

# Discretionary Charges as Firm Output Distortions: Evidence from China\*

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January 28, 2015

## Abstract

This paper studies discretionary charges, which I define to be fees and levies that are collected at the discretion of local officials, as firm output distortions. I document that there is an inverted-U relationship between size of firms and discretionary charges paid by firms in China. I build a model of heterogeneous firms with idiosyncratic endogenous output distortions. The model explains why discretionary charges fall most heavily on medium-sized firms and, consequently, reduce the number of medium-sized firms. Using the 2002 Chinese Income Tax Reform as a natural experiment, I find that a 1 percentage-point reduction in local tax revenue increased discretionary charges on firms by approximately 2 percent on average, with medium-sized firms experiencing the largest increase of 4.7 percent. In addition, a 1 percentage-point reduction in local tax revenue resulted in a 1.2 percent decline in the number of medium-sized firms as a share of total firms. This decline was most likely driven by slower growth rates of existing small firms. These results suggest that low tax revenues and lack of legal protection on firms may help explain why there are relatively fewer medium-sized firms and why there is larger firm productivity dispersion in low-income countries.

**Keywords:** Discretionary Charges, Distortions

**JEL classification:** H32, H71, L11, O43

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\*I am indebted to Nancy Qian, Christopher Udry, David Atkin, and Eric Weese for their guidance and support. I am grateful to Nicholas Bloom, Dean Karlan, Dan Keniston, Robert Jensen, Naomi Lamoreaux, Xiang Ma, Kota Mori, Mark Rosenzweig, Christopher Woodruff, and Xiaoxue Zhao, as well as other attendees at the Yale Development Prospectus Workshop and Seminar for their helpful comments. Special thanks to the Universities Service Centre for China Studies at the Chinese University of Hong Kong for providing me with relevant data. All errors are my own.

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# 1 Introduction

A recent literature has attributed the large income differences between rich and poor countries to misallocation of resources across firms. For instance, [Hsieh and Klenow \(2009\)](#) found that misallocation of resources contributed to greater productivity dispersions in India and China than in the United States. One reason why resource misallocation occurs is that firms are faced with different capital and output distortions.<sup>1</sup> Although it has been quite successful to account for a significant part of differences in total factor productivity (TFP) across countries, this explanation is less satisfactory in addressing the causes of these distortions. Meanwhile, another line of research finds that developing countries tend to have a large number of small firms and fewer medium-sized firms than developed countries.<sup>2</sup> A natural question to ask then is what has caused these distortions, whether it has impacted the firm size distribution, and what is the magnitude of efficiency loss due to the cause.

This paper studies the role of discretionary charges, which I define to be fees and levies that are collected at the discretion of local officials, in creating firm output distortions and affecting firm size distribution. The goal of this paper is to i) provide a plausible explanation for why output distortions may arise and differ by firm size, and ii) present empirical evidence that these distortions may have contributed to a large number of small firms and fewer medium-sized firms and, consequently, a lower aggregate efficiency in low-income countries.

“Discretionary charges,” which I use to refer to fees and levies that are collected at the discretion of local officials, are prevalent in low-income countries. Unlike taxes, there are usually no explicit rules for these fees and levies imposed by officials, and the legality of demanding such payments can be questionable. In some cases, these charges add to local government revenue and, thus, improve local public good provision. In a recent study by [Olken and Singhal \(2011\)](#), the authors showed evidence that informal taxation is widespread and can form a substantial share of local revenue in developing countries. In other cases, however, these charges may be outright extortions that end up in local officials’ private pockets. [Svensson \(2003\)](#) showed that 81 percent of a sample of Uganda firms must pay bribes to continue operation. [Olken and Barron \(2009\)](#) found that illegal payment to officials consists of a significant proportion in the total cost for truck drivers in Indonesia. In addition, the boundary between the legal and the illegal part of discretionary charges is often indefinite. Although discretionary charges can be collected for the purpose of public spending and officially

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<sup>1</sup>[Restuccia and Rogerson \(2008\)](#) and [Hsieh and Klenow \(2009\)](#) showed that misallocation of resources within narrowly defined industries could account for a large efficiency loss in terms of total factor productivity (TFP). [Guner, Ventura and Xu \(2008\)](#), [Alfaro, Charlton and Kanczuk \(2009\)](#), [Banerjee and Moll \(2010\)](#), [Collard-Wexler, Asker and Loecker \(2011\)](#) and [Bartelsman, Haltiwanger and Scarpetta \(2013\)](#) also contributed to this literature.

