy burden hypothesis and the empirical model**

### **2.2.1 The employment burden in China's SOEs**

The issue of the employment burden has a special importance in China, predominantly because of the central government's great concern over social stability. Without a well-functioning social security system, the Chinese government has had to rely on SOEs to provide social welfare in both pre- and post-reform periods. While the ostensible objective of enterprise reforms is to improve the financial performance of SOEs through market discipline, the state has been hesitant to relieve SOEs of their traditional roles as guarantors of job security and welfare of their employees (Dong and Putterman, 2003). Maintaining high employment in the state sector has always been the major concern of the central government during the entire reform era, including the late 1990s. In an important speech in 1997, Vice Premier Zhu Rongji stressed that the problem of excessive workers in SOEs should not be resolved through massive layoffs (Bai et al., 2000). Thus, it is no surprise to see that many SOEs bear heavy employment burdens under pressure from different levels of government.

The employment burden comes from both retired and non-retired employees. According to the China Statistical Yearbooks, the ratio of retired to on-duty SOE employees rose rapidly from 1:26 in 1978 to 1:4.6 in 1995. The accumulation of large amounts of retirees poses a great burden to the enterprises in terms of pensions, housing, medical care and other needs (Lin et al., 1998). This was true even in the late 1990s, when the pensions of legally retired workers started to be paid out of the social security account, because retirees still receive other forms of welfare compensation from their affiliated enterprises. Also, many non-retired workers are made redundant

in the form of *xiagang* and internal retirement. *Xiagang* represents a type of interim unemployment whereby a worker stops working but still keeps a nominal tie with the enterprise through work registration. *Xiagang* workers receive a living allowance from the re-employment center of their SOE for three years or until they are re-employed. Internally retired workers are those that take an early retirement but continue to receive a wage from the enterprise (typically at a lower rate) until they reach the legal retirement age.

The magnitude of redundant labor is substantial. A director of the social security department in the Ministry of Labor once said "... about 20 percent of the workers and employees in SOEs are surplus labor" (Broadman, 1995). Using a panel dataset of Chinese provinces from 1986 to 1996, Li and Xu (2002) find that if all redundant workers were to be released out of the enterprises, the average urban unemployment rate could reach as high as 25%. In a survey of 769 SOEs, Bodmer (2002) finds that 83% of SOEs report excess employment labor in 1994, and the actual number of workers employed exceeds the desired level by around 20%. Using another survey of 700 SOEs, Dong and Putterman (2003) show that the mean labor redundancy rate has increased from 28.6% in 1991 to 44.4% in 1994. Overall, these studies suggest that the redundancy rate in Chinese SOEs was at least 20% in the mid-1990s before *gaizhi* began.

### **2.2.2 The policy burden hypothesis**

As Kornai et al. (2003) have pointed out, the early SBC literature examines almost exclusively enterprises under state ownership. The consequence is too much attention on firms' government ownership and not enough attention on other important factors. This has been changed in the recent literature. For example, Shleifer and Vishny (1994) discuss politicians' influence on enterprises' employment and production decisions. Dewatripont and Maskin (1995) relate the SBC problem with the creditor's lack of information and commitment, and therefore break the specific relationship between the soft budgets and the socialist system. Bai and Wang (1998) bring the politicians' influence and information asymmetry together and emphasize the agency problem of the bureaucrats.

The policy burden theory inherits the information perspective of these theories, and is based upon the moral hazard problem involving the enterprise manager's behavior (Lin and Li, 2006). Various policy burdens put SOEs in a disadvantaged situation in competition with non-state enterprises. As long as there is a policy burden on an enterprise, the government has to assist it at times of difficulty. In theory, the government should be responsible only for the losses arising from policy burdens. However, due to information asymmetries, managers have an incentive to attribute all their losses to policy burdens, and the government is not able to distinguish whether the losses are due to policy burdens or caused by operational failures. Thus the government has to bear all the losses by providing subsidies, cutting taxes or offering other forms of compensations to those enterprises in difficulty (Lin et al., 1998; Lin and Tan, 1999). These compensations will soften the budget constraint of SOEs (or any enterprises that bear policy burdens and receive aids). The major policy burdens include the employment burden, the strategic burden and price distortions. In this paper, I focus exclusively on the employment burden. The causal effect of the employment burden means that at least some kinds of policy burdens are responsible for soft budgets, thus the positive results of a link between the employment burden and soft budgets could be interpreted as supporting the policy burden hypothesis.

Since the ownership hypothesis does not exclude the employment consideration of the government, people might ask "what is the difference between these two theories?" According to the policy burden hypothesis, privatization will not eliminate soft budgets if the burdens are not removed, because government ownership is not the ultimate determinant for soft budgets. In fact, as long as any enterprise still bears burdens such as maintaining employment, soft budgets will continue to exist regardless of its ownership type. This stands in contrast to the prediction of the ownership hypothesis.

This point has also been made in other studies. Kornai et al. (2003) mention that "it is not rare for firms in private ownership to be rescued from financial straits; this has been particularly evident in post-socialist transition where privatization has by no means ended the practice of bailouts." According to the World Bank (2002), many firms in Eastern Europe actually received more government subsidies after

privatization than before. In China, as I will demonstrate later, employment burdens have remained prevalent even after *gaizhi*. Thus, I would expect to see that soft budgets continue to exist, if the policy burden hypothesis holds.

To test these two hypotheses, I include measures of both the employment burden and ownership as independent variables in the regression. This allows me to identify which factor is responsible for soft budgets and to distinguish one hypothesis from the other. If the policy burden hypothesis holds, there should be a positive effect of employment burden on soft budgets, while private ownership should not make much difference from government ownership. Alternatively, if the ownership hypothesis holds, we should find a positive effect of state ownership but not employment burden.

### **2.2.3 Measurement of soft budgets and the employment burden**

As stated in Kornai et al. (2003), there are various means of softening an enterprise's budget constraint. Accordingly, soft budgets can take different forms, such as soft subsidies, soft taxes, soft credit and soft administrative pricing (Sjoberg and Gang, 1996). Since I do not have information on managers' expectation of being bailed out (Anderson et al., 2000), I use four different forms of aids received by firms to measure soft budgets. These measures cover various means of "softness" mentioned in the literature. In particular, I have one variable to measure soft budgets from banks and three variables to measure soft budgets from the government. Following Garnaut et al. (2005), overdue bank debts, which equal to the sum of overdue loans and overdue interest payment, are used to measure the bank soft budget. I have three variables to measure soft budgets from governments: government subsidies, soft taxes (overdue taxes plus tax exemptions) and overdue social security payments. While government subsidies are a direct measure of aid from the government, the other three measures capture the idea that the government or banks impose weak financial or fiscal discipline on firms and allow the extension of overdue payments. These measures also distinguish between "stocks" (such as overdue bank debts) and "flows" (such as government subsidies). Thus, they satisfy the standards set by Schaffer (1998), who emphasizes the importance of "actually receiving net financing" as evidence of soft budgets. In sum, the measures of soft

budgets include *overdue bank debts*, *government subsidies*, *soft taxes* and *overdue social security payments*. They satisfy my requirements and are consistent with the literature (see summary in Table 1 of Kornai et al. 2003). To make these variables comparable across enterprises with different sizes, I normalize them by the total assets of each enterprise.

To measure the employment burden, I use two variables that capture different sides of the employment adjustment story. In one case, I use the *redundancy rate* to measure the direct burden from redundant workers, which are defined as the sum of *xiagang*, internally retired and retired workers. These workers make no contribution to their affiliated enterprises, but they are still partly supported by those enterprises and therefore become a “burden” (Song and Yao, 2004). The redundancy rate is defined as the ratio of redundant workers to total workers of that firm. In the other case, I use the *layoff rate* to measure the reduction of the employment burden. Instead of keeping redundant workers in the firm, laying them off alleviates the firm’s employment burden. Thus, the layoff rate and the redundancy rate have opposite meanings. Here the layoff rate is defined as the number of workers being laid off divided by number of total workers plus the number of workers being laid off in that year. The reason for including the number of workers being laid off in the denominator is to keep the layoff rate between zero and one, given that some enterprises discharge most of their workers in a single year. In sum, a heavier employment burden implies a higher redundancy rate and a lower layoff rate. The effect of the *redundancy rate* on soft budgets should be positive while that of *layoff rate* should be negative, if the policy burden hypothesis holds.

#### **2.2.4 Empirical model**

The empirical model is  $Y_{it} = \alpha_t + G_{it}'\beta_1 + Z_{it}'\beta_2 + X_{it}'\beta_3 + e_{it}$ . For firm  $i$  in year  $t$ ,  $Y_{it}$ ,  $G_{it}$  and  $Z_{it}$  represent the soft budgets, ownership and employment burdens, respectively. Year dummies ( $\alpha_t$ ) are used to capture the impact of nation-wide policy changes. Other variables ( $X_{it}$ ) are included to control for factors that may have an impact on the soft budget, such as enterprise size and performance. Finally,  $e_{it}$  represents the error term. I add a full set of city dummies in the regression to control

for city-level policies and market conditions that may affect soft budgets. This method is consistent with other studies that use the same data, such as Guo and Yao (2005). Although controlling for firm fixed-effects could help to address the omitted variable problem, I choose not to control for them because I want to fully utilize the between-firm variation. The ownership and control variables are measured in the following way.

Ownership dummy  $G$ : As described in the introduction section, *gaizhi* takes many different forms, not all of which involve privatization. For example, a firm could be internally restructured without transfer of ownership from the state to private hands. Internal restructuring further include several forms, such as incorporation, spin-off and debt-equity swaps. Even in cases where private shares are introduced, the government may still keep controlling shares in that firm. Thus I distinguish between three different types of ownership. A *state controlled* firm is one where different levels of government or SOEs hold more than half but not all of its shares. This type of firm is also called partial SOEs.<sup>3</sup> A *privately controlled* firm, by contrast, is one where private or foreign firms control at least half the shares. The third type is a *fully state owned* enterprise, which serves as the omitted group in the regression.

Control variables  $X$ : Firm performance and firm size are likely to affect soft budgets, in ways that have nothing to do with the employment burden or ownership type. For example, if a firm is making a large profit, its overdue bank debts are unlikely to be extended, regardless of how much employment burden it bears. On the other hand, the chance for a very large firm to receive subsidies at a time of difficulty may be much higher than that of a small firm, because the government fears the negative spillover effects of the large firm's possible bankruptcy. This is usually called "too big to fail" in the literature. Thus I want to control for the performance and size of firms in the regression. Since the dependent variable is defined by either overdue bank debts, government subsidies, soft taxes or overdue social security payments divided by total assets, I should not use "total assets" as a control for size on the right-hand side; otherwise this built-in negative correlation would induce bias in my estimates. Therefore, I use the total number of workers as a size control (with a

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<sup>3</sup> See Sun et al. (2005) for a theory of optimal state share in partial SOEs.

log transformation to reduce the effect of outliers). The variable used to control for performance is the one-year lagged *sales per worker*, which is defined as total sales divided by the number of on-duty workers.

### 2.3 Data

The data comes from an International Finance Corporation survey conducted in 11 Chinese cities in 2002. The survey was administered by the State Economic and Trade Commission (SETC) and its counterparts in the sample cities. The surveyed enterprises are industrial firms that were owned by the municipal government of the 11 cities at the end of 1995. These cities are Harbin, Fushun, Tangshan, Lanzhou, Weifang, Xining, Zhenjiang, Huangshi, Chengdu, Hengyang, and Guiyang. Their locations, GDP levels and other economic conditions varied dramatically, allowing researchers to account for the significant regional variation of enterprise reforms in terms of form, pace and outcome. About 1,100 questionnaires were sent out, with 683 questionnaires collected. The survey questionnaires cover basic enterprise information, as well as detailed information about *gaizhi*. Moreover, the financial information, employment condition and share structure data is recorded annually from 1995 to 2001.

Most of the sample firms were in manufacturing sectors, especially in machinery (14%), petro-chemicals (13%) and textiles (10%). This is roughly consistent with the sector distribution at the national level. In terms of employment size, the sample is over-represented by large firms, with an average of 864 workers, which is about 4 times of the national average. With regard to the pace of *gaizhi*, 54% of the SOEs in the sample reported that they had undertaken *gaizhi* by the end of 2002, while about 86% of the China's SOEs had finished *gaizhi* by the end of 2001 (Garnaut et al., 2005). The over-representation by larger firms with slower paced *gaizhi* in the sample is probably due to the fact that the survey was conducted by the SETC, so that larger and non-private firms tend to have a higher response rate. However, this should not pose a problem to my empirical tests because the variation in the employment burden and ownership is nonetheless quite substantial. For example, the share of state controlled firms is 2.1% in 1995 and 8.4% in 2001, while the share of privately

controlled firms rises from 0.7% in 1995 to 12.1% in 2001. In sum, the survey generates a sample that is appropriate for my testing purpose.<sup>4</sup>

In the sample, most firms have 7 years of observation. However, the panel dataset is unbalanced since some firms have a shorter life span due to a merger or a split. Table 1 shows the definition and summary statistics of variables. In addition to the total number of observations, mean and standard deviation, I also list the number of observations that take the value of zero and the mean value in 1995 and 2001. It is easy to see that the mean redundancy rate (41%) in my sample is comparable to other studies listed in section 2.2.1. Despite the high redundancy rate, the layoff rate remained at a very low level until 2001. The four measures of soft budgets, redundancy rates and layoff rates all increased while employment size decreased during the sample period.

## 2.4 Econometric problems

There are two major econometric problems that need to be addressed before I start to run regressions. The first one is the endogeneity issue. On the one hand, ownership is often viewed as endogenous in the empirical literature due to either omitted-variable bias or reverse causality (Djankov and Murrell, 2002). On the other hand, both the *redundancy rate* and the *layoff rate* could be affected by the inherent quality of a firm, which is not fully captured by the performance control.<sup>5</sup> The inherent quality could affect the firm's arrears to banks and the government, and therefore make the estimates biased. For example, a firm with poor inherent quality usually has a high redundancy rate and a high layoff rate together, and owes a lot of arrears as well. In that case, the coefficients on the redundancy rate and layoff rate would both be positive for reasons that have nothing to do with the employment burden. Thus we could reject the policy burden hypothesis just because of the omitted-variable bias. To solve these endogeneity problems, I use instrumental variables (IVs) that capture the exogenous changes in ownership, the redundancy rate and the layoff rate. I estimate the model using a two-stage least square (2SLS) method.

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<sup>4</sup> For more details about this survey, see Garnaut et al. (2005).

<sup>5</sup> For more details on the inherent quality hypothesis, see Li and Rozelle (2004).

The first instrument is a dummy variable that measures the manager's importance in the firm's employment decision. It equals one if the manager self-reports that he or she has a decisive influence on employment issue relative to other related parties of the firm, and equals zero otherwise.<sup>6</sup> Obviously, the manager's influence will directly affect the redundancy and layoff rate of a firm. Also, such influence is only inside the firm, and is unlikely to be directly causally related to the soft budgets. Moreover, a manager's influence largely depends on personal characteristics, and it is not likely to be reversely affected by ownership type. Therefore, this first instrument is validly excluded from the main equation. The second instrument is the age of the firm. Under the slogan "grasping the large and letting go the small" the central government adopted different reform policies for enterprises with different sizes that were historically determined (Xu et al., 2005). Also, the number of retired workers is usually positively correlated with the age of a firm. Thus the firm size and firm age provide important identification information for *gaizhi* and the employment condition. Since firm size has been used as a control variable, here only firm age is adopted as an instrument. As managerial and political turnover became more common in the reform era, old SOEs lost their close ties with government officials, so that the direct correlation between *firm age* and soft budgets is weak. Firm age thus serves as a valid instrument. I calculate each firm's age at 2002. The mean is 33 years, and the maximum is 124 years. Similar to the employment size, a log transformation is applied to this variable to reduce effect of outliers.

Besides these two variables, I include a set of *region-sector interaction dummies* as additional instruments. Since there exist specified labor markets for different sectors in different cities (e.g., the job market for textile workers in Harbin city), the demand and supply in these labor markets will have an impact on firms' redundancy and layoff rate. More importantly, what these interaction dummies actually capture are the characteristics of the specified labor markets, not those of cities or industries. Therefore, they are not likely to be directly correlated with soft budgets and these interaction dummies are excluded from the main equation. To limit the number of

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<sup>6</sup> These different parties include government, board of directors, labor union, shareholder conference, communist party branch, supervision board and parent company.

instruments, I combine the city dummies into 2 regional dummies: south and north. The northern cities include Harbin, Fushun, Tangshan, Xining, Weifang and Lanzhou, and the remaining cities belong to the south. The 2-digit coded industries are combined into 3 major sectors: mining and quarrying, manufacturing, and other sectors, which include utility supply, transportation, trade services etc. In total, I have 6 *region-sector interaction dummies*. As will be shown in section 2.5, these interaction dummies survive the over-identification test after I control for city and sector dummies in the main equation. This gives further justification regarding their validity as instruments.

The second econometric problem lies in the fact that the dependent variables equal zero in many cases. For example, of the total 2949 observations of *overdue bank debts* shown in Table 1, 1207 cases equal zero. These zeros could be viewed as corner solution outcomes, which generate censored data. Thus I adopt a tobit model.

I assume that the latent amount of soft budgets is  $Y_{it}^* = \alpha_i + G_{it}'\beta_1 + Z_{it}'\beta_2 + X_{it}'\beta_3 + e_{it}$

and we observe  $Y_{it} = \max(0, Y_{it}^*)$ . That is, the latent amount of soft budgets  $Y_{it}^*$  is determined by the firm's ownership, employment condition and other characteristics including performance. In case  $Y_{it}^* > 0$ , subsidies (or other kinds of aid) will be given accordingly; if  $Y_{it}^* \leq 0$ , the government will simply not compensate the firm, instead of making a "negative" subsidy.

Addressing the endogeneity problem and accommodating corner solution outcomes simultaneously, I estimate a two-step tobit model with endogenous regressors (Newey, 1987). I will present the two-step tobit results along with those from 2SLS estimation. Furthermore, given the tobit model's limitation of using a single mechanism to determine the choice between  $Y = 0$  versus  $Y > 0$  and the value of  $Y$  conditional on  $Y > 0$ , I estimate a simple two-tiered model as a robustness test.<sup>7</sup> If there does exist two different mechanisms, the estimates would allow me to check whether the effect of the employment burden on soft budgets is consistent in these two mechanisms.

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<sup>7</sup> See chapter 16.7 of Wooldridge (2002) for more details.

## 2.5 Empirical results

### 2.5.1 First-stage results

Table 2 shows the first-stage regression results.<sup>8</sup> The F-statistics are generally high and the corresponding p-values are all smaller than 0.002. This demonstrates that these instruments are generally powerful in the first-stage.

The first-stage regressions display some interesting patterns regarding the employment adjustment and *gaizhi* at the firm level. For the first instrument, if the enterprise manager has a decisive influence on employment, more workers are likely to be put on redundancy, but the layoff rate is not significantly affected. This result demonstrates that the scope of managers' employment decision is quite limited: they generally do not have the ability to fire non-productive workers.<sup>9</sup> Nonetheless, managers could make non-productive workers redundant, by means of *xiagang* or internal retirement, possibly with efficiency in mind.

For the second instrument, firm age, the regression shows that older firms are more likely to have a higher redundancy rate and a higher layoff rate. Besides this, older firms are less likely to be partial SOEs as opposed to remaining fully owned by the state. However, being older does not prevent a firm from undergoing privatization.

In Table 2, I also present the coefficients on the region-sector interaction dummies by taking the mining and quarrying industries in the south as the omitted group. The first interesting finding is that firms located in northern cities generally have a higher redundancy rate, and are less likely to be privatized. These results are consistent with the observation that firms in northern China have heavier employment burdens and a slower reform pace, due to the concentration of heavy industries. Another interesting point is that the layoff rate in "other" sectors in the south is significantly low relative to manufacturing, mining and quarrying sectors in the south and all sectors in northern cities. This finding is also consistent with the economic

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<sup>8</sup> Different measures of soft budgets correspond to different sample sizes, so the number of observations of first-stage varies. Table 2 shows results based on the sample size of overdue bank debts. The first-stage results based on other sample size are very similar to table 2.

<sup>9</sup> This is consistent with Dong and Putterman (2003), who claim that managers of SOEs were unable to adjust the size of their labor forces in response to changes in demand and technology up until the late 1990s.

prosperity and increasing demand for various business services in southern China, which makes these “other” sectors perform relatively well in terms of job creation.