<sup>2</sup>[Tybout \(2000\)](#) found that the employment shares of mid-sized firms in developing countries are lower compared with those in developed countries. [Hsieh and Olken \(2014\)](#) found that there are a large number of small firms and fewer mid-sized firms and fewer large firms in India, Indonesia, and Mexico.

considered part of the local budget, the revenue may still be used *de facto* for officials' own benefit.<sup>3</sup>

In China, discretionary charges are widespread and contribute significantly to local government revenue.<sup>4</sup> Using non-tax revenue to proxy for discretionary charges at the county level, I find that on average, between 1998 and 2001, non-tax revenue contributed to 28 percent of county governments' revenue. Data from the *World Bank Enterprise Surveys* (China, 2004) reveal the prevalence of discretionary charges on Chinese firms. Of the 12,400 firms surveyed in 123 cities in 2004, 40 percent reported being charged more than one type of administrative expense, with some firms reporting charges for up to 64 different types of administrative expenses. On average, these expenses amounted for 7 percent of the firms' after-tax profits. Using data from *Chinese Private Enterprise Surveys* (1994-2001), which covered a random sample of private firms, I find that 58 percent of the sampled firms were charged levies, which is a type of discretionary charges that are collected by the local government. The average value of these levies amounted to 11 percent of the firms' after-tax profits.

I document four stylized facts in the Chinese data by using government levies to proxy for firm-level discretionary charges. First, I find an inverted-U relationship between firm size and levy rate.<sup>5</sup> This relationship holds robust to different measure of firm size and levy rate; similar inverted-U curves also exist across years and across regions. Second, smaller firms are on average less likely to be levied but, conditional on being levied, pay higher rates than larger firms. Together, these two relationships—one decreasing and the other increasing—explained the inverted-U relationship. Third, regions with lower incomes have higher average levy rates. This fact is still robust after we control for firm size and firm industry. Fourth, conditional on being levied, the total value of levies increases with firm size.

Motivated by these stylized facts, I build a simple model incorporating interactions between heterogeneous firms and a local official. The key insight of this model is that medium-sized firms are large enough to be targeted for expropriation but not yet large enough to receive protection against expropriation. Thus, medium-sized firms are most vulnerable in a context where legal protection on firms are generally lacking and local officials seek revenue from firms. To formalize this idea, I model the interactions between firms and the local official in a one-period Stackelberg game, in which firms act first and the official acts second. In the first stage, firms choose inputs to maximize profits. The revenue of the firm, however, will affect the amount of discretionary charges to be collected by the official in the second stage. It creates a distortion for

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<sup>3</sup>Prudhomme (1992) notes that most of the expenditures in Zaire consist of wages, allowances, and bonuses paid out to semi-local government employees. Henderson and Kuncoro (2004) show that the extent of local red tapes depends on local fiscal situations. These red tapes generate direct revenues as well as indirect revenues through bribes.

<sup>4</sup>One notable example is that a franchise of McDonald's in Beijing was charged 31 different fees in 1997. Most of these fees were unauthorized by the central government, but were not considered corruption. Local agencies simply were being forced to raise their own revenues in the face of government funding cuts (*Pittsburgh Post-Gazette*, 1997).

<sup>5</sup>These inverted-U curves are observed across all years (1993-2005) in China. In Section 6 of this paper, I show similar inverted-U relationships exist in many other developing countries as well.

firm production. In the second stage, the official visits firms and imposes discretionary charges to maximize his or her utility from revenue. The revenue is a sum of an exogenously endowed revenue and the revenue from discretionary charges. I assume that there is a fixed cost for the local official to visit a firm regardless of firm size. I also assume that there is a probability for the official to receive a punishment from a central auditing agency once he demands these charges.

This model provides implications that are consistent with the stylized facts in Figure 1. It also generates three testable hypotheses. First, when there is a reduction in the endowed revenue for the official, there will be an increase in revenue from discretionary charges. Second, when there is a reduction in endowed revenue for the official, firms will experience differential increases in discretionary charges depending on firm size. Firms with size in the medium range may experience the largest increase in discretionary charges. Third, when there is a reduction in endowed revenue for the official, we may observe a decline in the number of medium-sized firms and an increase in the number of small firms.