### 2.5.2 Main results

The main regression results are shown in Table 3. The dependent variables are the measures of soft budgets: *overdue bank debts*, *government subsidies*, *overdue social security payments* and *soft taxes* (all normalized by total assets). For each dependent variable, I first show the ordinary least square (OLS) results, then the results for 2SLS and two-step tobit models. Independent variables include employment burden measures, ownership dummies, control variables and a full set of year and city dummies. As expected, the coefficient on *one-year lagged sales per worker* is generally negative, which means that firms tend to have softer budgets if their performance is bad. One interesting finding about firm size is that the coefficient on *log (total workers)* is negative and significant in most cases. This contradicts the common view that larger firms have better access to government aid under the logic of “too big to fail”, thus inspiring us to look for deeper reasons for soft budgets.

I conduct Hausman tests to compare the coefficient on layoff rate between the OLS and 2SLS models. The test results (not shown in the table) suggest that these coefficients are significantly different in most specifications (except when the dependent variable is government subsidies) and support the use of the instruments. One example is the comparison between column 1 and column 2. By using 2SLS instead of OLS, the coefficient on the layoff rate changes from positive to negative. This demonstrates that instruments are necessary in order to solve the omitted-variable problem, possibly caused by an unobserved inherent quality. In addition, I test the over-identification restrictions for the 2SLS models. The null hypothesis is that, if at least one instrument is valid, all instruments are jointly valid. Table 3 shows that the p-values of the Sargan statistics of the over-identification tests are generally high, suggesting that we cannot reject the null. In sum, the test results provide some evidence on the validity of my instruments.

The results shown in Table 3 generally support the policy burden hypothesis. A higher redundancy rate or a lower layoff rate is associated with a softer bank budget

constraint and a softer government budget constraint as well (except for soft taxes, which will be discussed in the next sub-section). According to the 2SLS results, a one percentage point decrease in layoff rate from the mean (1.1%) corresponds to an 8.9 percentage points increase in overdue bank debts (mean is 32%) and an 1.01 percentage points higher overdue social security payments to the government (mean is 2.8%). A ten percentage points increase in redundancy rate from the mean (41%) corresponds to 16.9 percentage points higher overdue bank debts. The coefficients on the layoff rate and redundancy rate in the two-step tobit regressions are generally bigger than 2SLS estimates in absolute value. However, what they represent is the marginal effect on the “latent” outcome. Their corresponding marginal effects on “observed” outcome are usually smaller, and are roughly comparable to the 2SLS results.<sup>10</sup> Overall, the estimated economic effects are substantial.

In contrast, the results in Table 3 do not support the ownership hypothesis. The coefficient on the *privately controlled* dummy is generally negative, but not statistically significant in most specifications. This means that privately controlled firms are not substantially different from fully state owned firms in receiving various kinds of soft budgets. On the other hand, the coefficient on the *state controlled* dummy is significantly positive in several specifications, suggesting that partial SOEs have higher overdue bank debts and receive more subsidies from the government than fully state owned firms. The reason may lie in the nature of their partial state ownership. During *gaizhi*, the government usually introduces some private shares into, but nonetheless keeps controlling shares in, enterprises that have special importance (Sun et al., 2006). As a result, the controlling state ownership may actually represent strong bargaining power or close ties with the government, thus allowing these firms to get more help than fully state owned ones in general. In particular, let us look at the example of overdue social security payments. In 1998, China introduced a social security system that requires firms to pay around 20% of their payroll into social security funds which are then pooled together at the city level and ultimately at the

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<sup>10</sup>Theoretically, these marginal effects could be manually calculated by multiplying the two-step tobit estimates by an adjustment factor. However, the two-step estimator does not identify the adjustment factor computationally, so I am not able to calculate the exact amount of the marginal effects. See chapter 16.2 of Wooldridge (2002) for details.

national level. The high payments and the requirement of fund-pooling might have induced some firms to default on their payments, especially those with strong bargaining power. Therefore, it is not surprising to find that these state controlled firms owe more social security payments relative to other types of firms.

### **2.5.3 Soft taxes and incentives of the local and central governments**

Regressions on *soft taxes* (Table 3, columns 10-12) appear to be abnormal. Of all the employment burden and ownership variables, only the layoff rate has a significant effect; and it is in an unexpected way: a higher layoff rate is associated with more soft taxes. Except this, soft taxes are unresponsive to both the redundancy rate and ownership type. This finding is consistent with the findings in Table 6 and 9 of Luo (2005). So why is there such a big difference between soft taxes and other forms of soft budgets?

Different sources of soft budgets correspond to government at different levels, and these different levels of government may have divergent incentives. To understand the difference between soft taxes and other forms of soft budgets, and more generally, to analyze the link between the employment burden and soft budgets, one needs to examine the incentives of different levels of government and their impact on soft budgets. This idea is consistent with Anderson et al. (2000), who suggest that “analysis of enterprise-state relations in transition countries offers considerable potential for identifying the determinants of soft budgets”. The main source of industrial enterprise loans comes from the four major State Owned Commercial Banks (SOBs), while various kinds of government soft budgets are mainly related to city governments in my sample. Next I will discuss the fiscal incentives and role of city-level governments, and then move to the central government and the SOBs.

As the representative of “local government” in the paper,<sup>11</sup> city governments play a mixed role in relation to enterprises. First, city governments are responsible for social stability. Since social stability is a crucial component in China’s cadre

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<sup>11</sup> Provincial government lies between the city government and the central government. Since it does not directly own SOEs in my sample, I will not discuss them here.

evaluation system (sometimes even with a “veto” power), there is an incentive for city governments to help enterprises that bear a heavy employment burden. Second, city governments are the direct owner of most enterprises in my sample. The performance of these SOEs will also have an important effect on the promotion of city officials (Li and Zhou, 2005). Thus city officials are concerned with the profitability of these city-owned SOEs, and may take relatively long-term views about firms’ financial difficulty, instead of simply bailing them out just because they are owned by city governments. Third, as the tax collector and public service provider, city governments need tax revenues from those enterprises. This role gives city governments an incentive to enforce tax rules. Jin et al.’s (2005) finding of stronger fiscal incentives of local governments in the reform era supports this argument.<sup>12</sup> Thus, I expect that the budget on tax payment to be much harder than that on social security payments. Compared to other forms of government soft budgets, soft taxes should be less responsive to changes in employment burden or ownership. My regression results confirm that neither the redundancy rate nor ownership plays an important role in determining city governments’ tax enforcement. In sum, the mixed role of city governments do not support the ownership hypothesis, while fiscal incentives may harden enterprises’ budget in regard to tax payment.

Although aware of the SBC problem, policy makers at the central level usually choose to first deal with the most urgent concerns. Bai et al. (2005) argue that the central government places more weight on social stability than the local government. Therefore, in short term, the central government may put pressure on both city governments and the SOBs to let them support enterprises that bear an employment burden. For the SOBs’ city branches, although they report directly to higher-level managers, there is evidence that they were still subject to the influence of city governments throughout the 1990s. For example, Wei and Wang (1997) find evidence that the loans of China’s SOBs favor SOEs in the early 1990s, while Cull and Xu (2003) find that the association between bank finance and profitability

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<sup>12</sup> The tax reform of 1994 introduced a clear distinction between national and local taxes. The major tax levied on city-owned SOEs, the value-added tax, is shared by central and local government. Another important tax, business tax, which is mainly for the service sectors, is collected by local government. Thus the fiscal incentive argument for city government is still valid after the 1994 tax reform.

weakened in the 1990s as banks increasingly assumed bailout responsibility. Thus, under the pressure of central government and the influence of city governments, SOBs would have had a bad discipline with respect to loans and interest payments, sometimes by simply writing off arrears. The reason to do such bailout is to let firms take employment burdens, especially when the unemployment concern rises with *gaizhi*. The empirical results shown above are consistent with this argument.

In sum, the concern over social stability gives both the city and central government an incentive to take actions that could soften the budget constraints of firms. However, soft taxes are less responsive due to the fiscal incentives of city governments. The difference between soft taxes and other means of soft budgets reflects the mixed role of city governments.

#### **2.5.4 Robustness tests**

I conducted several robustness tests to make sure that my main results still hold in alternative model specifications (Table 4). First, it may be argued that current soft budgets may actually depend on the employment burden that had already been taken by firms in the past. So I try the specification with the one-year lagged layoff rate and redundancy rate as independent variables. Also, I control for the one-year lagged log of total workers. The results for overdue bank debts are shown in columns 1 and 2. A higher redundancy rate or a lower layoff rate is associated with more overdue bank debts, but ownership dummies are not significant. The economic effect of the one-year lagged layoff rate is even stronger than the current-year value. This suggests that the employment burden may have a continuous effect on soft budgets for several years.

Second, the mechanism determining whether or not the enterprise is subject to soft budgets may be different from the one that determines the amount of soft budgets given that the budget constraint is soft. One limitation of the tobit model is that it does not distinguish between these two mechanisms. Here I conduct a two-tiered model (also known as hurdle model or two-part model) as an additional check. My goal is not to test the two-tiered model against the tobit model, but to check whether the sign of the effect of employment burden is consistent in these two mechanisms. I

take a simple approach suggested by Wooldridge (2002). I first estimate a probit model to explain whether or not a firm has any overdue bank debts; then I run a linear regression on observations that have positive overdue bank debts, so as to estimate the conditional effect of the employment burden. As usual, both the probit and linear models are estimated in a two-step way using instruments. Columns 3 and 4 show the results of these two estimations. While a higher redundancy rate or a lower layoff rate increases a firm's chance to get any extension on bank debts, a lower layoff rate helps firms that already have positive overdue bank debts to increase the overdue amount. Thus, both mechanisms suggest a causal relationship between employment burden and overdue bank debts. The ownership dummies are not significant in either regression.

Third, Garnaut et al. (2005) point out that whether or not the firm has new overdue bank debts may be a better indicator of bank soft budgets than the stock of past overdue debts. Following this argument, I change the dependent variable into a binary one, representing whether or not the enterprises have any new overdue bank loans in that year, and estimate a two-step probit method. The results in column 5 show that the probability of getting new overdue bank debts is positively associated with the redundancy rate. Although the coefficient on the layoff rate is not significant, its sign is still negative. This result is consistent with the policy burden hypothesis. On the other hand, ownership dummies are still not significant.

Finally, I add industry dummies in the regressions to control for any possible bias of soft budgets towards specific industries. The nine industry dummies include primary, food, textile, furniture, chemicals, metal, machinery, electronics, and service. The regression results for overdue bank debts, government subsidies and overdue social security payments in the two-step tobit model are shown in columns 6-8. These dummies are usually jointly significant in the regressions. We can see that the major results still hold.

## **2.6 Conclusion**

As an important SBC theory, the policy burden hypothesis challenges the traditional ownership hypothesis by singling out policy burdens as the ultimate causal

factors of soft budgets. Taking the employment burden as an example of policy burdens, this paper conducts an empirical test on these two competing hypotheses using firm-level data from China. The results broadly support the policy burden hypothesis, but not the ownership hypothesis. It should be noted that both the policy burden theory and my empirical test are based on the Chinese experience. With substantial differences in institutional environment, any attempt to translate the policy burden theory and results to other transition countries should be done with caution.

This paper addresses some of the important econometric issues in the transition literature, such as the endogeneity problem and the corner solution outcome. I interpret the empirical results within the Chinese institutional background by analyzing the incentives of the different levels of government in China. However, future research is needed in order to disentangle the complex relationship among the government's concerns over unemployment, its privatization strategy and soft budgets. In this sense, more detailed data that covers a longer period would be very helpful.

The SBC problem is closely related to the employment and social security issues in China. To solve the SBC problem and improve enterprise performance, efforts should be made to create jobs and improve the social safety net. The central government has already realized that “a sound social security system is vital for China's development in the next 20 years”. (China Daily, 2004)

Table 1. Summary statistics

Variables	Definition	N	N (value= 0)	Mean	Standard deviation	Mean (1995)	Mean (2001)
State controlled	=1 if government share between 50% and 100%; =0 otherwise	4,408	4,188	0.050	0.20	0.021	0.084
Privately controlled	=1 if private share more than 50%; =0 otherwise	4,408	4,224	0.042	0.18	0.007	0.121
Redundancy rate	$\frac{xiaogang + \text{internal retired} + \text{retired workers}}{\text{total workers}}$	3,903	216	0.41	0.27	0.031	0.50
Layoff rate	$\frac{\text{laidoff workers}}{(\text{total worker} + \text{laidoff workers})}$	3,881	3,110	0.011	0.06	0.006	0.017
Overdue bank debts	$\frac{\text{overdue loans and interest}}{\text{total assets}}$	2,949	1,207	0.32	0.44	0.28	0.40
Government subsidies	$\frac{\text{government subsidies}}{\text{total assets}}$	800	363	0.011	0.02	0.009	0.011
Soft taxes	$\frac{\text{overdue taxes} + \text{tax exemptions}}{\text{total assets}}$	3,162	1,250	0.025	0.07	0.021	0.028
Overdue social security payments	$\frac{\text{overdue social security payments}}{\text{total assets}}$	1,361	343	0.028	0.07	0.014	0.044
Sales per worker (10,000 RMB/person)	$\frac{\text{sales}}{\text{on-duty workers}}$	3,240	N.A.	6.88	28.2	6.56	9.08
Total workers	number of total workers	4,063	N.A.	864	1388	876	813
On-duty workers	number of on-duty workers	4,014	N.A.	573	982	681	464
Total assets (10,000 RMB)	total assets of the firm	3,621	N.A.	5197	6355	4501	5134

Table 2. First-stage regression results

	Dependent variable:			
	Layoff rate	Redundancy rate	State Controlled	Privately Controlled
	(1)	(2)	(3)	(4)
Manager has a decisive influence on employment	-0.0023 (0.0020)	0.018* (0.010)	-0.018* (0.010)	0.035*** (0.0096)
Log(firm age)	0.0050*** (0.0017)	0.11*** (0.0083)	-0.064*** (0.008)	0.0049 (0.0078)
<i>Region-sector interaction dummies</i>				
North-mining and quarrying	-0.0098 (0.013)	0.090 (0.064)	-0.083 (0.063)	-0.23*** (0.060)
North-manufacturing	-0.0034 (0.009)	0.18*** (0.046)	-0.061 (0.046)	-0.12*** (0.043)
North-other sector	-0.0040 (0.010)	0.13** (0.052)	-0.010 (0.051)	-0.095* (0.049)
South-manufacturing	-0.0030 (0.0057)	0.010 (0.029)	0.024 (0.028)	0.030 (0.030)
South-other sector	-0.013*** (0.0042)	0.030 (0.021)	0.0031 (0.021)	-0.017 (0.020)
Log(total workers)	-0.0021** (0.0011)	-0.012** (0.0053)	0.046*** (0.0053)	-0.012** (0.005)
One-year lagged sales per worker	0.00001 (0.0001)	-0.0043*** (0.0005)	0.0021*** (0.0005)	-0.0009* (0.00048)
R-squared	0.04	0.31	0.09	0.11
Test of excluded instruments: F-statistics (p-value in parenthesis)	3.54 (0.002)	32.17 (0.000)	9.26 (0.000)	9.85 (0.000)
Number of observation	1,976	1,976	1,976	1,976

Notes: 1. All regressions include a full set of year and city dummies. 2. Standard errors are in parenthesis; Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*. 3. The null hypothesis of the test of excluded instruments is that IVs are jointly non-significant.

Table 3. Regression analysis - main results

	Dependent variable:											
	Overdue bank debts			Government subsidies			Overdue social security payments			Soft taxes		
	(1) OLS	(2) 2SLS	(3) 2-step Tobit	(4) OLS	(5) 2SLS	(6) 2-step Tobit	(7) OLS	(8) 2SLS	(9) 2-step Tobit	(10) OLS	(11) 2SLS	(12) 2-step Tobit
<i>Employment burden</i>												
Layoff rate	0.38* (0.20)	-8.91* (4.78)	-15.69** (6.93)	-0.046** (0.018)	-0.026 (0.69)	0.53 (1.21)	-0.11*** (0.038)	-1.01** (0.41)	-1.59*** (0.52)	-0.044 (0.035)	1.92** (0.89)	3.05** (1.41)
Redundancy rate	0.46*** (0.038)	1.69*** (0.54)	2.36*** (0.80)	0.0027 (0.0044)	0.21 (0.25)	0.51** (0.23)	0.059*** (0.009)	0.20 (0.23)	0.17 (0.29)	0.029*** (0.0067)	-0.051 (0.085)	-0.013 (0.13)
<i>Ownership</i>												
State controlled	-0.17*** (0.040)	1.64** (0.98)	0.90 (1.46)	0.0084 (0.0046)	-0.60 (0.71)	1.67** (0.82)	0.016* (0.0088)	0.14 (0.11)	0.12 (0.14)	-0.0092 (0.0072)	-0.27 (0.19)	-0.34 (0.30)
Privately controlled	-0.093** (0.043)	-0.42 (0.75)	-1.40 (1.11)	-0.0063 (0.005)	0.11 (0.20)	-0.81 (0.61)	0.0068 (0.011)	-0.13 (0.25)	-0.13 (0.32)	0.017** (0.0077)	-0.001 (0.12)	-0.16 (0.19)
<i>Control</i>												
Log(total workers)	-0.027*** (0.0089)	-0.11*** (0.039)	-0.11** (0.057)	-0.007*** (0.0009)	0.021 (0.033)	-0.087** (0.044)	-0.011*** (0.0021)	-0.021** (0.009)	-0.023** (0.011)	0.0016 (0.0015)	0.010 (0.007)	0.031*** (0.011)
One-year lagged sales per worker	-0.0018* (0.0009)	-0.001 (0.002)	-0.002 (0.003)	-0.0002* (0.0001)	-0.001 (0.001)	0.0007 (0.0008)	-0.0005** (0.0002)	-0.0006 (0.0005)	-0.003*** (0.0008)	-0.00003 (0.0001)	0.0001 (0.0002)	-0.0005 (0.0005)
Over-ID test: p-value		0.34			0.77			0.86			0.05	
Number of Observation	2,210	1,976	1,976	602	562	562	1,066	970	970	2,372	2,138	2,138

Note: 1. All regressions include a full set of year and city dummies. 2. Standard errors are in parenthesis; Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*. 3. Over-ID (Over-identification) test is conducted for the 2SLS regressions, the p-value of the Sargan statistics is listed.

Table 4. Regression analysis - robustness tests

	Dependent variable:		Overdue bank debts		Overdue bank debts		Overdue bank debts		Government subsidies		Overdue social security payments				
	1= (overdue bank debts >0)	0= (overdue bank debts =0)	1= (new overdue bank debts>0)	0= (new overdue bank debts=0)	Overdue bank debts	Government subsidies	Overdue social security payments	(1) 2SLS	(2) 2-step Tobit	(3) 2-step Probit	(4) 2SLS	(5) 2-step Probit	(6) 2-step Tobit	(7) 2-step Tobit	(8) 2-step Tobit
<i>Employment burden</i>															
Layoff rate															
			-35.41**	-4.62**	-11.78	-17.0*	-0.42	-2.04*							
			(15.19)	(2.36)	(11.08)	(9.5)	(0.68)	(1.13)							
Redundancy rate			3.76**	0.44	1.98*	2.63***	0.17***	0.35							
			(1.70)	(0.44)	(1.11)	(0.86)	(0.065)	(0.26)							
One-year lagged layoff rate	-14.2**	-24.1***													
	(5.97)	(9.22)													
One-year lagged redundancy rate	1.34***	1.79***													
	(0.47)	(0.74)													
<i>Ownership</i>															
State controlled	1.04	-0.001	-2.04	0.60	2.43	0.91	0.58***	0.18							
	(0.89)	(1.41)	(3.06)	(0.62)	(2.22)	(1.74)	(0.190)	(0.20)							
Privately controlled	-0.70	-1.86	-2.99	-1.63	-2.82	-1.55	-0.22	-0.30							
	(0.69)	(1.08)	(2.35)	(0.73)	(2.41)	(1.22)	(0.18)	(0.23)							
<i>Control</i>															
Log(total workers)			-0.043	-0.11***	-0.24***	-0.15*	-0.041***	-0.037**							
			(0.12)	(0.026)	(0.079)	(0.07)	(0.014)	(0.018)							
One-year lagged log(total workers)	-0.046	-0.010													
	(0.041)	(0.064)													
One-year lagged sales per worker	-0.0027	-0.0042	-0.002	-0.001	-0.006**	0.0008	-0.0001	-0.003***							
	(0.0018)	(0.0028)	(0.007)	(0.002)	(0.0028)	(0.0037)	(0.0004)	(0.001)							
Over-ID test: p-value	0.45			0.31											
Number of observation	1,981	1,981	1,976	1,196	2,411	1,976	562	970							

Note: 1. All regressions include a full set of year and city dummies. 2. Regressions in column 6, 7 and 8 include nine industry dummies. 3. Standard errors are in parenthesis; Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*. 4. Over-identification test is conducted for the 2SLS regressions, p-value of the Sargan statistics is listed.

## Chapter 3: The Incentive Role of Creating "Cities" in China

### **Abstract**

This Chapter examines a distinctive mechanism of providing incentives to local governments – the creation of "cities". In China, awarding city status to existing counties was the dominant way of creating new urban administrative units, during which the local government gets many benefits. Using a large panel data set covering all counties in China during 1993-2004, I investigate the determinants of upgrading. I find that the official minimum requirements for upgrading are not enforced in practice. Instead, a county's economic growth rate plays a key role in obtaining city status. An empirical test is then conducted to distinguish between a principal-agent incentive mechanism and political bargaining. The findings are consistent with the hypothesis that the central government uses upgrading to reward local officials for high growth, as well as aligning local interests with those of the center. This paper highlights the importance of both fiscal and political incentives facing the local government. The comparison between incentive and bargaining mechanisms sheds light on an important question about China's politics of governance: where does power lie in China?

### **3.1 Introduction**

In transition and developing countries, a major concern of the central government is to give local governments incentives to spur market development and economic growth. Democracy achieves this goal by putting the local government under the direct pressure of citizens who vote. In contrast, under a hierarchical political system, the central government relies on its political authority to create incentive mechanisms. For example, the "Cadre Evaluation and Appointment System" is used by the Chinese central government to induce desirable economic outcomes (Huang, 2002; Mei, 2007). At the same time, fiscal decentralization provides market incentives to local governments by making them the residual claimant over local revenues (Montinola et al., 1995; Jin et al., 2005).

This paper examines a very distinctive mechanism of providing incentives. In China, awarding "city" status to existing counties is the dominant way of creating new urban administrative units. The creation of a large number of cities through so-called "county-to-city upgrading" has changed China's basic administrative structure, making the Chinese city system unique. City status gives localities both political and fiscal benefits, thus providing the center with an effective tool to reward counties. I present evidence that the central government uses the creation of cities as part of the incentive system for local officials, which is very important for China's successful reform (Zhuravskaya, 2000; Li and Zhou, 2005).

By viewing county-to-city upgrading as an incentive mechanism, I cast the relationship between center and localities as that between principal and agents. However, it has been argued that the central-local relationship in China is much more like political bargaining (see Shirk, 1993). As local governments get more and more discretion through decentralization, they gradually accumulate bargaining power that is used to negotiate political benefits from the center. Bargaining differs from the incentive mechanism in that it is based on the assumption of much stronger local political power. Despite the great attention paid to the central-local relationship by both political scientists and economists, few studies have attempted to determine whether political bargaining or principal-agent incentive mechanisms occur. My

paper fills this gap by differentiating between these two mechanisms and providing evidence on the dominance of incentive mechanisms in China's central-local relationships.

Using a large panel data set covering all counties in China during 1993-2004, I show that the formal minimum requirements for county-to-city upgrading are not enforced in practice. Instead, economic performance has played a critical role in upgrading. I provide strong and robust evidence on the relationship between a county's growth rate and its probability of getting city status after controlling for the formal requirements (industrialization, population and fiscal strength). Moreover, I conduct an empirical test to distinguish between the incentive mechanism and the bargaining mechanism. Using local extra-budgetary funds as an example of divergence between central and local interests, I find that city status helps the center to align local interests with national ones. This finding is consistent with the hypothesis that upgrading is used as an incentive mechanism, instead of being the result of bargaining.

This paper relates to the existing literature in three ways. First, it highlights the coexistence of fiscal decentralization and political centralization in China. This unique institutional setting is attracting growing interests among economists (Blanchard and Shleifer, 2001; Zhang, 2006; Zhuravskaya, 2007). While the role of fiscal decentralization in stimulating economic growth has been discussed extensively (see, e.g. Qian and Weingast, 1997; Qian and Roland, 1998; Zhang and Zou, 1998), some recent studies argue that the centralized power structure is also crucial in creating yardstick competition among local officials and constraining them from practicing local protectionism (Cai and Treisman, 2004; Li and Zhou, 2005; Bai et al., 2007). Blanchard and Shleifer (2001) explicitly point out the complementarities between the "carrot" of fiscal revenue sharing and the "stick" of political centralization. In this paper, upgrading is the result of centralized decision-making; at the same time, it gives local officials more discretion over revenue collection. Thus, it reflects both political centralization and fiscal decentralization.

Second, since upgrading provides many benefits to local officials, it is a substitute for promotion. This paper thus relates to the literature on the career concerns and political mobility of local officials.<sup>13</sup> Although it is generally accepted that ideological loyalty has become much less important in determining the promotion of local officials during China's reform era, there still exists doubt concerning to what extent economic performance matters. For example, Landry (2003, p. 31) concludes that "economic performance of cities has little substantive impact on promotion or removal from office". Moreover, even among studies that admit the important role of economic performance, there is no consensus on which factor carries most weight in evaluating economic performance. For example, Li and Zhou (2005) conclude that GDP growth rate is the most critical performance measure for provincial top leaders, but Bo (2002) finds that fiscal performance is more important. In this paper, I show that the growth rate of gross output has a strong effect on the probability of getting an upgrade, thus confirming the importance of economic performance in the evaluation of local officials.

Third, the emergence of a large number of cities has had a big impact on China's administrative structure and attracted lots of attention both inside and outside China (see, e.g., Au and Henderson, 2006). Existing studies on this subject are mainly concentrated in the field of urban economics and regional science. The political economic reasons for the emergence and expansion of cities in China have largely been ignored.<sup>14</sup> This is the first paper to discuss the incentive role of creating cities.

Two specific advantages of examining county-to-city upgrading bear highlighting. First, using county-level data gives many more observations than other studies that use provincial-level data, thus increasing the power of tests. Second, as a measure of rewards, upgrading suffers much less from omitted variable bias relative to the promotion of local officials, which is the focus of many other studies. The reason is that upgrading is very costly and irreversible, so that the decision is made in a very deliberate way (Wu, 2000). In fact, applications for upgrading are submitted to

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<sup>13</sup> See Bo (2002, Chapter 1) for a literature review on the political mobility of Chinese local officials; see Besley and Case (1996) for theoretical and empirical evidence on how career concerns generate yardstick competition in a democratic situation.

<sup>14</sup> For a general discussion of the political economy of city formation, see Henderson and Becker (2000).

the Ministry of Civil Affairs through the provincial government, and the final decision is made directly by the State Council. Such a strict process is well beyond the influence of county leaders. In contrast, the promotion decision of local officials is made by the government at just one level higher so that the unobserved factors, such as personal relationships, could be relatively important.

The remainder of the paper is organized as follows. Section 3.2 describes the background of county-to-city upgrading. Section 3.3 lays out the model and hypotheses. Section 3.4 describes the data. Section 3.5 presents the results. The last section concludes.

### **3.2 Institutional background: county-to-city upgrading in China**

There are four local administrative levels in China: province, prefecture (*diqu*), county and township. At each level, jurisdictions are classified either in a normal form, or in a form with the name "city" or "town".<sup>15</sup> For example, a province and a City Directly under the Central Government (*zhixiashi*) are both at the provincial level; a prefecture and a prefecture-level city are both at the prefecture level; a county and a county-level city are both at the county level, a *xiang* and a town are both at the township level.<sup>16</sup> In this paper, county-to-city upgrading refers to the reclassification of a county into a county-level city.<sup>17</sup> From 1983 to 2001, 430 county-level cities were established, most of which were created through upgrading. Only 25 of them were created by separating out a relatively urbanized area from a county and setting up a new government.

This unique way of creating cities through upgrading stands in contrast to the experience of most countries, where "city" and "county" fall into different administrative categories. Cities only govern urban centers where population is concentrated. Instead of reclassifying a whole county, a new city is normally created inside a county. Also, the decision is often made through a voting. For example, in

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<sup>15</sup> For a detailed description of the Chinese city system, see Chung (1999).

<sup>16</sup> Here are examples of cities at different levels. Shanghai city is at the provincial level, Suzhou city (which attracts the largest amount of foreign investment among all cities) of Jiangsu province is at the prefecture level, Shunde city (where massive enterprise privatization took place) of Guangdong province is at the county level.

<sup>17</sup> Similarly, there is another type of upgrading, prefecture-to-city upgrading, during which the entire prefecture is labeled "city". From 1983 to 2001, more than 160 prefectures were upgraded to prefecture-level cities.

the U.S., a new city could be created by adopting a home rule charter. While in Brazil, new municipalities are established through local voting.

The administrative units created through upgrading are different from cities as we normally understand the term (Chan, 1997). During upgrading, the entire county, not just the town, is labeled "city". Thus, cities include not only the urban centers, but also large expanses of rural areas. For example, Eerguna is a county-level city in Inner Mongolia. It covers an area of 11,000 square miles, 2/3 of which is forest and another 1/6 is grassland. Its population density is about 7 persons per square mile, only 1/50 of the national average. Such a "rural" administrative unit was nonetheless awarded "city" status in 1994. *The Economist* (2007b) describes Sansha, a county-level city consisting mostly of water and desolate islands in the South China Sea, as "no more than a bizarre misnomer". The confusion caused by upgrading even leads to a lawsuit (see the story about "Huangshan" city in Chung and Lam, 2004). Overall, the magnitude of upgrading is very substantial. Since 1983, nearly 15 percent of counties have obtained city status.

The official rule to regulate county-to-city upgrading first appeared in 1983, when cities had shown advantages in attracting investment and the demand for city status increased in the coastal provinces. Under some rough requirements proposed by the Ministry of Personnel and Ministry of Civil Affairs, nearly 100 cities got city status during 1983-1986. In 1986, the minimum requirements for city status were raised, but were still considered to be very low (Chung and Lam, 2004). The number of cities continued to rise and the central government further raised the requirements in 1993, setting different standards for counties with different population density. As more and more counties became enthusiastic about city status, the central government realized that urbanization cannot be achieved by just changing the title to "city". Instead, large-scale upgrading has exaggerated the actual urbanization level. It eventually stopped approving county-to-city upgrading in mid 1997, only allowing prefectures to be eligible to get city status. Until now, county-to-city upgrading has not been resumed.

Table 1 summarizes the main minimum requirements on industrialization level, urbanization level and fiscal strength announced in 1993. One interesting thing to note is that counties with lower population density were given lower standards. This seems to imply that, in the mind of the central government, setting up cities does not follow the usual idea of urban agglomeration.

By winning city status, local governments gain more administrative authority and better political treatment, despite that formal rules tell that the government of county-level city should share the equal rank in the administrative hierarchy as its predecessor. This is the reason to use the term "upgrading". Some county-level cities have been awarded deputy-prefecture status, such as Tianmen city in Hubei province and Jiyuan city in Henan province. In some other provinces, although deputy-prefecture status is not explicitly given, the budget of county-level cities has been placed under the direct supervision of the provincial government, thus bypassing the prefecture level.<sup>18</sup> In addition, local leaders gain some political privileges. For example, the party secretaries of many county-level cities are able to enter the standing committee of the prefecture-level party committee and enjoy deputy-prefecture political rank.

Besides administrative and political upgrade, there are many other benefits associated with city status (see Table 2 for an incomplete list). Among them, the most prominent one is that cities obtain a huge amount of revenues from the conversion of land into non-farm usage (Ping, 2006). My data shows that the average land revenue collected by a county-level city is about four times as big as that of a county. Moreover, these benefits are not only for the local officials (such as bigger government size and higher official rank), but for the whole county as well. For example, additional revenues and favorable policies usually result in more urban infrastructures. In sum, city status gives localities both political and fiscal benefits.

The political privilege mentioned above also brings the local government closer to higher-level governments, thus facilitating the supervision from above. As will be shown later, such supervision will make local governments less likely to defy the

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<sup>18</sup> This is called "line item under province", or *Shengji Jihua Danlie*.

center when they have potential conflict with the center. However, these small costs are overshadowed by the political and fiscal benefits and we observe great enthusiasm for city status among counties in the mid 1990s.

At the same time, upgrading is very costly to the central government. For example, the expanded size of the local government will increase salary expenses, which are paid out of the central budget. In many upgrading cases, the central government also makes subsidies to new cities for their infrastructure construction. Besides fiscal burdens, the central government also worries about inequality across regions. In my sample, counties that got an upgrade are generally rich ones. Since they will further receive favorable policies and fiscal subsidies once getting city status, the disparity between poor and rich jurisdictions is likely to increase. Finally, since no city has been downgraded to a county, upgrading could be viewed as an irreversible administrative change. This irreversibility greatly increases the cost of upgrading if it is not desirable to the central government *ex post*.

### **3.3 Hypotheses and empirical strategy**

In this section, I formalize my arguments about the incentive role of upgrading into three hypotheses. The first hypothesis relates to the non-enforcement of formal rules, the second states the importance of economic growth in determining upgrading. The third one is actually composed of two alternative hypotheses that correspond to the incentive mechanism and bargaining mechanism, respectively. I also present empirical methods used to test each hypothesis.

#### **3.3.1 Non-enforcement of the formal rules**

As shown in section 3.2, the central government sets certain rules to regulate upgrading. A natural hypothesis is that the center wants to enforce these rules. However, many scholars (e.g., Liu and Wang, 2000; Chung and Lam, 2004) have observed that the official minimum requirements for upgrading are not enforced in practice: while eligible counties compete to get upgraded, some ineligible counties nevertheless received city status because of special treatment. Chung and Lam (2004, p. 953) even report that the Ministry of Civil Affairs "was not in a good position to

verify the statistics supplied by the counties". Thus, the first hypothesis of this paper is:

*Hypothesis 1:* The formal minimum requirements for upgrading are not enforced.

The data allows me to directly check whether all counties that got an upgrade meet the requirements listed in Table 1. To formally test this hypothesis, I will use an empirical model (to be introduced in the next sub-section) to examine the importance of these requirements. More details will be discussed in section 3.5.

### **3.3.2 Main hypothesis and the empirical model**

If those formal rules were not enforced, upgrading should not be just an instrument for urbanization. The central government must have other considerations when making decisions. That is, there exist some informal rules that were not explicitly written in policy documents, but nonetheless played an important role in the upgrading decision. It turns out that growth rate is such a critical factor. To give a brief preview of the importance of growth rate, Figure 1 displays the number of upgrading cases from 1981 to 1997, as well as the corresponding yearly GDP growth rate. We can clearly observe several waves of upgrading around 1983, 1988 and 1993, which roughly correspond to the periods when China experienced high economic growth. Figure 1 suggests that high growth may generate high demand for upgrading, thus inspiring me to examine the correlation between growth rate and probability of upgrading at the county level.

To test the positive effect of growth rate on the probability of upgrading, I need to control for other confounding factors that affect upgrading. Obviously, urbanization level, industrialization level and fiscal strength are important socio-economic factors since they were listed in the official documents. Thus, the main hypothesis of this paper is:

*Hypothesis 2:* Conditional on urbanization level, industrialization level and fiscal strength, the higher the growth rate, the more likely an upgrading would happen.

To test this hypothesis, I view upgrading as a discrete choice problem faced by the central government and model it in the following way.<sup>19</sup> For county  $i$  at year  $t$ , the central government could choose either to upgrade it or not. A latent value  $Y_{it}^*$  represents the difference of utility the center expects to achieve under upgrading and non-upgrading.  $Y_{it}$  is a binary variable representing upgrading status. The approval decision is made according to:

$$Y_{it} = 1 \text{ (upgrading) if } Y_{it}^* > 0; Y_{it} = 0 \text{ (non-upgrading) otherwise}$$

Here  $Y_{it}^* = f(G_{it}, Z_{it}, \varepsilon_{it})$ , in which  $G_{it}$  represents growth rate,  $Z_{it}$  is a vector that includes other county-specific variables such as urban population, industrial output and fiscal revenue. The error term  $\varepsilon_{it}$  contains unobserved factors in the decision-making process.

Assume that  $f(G_{it}, Z_{it}, \varepsilon_{it})$  has a linear form, so that  $Y_{it}^* = \beta_0 + G_{it}\beta_1 + Z_{it}\beta_2 + \varepsilon_{it}$ , and  $\Pr(Y_{it} = 1) = \Pr(\varepsilon_{it} > -\beta_0 - G_{it}\beta_1 - Z_{it}\beta_2)$ . Further assume  $\varepsilon_{it}$  follows a logistic distribution, I get the following logit model:

$$\begin{aligned} (1) \quad \Pr(Y_{it} = 1) &= \Phi(\beta_0 + G_{it}\beta_1 + Z_{it}\beta_2) \\ &= \exp(\beta_0 + G_{it}\beta_1 + Z_{it}\beta_2) / (1 + \exp(\beta_0 + G_{it}\beta_1 + Z_{it}\beta_2)) \end{aligned}$$

### 3.3.3 Why growth matters? Two alternative hypotheses

Hypothesis 2 predicts that growth matters in getting city status. However, it does not tell us the specific mechanism through which growth plays a role. In fact, there are two possible interpretations of the positive effect of growth rate on upgrading. The obvious one is that the central government uses upgrading to reward counties with a high growth rate, which suggests that upgrading serves as an incentive mechanism. This interpretation casts the central-local relationship in a principal-agent framework. Since the central government (principal) cannot perfectly monitor the localities' (agents) effort in economic development, it uses incentives to elicit good

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<sup>19</sup> If we assume that 1) all counties started to have a chance of upgrading since 1983, the beginning year of the policy, and 2) upgrading to city status is an ultimate outcome for most counties if time horizon is long enough, then upgrading could be alternatively treated as a duration problem. Getting city status means the ending of spells, and the probability of upgrading is the corresponding hazard. However, since I can only follow the subjects for 5 periods, it is very difficult to extract any meaningful information on the shape of the hazard of ending the spell. Thus, duration model is not adopted in this paper.

economic outcomes represented by a high growth rate. In contrast, some scholars have argued that the central-local relationship in China is much more like bargaining (e.g., Shirk, 1993; Lampton, 1992). Accordingly, upgrading could be viewed as a result of central-local bargaining. High levels of economic performance, approximated by a high growth rate, strengthen the bargaining power of the local government and provide it with greater leverage in getting an upgrade. This interpretation is also consistent with the result that a higher growth rate leads to a higher probability of upgrading. Thus I have the following competing hypotheses:

*Hypothesis 3A (Incentive):* The central government uses upgrading as a reward in an incentive mechanism.

*Hypothesis 3B (Bargaining):* Upgrading is a result of political bargaining, in which a high growth rate strengthens the bargaining power of a county, and thus helps it to get city status.

These two hypotheses display different pictures of China's politics of governance. The incentive hypothesis assumes that the center has a tight control over local governments. The bargaining hypothesis, however, puts local officials in a more powerful position in playing political games with the center. Although the Chinese *Nonmenklatura* system gives authority over local official appointments to the center, it does not mean that the local government is always weak. As the notorious saying tells, "Whenever there is a policy top-down, there is a strategy bottom-up (*Shang you zhengce, xia you duice*)."<sup>1</sup> Sometimes non-cooperation is used as a strategy to confront the center (see examples in Shirk, 1993). To some extent, the bargaining hypothesis suggests that the center is "forced" by local governments to give out the city recognition. When describing the administrative changes in the reform era, Chan (1997, p. 86) mentions "pressure from below for faster change and more thorough reform builds up". Chung and Lam (2004, p. 953), based on interviews with government officials, claim that the Ministry of Civil Affairs "often found it difficult to resist political pressure from below" in approving upgrading applications. The non-enforcement of the official upgrading requirements further raises doubts on the central government's capacity to enforce pre-announced policies and gives some

support to the bargaining argument. Besides, anecdotal evidence also suggests the existence of case-by-case negotiation in upgrading (Su, 2000).

In fact, the bargaining hypothesis has a long tradition in the study of Chinese politics. Lampton (1992, p. 34) writes that "bargaining is one of the several forms of authority relationship in China. ... bargaining remains a key feature of the system [after 1989]." Naughton (1992) states that both the center and localities are equipped with bargaining power when specific policies of economic reform are being decided. Shirk (1993) studies the central-local relation from the perspective of accountability. Since the top leaders were elected by the Central Committee members, among whom provincial leaders form the largest bloc, she argues that central leaders have to compete for the support of provincial leaders, so that the bargaining position of the provinces is strengthened. These studies treat bargaining and mutual compromise as the working mechanism between the central and local governments, and form the background for my bargaining hypothesis.

Mei (2006) uses a simple graph to highlight the difference between the principal-agent and the bargaining models based on their emphasis on local officials' political power (Figure 2). By putting these two models together with models of full centralization and full decentralization, this graph clearly shows that the bargaining model ascribes more political power to the localities relative to the principal-agent model, while both models are not as extreme as full centralization or decentralization.