I empirically test these three hypotheses by exploiting variation in local governments' demand for revenue from discretionary charges due to the 2002 Chinese Income Tax Reform. This reform has changed the sharing-rule in income taxes between the local and the central government after 2002 and cut local government revenues from income taxes by half since then. Although the new policy was implemented nationwide, the reform had different impacts in different regions, depending on the reliance of each region's revenue on income taxes prior to the reform. I use a difference-in-differences estimator to compare regions with initially high and low income tax revenue shares across pre-reform and post-reform periods. The county government finance statistics are from the *Fiscal Yearbook of Chinese Prefectures and Counties* (1997-2007) that include government revenues and expenditures of counties in each fiscal year. The firm-level data come from the *Chinese Private Enterprise Surveys* (1999-2005) that cover random samples of Chinese private firms biennially. I merge these two data sets to create a unique county-level panel data set. I then use this data set and the 2002 Chinese Income Tax Reform as a natural experiment to test these hypotheses.

Consistent with my first hypothesis, I find that a 1 percentage-point reduction in local tax revenue increased discretionary charges on firms by approximately 2 percent on average. Consistent with my second hypothesis, I find that medium-sized firms are affected disproportionately, experiencing an increase in discretionary charges of 4.7 percent compared with other firms. Consistent with the second hypothesis, I find that a 1 percentage-point reduction in local tax revenue resulted in a 1.2 percent decrease in the share of medium-size firms. My results suggest that this decline was most likely driven by slower growth rates of existing small firms, rather than by firm entry and exit.

This paper makes several contributions to the literature. It provides a plausible explanation for heterogeneous firm output distortions that may impact firm size distribution and reduce aggregate output. It is also

closely related to the recent studies addressing the effects of taxes and government regulations on firms in developing countries (e.g., McKenzie and Sakho (2010), Bruhn (2011), De Mel, McKenzie and Woodruff (2013)), and research investigating the effects of corruption on firms (e.g., Svensson (2003), Fisman and Svensson (2007)). To my knowledge, however, this paper is the first study to (i) document that there is an inverted-U relationship between firm size and discretionary charges paid by firms and (ii) investigate the effect of discretionary charges on firms. My model highlights how discretionary charges can impact firm production decisions and affect firms differentially. As such, it furthers our understanding on the causes of heterogeneous firm output distortions and consequences of these distortions. While the empirical estimates are specific to the context of my study, the key insight is generally applicable other developing contexts: medium-sized firms are large enough to be expropriated but not yet large enough to receive protection against expropriation. This paper also contributes to the fiscal decentralization literature (e.g., Bardhan (2002), Treisman (2006), Bardhan and Mookherjee (2006)) by showing that tax-sharing rules between the central and the local government have heterogeneous effects on firms through affecting local governments' incentives to collect discretionary charges. It makes policy implications that we need to take these heterogeneous effects into consideration when we are designing tax-sharing rules and inter-governmental transfers among different levels of government in developing countries. This paper suggests that low tax revenue, especially low local tax revenue, and lack of legal protection on firms may help explain why there are relatively fewer medium-sized firms (e.g., Ayyagari, Demirgüç-Kunt and Maksimovic (2011); Hsieh and Klenow (2014)) and why there is more firm productivity dispersion (e.g., Hsieh and Klenow (2009)) in low-income countries.

The rest of the paper proceeds as follows. Section 2 presents a simple model from which testable hypotheses are derived. Section 3 introduces a natural experiment, the 2002 Chinese Income Tax Reform, to empirically test the hypotheses. Section 4 describes the data. Section 5 presents the empirical results. Section 6 discusses the relevance of discretionary charges to economic development. Section 7 concludes the paper.

## **2 A Simple Model**

This section presents a simple model to explain the stylized facts in Figure 1 and guide our empirical analysis. In this model, firm output distortions are endogeneously determined by interactions of firms and a local official. To make it tractable, I model these interactions in a one-period Stackelberg game. There are two players in the model: a continuum of heterogeneous firms and a local official. The timing of the game is as follows:

1. Firms choose inputs to maximize profits.

2. Local official collects discretionary charges to maximize utility.

In the model, the local official can not make commitment to his or her actions before firms act. This model produces results that are consistent with the four stylized facts presented in Section 1 and generates several testable implications.