Empirical literature on the bargaining mechanism is also fairly developed. For example, several studies have shown that the provincial representation in the Central Committee affects resource allocation across provinces (Sheng, 2005; Su and Yang, 2000; Huang, 2001). Maskin et al. (2000) further shows that the provincial representation depends on the power of a province composed by its population, economic size and fiscal contribution. Thus, this strand of literature is consistent with the bargaining hypothesis that economic performance strengthens local bargaining power. This paper goes one step further by directly comparing the bargaining mechanism with the principal-agent incentive mechanism.

Since counties cannot negotiate directly with the central government due to their low position in the administrative hierarchy, bargaining takes place between governments at adjacent hierarchical levels. For example, as the county government gains political privileges and administrative authority through upgrading, the prefecture government will necessarily lose some power. Therefore, to get the prefecture government's support on its upgrading attempt, the county will have to negotiate with the prefecture. More importantly, since the application for upgrading is through the provincial government, there is central-local bargaining between the center and provinces. In fact, provincial governments, as representatives of their subordinate counties, have played an important role in competing for city status.<sup>20</sup> The bargaining power of provincial governments will be partly determined by the economic performance of counties in candidacy.

### **3.3.4 Incentive versus bargaining: empirical strategy**

In order to distinguish the two competing hypotheses, incentive versus bargaining, I examine the behavior of city and county governments in situations where the center and localities have divergent interests. The logic is as follows.

According to the bargaining hypothesis, counties get upgraded because of their strong bargaining power. Since city status brings political privileges to local officials, local bargaining power should be strengthened through upgrading. Thus, when there is any divergence between central and local interests, city status makes localities more capable of pursuing local interests against the center. Moreover, the best time to do so is right after upgrading when local bargaining power would be at its strongest. Thus, we should see a larger divergence between central and local interests just after cities get upgraded.

In contrast, the incentive hypothesis suggests that the center maintains control over localities, and it alone decides which counties get upgraded. City status facilitates administrative supervision, thus helping the center to better align local interests. This hypothesis predicts that cities are less likely to act against the central

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<sup>20</sup> Ren and Wang (1999) give an example showing that the provincial bureau of civil affairs is crucial in helping counties to get city status.

government. Moreover, the detailed evaluation process of upgrading is equivalent to performing *ex ante* monitoring over local governments, meaning that newly upgraded cities are least likely to make decisions that diverge from the center's wishes.

In sum, these two hypotheses have different predictions on how city status is going to affect the way local governments promote local interests at the cost of the central government. By comparing counties with cities, and furthermore, newly upgraded cities and old ones, I can test one hypothesis against the other.<sup>21</sup> This requires me to have a proper measure of divergence between central and local interests.

In China, the existence of local "extra-budgetary funds" (EBFs) as opposed to "within-budgetary fiscal revenue" provides a good example of the conflict between central and local interests. EBFs consist of all resources managed directly or indirectly by administrative branches of the government outside the normal budgetary process. EBFs are concentrated at the sub-national level and not subject to treasury management or budgetary oversight (Wong and Bird, 2005). In times of fiscal stress, local governments respond by introducing a large number of fees, charges, and revenues from land leasing, which are not shared with, and often not reported to, higher-level governments. Park et al. (1996) show that local governments have become very practiced in hiding income in extra-budgetary accounts, making it extremely difficult for the central government to monitor true deficits. Ping (2006) finds that increases in the administrative cost of local governments are due to EBFs, which softens the budget constraint of local governments.

With these properties, EBFs could be regarded as a measure of local budgetary independence. It is in local governments' interest to maintain or even increase the size of EBFs. In contrast, the central government would like to minimize EBFs relative to within-budgetary fiscal revenue in order to extend its control over local officials. In fact, the central government has tried hard to curb local extra-budgetary fiscal activities. Huang (1996, p. 47) has shown that "since 1986 the State Council has

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<sup>21</sup> This method is consistent with Huang (1996, Chapters 6-8), who distinguishes between a bargaining hypothesis and a control hypothesis when studying the impact of local officials' bureaucratic status on the way they handle the central-local investment conflicts.

attempted to manage the extra-budgetary revenues and expenditures more centrally." In 1997, the central government ruled that as many as thirteen types of "local government funds" should be brought from extra-budget to within-budget, which shows the center wanted to impose supervision over EBFs. However, this rule failed to fundamentally change the ability of local government to control EBFs (Ping, 2006). The fight over EBFs is going on between the center and localities and it clearly reflects the conflict of central and local interests.

Thus, the ratio of EBFs to within-budgetary fiscal revenue serves as a good proxy for the divergence of local interests from the center's. If the bargaining mechanism dominates, I expect this ratio to be higher for cities than for counties. Among cities, newly upgraded ones should have a ratio higher than old ones. If the incentive mechanism dominates, the opposite should happen. The empirical model is:

$$(2) Ratio_{it} = \beta_0 + City_i * \beta_1 + Newcity_i * \beta_2 + Z_{it} * \beta_3 + v_t + \varepsilon_{it}$$

Where  $Ratio_{it}$  is the ratio of EBFs to within-budgetary fiscal revenue for county  $i$  at year  $t$ .  $City_i$  is a dummy for city status.  $Newcity_i$  is a dummy for newly upgraded cities, whose definition will be discussed in section 3.5.  $Z_{it}$  is a vector of control variables that include GDP, population and growth rate;  $v_t$  is the year dummy;  $\varepsilon_{it}$  is the error term. While  $\beta_1$  is the effect of having city status,  $\beta_2$  is the additional effect of being newly upgraded conditional on having city status. The sign of the estimate of  $\beta_1$  tells us which mechanism dominates. Whether  $\beta_2$  has the same sign with  $\beta_1$  will provide a further check. In short, the prediction of the two competing hypotheses is:

Incentive hypothesis:  $\beta_1 < 0, \beta_2 < 0$

Bargaining hypothesis:  $\beta_1 > 0, \beta_2 > 0$

### 3.4 Data

Systematic data on Chinese jurisdictions below the provincial level is difficult to collect. While several papers have employed prefecture-level data to study the incentives of local officials (e.g., Li and Bachman, 1989; Landry, 2003), no one has ever used county-level data to do so. The difficulty is two-fold. On the one hand, statistics of counties are distributed in the statistical yearbooks of each province,

making it hard to build a relatively complete data set. On the other hand, the traditional measure of rewards to local officials, promotion, is almost impossible to find for county-level officials.

To overcome these difficulties, I use data from the yearly series of *Sub-Provincial Public Finance Statistics*, which was publication by the Ministry of Finance starting in 1999. It covers all county-level jurisdictions during 1993-2004 and includes detailed public finance information, as well as data on basic socio-economic conditions. At the same time, I use upgrading to capture the rewards to county leaders, substituting for the usual measure, promotion. The specific benefits associated with city status make upgrading a reliable measure of rewards. In contrast, the existing literature using promotion as a measure of rewards often faces the problem of identifying when mobility is a true promotion and when it is not.<sup>22</sup> The reason is that, under the Chinese bureaucracy, the actual power and political benefits enjoyed by an official may be hard to discern using only his or her job title (Mei, 2006).

As summarized in Rawski and Xiao (2001), there exist concerns about the accuracy of China's economic statistics in the literature. Specifically, the administrative use of statistical indicators as a way to evaluate local officials' performance has been argued to supply the chief motivation for false reporting (Zhang, 1999). Here, I address the issue of data quality from three perspectives. First, the public finance data set used in this paper is compiled by the Ministry of Finance. It is very difficult to manipulate statistics on local public financial activities, such as fiscal revenue, because every single statistic reflects actual money flow. In fact, Tsui (2005) and Zhang (2006) have used this data set and do not find any significant problem. Second, if the local officials in a county did try to inflate statistics for the reason of getting city status, they should first inflate urban population, industrial output and fiscal revenue in order to meet the official requirements which are explicitly announced. Thus, checking whether these requirements are satisfied could tell us useful information on local officials' behavior. If the majority of counties that

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<sup>22</sup> For example, when a provincial governor is assigned to be the director of the provincial People's Congress, this seemingly lateral move usually means loss of power and usually marks the end of his or her political career. While Li and Zhou (2005) attribute this kind of mobility into the same category as demotion, Bo (2002) simply drops such kind of observations from his study.

got an upgrade did not meet these requirements, then it is hard to believe that false reporting is widespread. The test of hypothesis 1 in the next section will do this job. Third, even if inflating statistics is a common action, every county should have the same incentive to do so. Because my analysis is based on comparing the relative economic performance across counties in different regions, it suffers little in such a case. For example, if every county inflates its growth rate by a same percentage points, the conclusion that high growth rates lead to higher chance of upgrading still holds.

To estimate equation 1, I use data from 1993, the beginning year of the published data, to 1997, when upgrading stopped. For county-level cities, I drop observations in their post-upgrading period because there is no chance of downgrading or further upgrading available (see Table 3 for more details). Data of Xizang (Tibet) is dropped because it obviously faces a different policy on jurisdiction administration.

The key independent variable is the growth rate of Gross Value of Industrial and Agricultural Output (GVIAO). Since GDP data is not available before 1998, GVIAO is the best consistent measure of a county's total economic activity. Similar to Li and Zhou (2005), I use its growth rate to measure economic performance. As stated above, I control for urbanization level, industrialization level and fiscal strength since they are official requirements on upgrading. Measures for these requirements include industrial output value, share of industrial output value in GVIAO, urban population, share of urban population in total population and per capita (within-budgetary) fiscal revenue.

Table 4 shows the comparison of mean values for upgrading and non-upgrading cases. A total of 99 upgrading cases are identified through official records from the Ministry of Civil Affairs.<sup>23</sup> All the output and revenue measures have been adjusted to 1993 constant prices using the yearly GDP deflator (growth rate is calculated using adjusted values). While growth rate of GVIAO starts from 1994, growth rate of fiscal revenue starts only from 1995. The reason is that the 1994 tax reform fundamentally changed local tax base and makes the growth rate of fiscal revenue from 1993 to 1994

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<sup>23</sup> Available online at <http://www.xzqh.org>.

meaningless. Except for these two growth rates, all other variables are averaged from 1993 to 1997. I also present the p-values of t-test of mean differences between upgrading and non-upgrading observations. It is clear to see that there are significant differences between these two groups.

In estimating equation 2, the ratio of local EBFs to within-budgetary fiscal revenue captures the divergence between local interests and those of the center. There is no direct report of EBFs at county level in my data set. However, various types of "local government funds" were reported since 1999. As I have shown in section 3.3, these funds were nominally brought from extra-budget into budget by the central government in 1997, but were still under the control of local governments (Ping, 2006; Wong and Bird, 2005). Thus, these "local government funds" could reflect the conflict between central and local interests. I use the sum of them as a proxy for EBFs, so that the actual dependent variable is the ratio of the sum of local government funds to within-budgetary fiscal revenue.

Table 5 compares the mean values of key variables between counties and county-level cities. The sample is from 1999 to 2004 because local government funds are not available before 1999. Since upgrading has already been stopped in 1997, there is no change in city status in this period, and consequently, I cannot perform a difference-in-difference exercise. This also explains why  $City_i$  and  $Newcity_i$  do not vary across time in equation 2. In this period, GDP data is available, so I use it to substitute GVIAO as the measure of local development level. From the table, it is obvious to see that cities, with a greater power on revenue collection, raise much more "local government funds" than counties in average. However, the raw mean of the ratio of local government funds to within-budgetary fiscal revenue is not very different between cities and counties. In the following section, I will compare cities with a certain group of counties that have similar properties to cities, as well as controlling for confounding variables in the regressions.

### **3.5 Empirical results**

This section presents the empirical results. First, I estimate the logit model (equation 1) to show that growth rate has a positive effect on a county's probability of

upgrading. The results are robust to alternative definitions of growth rate, as well as various specifications and estimation techniques. Then I use stylized facts and results from a formal test to show that upgrading requirements are not enforced. After that, I estimate equation 2 to test the incentive hypothesis against the bargaining hypothesis.

### **3.5.1 The positive effect of growth rate on upgrading**

#### **Baseline results**

To estimate the effect of growth rate conditional on urbanization level, industrialization level and fiscal strength, I closely follow those official requirements on upgrading and control for industrial output value, share of industrial output in gross output, urban population, share of urban population in total population, and per capita fiscal revenue in equation 1. All these control variables are lagged by one year. Since counties vary greatly in size, I apply a log transformation to these variables (except the two shares) to reduce the effect of outliers.

Table 6 shows the results estimated using Maximum Likelihood Estimation (MLE). Column 1 reports the baseline results, listing coefficients and robust standard errors clustered at the prefecture level. The key independent variable, growth rate of GVIAO, has a positive and significant coefficient. This finding is consistent with hypothesis 2: conditional on urbanization level, industrialization level and fiscal strength, a higher growth rate increases the probability of achieving city status.

Among the five control variables that measure the official requirements, only urban population and fiscal revenue per capita have positive and significant coefficients. While industrial output and its share in total output have positive and insignificant coefficients, share of urban population has a negative and insignificant coefficient. To further control for the variation of upgrading policy across years, which is common for all counties, I add year dummies (column 2). The only change to the estimates is that the coefficient on share of urban population becomes positive, but it remains insignificant. In column 3, I add province dummies. This is equivalent to ask such a question: what makes a county stand out from its peers in the same province in competing for city status? The results are very similar to column 1,

suggesting that growth is important in the within-province competition. Examining the coefficients of provincial dummies (not shown in the table), I find that counties in some poor provinces in western China, such as Guizhou and Gansu, have a higher probability of getting city status than the average county. This suggests that the upgrading policy may be slightly biased in favor of western provinces. Then I control for both year and province fixed effects (column 4). There is little change to the estimates of control variables, except that industrial output becomes significant. In all these models, growth rate remains positive and significant.

Since several provinces do not govern any county that was upgraded during the sample period, adding provincial dummies means dropping counties in these provinces from the estimation. To compare the size of coefficient between the baseline model and the one with year and province fixed-effects, I estimate the baseline model again on the sample used in column 4. The results are shown in column 5. Overall, adding province and year dummies generate a larger coefficient on growth rate, which suggests that they capture the effect of omitted variables. For example, if the upgrading policy is in favor of provinces with low growth potential, such a bias in policy would have a negative correlation with growth and a positive correlation with upgrading. Therefore, including province dummies eliminates the effect of this omitted variable and increases the coefficient on growth rate. In the following alternative specifications, I always report result both with and without fixed effects.

To assess the economic magnitude of the effect of growth rate, I calculate the marginal effect. In column 4, the mean value of  $dPr(upgrading)/dG$  equals 0.01. This implies that when the annual growth rate increases, for example, from 16% (the average growth rate) to 26%, the probability of upgrading will increase by 0.001, which is about 7% of the average upgrading probability (0.015).<sup>24</sup> For the control variables that are significant in column 4, I also calculate their mean marginal effect. The change in probability of upgrading if industrial output moves from the 25<sup>th</sup>

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<sup>24</sup> This magnitude is comparable to the one estimated in Li and Zhou (2005). They show that if the GDP growth rate of a province increases by same amount (10 percentage points from mean), the probability of provincial leaders getting a promotion increases by an amount that is 24% of the average probability.

percentile to the 75<sup>th</sup> percentile is 0.022, which is about 1.4 times the average upgrading probability. For urban population and fiscal revenue per capita, this value is 0.026 and 0.007, respectively. These values are in the reasonable ranges.

### **Robustness checks**

To check the sensitivity of the baseline results, I adopt alternative definitions of growth rate, as well as using various specifications and techniques to estimate the logit model.

Since upgrading decisions are made throughout a year, it is likely that the center refers to last year's performance when making decisions, especially early in the year. To check whether the results are sensitive to different timing, I lag growth rate by one year and lag all other independent variables (except the year dummies) by two years. The results (Table 7, columns 1 and 2) are generally similar to the baseline results shown in Table 6, with an even larger coefficient on growth rate. Thus, the results are robust to different timing of variables.

By including all counties in the estimation, I implicitly assume that every county has a chance of upgrading and thus faces an incentive. However, some counties actually have very little chance of getting city status, so that the upgrading policy does not provide any incentive for them to increase growth rates. One group of these counties is the nationally designated poor counties, which receive special subsidies from the center every year. The official document explicitly states that these counties generally should not be considered for an upgrading. Thus, I drop these counties and re-estimate the baseline model (columns 3 and 4). The results are little different.

Furthermore, I adopt alternative measures for growth rate. Chen et al. (2005) show that relative performance matters more than absolute performance in evaluating provincial officials. Following their method, I subtract the average growth rate of counties in the same prefecture from each county's growth rate and generate an alternative growth measure – relative growth rate. Without controlling for fixed effects, the relative growth rate is positive but not significant (column 5); once province and year dummies are included, relative growth rate has a positive and significant coefficient, and the size is even larger (column 6). This suggests that

relative growth rate is also used as a reference in the evaluation of counties' economic performance.

Following Li and Zhou (2005), I use average growth rate as another alternative growth measure, where the averaging is from the beginning of the sample to the current year. This measure corresponds to an evaluation process that is based on both the current and past growth rates. The results (columns 7 and 8) show that, although not statistically significant, the coefficient on average growth rate has a magnitude similar to the baseline results. These exercises show that the baseline results are not driven by a particular definition of growth rate.

On the other hand, some studies have argued that the central government is most interested in seeing continuously growing fiscal revenues (e.g., Bo, 2000). So I put both the growth rate of fiscal revenue and the growth rate of gross output in the regression to see which growth rate is more important (Table 8, columns 1 and 2). To estimate this model, I only use data from 1995 to 1997 because the growth rate of fiscal revenue starts from 1995. The results show that the coefficient on growth rate of fiscal revenue is not significant either statistically or economically; whereas the growth rate of gross output remains to be significant. A plausible explanation is: local governments sometimes lower the actual tax rate to attract investment, so that the high economic growth rates are achieved at the cost of fiscal revenues. Thus, the growth rate of fiscal revenue is not used as an important performance indicator in practice.

Since my sample covers several years, upgrading policy may vary during this period. Additionally, growth rates in different years may not be comparable to each other. For example, a growth rate of 14% is below average in 1994, but is above average in 1997. Controlling for year fixed effects could partly take care of this problem by allowing different intercepts for different years, but the marginal effect of growth rate may also change across time. Ideally, I should estimate the model year-by-year to check the robustness of the effect of the growth rate in different years. Due to the limited sample size and rareness of upgrading cases in some years, I split the sample into two periods: 1994-1995 and 1996-1997. The estimation on these two sub-

samples (columns 3-6) is consistent with the baseline model. In addition, I estimate the logit model using a cross-section where each county counts as one observation. In this specification, the dependent variable equals one if the county gets upgraded during the sampling years, and the independent variables are the average values across these years. Although the small sample size generates larger standard errors, the results provide evidence on the positive effect of growth rate (columns 7 and 8). These tests demonstrate that, even though the upgrading policy may vary across time, growth rate always plays a crucial role.

One caveat to my empirical model is sample attrition. Once a county gets city status, it loses the chance of receiving any further reward through upgrading, and its probability of having  $Y_{it} = 1$  is zero since then. Thus, in the discrete choice framework, post-upgrading observations are ruled out of the estimation (explained in Table 3). In this unbalanced panel, the attrition could be non-random if error terms have serial correlation across time. For example, if a county suffers a positive shock to the idiosyncratic error term in a specific year and gets upgraded, it is dropped out of the sample in subsequent years, during which the error term could continue to get positive shocks if there is a positive serial correlation. In this case, sample selection is correlated with the error term and could induce inconsistency of estimates. To correct for this problem, I adopt a probit model with sample selection (Van de Ven and Van Pragg, 1981). It is similar to the Heckman selection model (Heckman, 1979), except that the dependent variable in the outcome equation is also binary. Following Wooldridge (2002, Chapter 17.7), I treat the probit of the previous year as the selection equation, where non-upgrading means being selected into current year's outcome estimation. Specifically, a probit model of upgrading on growth rate and control variables using data of 1994-1996 serves as the selection equation, and the outcome equation is another probit using data of 1995-1997. By assuming a bivariate normal distribution of the error terms of these two equations, this model is estimated using MLE.<sup>25</sup>

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<sup>25</sup> The probit model is very similar to equation 1, except that the error term  $\varepsilon$  is assumed to follow a normal distribution instead of a logistic distribution. In the selection equation, the dependent variable equals one in case of non-upgrading; in the outcome equation, the dependent variable equals one if upgrading happens. The reason I estimate a probit model with sample selection rather than a logit model with sample selection is computational

The results are shown in Table 9. Column 1 shows the results of a usual probit estimation. The coefficient on growth rate is almost significant (p-value is 0.12). The corresponding mean marginal effect (0.0044) is about the same as in a logit model (0.0042), which shows that the probit model is little different from the logit model. Column 2 shows the results of probit with sample selection. The estimates are very similar to column 1. Moreover, the correlation coefficient between error terms of the selection equation and outcome equation is not statistically significant, suggesting that sample selection is not a severe problem and it does not bias my estimates.