## 2.1 Setup

### Firms

There is a continuum of firms, each producing a different good,  $\omega$ , using labor. The economy is endowed with total amount of labor,  $L$ . Firm production follows a simple technology:

$$y_\omega = A_\omega l_\omega, \quad (1)$$

where  $A_\omega$  is firm-specific labor productivity, and  $l_\omega$  is unit of labor hired by the firm. I assume that there is a downward-sloping demand for each product. For simplicity, I assume the demand is  $y_\omega = Y p_\omega^{-\sigma}$ .<sup>6</sup> There are two costs firms are faced with: costs from hiring labor and costs from paying discretionary charges. I assume that labor market is perfectly competitive with labor wage equals  $w$ . Each firm expects the official to impose an amount of discretionary charges,  $e_\omega$ , which is a function of firm revenue. Each firm chooses optimal amount of labor to maximize profits:

$$\pi_\omega = \max_{l_\omega} p_\omega y_\omega - w l_\omega - e(p_\omega y_\omega). \quad (2)$$

### Local Official

There is one local official, who visits firms and collect discretionary charges. The official generates utility from total revenue, which is a combination of an exogeneous endowed revenue,  $T$ , and the revenue from discretionary charges,  $E$ . I assume the utility function to be concave. Collecting discretionary charges involves two costs for the official. First, there is a fixed cost,  $F$ , to visit each firm, regardless of the firm size. Second, there is a convex cost when imposing charges on each firm. On the one hand, this cost decreases with firm size so that larger firms have higher amount of discretionary charges. On the other hand, it does not decrease too fast so that larger firms still pay lower fractions of their revenue.

One plausible way to model this convex cost is as follows. I assume that there is a probability,  $q$ , for

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<sup>6</sup>This assumption is consistent with Melitz (2003) and Hsieh and Klenow (2009), where  $Y$  is the aggregate output and  $\sigma$  is the constant elasticity of substitution. The aggregate output,  $Y$ , can be expressed as  $Y = \left[ \int_{\omega \in \Omega} y_\omega^{\frac{\sigma-1}{\sigma}} d\omega \right]^{\frac{\sigma}{\sigma-1}}$ .

the official who imposed these charges to be punished by a central auditing agency. The punishment is proportional to the amount of charges the official has collected. The official chooses an amount of charges,  $\{e_\omega\}$ , on each firm to maximize utility from total revenue: ( $I$  is an indicator that equals 1 if  $e$  is positive and 0 if otherwise.)

$$W = \max_{\{e_\omega\}} u(T + E) - c(E) \quad (3)$$

$$s.t. E = \int_{\omega \in \Omega} (e_\omega - FI_\omega) d\omega; \quad (4)$$

$$c(E) = \int_{\omega \in \Omega} q_\omega c e_\omega d\omega. \quad (5)$$

To model the probability of the official to be punished,  $q$ , I decompose  $q$  into two parts: the probability of the firm being audited,  $q_1$ , and the probability of the official to be exposed once the firm is audited,  $q_2$ . And  $q$  equals the product of  $q_1$  and  $q_2$ . First, I assume that the probability of the firm being audited,  $q_1$ , is increasing in firm revenue  $py$ . This assumption is based on the fact that larger firms tend to receive more protection against charges from local officials. Second, I assume that the probability of the official to be exposed once audited,  $q_2$ , increases in the fraction of firm revenue taken as discretionary charges,  $e/py$ . This assumption is to capture the idea that the boundary between the legal part and illegal part of discretionary charges could be vague. It requires a cross comparison between the fraction taken as discretionary charges and the fraction of other firm expenses, such as taxes, to find the illegal part of these charges. And the court must be able to distinguish between honest errors in judgement and outright corruption in order to circumscribe corruption among government officials (Stiglitz (2009)). For simplicity, I assume  $q_1 = (py)^\beta$  and  $q_2 = (\frac{e}{py})^\gamma$ . I assume  $\gamma$  is greater than  $\beta$ : it captures the idea that officials who impose the same amount of discretionary charges on smaller-scale firms are more likely exposed and to receive punishment.

## 2.2 Equilibrium

To solve the model, we start from the second stage. Once having observed firms' output, the local official chooses which firms to visit and the amount of discretionary charges to be collected from each firm. The marginal utility from collecting the optimal amount of charges will be equal to the marginal cost:

$$u'(T + E) = (\gamma + 1) c e^\gamma (py)^{\beta - \gamma}, \quad (6)$$

where  $E$  is the value of total discretionary charges and  $py$  is the observed firm revenue. Thus, the optimal

amount of discretionary charges is:

$$e^* = \left[ \frac{u'(T+E)}{c(\gamma+1)} \right]^{\frac{1}{\gamma}} (py)^{1-\frac{\beta}{\gamma}}. \quad (7)$$

Since the official faces a fixed cost,  $F$ , to visit a firm. To make it worthwhile to visit the firm, the benefit should outweigh the cost.<sup>7</sup> Thus, there is a cutoff revenue above which the official will visit the firm:

$$py^{cutoff} = \left[ \frac{\gamma+1}{\gamma} F \right]^{\frac{\gamma}{\gamma-\beta}} \left[ \frac{c(\gamma+1)}{u'(T+E)} \right]^{\frac{1}{\gamma-\beta}}. \quad (8)$$

In the first stage, the firms take the official's optimal responses into consideration. Each firm chooses the optimal amount of capital and labor inputs to maximize firm profits:

$$\pi_\omega = \max_{l_\omega} p_\omega y_\omega - w l_\omega - e(p_\omega y_\omega). \quad (9)$$

$$s.t. \ e = \begin{cases} e^* & \text{if } py > py^{cutoff} \\ 0 & \text{if } py \leq py^{cutoff} \end{cases}. \quad (10)$$

Given initial condition on  $(E, L, Y)$ , we can solve for the  $(e_\omega^*, l_\omega^*, y_\omega^*)$ . In equilibrium, the following conditions should hold:<sup>8</sup>

$$\int_{\omega \in \Omega} e_\omega^* d\omega = E; \quad (11)$$

$$\int_{\omega \in \Omega} l_\omega^* d\omega = L; \quad (12)$$

$$\left[ \int_{\omega \in \Omega} y_\omega^{*\frac{\sigma-1}{\sigma}} d\omega \right]^{\frac{\sigma}{\sigma-1}} = Y. \quad (13)$$

In equilibrium, firms with low productivity ( $A \leq \underline{A}$ ) will produce an optimal amount of output that is below the cutoff size.<sup>9</sup> Since their output is below the cutoff, there will be no discretionary charges imposed on these firms. Firms with higher productivity ( $\underline{A} < A \leq \bar{A}$ ) will produce output at the cutoff size, in order to evade discretionary charges. Firms with even higher productivity ( $A > \bar{A}$ ) will produce optimal output and

<sup>7</sup>In other words:

$$u'(T+E^*)(e^* - F) > (py)^\beta \left( \frac{e^*}{py} \right)^\gamma c e^*.$$

<sup>8</sup>It is hard to find a closed-form solution for the equilibrium. Below I simulate the equilibrium by assuming parameter values. The simulation results suggest that there exists a unique equilibrium. To formally prove the existence and uniqueness of the equilibrium, however, is beyond the purpose of the paper.

<sup>9</sup>The cutoff firm output is:

$$\bar{y} = Y^{*\frac{1}{\sigma-1}} \left[ \frac{\gamma+1}{\gamma} F \right]^{\frac{\gamma\sigma}{(\gamma-\beta)(\sigma-1)}} \left[ \frac{c(\gamma+1)}{u'(T+E^*)} \right]^{\frac{\sigma}{(\gamma-\beta)(\sigma-1)}},$$

where  $Y^*$  is the equilibrium aggregate output and  $E^*$  is the equilibrium total revenue from discretionary charges.



pay discretionary charges.<sup>10</sup>

Figure 2 exemplifies how, in equilibrium, the allocation of firm-level discretionary charges varies according to firm productivity.<sup>11</sup> In equilibrium, the local official only visit firms with productivity above 0.85. These firms are large enough for the official to collect enough discretionary charges to cover the fixed cost. Meanwhile, the amount increases with firm productivity in equilibrium. The intuition is that, for the same amount of charges, it is less likely for the official to be punished if the firm has a larger revenue (i.e.  $\gamma > \beta$ ). Thus, firms with larger productivity—they produce more output and have larger revenue—will see larger amounts of discretionary charges.

In Figure 3, the solid line demonstrates the relationship between firm productivity and firm revenue in equilibrium. When productivity is below 0.62, the firm will choose an optimal amount of labor to produce and there are no discretionary charges. Firm revenue is below the cutoff level,  $py^{cutoff}$ , which equals 0.107. When productivity is between 0.62 and 0.85, the firm will choose to produce  $py^{cutoff}$ . It is optimal strategy for the firm to under produce and evade charges in this case. When productivity is above 0.85, the firm will choose to produce above  $py^{cutoff}$  and pay discretionary charges. In this case, the cost of staying small and evade charges will be overly high.