In sum, various robustness tests provide strong and consistent evidence for hypothesis 2, which states that growth rate plays a crucial role in upgrading.

### **3.5.2 Testing the non-enforcement of upgrading requirements**

#### **Stylized facts**

To test the non-enforcement of formal minimum requirements for upgrading, I first use the data to check whether counties being upgraded during my sample period meet these requirements. Table 1 has listed the three main requirements, which are concerned with urbanization level, industrialization level and fiscal strength, respectively. Each requirement has an absolute criterion and a relative (or per capita) one. For counties with different population densities, these upgrading requirements are also different. Based on the 1993 data, I summarize the number and percentage of counties that meet each criterion, as well as those that meet both criteria of a specific requirement (Table 10).<sup>26</sup> This table shows that the requirement on urbanization level is quite stringent, while the requirement on fiscal strength is less binding.

Based on the three requirements, I construct three dummy variables: urbanization dummy, industrialization dummy and fiscal dummy. Each dummy equals one if both criteria of the corresponding requirement are met. For example, for a county with a

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convenience. The actual estimation is performed by Stata's command "heckprob".

<sup>26</sup> In the official requirements shown in Table 1, "urban population" refers to those who are engaged in non-agricultural production. There is no direct report of this variable in my data set. Instead, I have data on population with urban registration status. For each population density group, I also know the average ratio of population engaged in non-agricultural production to those with urban registration status. Therefore, I am able to get an estimate of population engaged in non-agricultural production for each county. I use this estimated urban population to tell whether a county meets the requirement on urbanization level.

population density above 400 per square km, if its fiscal revenue exceeds 60 million RMB *yuan* and the per capita revenue exceeds 100 RMB *yuan* in a particular year, then its fiscal dummy equals one in this year; otherwise this dummy equals zero. I assign all county-year observations in 1994-1997 into different cells according to the value of these three dummies and whether there is an upgrading. I count the number of observations in each cell and summarize them in Table 11. I further combine those cells with an equal number of requirements been satisfied and count the number of observations in each cell (Table 12). Among the 99 cases of upgrading, only 6 meet all three requirements; 39 meet two, 30 meet only one, and 24 meet none. On the other hand, there are 36 cases where all three requirements are met but the counties did not get upgraded. This table clearly demonstrates that the official requirements are not enforced in practice.

Given the large variance of development level across regions, it is interesting to know whether the degree of enforcement varies in different regions. To do this, I divide all counties into three groups according to their geographical location: coastal (431 counties), middle (511 counties) and western (736 counties). The upgrading cases in these regions are 49, 30, and 20, respectively. The total number of counties in the coastal region is only 3/5 of that in the western region, but there are more than twice upgrading cases in the former group. This suggests that the upgrading quota is not distributed in proportion to the total number of counties in each region. Instead, economic growth should play a key role, so that the coastal region has more upgrading cases because of its economic prosperity. I then check whether each upgrading case meets the three requirements and list the results for the three regions respectively (Table 13). It is common to see non-enforcement in all three regions, which supports the proposition that upgrading requirements are not enforced in all regions.

However, this table also shows that the non-enforcement of minimum upgrading requirements is more severe in the western region. Together with the positive effects found on the provincial dummies of some poor provinces in western China, this suggests that the chance of getting city status for counties in the under-developed provinces may exceeds what their economic performance deserves. This is probably

due to the central government's consideration of balancing regional disparities in urbanization level.

### **Tests based on the logit model**

To formally test hypothesis 1, I extend equation 1 to allow for flexible functional form in the logit model. Basically, I add the three dummies (urbanization dummy, industrialization dummy and fiscal dummy) and some interaction terms in the model to check whether meeting one or several requirements increase the average probability of upgrading and the marginal effect of independent variables. Once these results are shown, it will be straightforward to draw the conclusion that the official requirements are not enforced.

In the first step, I allow the marginal effect of three control variables (industrial output, urban population and fiscal revenue) to change once the corresponding requirement is met. To do this, the three dummies and their respective interactions with the three control variables are included in the model. While the coefficients on these dummies tell us whether the average probability of upgrading has a jump once a requirement is satisfied, the coefficients on the interaction terms would indicate whether meeting a requirement changes the marginal effect of the corresponding control variable. The results (Table 14, column 1) show that only fiscal strength requirement is important. The coefficient on the fiscal dummy is very substantial, meaning that once the fiscal requirement is satisfied, the average probability of upgrading will jump up by about four times of the average value. The coefficient on the interaction term is negative, with a size almost the same as the coefficient on fiscal revenue per capita. This means that the marginal effect of fiscal revenue per capita entirely fades out once the fiscal requirement is satisfied.

In the second step, I allow observations that meet some requirement and those that fail to meet to have different intercepts and different marginal effects on growth rate. To do this, I create a dummy "Meet  $\geq 2$  requirements", which equals one if at least two requirements listed in Table 1 are satisfied by the county. I put this dummy and the interaction term with growth rate in the model (column 2). Another dummy "Meet  $\geq 1$  requirement" is defined in a similar way, and the results are shown in

column 3. In both columns, growth rate and the dummy are both positive and significant, while the interaction term is not statistically significant. This implies that satisfying some requirements increases average probability of upgrading, but does not change the marginal effect of growth rate.

In the third step, both growth rate and control variables are allowed for flexible functional form (columns 4 and 5). The results are consistent with those displayed in the first and second steps. The only exception is that growth rate, "Meet  $\geq 2$  requirements" dummy and "Meet  $\geq 1$  requirement" dummy become less significant. However, the size of their coefficients remains similar.

These results are summarized in Figure 3. The upper-left diagram shows that meeting some requirements put the county in an advantageous situation to compete for city status (i.e., with a higher average probability); but it does not change the marginal effect of growth rate does not change. The lower-right diagram shows that before meeting the fiscal requirement, an increase in fiscal revenue per capita would increase the probability of upgrading. Once meeting this requirement, the average probability of upgrading will jump up to a higher level, but additional increase in fiscal revenue would not matter any more. The other two diagrams show that industrialization requirement and urban population requirement are not important. Overall, the fiscal requirement seems to be enforced more strictly than other two requirements, which seems to be consistent with the fact that there are more counties meet the fiscal requirement than those that meet the other two requirements.

Since these requirements are supposed to be the "minimum" level for a county to be eligible for upgrading, it is reasonable to conclude that the official requirements on upgrading are not enforced in practice. Thus, upgrading is not just an instrument for urbanization. Whether it is a tool of rewarding or a result of center-local bargaining is the task of the next sub-section.

### **3.5.3 The incentive hypothesis versus the bargaining hypothesis**

To empirically distinguish the incentive hypothesis from the bargaining hypothesis, I examine the effect of city status on the divergence of local interests

from those of the center, as measured by the ratio of the sum of local government funds to the within-budgetary fiscal revenue.

As mentioned before, the central government stopped approving county-to-city upgrading in 1997. In fact, this change of policy was not publicly announced until 2000 (*Xin Changzheng*, 2000), and counties still had an incentive to compete for city status in years immediately following 1997. This actually allows me to construct propensity scores for hypothetical upgrading probability in 1998 and 1999, which could help to control for differences between cities and counties. Specifically, I first estimate the baseline logit model using 1994-1997 data. Then I apply the estimated parameters to the 1998-1999 data to construct propensity scores. Had the upgrading policy continued, propensity scores should represent the probability of getting city status predicted by their observed characteristics, and counties with high scores should have a fairly large chance to win city status. Thus, except for not having the city title, these counties are otherwise similar to cities, especially newly upgraded ones. I choose 85 counties with scores higher than 0.1 in any of these two years to construct a restricted county sample.<sup>27</sup> Based on observed characteristics, these counties are similar to cities, and the comparison between cities and this restricted county sample is more meaningful. Henceforth, I call these 85 counties as the "comparison group counties."

Figure 4 draws the average ratio of the sum of local government funds to the within-budgetary fiscal revenue for these "comparison group counties," newly upgraded cities (upgraded during 1994-1997) and all cities, respectively. Obviously, these "comparison group counties" have a much higher ratio than cities, and the difference increases overtime. This graph shows that city status makes a difference on local governments' behavior with respect to violating the central interests.

To estimate equation 2, I run OLS regressions using pooled data from 1999-2004. The control variables for county size and development level include log population, log GDP and growth rate of GDP. Since local government funds have a strong time trend, I also include year dummies. I first show the results without including the

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<sup>27</sup> The reason to set the cutoff value at 0.1 is to make the number of these counties roughly equal to the number of newly upgraded cities. The results are robust to other cutoffs, such as 0.05 and 0.15.

*Newcity* dummy. Column 1 of Table 15 reports the result on the full sample, which contains all counties and all county-level cities. In column 2, I restrict the sample to those "comparison group counties" plus all cities, and call this "restricted sample". I then add the *Newcity* dummy and run regressions on the full sample (column 3) and the restricted sample (column 4). I further restrict the sample to only cities and run a regression without the *City* dummy (column 5). This specification directly compares newly upgraded cities with old cities. I adopt three different alternative definitions for the *Newcity* dummy. In columns 3-5, newly upgraded cities are those that received city status between 1994 and 1997 (no upgrading happens after 1997). According to this definition, 99 cities are classified as newly upgraded; the remaining 231 cities are classified as "old" cities, which were upgraded between 1975 and 1993. The cutoff year for newly upgraded cities is moved to 1992 in columns 6-8, and 1990 in columns 9-11.

In columns 1 and 2, the *City* dummy has a negative and significant coefficient, which is consistent with the incentive hypothesis. In other columns, the coefficients on *City* and *Newcity* are all negative, and the F-tests show that they are jointly significant in most cases. Since newly upgraded cities are a subset of cities, it is no wonder that the inclusion of *Newcity* dummy reduces the significance of *City* dummy. The important thing about this specification is that, as stated in section 3.3, having a negative coefficient on both dummies provides additional evidence on the incentive hypothesis. The coefficient on *City* dummy is of bigger size in the restricted sample than in the full sample, and the significance level is also higher. Since the counties in the restricted sample (i.e., "comparison group counties") are a more meaningful comparison group, these results give strong support to the incentive hypothesis. In sum, the empirical results show that city status helps the center to align local interests with the center. Conditional on being cities, a recent upgrading aligns local interests with the center even closer.<sup>28</sup>

As shown before, cities generally have greater access to revenue collection than counties. For example, the huge amount of revenues associated with land conversion

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<sup>28</sup> See Huang (2002) for other tools that align local interests with the center.

provides city governments with additional flexibility in generating EBFs. My results, however, show that city governments nonetheless gave up such power and maintain a low level of EBFs relative to within-budgetary revenues. This kind of behavior could only be explained by the incentive hypothesis, which tells that the center maintains strong control over localities, and city status further facilitates the center's supervision.

My results provide evidence that the Chinese central government still maintain a tight control over local officials. This raises a puzzle: if the center has enough authority, why does it fail to enforce those pre-announced upgrading requirements? The answer may lie in the experimental and flexible nature of China's policies. During the whole reform era, many policies were first tried in one or several provinces and, after proving their effectiveness, were implemented to nationwide (Qian, 2000). Also, the actual implementation is often very flexible. As long as the underlying issue is not urgent (such as family planning and public safety), policies are not implemented with strong central pressure (Lin et al., 2005). In the example of county-to-city upgrading, the central government announces formal rules to regulate the intense competition at the beginning. Upon receiving a specific application, it mainly considers economic performance. Since upgrading serves as a rewarding tool, the center also faces the choice between upgrading the county and promoting its officials. The decision is likely to be affected by the number of vacant positions that could be used in promotion. Thus, the actual decision may well be inconsistent with those pre-announced requirements.

### **3.6 Conclusion**

Although based on a very specific example, the creation of "cities" in China, this paper studies a question that is of very general economic interest: how to provide incentives to local governments? I show that economic growth plays an important role in determining whether a county was upgraded to a county-level city. I construct an empirical test to distinguish whether the effect of growth is a reflection of an incentive mechanism or political bargaining. I find that city status helps to align local interests with those of the center, which is consistent with the hypothesis that the

central government uses upgrading to reward local officials for high growth. The central themes of this paper reflect Fogel's (2007, p. 10) observation: "the successful unfolding of autonomy requires a center strong enough to integrate national and local goals ... and to provide rewards to those who advance it [the progress of reform]." However, one should be noted that upgrading only provides one-time incentive to localities and the rewards are for their past economic performance. The fact that the average growth rate of new cities quickly returns to normal after upgrading reflects the lack of long-term effect. In fact, some scholars consider county-to-city upgrading as a policy failure (Liu and Wang, 2000; Chung and Lam, 2004).

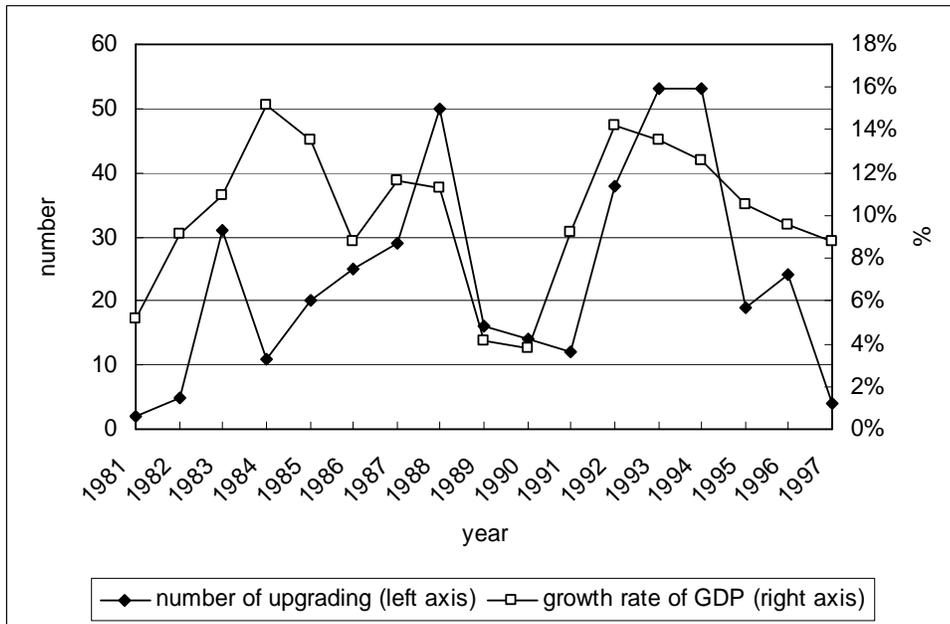
As Au and Henderson (2006) have pointed out, a large fraction of cities in China are smaller than the optimal size. The non-enforcement of upgrading requirements has certainly contributed to this problem. Cities are not a natural outcome of urban agglomeration in China. Instead, as I have shown in this paper, the title of "city" has been used as an incentive instrument to serve political purposes. In this sense, my paper sheds light on the urban economics literature about city formation and expansion in China.

Additionally, the large-scale upgrading has fundamentally changed China's administrative structure. Many county-level cities are now governed by prefecture-level cities. The Constitution of the People's Republic of China does not provide for the administration of one city (county-level) by another (prefecture-level) (Chan, 1997). There are intense debates on reforming the current administrative system, some of which suggests reducing the number of levels of government. Zhejiang province, for example, has started an experiment to let counties and county-level cities bypass prefecture-level cities and directly report to the provincial government. A recent article on the China Youth Daily (2007) describes how eager the localities are to obtain more administrative independence. My paper will facilitate an understanding of the current administrative system from a political economic perspective.

I conclude this paper by providing one more insight. Comparing China with Russia, Zhuravskaya (2007) points out that "the necessary condition for

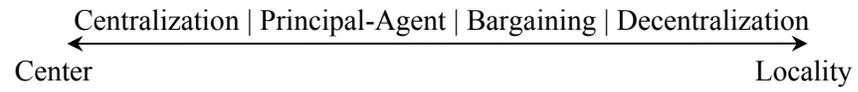
administrative centralization without local democracy to discipline local governments ... seems to be efficient growth objectives of central officials." This paper shows that the Chinese central government does have a strong growth objective, which has been embedded in the policy of setting up cities. However, the emphasis on growth also makes local governments obsessed with growth rates and produces some unintended consequences, such as severe pollution (*The Economist*, 2007a).

Figure 1. Number of county-to-city upgrading cases and yearly GDP growth rate



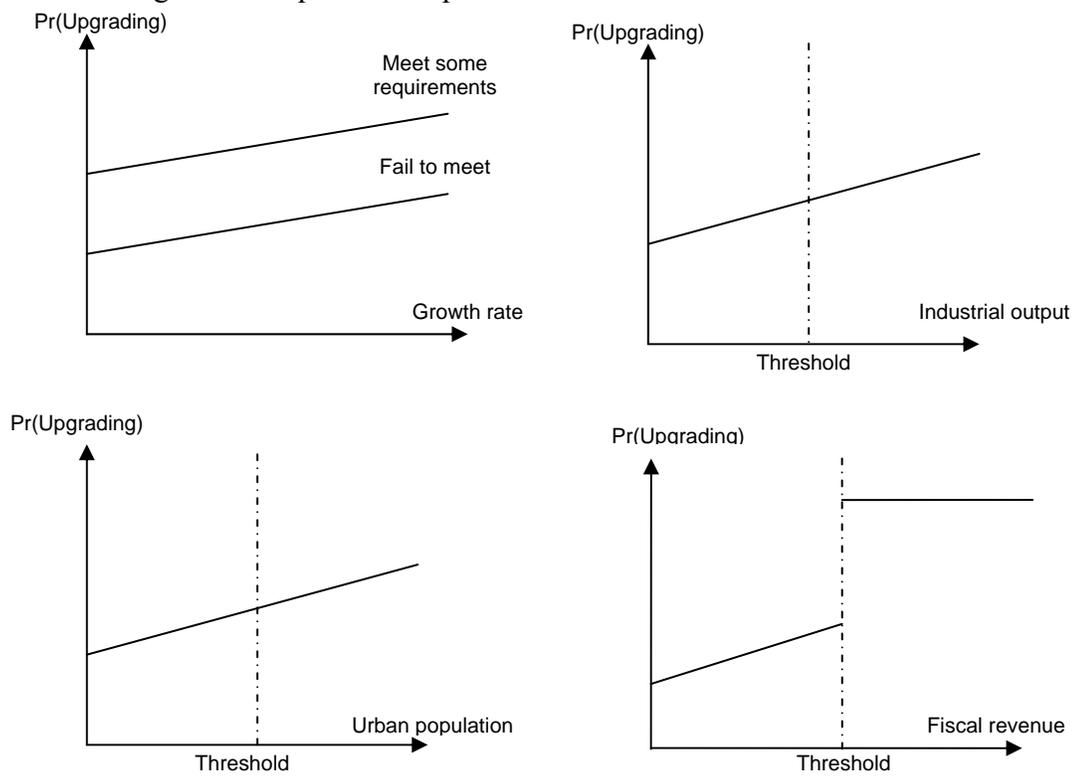
Source: GDP is from China Statistical Yearbook (2006); number of upgrading is from Liu and Wang (2000).

Figure 2. The spectrum of political models on China's central-local relationship



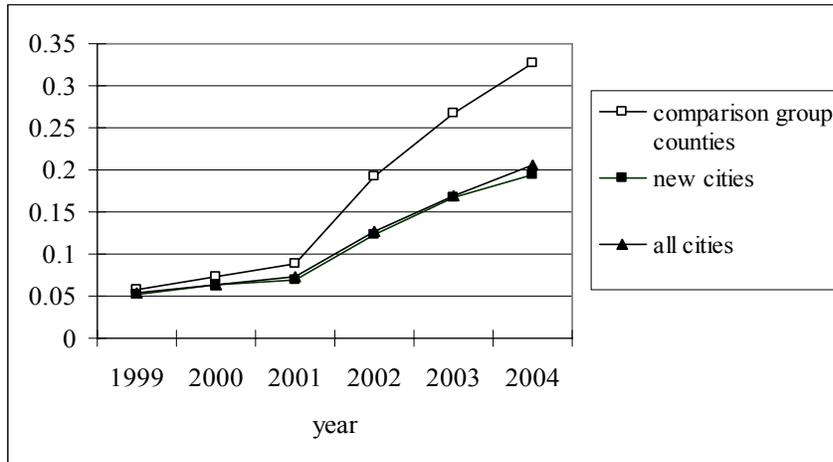
Source: Mei (2006).