The dashed line in Figure 3 shows the relationship between firm productivity and firm revenue in equilibrium when the official does not collect discretionary charges. This scenario happens when i) the endowed revenue,  $T$ , is very high, or ii) the fixed cost of visiting a firm,  $F$ , is very high, or iii) the punishment on the local official,  $c$ , is very high. Thus, discretionary charges will be prevalent when the endowed revenue is low and the cost of collecting charges is low.

When we compare the solid line with the dashed line, we observe discrepancies between them. The imposition of discretionary charges not only directly impacted firms' optimal output but also indirectly affected firms' production through altering input prices. Firms with productivity between 0.62 and 0.85 choose to stay small to evade charges. Discretionary charges lowered factor demand and, thus, reduce input prices. At the aggregate level, distortions from discretionary charges lower the aggregate output significantly. Compared with the case without distortions, the aggregate output with distortions is 7 percent lower.

<sup>10</sup> $\bar{A}$  is defined as the firm productivity at which the firm's optimal output without discretionary charges is the cutoff output,  $py^{cutoff}$ , in equilibrium;  $\bar{A}$  is defined as the firm productivity at which the firm's optimal output with discretionary charges is the cutoff output,  $py^{cutoff}$ , in equilibrium.

<sup>11</sup>I assume that the official's utility following a simple functional form,  $u(x) = x^\theta$ . I assume that the productivity of a firm is randomly drawn from a uniform distribution,  $A \sim U(0, 1)$ . For illustration purpose, I set  $\sigma$  to be 3,  $\theta$  to be 0.1,  $\beta$  to be 0.5,  $\gamma$  to be 1,  $F$  to be 0.005, and  $c$  to be 0.1. I set the endowed revenue,  $T$ , to be 20.94, and the endowed labor,  $L$ , to be 0.83.

## 2.3 Implications

This simple model generates a few testable implications. In Figure 4, I show the relationship between firm revenue,  $py$ , and firm-level discretionary charges in equilibrium. The solid line describes a increasing relationship between firm revenue and amount of charges placed on firms. It is consistent with the stylized fact that levies increase with firm size shown in Section 1. The dashed line shows a non-monotonic relationship between firm revenue and the fraction of firm revenue taken as discretionary charges: it increases when firm size is small and decreases when firm size is large. The intuition is that the local official will only visit firms with revenue above the cutoff,  $py^{cutoff}$ ; Since the probability for the official to receive punishment increases with the proportion of firm revenue taken as discretionary charges and larger firms are more likely to receive audit, the local official will impose a smaller fraction on a larger firm. It is consistent with the stylized fact that there is an inverted-U relationship between the firm size and levy rate shown in Section 1.

In Figure 5, I show the effect of a reduction in endowed revenue,  $T$ , on the allocation of discretionary charges on firms of different sizes by the local official. When there is a reduction in  $T$ , the local official will have higher incentives to collect discretionary charges. As such, he or she will visit smaller firms and impose larger amounts of charges. This is also consistent with the stylized fact shown in Section 1: firms in poorer regions are faced with greater discretionary charges compared with their counterparts in richer regions.

**Hypothesis 1:** A reduction in  $T$  will lead to a larger amount of aggregate discretionary charges,  $E$ .

**Hypothesis 2:** A reduction in  $T$  will have differential impacts on firms of different sizes. Firms of small size will not likely be affected. Discretionary charges on medium-sized and large-sized firms will likely increase. In particular, firms of medium size will likely experience disproportional increases in discretionary charges.

In Figure 6, I illustrate the effect of a reduction in endowed revenue,  $T$ , on the firm size distribution. The horizontal axis denotes firm revenue; the vertical axis describes the cumulative distribution of firm revenue in equilibrium. The solid line shows the cumulative distribution function of firm revenue when  $T$  is high ( $T_{high} = 20.94$ ); the dashed line shows the cumulative distribution function of firm revenue when  $T$  is low ( $T_{low} = 1.14$ ). After a drop in  $T$ , firms choose to produce lower outputs and, thus, the firm size distribution skews to the right. The intuition is that firms have higher incentives to produce lower outputs in responses to the increased discretionary charges. After the drop in  $T$ , however, the decline in the number of smallest firms reflect the fact that firms of lowest productivity produce more in responses to the lower input prices.

**Hypothesis 3:** A reduction in  $T$  will skew the firm size distribution to the right. And we may observe a decline in the number of medium-sized firms but an increase in the number of small firms and large firms.