Figure 3. Graphical interpretation of results drawn from Table 14



Note: The upper-left diagram shows the relation between growth rate and upgrading. The other three diagrams show the relation between upgrading and industrial output, urban population and fiscal revenue, respectively. "Threshold" means the cutoff value in the official minimum requirements.

Figure 4. Comparison of interest divergence: counties and cities



Note: The vertical axis is labeled by the ratio of the sum of local government funds to within-budgetary fiscal revenue, which measures the divergence of local interests from the center. The three lines represent the average value of this ratio for the three groups, respectively. The "comparison group counties" refer to counties that have a predicted probability of upgrading higher than 0.1 in 1998 or 1999.

Table 1. Minimum requirements for county-to-city upgrading

Population density (person/km <sup>2</sup> )		>400	100 - 400	<100
Percentage of counties in this category		25%	45%	30%
Industrialization level	Industrial output value ( <i>yuan</i> )	1.5 billion	1.2 billion	8 billion
	Share of industrial output value in gross value of industrial and agricultural output	80%	70%	60%
Urbanization level	Size of urban population (engaged in non-agricultural production)	150k	120k	100k
	Share of urban population in total population	30%	25%	20%
Fiscal strength	Fiscal revenue ( <i>yuan</i> )	60 million	50 million	40 million
	Per capita fiscal revenue ( <i>yuan</i> )	100	80	60

Source: "The Report on Adjusting the Criteria for the Designation of New Cities." Ministry of Civil Affairs, 1993. Available in English in Zhang and Zhao (1998).

Table 2. Benefits of being a city: an incomplete list

Category	Benefits	Source
Tax and fee	Cities enjoy a higher urban construction tax (7% compared to 5% for counties); could collect the surcharges levied on the issuing of motorcycle registration. In Liaoning province, cities could get 1 to 2 million additional subsidies each year after upgrading.	Chung and Lam (2004) Zhang and Zhao (1998)
Land-related	Cities generally convert more land to non-farm use and retain larger share of revenue from land sale.	Zhang (2006) Ping (2006)
Favorable policy	After achieving the status of "line item under province" ( <i>Shengji Jihua Danlie</i> ), cities could report directly to the provincial administration to ask for investment project	Su (2000) Zhang and Zhao (1998)
Administrative power	Cities have more authority on foreign trade and exchange management; gains authority over police recruitment and vehicle administration; could establish the branch of custom and large State-Owned banks; could approve projects with higher cap of investment.	Chung and Lam (2004) Du (1993)
Government size	Cities could establish more branches of government and have a larger size of government employees	Ren and Wang (1999)
Rank and salary	Sometimes the bureaucratic rank and salary of officials are raised after upgrading.	Liu (2005)
Reputation	Cities generally carry greater prestige and are more attractive to investors from outside.	Gu (1997) Chung and Lam (2004) Wang et al. (1998)

Note: Given the volatility of Chinese policies, the benefits are continuously changing overtime, and benefits listed are not necessarily effective during the same period.

Table 3. Construction of the sample for the logit model

Type of jurisdiction	Number	Observation year (shaded observations are dropped)				
		1993	1994	1995	1996	1997
Always a county, never upgraded	1,579	county	county	county	county	county
Upgraded to a city in 1997	4	county	county	county	county	city
Upgraded to a city in 1996	23	county	county	county	city	city
Upgraded to a city in 1995	19	county	county	city	city	city
Upgraded to a city in 1994	53	county	city	city	city	city
Already a city in 1993	231	city	city	city	city	city

Note: To estimate equation 1, the sample is from 1993 to 1997 because upgrading was stopped after 1997. Once a county gets city status, it loses the chance of receiving any further reward through upgrading. Therefore, in the discrete choice model, post-upgrading observations (in shaded cells) are excluded from the estimation.

Table 4. Comparison of mean values: upgrading and non-upgrading cases (1993-1997)

Variable	Non-upgrading		Upgrading		mean difference test: p-value
	Obs	Mean	Obs	Mean	
Gross value of industrial and agricultural output (GVIAO ,10,000 <i>yuan</i> )	8,048	145,563	99	651,154	0.000
Per capita GVIAO ( <i>yuan</i> )	8,048	3,220	99	8,896	0.000
Growth rate of GVIAO (1994-1997)	6,349	0.164	99	0.246	0.028
Industrial output value	8,048	97,858	99	558,708	0.000
Share of industrial output value in GVIAO	8,046	0.527	99	0.732	0.000
Population (10,000)	8,053	41.7	99	65.2	0.000
Urban population (10,000)	8,046	4.93	99	11.04	0.000
Share of urban population in total population	8,046	0.138	99	0.198	0.000
Within-budgetary fiscal revenue (10,000 <i>yuan</i> )	8,053	4255	99	6,980	0.000
Growth rate of fiscal revenue (1995-1997)	4,753	0.185	46	0.168	0.22
Fiscal revenue per capita ( <i>yuan</i> )	8,051	92.0	99	122	0.000
Number of public employees per 100 people	8,053	3.07	99	2.43	0.000

Note: All the output and revenue measures have been adjusted to 1993 constant prices using the yearly GDP deflator. Growth rate is calculated using adjusted values.

Table 5. Comparison of mean values: counties and county-level cities (1999-2004)

Variable	County		County-level city	
	Obs	Mean	Obs	Mean
Local government funds (10, 000 <i>yuan</i> )	8,683	613	1,857	2,614
Per capita local government funds ( <i>yuan</i> )	8,683	15.1	1,855	35.6
Within-budgetary fiscal revenue (10, 000 <i>yuan</i> )	8,683	5,422	1,857	18,377
Ratio of local government funds to within-budgetary fiscal revenue	8,683	0.105	1,857	0.116
Population (10,000)	8,683	43.6	1,855	72.9
GDP (10, 000 <i>yuan</i> )	8,661	150,811	1,854	544,184

Note: All output and revenue variables are measured in 1993 constant prices.

Table 6. Positive effect of growth rate on upgrading - baseline results

	Dependent variable: 1 = upgrade; 0 = no upgrade				
	(1)	(2)	(3)	(4)	(5)
Growth rate of GVIAO	0.473** (0.211)	0.519** (0.261)	0.546** (0.224)	0.749*** (0.275)	0.426* (0.219)
Log(industrial output)	0.033 (0.257)	0.367 (0.301)	0.456 (0.282)	1.130*** (0.323)	0.103 (0.267)
Share of industrial output	1.934 (1.267)	1.322 (1.375)	2.028 (1.345)	0.950 (1.454)	1.903 (1.788)
Log(urban population)	1.949*** (0.369)	1.584*** (0.396)	2.189*** (0.404)	1.600*** (0.395)	1.978*** (0.367)
Share of urban population	-1.304 (1.864)	0.976 (2.002)	-1.374 (2.334)	2.251 (2.313)	-1.341 (1.317)
Log(fiscal revenue per capita)	1.097*** (0.272)	0.650* (0.352)	1.390*** (0.276)	0.684** (0.332)	1.360*** (0.244)
Year Dummies		Yes		Yes	
Province Dummies			Yes	Yes	
Sample size	6,436	6,436	5,649	5,649	5,649

Note: Coefficients and robust standard errors clustered at the prefecture level are listed.

Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*. Except for growth rate and year dummies, all other independent variables are lagged by one year.

Table 7. Positive effect of growth rate on upgrading – robustness checks

	Dependent variable: 1 = upgrade; 0 = no upgrade							
	Lagged timing		Exclude poor counties		Relative growth rate		Average growth rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Growth rate of GVIAO	0.790** (0.362)	1.107** (0.470)	0.448** (0.207)	0.739*** (0.282)	0.326 (0.379)	0.814** (0.392)	0.609 (0.405)	0.663 (0.438)
Log(industrial output)	0.313 (0.388)	1.191** (0.482)	-0.060 (0.262)	1.011*** (0.337)	-0.019 (0.255)	1.06*** (0.315)	-0.043 (0.251)	0.98*** (0.317)
Share of industrial output	0.099 (1.736)	1.514 (2.158)	2.027* (1.212)	1.169 (1.488)	-1.620 (1.831)	1.903 (2.259)	2.145* (1.217)	1.617 (1.421)
Log(urban population)	1.572*** (0.495)	1.679** (0.664)	1.965*** (0.370)	1.618*** (0.400)	2.01*** (0.369)	1.66*** (0.395)	2.04*** (0.369)	1.73*** (0.400)
Share of urban population	0.700 (2.117)	-0.468 (2.741)	-1.889 (1.975)	1.579 (2.390)	2.103* (1.256)	1.121 (1.429)	-1.689 (1.858)	1.332 (2.314)
Log(fiscal revenue per capita)	1.076*** (0.299)	1.574*** (0.444)	1.041*** (0.269)	0.625* (0.335)	1.12*** (0.272)	0.732** (0.331)	1.13*** (0.271)	0.752** (0.335)
Year dummies		Yes		Yes		Yes		Yes
Province dummies		Yes		Yes		Yes		Yes
Sample size	4,754	3,904	4288	3885	6,436	5,649	6,441	5,654

Note: Coefficients and robust standard errors clustered at the prefecture level are listed. Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*. In columns 1 and 2, growth rate is lagged by one year, all independent variables, except for year dummies, are lagged by two years.

Table 8. Positive effect of growth rate on upgrading – robustness checks (continued)

	Dependent variable: 1 = upgrade; 0 = no upgrade							
	With growth rate of fiscal revenue		Split sample: 1994-1995		Split sample: 1996-1997		Average value for all variables	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Growth rate of GVIAO	0.642** (0.348)	0.718* (0.441)	0.343 (0.297)	0.592* (0.345)	0.659* (0.390)	0.686* (0.401)	1.27* (0.696)	0.967 (0.884)
Growth rate of fiscal revenue	0.023 (0.802)	0.206 (0.851)						
Log(industrial output)	0.602* (0.354)	1.32*** (0.469)	0.177 (0.312)	1.128*** (0.382)	0.176 (0.528)	1.272** (0.591)	-0.541 (0.396)	-0.276 (0.474)
Share of industrial output	1.52 (1.87)	0.777 (1.99)	1.844 (1.731)	1.451 (1.770)	1.531 (2.521)	-0.704 (2.524)	3.00* (1.78)	2.50 (2.03)
Log(urban population)	1.60*** (0.563)	1.60** (0.628)	1.978*** (0.459)	1.648*** (0.481)	1.376** (0.554)	1.739** (0.722)	2.92*** (0.546)	3.58*** (0.598)
Share of urban population	0.952 (2.39)	1.07* (0.561)	-1.527 (2.811)	1.518 (3.209)	1.985 (2.865)	6.517* (3.470)	-6.76*** (3.02)	-8.01*** (3.57)
Log(fiscal revenue per capita)	0.861* (0.524)	1.15** (0.513)	0.919*** (0.264)	0.340 (0.444)	0.767 (0.488)	1.348** (0.575)	2.55*** (0.512)	3.65*** (0.487)
Year dummies		Yes		Yes		Yes		
Province dummies		Yes		Yes		Yes		Yes
Sample size	4785	3931	3266	2699	3170	2511	1685	1486

Note: Coefficients and robust standard errors clustered at the prefecture level are listed. Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*. In columns 7 and 8, the independent variables are the average values during 1994-1997 (or from 1994 to the upgrading year if upgrading happens).

Table 9. Correction for sample attrition – probit model with sample selection		
	Dependent variable: 1 = upgrade; 0 = no upgrade	
	Probit	Probit with sample selection
	(1)	(2)
Growth rate of GVIAO	0.23 (0.15)	0.23 (0.15)
Log(industrial output)	0.097 (0.16)	0.096 (0.16)
Share of industrial output	0.33 (0.74)	0.31 (0.73)
Log(urban population)	0.68*** (0.21)	0.65*** (0.19)
Share of urban population	0.12 (0.99)	0.17 (0.97)
Log(fiscal revenue per capita)	0.51*** (0.18)	0.48*** (0.16)
Year dummies	Yes	Yes
Correlation coefficient between error terms in selection equation and outcome equation		0.23 (0.46)
Sample size	4910	4910

Note: Coefficients and robust standard errors clustered at the prefecture level are listed. Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*. Column 1 is estimated using data of 1995-1997. In column 2, selection equation is estimated using data of 1994-1996; outcome equation is estimated using data of 1995-1997.

Table 10. Number of counties that satisfied each upgrading requirement in 1993

Requirement	Criterion for this requirement	Number of counties (total is 1,678)	
		Meet one criterion	Meet both
Industrialization level	Industrial output	329 (19.6%)	158 (9.4%)
	Share of industrial output	290 (17.3%)	
Urbanization level	Urban population	212 (12.6%)	67 (4.0%)
	Share of urban population	212 (12.6%)	
Fiscal strength	Fiscal revenue	444 (26.5%)	352 (21.0%)
	Per capita revenue	976 (58.2%)	

Note: See Table 1 for the threshold value of each criterion.

Table 11. Number of county-year observations by upgrading status and requirements satisfied (1994-1997)

		Non-upgrading cases		Upgrading cases	
		Industry dummy=0	Industry dummy=1	Industry dummy=0	Industry dummy=1
Urbanization dummy=0	Fiscal dummy=0	4583	212	24	14
	Fiscal dummy=1	1005	375	14	31
Urbanization dummy=1	Fiscal dummy=0	96	5	2	0
	Fiscal dummy=1	83	36	8	6

Table 12. Number and percentage of county-year observations by upgrading status and number of requirements satisfied (1994-1997)

Number of requirements satisfied	0	1	2	3	total
Non-upgrading cases	4,583	1,313	463	36	6,395
	(71.6%)	(20.0%)	(7.0%)	(0.5%)	(100%)
Upgrading cases	24	30	39	6	99
	(24.2%)	(30.3%)	(39.4%)	(6.1%)	(100%)

Table 13. Number of upgrading cases that satisfied each requirement by region

Region	Total upgrading	Industry dummy=1	Industry dummy=0	Fiscal dummy =1	Fiscal dummy =0	Urbanization dummy=1	Urbanization dummy=0
Coastal	49	31	18	34	15	8	41
Middle	30	15	15	14	16	5	25
Western	20	5	15	11	9	3	17

Table 14. Non-enforcement of upgrading requirements – allowing flexible intercept and marginal effect

	Dependent variable: 1 = upgrade; 0 = no upgrade				
	(1)	(2)	(3)	(4)	(5)
Growth rate of GVIAO	0.731** (0.289)	0.668* (0.376)	0.799** (0.358)	0.643 (0.392)	0.740* (0.378)
Meet $\geq$ 2 requirements		0.722** (0.365)		0.577 (0.377)	
Meet $\geq$ 1 requirement			0.789** (0.370)		0.636 (0.416)
Interaction term		0.025 (0.591)	-0.181 (0.490)	0.093 (0.580)	-0.101 (0.511)
Log(industrial output)	0.917** (0.386)	1.052*** (0.327)	1.033*** (0.322)	0.882** (0.383)	0.831** (0.381)
industrialization dummy	-0.566 (4.824)			-0.784 (4.862)	-1.766 (4.799)
Interaction term	0.114 (0.390)			0.116 (0.392)	0.201 (0.387)
Log(urban population)	1.715*** (0.457)	1.541*** (0.407)	1.569*** (0.396)	1.705*** (0.463)	1.726*** (0.452)
Urbanization dummy	0.131 (2.117)			0.374 (2.053)	0.043 (2.136)
Interaction term	-0.334 (0.790)			-0.467 (0.769)	-0.333 (0.794)
Log(fiscal revenue per capita)	1.088** (0.449)	0.465 (0.380)	0.469 (0.346)	1.003** (0.470)	1.003** (0.461)
Fiscal dummy	5.223** (2.056)			5.484*** (2.119)	5.090** (2.109)
Interaction term	-1.039** (0.432)			-1.113** (0.448)	-1.040** (0.444)
Share of industrial output	0.190 (1.404)	0.761 (1.446)	0.753 (1.501)	0.232 (1.385)	0.179 (1.440)
Share of urban population	3.608 (2.545)	2.087 (2.351)	2.173 (2.351)	3.589 (2.536)	3.528 (2.576)
Sample size	5649	5649	5649	5649	5649

Note: All columns include year and province dummies. Coefficients and robust standard errors clustered at the prefecture level are listed. Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*. Except for growth rate, requirement dummy and their interaction term, all other independent variables are lagged by one year.

Table 15. Incentive versus bargaining- results

	Dependent variable: ratio of the sum of local government funds to within-budgetary fiscal revenue										
	"Newcity" = 1 if upgrade after 1994			"Newcity" = 1 if upgrade after 1992			"Newcity" = 1 if upgrade after 1990				
	(1) full sample	(2) restricted sample	(3) full sample	(4) restricted sample	(5) city sample	(6) full sample	(7) restricted sample	(8) city sample	(9) full sample	(10) restricted sample	(11) city sample
City	-0.013* (0.007)	-0.047** (0.022)	-0.012 (0.007)	-0.047** (0.022)		-0.006 (0.008)	-0.040* (0.023)		-0.002 (0.009)	-0.036 (0.023)	
Newcity			-0.004 (0.011)	-0.0002 (0.011)	-0.002 (0.011)	-0.018 (0.011)	-0.015 (0.011)	-0.017 (0.011)	-0.020* (0.011)	-0.019 (0.011)	-0.020* (0.011)
Log(GDP)	0.021*** (0.006)	0.023** (0.012)	0.021*** (0.006)	0.023** (0.011)	0.025** (0.010)	0.021*** (0.006)	0.022* (0.012)	0.024** (0.011)	0.021*** (0.006)	0.023* (0.012)	0.025** (0.011)
Log(population)	-0.011 (0.011)	-0.005 (0.017)	-0.011 (0.011)	-0.005 (0.017)	-0.008 (0.014)	-0.011 (0.011)	-0.004 (0.017)	-0.008 (0.014)	-0.011 (0.011)	-0.003 (0.018)	-0.007 (0.014)
Growth rate of GDP	-0.013 (0.009)	-0.012 (0.016)	-0.013 (0.009)	-0.012 (0.016)	-0.016 (0.017)	-0.013 (0.009)	-0.012 (0.016)	-0.016 (0.017)	-0.013 (0.009)	-0.012 (0.016)	-0.015 (0.017)
Joint F-test on "City" and "Newcity": p-value			0.14	0.11		0.04	0.03		0.03	0.02	
R-squared	0.08	0.19	0.08	0.19	0.17	0.08	0.19	0.18	0.08	0.19	0.18
Sample size	10,269	2,098	10,269	2,098	1,823	10,269	2,098	1,823	10,269	2,098	1,823

Note: All columns include a full set of year dummies. Coefficients and robust standard errors clustered at the prefecture level are listed. Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*. Full sample include all counties and county-level cities; restricted sample include all cities and "comparison group counties"; city sample only include cities.

## Chapter 4: The Consequences of Administrative Decentralization: Evidence from China's County-to-City Upgrading

### **Abstract**

In China, awarding the title of city to existing counties has created more than 400 new urban administrative units from 1983 to 1997. Using this county-to-city upgrading as an example, this paper examines the consequences of administrative decentralization. I merge data from the local public finance statistics with the population census and get a rich set of economic, fiscal and public services outcome variables. Using difference-in-differences (DID), propensity score matching, and DID propensity score matching methods, my finding confirms that city status increases government size and revenues, and creates more urban employment opportunities. However, there is no significant improvement in local public services after upgrading, and the high economic growth rates in pre-upgrading period drop to a normal level after upgrading. These results are interpreted by analyzing the incentive structure of local government officials.

## 4.1 Introduction

The theory of fiscal federalism says that decentralization has allocative benefits in providing public goods and services (Musgrave, 1959; Oates, 1972). Specific to China, there is a general consensus that fiscal decentralization promoted inter-jurisdictional competition and spurred market development during the reform era (Qian and Weingast, 1996; Lin and Liu, 2000; Jin et al., 2005). However, a few studies also raised the concern that decentralization may have had a negative distributional effect (Park et al., 1996; Knight and Li, 1999; Zhang and Zou, 1998). Zhang (2006) further argues that under a centralized governance structure, fiscal decentralization could lead to severe regional disparity in income level.

In fact, decentralization took place not just in the form of devolving taxing and spending powers to the local government. Cai and Treisman (2006) distinguish three types of decentralization in China: administrative decentralization, fiscal decentralization, and political decentralization. Although the Chinese political system has remained centralized (Blanchard and Shleifer, 2001; Zhang, 2006), there have been experiments of political decentralization at the grassroots-level. Zhang et al. (2004a) and Shen and Yao (2008) study the effect of village head elections and find positive effects on income equality and local public goods provision. In contrast to the attention paid to fiscal and political issues in the literature, few studies have investigated the effects of administrative decentralization. Whether the devolution of administrative powers to the local level improves local public services is especially under-researched.

My paper addresses this issue by looking at the practice of urbanization through administrative orders in China. From 1983 to 1997, more than 400 counties were awarded city status through “county-to-city upgrading” (Chung and Lam, 2004). Since upgrading greatly increases the administrative independence and revenue collection power of local governments, it could be viewed as an example of administrative decentralization. By increasing the number of cities and giving them more authority, the policy goal of upgrading is to speed up economic growth and urbanization process. Furthermore, it intends to give local governments some

leverage to provide better public services.<sup>29</sup> Thus, the focus of this paper is to examine whether upgrading achieved these policy goals, and furthermore, to explore the reasons.

Empirically, I evaluate the effect of upgrading using counties that have never been upgraded as the control group. In order to control for the pre-treatment differences, a difference-in-differences (DID) method is adopted. I merge data from two sources, namely the local public finance statistics and the population census. This gives me a large dataset that contains a rich set of economic, fiscal and public services outcomes at the county level. Because some variables are available only in the post-upgrading period, I use a propensity score matching method to conduct the evaluation. Combining the advantage of both DID and matching, a difference-in-differences propensity score matching method is also performed.

I report the following major findings. First, I find a larger increase in immigration and urban population in cities relative to counties. In addition, cities create more jobs in manufacturing and the service sectors. Second, fiscal revenue and the number of public employees increase more quickly in cities, suggesting an expansion of local government activities. The increase in total revenue is accompanied by a decrease of the share of agricultural tax. Third, economic growth rates of newly established cities drop from a high level to a normal level after upgrading. Last but not least, there is little difference in the level of educational achievement, public health outcomes and living conditions between counties and cities.

The first two findings are largely expected given the nature of upgrading. By winning city status, the localities gain many benefits. As shown in the previous chapter, they usually receive subsidies to improve urban infrastructures. Cities are allowed to impose a higher urban construction tax rate and have higher quotas to convert agricultural land into urban usage. At the same time, local governments also gain more administrative authority and revenue collection power. They are allowed to

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<sup>29</sup> In the “Report on the Sixth Five-Year Plan” made in 1982, Premier Zhao Ziyang states that “Except for special cases, the administrative power of enterprises should be decentralized to cities, bypassing the central ministries and provinces. We should change cities, especially big cities, into open, multi-functional, modern economic centers and form an economic network that based on big cities while including surrounding small cities and towns.” (Available at <http://www.people.com.cn/zgrdxw/zlk/rd/5jie/newfiles/e1170.html>)

set up more government branches and expand the number of public employees after getting city status. These favorable policies naturally lead to an increase in the level of urbanization and industrialization.

The other two findings are somewhat surprising. With all those favorable policies, it is normally expected that cities to have some advantage over counties in economic development and thus maintain a higher growth rate in the post-upgrading period. Similarly, with more administrative independence and revenue collection power, cities should be able to provide better public services according to the theory of federalism. In order to interpret these findings, it is important to examine the incentives of local officials. As shown in the previous chapter, upgrading serves to reward localities with high growth rates and thus gives the central government an informal way of providing incentives. Together with the formal approach, political promotion, it forms part of the incentive system for Chinese local officials.

Before upgrading, under both the formal and informal incentive mechanisms, local officials compete on economic growth to please the upper level government. After becoming cities, there is no further upgrading chance available to localities, resulting in the disappearance of the informal incentive mechanism. Since the formal incentive mechanism could not be expanded in a short period of time to make up the loss of the informal incentive mechanism, local officials may suddenly lose part of their incentives to maintain a high growth rate. Thus, given the importance of local officials' incentives in China (Li and Zhou, 2005; Xu et al., 2007), the change in the incentive structure of local officials must play a key role in the drop of growth rates.

In terms of the provision of public goods and services, city status does not bring in any new incentive. According to Mei (2006), local officials are multi-task agents in the political hierarchy. Without changes in the evaluation criteria, they always choose to perform tasks whose intended outcomes could be easily realized and are more predictable. Since providing public goods, such as education and health, does not have an immediate and certain effect on their economic performance, local officials always lack the proper incentive to work hard on these issues. Thus, it is not

surprising that I do not find any evidence of significant improvement in local public goods outcomes.

The remainder of this paper is organized as follows. Section 4.2 describes the data and gives some brief descriptive analysis. Section 4.3 discusses the empirical strategy. Section 4.4 reports the results of the DID estimation. Section 4.5 reports the matching results. Section 4.6 concludes.

## **4.2 Data and descriptive analysis**

The major outcome variables are from the two recent population censuses that were conducted in 1990 and 2000, respectively. Assembled at the county level, they provide rich information on education, health, migration, urbanization and employment. Merging the two censuses together, I get the “census dataset” that is a 2-period panel data with observations in 1990 and 2000.

The census dataset does not provide information on economic conditions and government activities, such as GDP, fiscal revenue and expenditure. In order to estimate the propensity score of upgrading and to get more outcome variables, I also use the yearly series of *Sub-Provincial Public Finance Statistics 1993-2004* (henceforth “public finance dataset”). Since it only starts in 1993, I drop cities that were upgraded before 1994 because I lack data to estimate their upgrading propensity score (one-year lagged data is used in the estimation). I keep all counties in the sample as the control group. Jurisdictions that were counties before 1994, but got upgraded to cities during 1994-1997 form the treatment group (upgrading policy stopped at 1997).

Merging the two datasets allows us to compare various outcomes both before and after upgrading. For most outcomes from the public finance dataset, I have data continuously from 1994 to 2004. Table 1 lists the 1994 and 2004 mean values for cities and counties, respectively. In total I have 1537 counties and 99 cities being upgraded during 1994-1997. Growth rate is not listed in this table because I do not have a continuous measure from 1994 to 2004. While GDP data is not available before 1998, Growth value of industrial and agricultural output (GVIAO) is not

available after 2000. Instead, I will use a graph to compare growth rate between 1994 and 2004 later in this section. There are several interesting observations from this table. First, the number of public employees increases much more quickly in the cities relatively to counties. Second, the increase in the share of productive public expenditure (basic construction expenditure plus expenditure supporting agricultural production divided by total expenditure) seems to be slower in cities. Third, cities have much more extra-budgetary revenue and land revenue than counties in 2004.

Table 2 lists the 1990 and 2000 mean values of variables from the population census dataset. I restrict the sample to those that will appear in the empirical studies in section 4.5. In total, there are 95 cities and 1001 counties. From a preliminary check on these raw means, it is easy to see that urban population, immigrants and employment in the manufacturing and service sectors growth faster in cities relative to counties. In contrast, for public service outcomes, such as education and crude death rate, there seems to be little difference in the change from 1990 to 2000 between cities and counties.

The previous chapter has shown that a high growth rate increases the chance of getting an upgrade; and this works through an incentive mechanism. Once counties have achieved city status, there is no further chance of getting reward through administrative upgrade. Thus, local officials' incentives may be weakened after upgrading, which could result in the decrease of growth rate. I use a graph to compare the average growth rate of all county-level jurisdictions and those that were upgraded from counties to cities during 1994-1997 (Figure 1). Since GVIAO is not available after 2000, I use the growth rate of GDP to show the trend of 2000-2004. It is easy to see that the growth rate of cities that were just upgraded dropped sharply after 1997 and became normal compared to counties in later years.

I then focus on cities that were upgraded from counties in recent years. Figure 2 depicts the trend of growth rate before and after their upgrading. Negative number on the horizontal axis represents years before upgrading, during which they were still counties. Similarly, positive numbers means years after upgrading. Both the actual growth rate and the growth rate relative to national average show the same trend: in

years right before upgrading and when upgrading happens, the average growth rate of these jurisdictions are at a high level; after upgrading, it starts to drop; two years after upgrading, it falls below the national average. After that, the average growth rate returns to the national average and stays around there. This figure shows that counties that eventually got an upgrade are not born with high growth rates. Instead, high growth rates are closely associated with the opportunity of getting an upgrade, which suggests that incentives play an important role in explaining the fluctuation of growth rates before and after upgrading.

### 4.3 Empirical strategies

#### 4.3.1 Difference-in-differences model

Assuming that cities and counties have a parallel time trend for outcome  $Y$ , then a straightforward way to estimate the effect of city status on  $Y$  is a DID model

$$(1) Y_{it} = \beta_0 + \beta_1 * Upgrade_i + \beta_2 * Post_t + \beta_3 * Upgrade_i * Post_t + \varepsilon_{it}$$

$Upgrade_i$  is a dummy for upgrading,  $Post_t$  is a dummy for post-upgrading period, and the interaction term,  $Upgrade_i * Post_t$ , equal one for cities in their post-upgrading years. Since each upgrading case happens in different years,  $Post_t$  varies across cities, and it is not even defined for counties. Thus, equation (1) needs to be modified before I can actually run the regression. I use a full set of year dummies to substitute  $Post_t$ . This, however, will not affect the definition of  $Upgrade_i * Post_t$ . So the actual empirical model is

$$(2) Y_{it} = \beta_0 + \beta_1 * Upgrade_i + \beta_2 * Year Dummy_t + \beta_3 * Upgrade_i * Post_t + \varepsilon_{it}$$

In equation (2),  $\beta_3$  measures the average effect of city status on outcome  $Y_{it}$  after controlling for pre-upgrading differences. This model could be applied to evaluate outcomes from both the public finance dataset and the census dataset, as long as they are available in both pre- and post-upgrading periods. For the public finance dataset, since I have yearly data from 1994 to 2004, I can decompose the average effect of upgrading into effects in different post-upgrading years. To do this, I replace the  $Upgrade_i * Post_t$  dummy with ten dummies:  $Upgrade_i * Post0_t, Upgrade_i * Post1_t, \dots$ ,

$Upgrade_i * Post9_t$ . Among them,  $Upgrade_i * Post0_t$  equals to one for cities in the year they were upgraded,  $Upgrade_i * Post1_t$  equals to one for cities in the first year after upgrading, etc. The model is

$$(3) Y_{it} = \beta_0 + \beta_1 * Upgrade_i + \beta_2 * Year Dummy_t + (\beta_{30} * Upgrade_i * Post0_t + \beta_{31} * Upgrade_i * Post1_t + \dots + \beta_{39} * Upgrade_i * Post9_t) + \varepsilon_{it}$$

where  $\beta_{30}$  measures the average effect of upgrading for the year of upgrading,  $\beta_{31}$  measures the average effect of upgrading for the first year after upgrading, etc. Equation (3) allows me to detect the time pattern of the effect, such as immediate vs. gradual and temporary vs. permanent.

#### 4.3.2 Propensity score matching

Some important outcome variables are available only in post-upgrading years. For example, land revenue is available in 2000-2004 in the public finance dataset, and housing condition is only available in 2000 in the census dataset. For these outcomes, I cannot perform DID estimation. In order to control for the pre-upgrading differences between counties and cities, I use propensity score matching to evaluate the effect of upgrading (Rosenbaum and Rubin, 1983; Dehejia and Wahba, 1999, 2002). Based on the analysis on the determinants of upgrading in the previous chapter, I use a logit model to estimate the propensity score of upgrading during 1994-1997

$$(4) \Pr(Y_{it} = 1) = \exp(\beta_0 + G_{it}\beta_1 + Z_{it}\beta_2) / (1 + \exp(\beta_0 + G_{it}\beta_1 + Z_{it}\beta_2))$$

where  $G$  represents growth rate,  $Z$  is a vector that includes lagged values of variables appearing in the upgrading requirements, such as urban population, industrial output and fiscal revenue.

Then I combine counties and cities with different observed characteristics but same propensity scores using matching. The matching estimator is

$$(5) \hat{\alpha}_{Matching} = \frac{1}{n} \sum_{i \in county \rightarrow city} \left\{ Y_i - \sum_{j \in county} W(i, j) Y_j \right\}$$

where  $i$  represents those that were upgraded from counties to cities during 1994-1997,  $j$  represents those that remain to be counties. The match for each upgrading case  $i$  is

constructed as a weighted average over the outcome of control group – counties. The weight,  $W(i, j)$ , depends on the distance between the propensity scores for  $i$  and  $j$ . In this paper, I construct the weights using a kernel function that is standard in the literature (Smith and Todd, 2005).

### 4.3.3 Difference-in-differences propensity score matching

For outcomes from the census data and are available in both 1990 and 2000, I also conduct the difference-in-differences propensity score matching (Heckman et al., 1997; Heckman et al., 1998). The estimator is defined as

$$(6) \hat{\alpha}_{DID} = \frac{1}{n} \sum_{i \in \text{county} \rightarrow \text{city}} \left\{ (Y_{i,2000} - Y_{i,1990}) - \sum_{j \in \text{county}} W(i, j)(Y_{j,2000} - Y_{j,1990}) \right\}$$

The difference of this DID propensity score matching estimator from the matching estimator in the previous subsection is that it allows for systematic differences between upgrading and non-upgrading outcomes after conditioning on observables. It is analogous to the standard DID regression estimator defined in Section 4.3.1, but it does not need to impose a linear functional form on the model and it re-weights the observations according to the weights used by the matching estimators. According to Smith and Todd (2005), DID matching generally performs better than cross-sectional matching estimators.

## 4.4 Difference-in-differences estimation results

Table 3 shows the DID estimation results based on equation (2). The outcome variables are from the public finance dataset, including fiscal revenue, number of public employee, fiscal revenue per public employee, share of productive expenditure in total expenditure, and share of agricultural tax in total revenue.<sup>30</sup> While the first two variables tell us about the expansion of government activities, the other three variables provide additional information on fiscal condition and revenue and expenditure structure of the local government. Since all these variables are available

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<sup>30</sup> It is possible that fiscal revenue and public employee follow log normal distributions, so that their log values are better dependent variables. I have conducted robustness checks using their log values as dependent variables and the findings are very similar. Since these variables are more meaningful in their levels, here I only report estimates using their levels as dependent variables.

continuously from 1994 to 2004, Table 4 further decomposes the average effect of upgrading into effects in different post-upgrading years based on equation (3).

Column (1) of Table 3 shows that the average effect of upgrading on fiscal revenue is positive, with a magnitude of about 15% of the average value. The decomposed effects shown in column (1) of Table 4 display an interesting time pattern. The effect is negative in the first three years after upgrading and then become positive starting in the fourth year. This may suggest that although upgrading gives more revenue collection power to the local government, it does not increase the local revenue in the short term. However, upgrading has a long-term effect on revenue generating.

As Luo and Zhang (2006) point out, under the centralized governance structure in China, the size of local government is largely in proportion to local population, unless there is a change in the governance structure. Column (2) of these two tables confirms the effect of such a change. There is an immediate increase in the number of total public employee right after upgrading, and it continues to grow in the post-upgrading period. The average increase is as large as twenty percent relative to counties.

The increase in fiscal revenue and public employees is consistent with the fact that local officials gain more administrative independence and discretionary power through upgrading. Analogous to firm managers who maximize their control rights, local officials prefer a larger government. Administrative decentralization gives them some leverage to expand the local government activities in terms of both revenue and employees.

Fiscal revenue per public employee is used to measure the fiscal dependent burden (Zhang, 2006). The lower the fiscal revenue per public employee, the less capable the local government is to support its employees, thus the heavier the fiscal dependent burden. With a huge increase in the size of public employees, it is interesting to know whether this burden becomes more severe after upgrading. In column (3) of Table 3, I do not find significant difference in revenue per public employee between cities and counties. But once this effect is decomposed into

different years (Table 4, column 3), it appears that the fiscal dependent burden actually becomes more severe in the first seven years after upgrading, suggesting that the expansion of the number of public employees overshadows the increase of revenue, at least in the short term.

Column (4) of both tables examines how the expenditure is distributed into productive investment in agriculture and basic construction. This part of expenditure is called “productive” as opposed to another important part, administrative costs of the government. The negative estimate suggests that cities spent a less proportion on this part. Again, this may be largely due to the quick expansion of government size.

Finally, column (5) of both tables shows a negative effect of upgrading on share of agricultural tax in the total revenue. This suggests that the focus of local government has shifted away from agriculture.

In sum, all these results shown in Table 3 and 4 are consistent with the proposition that upgrading decentralizes administrative power to the local government and allows them to generate more revenues and inflate the size of public sectors.

## **4.5 Matching results**

### **4.5.1 Estimating propensity scores**

The results for the logit estimation of the propensity score could be found in column 1 of Table 6 in the previous chapter. Figure 3 shows the histograms of the estimated propensity score for counties (upper panel) and cities (lower panel). Following Smith and Todd (2005), I match on the odds ratio of propensity score,  $P/(1-P)$ , rather than on the propensity score  $P$  itself, so that the estimates are robust to choice-based sampling. The histograms of the log odds ratio are shown in Figure 4. These figures give us a graphical assessment of the extent of common support. It looks that the propensity scores have a lot of overlap for cities and counties. In fact, only two cities do not have support in the counties, and they will be excluded in matching.

In order to check whether the conditioning variables used to construct propensity score in equation (4) satisfy the conditional independence condition, I need to perform a balancing test. The general idea is to test whether or not there are differences in these variables between cities and counties after conditioning on the propensity score. In this paper, I follow Smith and Todd (2005) to conduct a regression-based balancing test for each conditioning variable. For example, for growth rate  $G$ , I run the following regression

$$(7) \quad G_{it} = \beta_0 + \beta_1 \hat{P}_{it} + \beta_2 \hat{P}_{it}^2 + \beta_3 \hat{P}_{it}^3 + \gamma_0 Upgrade_{it} + \gamma_1 Upgrade_{it} \cdot \hat{P}_{it} + \gamma_2 Upgrade_{it} \cdot \hat{P}_{it}^2 + \gamma_3 Upgrade_{it} \cdot \hat{P}_{it}^3 + \varepsilon_{it}$$

where  $\hat{P}$  is the estimated propensity score. Then I test  $H_0: \gamma_0 = \gamma_1 = \gamma_2 = \gamma_3 = 0$ . Failing to reject  $H_0$  means that conditional on the polynomials of the propensity score, interactions of the treatment dummy with these polynomials have no additional impact on the conditioning variable. This suggests that the balancing condition is satisfied. The F-statistics for these tests are generally small, with p-values all above than 0.1, suggesting that I can not reject  $H_0$ . In sum, both the histograms and the balancing tests suggest that the conditional independence condition and common support conditions are satisfied.

In estimating the logit model (4) using data of 1994-1997, I get one propensity score for each jurisdiction-year observation. Thus, each county or city has multiple propensity scores. For the treatment group – cities, it is straightforward to keep the propensity score corresponding to treatment, that is, the one in the upgrading year. For the control group – counties, I keep propensity scores in all four years and treat them as independent control observations in the actual matching. This allows us to use all information of the control group in terms of the probability of getting treatment in any of these years.

#### 4.5.2 Matching results

Table 5 shows the propensity score matching results for three variables that are available only in the post-upgrading period. The first variable, floor space per person,

comes from the census dataset. The estimate is not significant, suggesting that cities do not outperform counties in this indicator of people's well-being.

The other two variables come from the public finance dataset.<sup>31</sup> I find that extra-budgetary funds are significantly higher in cities relative to counties. This is consistent with the fact that city status gives local governments more discretion over revenue collection, so that they can raise more funds from sources that are out of the budget.

Another outcome variable is the revenue from land. According to Zhang et al. (2004b) and Ping (2006), industrialization and urbanization have driven up the demand for land and dramatically increased land value since 1990s. Thus, local governments have a strong incentive to convert agricultural land into non-farm usage, and in turn increase their revenue. The significant result of land revenue shows that increased administrative authority brought by city status helps local governments to achieve this goal.

#### **4.5.3 Difference-in-differences propensity score matching results**

For outcome variables from the census dataset, I perform a DID propensity score matching to estimate the effect of city status on the change of their values from 1990 to 2000. Together with the DID propensity score matching, I also perform a regression-adjusted matching, which controls for the same set of conditioning variables as in matching, but uses a linear functional form. To do this, I run an OLS regression of the difference between 2000 and 1990 values on the *Upgrade<sub>i</sub>* dummy and all the conditioning variables used to construct propensity score in logit model.

Table 6 shows the estimated coefficients and corresponding t-ratios. The outcomes are grouped into four categories: public goods and services, urbanization, immigration and employment. For level variables, I present the estimates on their log values. To make this table easy to read, I use a positive t-ratio to indicate that cities are doing better than counties, and add a negative sign before t-ratios if the sign of the coefficient suggests that cities are doing worse. For example, since the increase in

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<sup>31</sup> The findings on extra-budgetary revenue and land revenue are also robust to the using of their log values as dependent variables

the attendance rate of secondary and above education is smaller in cities relative to counties (with a coefficient of -0.00026), the t-ratio is labeled as negative (-0.14). Thus, for those “bad” outcomes (illiterate ratio, crude death rate, disabled population, disability rate, percentage of people working as farmers, employment in agriculture and mining), a negative t-ratio means that cities have a smaller reduction or a bigger increase than counties.