In this simulated example, if we choose one third and two thirds of the largest firm revenue when there are no discretionary charges as the cutoffs (see the dashed line in Figure 3), we will observe a decline in the share of medium-sized firms and an increase in the share of small-sized firms and large-sized firms. The revenue cutoff points for small, medium, and large firms here are 0.029 and 0.114. After the reduction in  $T$ , the share of medium-sized firms drops from 54 percent to 11 percent, whereas the share of small-sized firms increases from 32 percent to 57 percent. The share of the large firms increase from 14 percent to 32 percent.

It is clear that distortions from discretionary charges lower the aggregate output compared with the case where distortions are absent. The model prediction on aggregate output after a reduction in  $T$ , however, is ambiguous. Although the specific example above gives a reduction of output by 1 percent, the direction in fact depends on the specific values of  $T_{high}$  and  $T_{low}$ . On the one hand, when  $T_{high}$  is large, some firms with large productivity choose to produce the cutoff revenue, which brings large efficiency loss. In this case, when  $T_{high}$  drops to  $T_{low}$ , we might see an efficiency gain and, thus, an increase in aggregate output. On the other hand, in an extreme case, when  $T_{high}$  is so large that local officials choose not to visit any firm, there is no efficiency loss. In this case, when  $T_{high}$  drops to  $T_{low}$ , we might see efficiency loss due to increases in distortions from discretionary charges and, thus, a decrease in aggregate output.

### 3 Empirical Strategy

This section introduces empirical strategies for testing the hypotheses in the previous section. I use government non-tax revenue at the county level and government levies at the firm level to proxy for discretionary charges. We study (i) how local tax revenue affects local official's incentive to collect discretionary charges (Hypothesis 1), (ii) how local tax revenue affects the change in discretionary charges on firms differentially depending on firm size (Hypothesis 2), and (iii) how discretionary charges affect firms' production decisions and therefore the firm size distribution (Hypothesis 3). The main empirical difficulty for this study is to address the endogeneity bias. To study (i) and (ii), the bias could arise from simultaneity problem between tax revenue and discretionary charges. For example, suppose we observe a negative relationship between local tax revenue and aggregate discretionary charges, the causal direction is unclear. On the one hand, tax revenue affects local governments' incentives to collect discretionary charges. On the other hand, discretionary charges affect firms' decisions, which, in turn, impact total local tax revenue. To study (iii), the bias could arise from omitted variables that are confounding with discretionary charges. For instance, suppose that we observe a negative relationship between discretionary charges and the number of medium-sized firms. It would be difficult to argue that discretionary charges cause less medium-sized firms, because both variables could be outcomes of a third factor such as low income. To address these concerns, I use an exogenous change

in the tax-sharing rule between the central and local governments to explain the changes in tax revenues of county governments. The change in tax-sharing rule induces an exogenous change in the demand of discretionary charges, which allows us to study the impact of discretionary charges on firm size distribution. The 2002 Chinese Income Tax Reform provides such a change.

### **3.1 The 2002 Chinese Income Tax Reform**

Before 2002, income taxes, including corporate and personal income taxes, were paid to different governments according to firms' or employers' affiliation. This affiliation system came to existence during the central-planning era, when firms—as part of their registration—were required to be affiliated with (*lishuyu*) a level of government. Different levels of government administered firms they were attached to and were held accountable for production of these firms. The affiliation also determined to which government a firm should pay income taxes to. In general, centrally affiliated remitted income taxes to the central government, and other firms remitted income taxes to different local governments.

Since the liberalization of the economy in the late 1970s, this affiliation system has been less functional and thus relaxed. State firms were still required to be affiliated with either the central or a local government. Private firms, however, had the right to opt out of the affiliation system. Firms affiliated with the central government are usually central government-owned or central government-controlled state enterprises. For example, 75 percent of centrally-affiliated manufacturing firms were state-owned firms and the state owned 44 percent equity of the remaining 25 percent firms in 2001. Firms affiliated with provincial, prefecture, county, are mostly either local government-owned or local government-controlled state firms. Firms affiliated with townships or districts are privately owned by an individual or a collective. Firms registered “others” as their affiliation are mostly private. For instance, 29 percent of manufacturing firms were affiliated with “others” in 2001. On average, the state owned only 2 percent of the total equity of these firms.