In terms of the provision of public goods, I generally do not find much difference between counties and cities. The attending rate in secondary and above-level schools, the crude death rate and the disabled population are not significant. In terms of the reduction in illiterate ratio, cities are doing even worse than counties, which may be due to a low initial value. The number of disabled population increases more quickly in cities (although not significant), which could be the result of increased demand for more dangerous jobs, such as construction workers.

For urbanization indicators, cities are found to be performing better. Urbanization process seems to be sped up by favorable policies accompanied with city status. Also, the number of farmers and employment in the agricultural and mining sectors decreases faster in cities. Results on the employment variables are largely consistent with the expectation that cities should provide more jobs in the industry and service sectors. More job opportunities in cities could also explain the significant increase in the number of immigrants in cities.

In sum, these results show that upgrading helps local jurisdictions to speed up urbanization process and create more urban employment opportunities. However, these economic prosperities do not seem to have led to better public goods and services, at least in the short term.

#### **4.6 Conclusion**

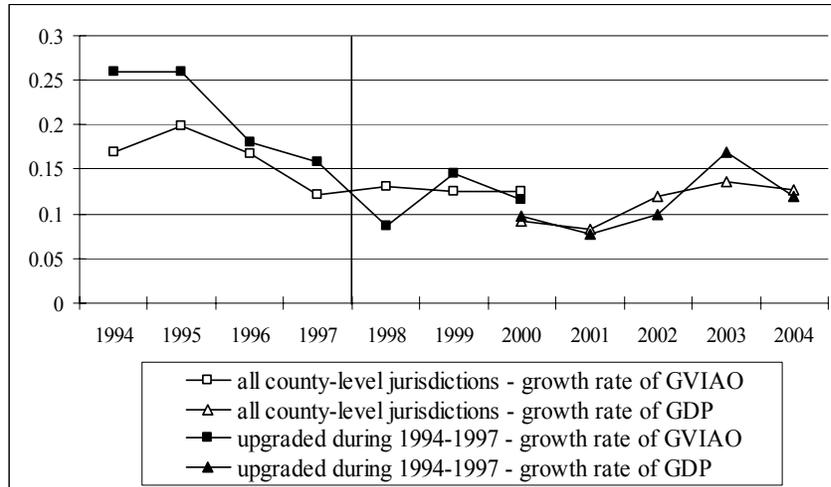
Using county-to-city upgrading as an example of administrative decentralization, this paper examines its consequences on a wide range of economic, fiscal and public service outcomes. I employ several empirical methods to conduct the estimations,

including difference-in-differences, propensity score matching, and difference-in-differences propensity score matching methods.

My results confirm the expansion of government revenue and public employees due to upgrading. Due to the loss of informal incentives in the incentive structure of local officials after upgrading, economic growth rate falls in newly upgraded cities. Although city status increases the urbanization level and helps to create more jobs in industrial and service sectors, it does not directly improve the provision of local public goods and services. The reason is that upgrading does not provide local officials with new incentive to work hard on these issues.

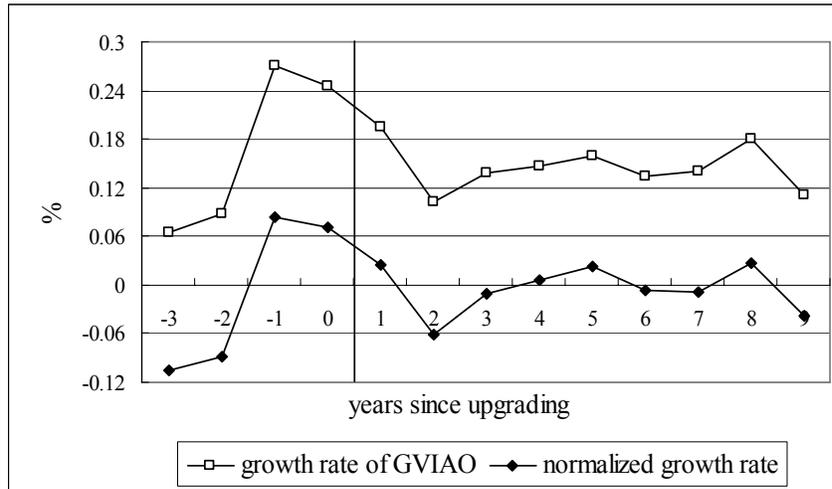
This paper highlights the importance of local officials in making policies effective in China. Even if the central government has designed a good policy, it is hard to get expected effect without the cooperation of local officials. In short, it is very important to “make incentives right.”

Figure 1. Comparison of growth rates: upgrading cases and the national average



Note: This graph compares the average growth rate of all county-level jurisdictions and those that were upgraded from a county to a city during 1994-1997. GVIAO means the growth value of industrial and agricultural output, which measures the total economic activity when GDP data is not available.

Figure 2. Trend of growth rates before and after upgrading



Note: This graph shows the trend of average growth rates for county-level jurisdictions that have experienced upgrading. The horizontal axis represents the time difference between current year and the year of upgrading. Year "0" means the year of upgrading, year "-1" means the year right before the upgrading year and year "1" means the year right after upgrading. The upper line draws the average growth rate of GVIAO. The earliest upgrading case reflected in this graph took place in 1985, and the growth rate in 1994 is used to calculate the average growth of year "9". In contrast, if upgrading took place in 1997, the growth rate of 1994 is used to calculate the average growth of year "-3". The lower line, normalized growth rate, is drawn in a similar way using growth rate relative to the national average value (average of all counties and county-level cities). For example, for a city upgraded in 1985, I subtract its 1994 growth rate by the 1994 national average value and use it to calculate the year "9" average growth rate.

Figure 3. Histogram of propensity score

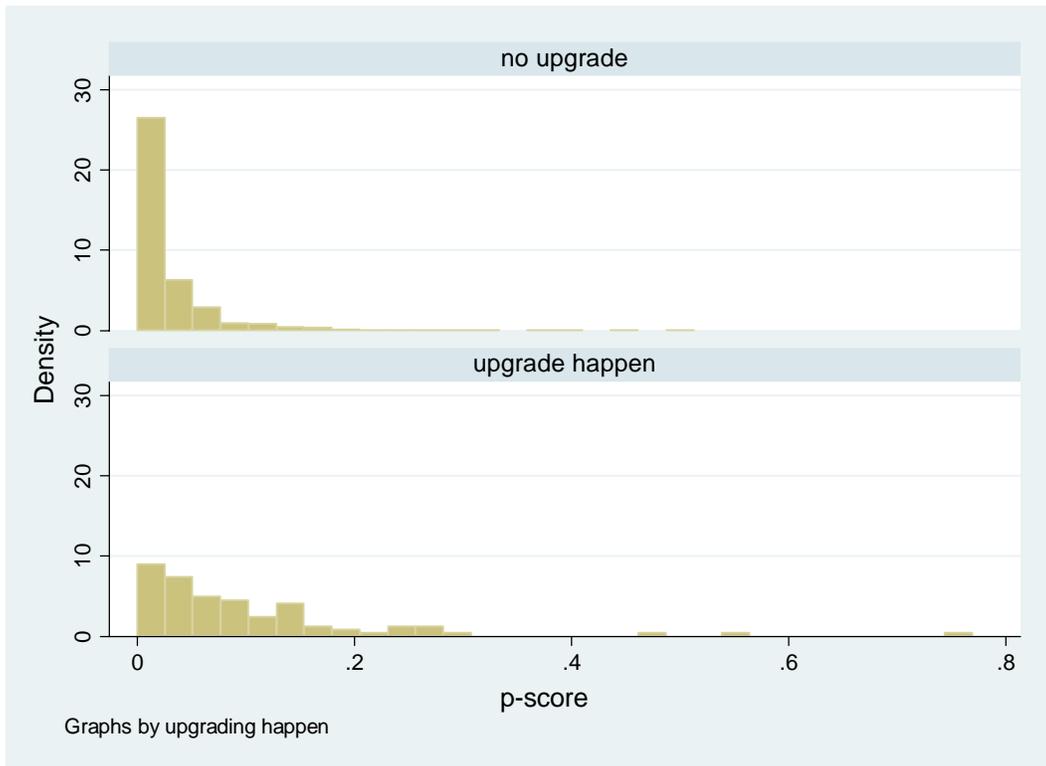


Figure 4. Histogram of log-odds ratio of propensity score

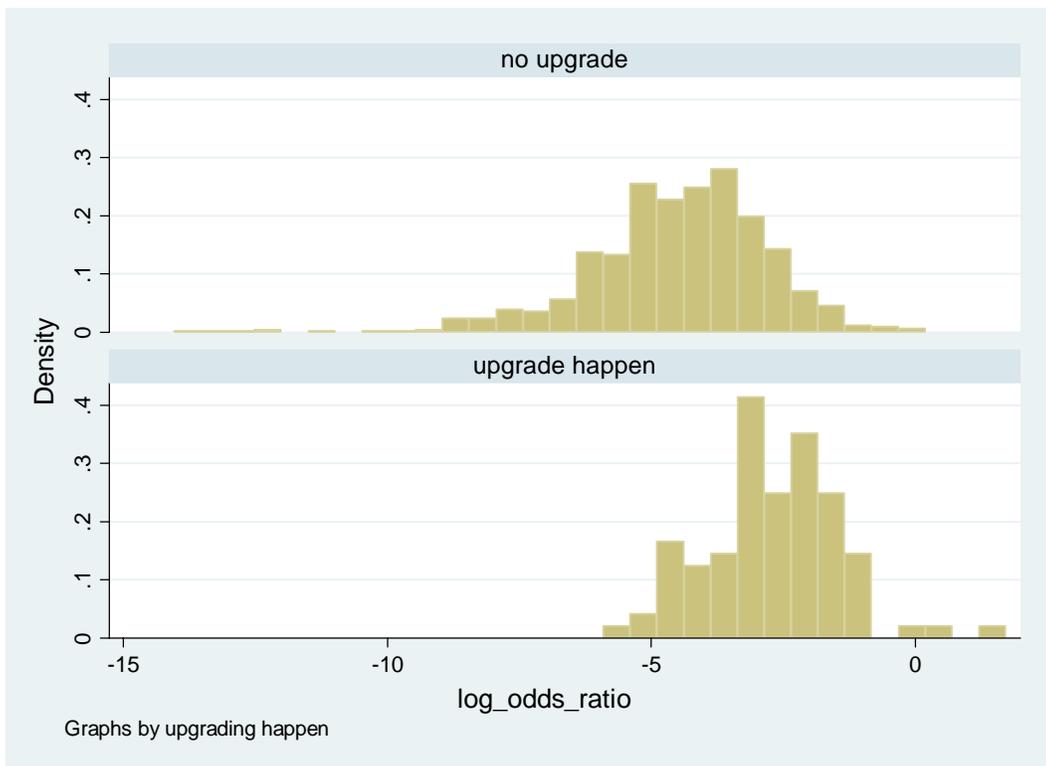


Table 1. Mean value of variables from public finance dataset

	treatment group:		control group:	
	city		county	
N	99		1537	
Year	1994	2004	1994	2004
Fiscal revenue (10,000 RMB)	6355	15720	2358	5683
Public employee	13112	17263	9010	11847
Public employee per 100 people	2.2	2.8	2.8	3.5
Fiscal revenue per public employee (10,000 RMB / person)	0.49	0.98	0.25	0.49
Industrial and business tax (10,000 RMB)	6522	9337	2294	3470
Industrial and business tax rate	5.11%	4.63%	7.83%	7.18%
Agricultural tax rate	3.20%	3.30%	2.80%	3.10%
Share of agricultural tax in total revenue	27.30%	18.50%	37.20%	26.50%
Provide any subsidies to the firms	18.68%	3.30%	25.76%	4.30%
Productive expenditure per capita (RMB)	22670	76863	18836	126753
Share of productive expenditure in total expenditure	8.36%	9.07%	7.97%	10.96%
Extra-budgetary funds (10,000 RMB)		3373		1049
Revenue from land (10,000 RMB)		1465		542

Note: All the output and revenue measures have been adjusted to 1993 constant prices using the yearly GDP deflator.

Table 2. Mean value of variables from population census dataset

N	treatment group: city		control group: county	
	95		1001	
year	1990	2000	1990	2000
population (1,000 people)				
total	629	658	412	421
> 6 yrs	555	615	360	393
> 15 yrs	455	502	292	320
Educational achievement				
% illiterate (among > 15yrs)	19.7%	8.1%	24.9%	11.2%
% illiterate (male, among > 15yrs)	10.6%	3.9%	15.4%	6.5%
% illiterate (female, among > 15yrs)	29.2%	12.4%	35.0%	16.1%
% illiterate (among > 6yrs)	18.2%	8.7%	22.9%	11.6%
% primary (among >6yrs)	44.8%	40.5%	44.4%	41.6%
% middle school (among >6yrs)	28.2%	38.3%	24.9%	35.7%
% high school (among >6yrs)	8.1%	10.5%	7.3%	9.3%
% above secondary(among >6yrs)	0.7%	2.0%	0.6%	1.8%
Health related				
# of children born alive per woman	2.15	1.42	2.25	1.49
# of surviving children per woman	2.02	1.40	2.07	1.45
Crude death rate (deaths per 1000)	6.16	6.18	6.47	6.27
disabled population	16.6k	21.9k	11.6k	14.5k
disability rate	3.7%	4.3%	4.0%	4.5%
Living condition				
space per person (square meters)		24.4		22.4
Urbanization				
Percentage of urban population	14.7%	19.8%	13.1%	16.1%
Immigration				
total immigrants (1,000 people)	12.4	57.1	6.5	23.1
immigrants from other province	4.7	18.4	2.3	4.7
immigrants / total population	2.49%	9.24%	2.25%	6.89%
Total employment rate (among >15yrs)	78.5%	79.8%	78.9%	82.4%
by occupation:				
technician	3.5%	3.4%	3.5%	3.4%
government agencies	1.1%	1.0%	1.0%	0.9%
office workers	1.1%	1.7%	1.1%	1.4%
sales and service	4.2%	6.1%	2.9%	4.5%
agriculture	56.3%	46.5%	63.1%	55.9%
production	12.3%	12.0%	7.3%	7.3%
Employment size by sector (1,000 people)				
agriculture	275	237	197	183
mining and quarrying	5.1	3.7	2.3	1.8
manufacturing	39.7	50.9	13	15.5
electric, gas, water supply	1.25	1.87	0.57	0.92
construction	5.5	11.3	1.98	4.3
geology and water	0.63	0.37	0.36	0.21
transportation and storage	5.8	8.8	2.8	4.3
commerce	12.7	21.3	6.2	10
banking and insurance	0.95	1.48	0.57	0.78
real estate	0.32	0.29	0.14	0.12
social service	1.70	5.70	0.86	2.50
health, sports	2.10	3.10	1.30	1.78
education and culture	6.69	7.82	4.40	4.84
research	0.18	0.18	0.09	0.10
government	5.19	6.89	3.32	4.20

Table 3. Difference-in-differences estimation results

	Fiscal revenue (1)	Public employee (2)	Fiscal revenue per public employee (3)	Productive expenditure share (4)	Agricultural tax share (5)
Upgrade	6023*** (909)	3600*** (615)	0.406*** (0.077)	1.487** (0.734)	-0.068*** (0.014)
Upgrade *Post	761* (390)	1896*** (457)	-0.073 (0.058)	-2.862*** (0.777)	-0.047*** (0.016)
Year dummies	Yes	Yes	Yes	Yes	Yes
R-squared	0.14	0.06	0.11	0.07	0.17
Obs	20215	20195	20190	11412	19985

Standard errors clustered at the prefecture level are in the parenthesis. Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*.

Table 4. Difference-in-differences estimation results after decomposition

	Fiscal revenue (1)	Public employee (2)	Fiscal revenue per public employee (3)	Productive expenditure share (4)	Agricultural tax share (5)
Upgrade	5983*** (908)	3584*** (615)	0.404*** (0.077)	1.508** (0.739)	-0.068*** (0.014)
Upgrade *Post0	-1688*** (515)	944*** (313)	-0.173*** (0.067)	-1.643*** (0.596)	-0.049*** (0.015)
Upgrade *Post1	-1381*** (488)	805** (404)	-0.160** (0.067)	-2.663*** (0.684)	-0.072*** (0.016)
Upgrade *Post2	-1352** (654)	1537*** (419)	-0.178*** (0.063)	-2.228*** (0.815)	-0.069*** (0.019)
Upgrade *Post3	-385 (440)	1968*** (478)	-0.151** (0.059)	-2.358*** (0.805)	-0.061*** (0.019)
Upgrade *Post4	331 (432)	2015*** (470)	-0.112** (0.048)	-2.635*** (0.860)	-0.052*** (0.017)
Upgrade *Post5	688 (831)	1977*** (492)	-0.073 (0.064)	-3.156*** (0.937)	-0.028* (0.017)
Upgrade *Post6	1704* (941)	1924*** (525)	-0.027 (0.061)	-3.160*** (0.918)	-0.036** (0.015)
Upgrade *Post7	2321** (1055)	2164*** (587)	0.029 (0.069)	-3.461*** (1.006)	-0.028* (0.017)
Upgrade *Post8	2673** (1115)	1954*** (614)	0.065 (0.080)	-3.163*** (1.029)	-0.044** (0.020)
Upgrade *Post9	4060*** (1503)	3422*** (928)	0.059 (0.078)	-3.950*** (1.075)	-0.046* (0.027)
Year dummies	Yes	Yes	Yes	Yes	Yes
R-squared	0.15	0.06	0.11	0.07	0.17
Obs	20215	20195	20190	11412	19985

Table 5. Propensity score matching results

	Coefficient	t-ratio
Floor space per person in 2000	0.296	0.37
Extra-budgetary funds (1999-2004)	970	6.0
Revenue from land (2000-2004)	333	2.4

Table 6. Difference-in-differences propensity score matching estimation

	difference-in-differences		difference-in-differences propensity score matching	
	coefficient	t-ratio	coefficient	t-ratio
<i>Public goods provision and outcomes</i>				
Illiterate ratio (above 15yrs)	0.00525	-0.81	0.0155	-2.71***
Ratio of attending secondary and above education (among > 6yrs)	-0.00086	-0.58	-0.00026	-0.14
Crude death rate	0.01	-0.11	0.3	-0.24
Log(disabled population)	0.020	-0.66	0.039	-1.43
Disability rate	-0.00102	0.86	0.00037	-0.33
<i>Urbanization</i>				
Urban population ratio	0.0048	1.15	0.0104	1.76*
<i>Immigration</i>				
Log(total immigrants)	0.111	1.79*	0.127	2.01**
Log(immigrants from other province)	0.223	2.45**	0.195	2.09**
Ratio of immigrants in population	0.00579	1.34	0.00477	0.79
<i>Employment</i>				
Percentage employed	0.0158	1.06	-0.00253	0.18
Percentage of people working in office	0.00074	1.36	0.00079	1.32
Percentage of people working in sales or service	0.00011	0.08	0.00054	0.33
Percentage of people working as farmers	-0.00096	0.11	-0.0139	1.51
Employment size by sector (1,000)				
Log(employment in agriculture)	-0.029	1.31	-0.044	1.55
Log(employment in mining)	-0.22	1.64*	-0.11	0.99
Log(employment in manufacturing)	0.069	1.26	0.066	1.24
Log(employment in electricity and water supply)	-0.063	-1.06	-0.070	-1.27
Log(employment in construction)	-0.026	-0.30	0.025	0.29
Log(employment in geology)	0.14	1.19	0.056	0.45
Log(employment in transportation and storage)	-0.014	-0.30	0.026	0.60
Log(employment in commerce)	0.006	0.17	0.018	0.48
Log(employment in banking and insurance)	0.034	1.4	0.087	2.8***
Log(employment in real estate)	0.21	2.1**	0.089	1.05
Log(employment in social service)	0.036	0.68	0.055	1.14
Log(employment in health and sports)	0.071	2.5***	0.102	4.1***
Log(employment in education and culture)	0.027	1.0	0.057	2.83***
Log(employment in research)	-0.015	-0.13	-0.077	-0.67
Log(employment in government)	0.067	2.14**	0.092	3.02***

Note: A positive t-ratio means cities are doing better than counties. A negative sign means cities are doing worse. For “bad” outcomes (illiterate ratio, death rate, disabled population, disability rate, employment in agriculture and mining), a negative t-ratio means a smaller reduction or a bigger increase in cities relative to counties. Significance levels of 10%, 5% and 1% are represented by \*, \*\* and \*\*\*.

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