A greater number of firms chose to affiliate with local governments or not affiliated at all since 1990s. Meanwhile, the profits of these firms had been increasing at an unprecedented speed. In the late 1990s, the total profits of these locally affiliated and non-affiliated firms overtook those of centrally registered firms. And consequently, income taxes received by the central government started to decline relative to those collected by the local government. For example, the central-local ratio for income tax revenue was 0.65 in 1997 and it declined to 0.49 after 3 years in 2000. As a result, it thwarted the central government's capacity in public spending and redistribution. The financial pressure pushed the central government to change the sharing rule of income taxes.

Starting on January 1, 2002, the central government implemented a new tax-sharing rule to increase its tax

revenues. All income taxes, except for those from “a few centrally affiliated firms,” were to be shared between the central and local governments in China, regardless of firms’ affiliation.<sup>12</sup> In 2002, the central government would share 50 percent of all income taxes, and the local government would keep the remaining 50 percent. After 2003, the central government took 60 percent of all income taxes and the local government 40 percent.<sup>13</sup> This reform has dramatically increased the revenue from income taxes for the central government but reduced income tax revenue for the local government. The central-local ratio for income tax revenue increased from 0.50 in 2001 to 0.67 in 2003.

### 3.2 Identification

Although the sharing rule was implemented nationwide, the impact of this reform on county governments varied, depending on the share of income taxes in total government revenue prior to the reform. Figure 7 shows that the reform cut county income tax revenues roughly by half in 2002. The horizontal axis denotes the county income tax revenue as a fraction of county total revenue in 2001. It measures how dependent the county revenue was on income taxes right before the reform. The vertical axis describes the change of county income tax revenue from 2001 to 2002, as a fraction of county total revenue in 2001. It measures the impact of the reform on county income tax revenue. This figure shows that for counties whose income tax share of total revenue was greater in 2001, the percentage loss of revenue from income taxes was larger. On average, a 1 percentage-point higher in income tax share in total county revenue in 2001 predicted a 0.55 percentage-point greater reduction in income tax share in 2002. To give one example, Yanchuan county in Shanxi province had an income-tax share of 58 percent in 2001 and it lost 33 percent of total revenue in 2002. Consider another county, Renqiu in Hebei province, 10 percent of its total revenue came from income taxes in 2001 and it lost 5 percent of total revenue in 2002. Compared with Renqiu, Yanchuan experienced a larger negative impact on county tax revenue after the reform because of its higher income tax share before the reform. Thus, I use difference-in-differences estimators to study the effects of the reform, for counties with initially high and low income tax revenue shares, across pre- and post-reform periods.

My empirical exercise follows four steps. First, I study the effect of the reform on county government revenues. Since the reform cut the income tax revenue of local governments by roughly half, the greater income tax revenue a county had in 2001, the larger amount of income tax revenue the county should have lost after 2002. It also may or may not have affected other types of government revenue, e.g. other taxes and

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<sup>12</sup>These “few centrally affiliated firms” include China Railroads, China Post Group, Industrial and Commercial Bank of China, Agricultural Bank of China, Bank of China, China Construction Bank, China Development Bank, Agricultural Development Bank of China, Export-Import Bank of China, China National Petroleum Corporation, China Petroleum&Chemical Corporation, and China National Offshore Oil Corporation.

<sup>13</sup>The details of the 2002 Chinese Income Tax Reform are well documented in *Intergovernmental Fiscal Relationship in China*, which was edited by the Budgetary Office at Ministry of Finance and published by the China Financial and Economic Publishing House in 2002.

inter-governmental transfers. I use a difference-in-differences strategy to explore the effects of the reform on county revenues. I normalize both the dependent variable and the independent variables by the county's total revenue in 2001 in order to reduce the potential correlation between changes in unobservables and the income tax revenue in 2001.<sup>14</sup> In specification (1), the dependent variable  $y_{i,t}$  includes county income tax revenue, other tax revenue, non-tax revenue, as well as inter-governmental transfers to county  $i$  in year  $t$ . The main regressor  $ITS_{i,2001}$  is the share of income tax revenue in total revenue of county  $i$  in 2001. I interact  $ITS_{i,2001}$  with a  $Post_t$  dummy, which equals 0 in pre-reform period and 1 in post-reform period. Quantitatively,  $\beta$  estimates the percentage-point changes in county's revenues in county total revenue in 2001, when there is a 1 percentage-point change in county's income tax share in 2001. I control for county pre-trends as well as a set of pre-reform county characteristics interacted with the  $Post_t$  dummy to alleviate the concern that  $ITS_{i,2001}$  may be correlated with changes in other county unobservables. County fixed effects and year fixed effects are added to control for the county-invariant and time-invariant unobservables. The standard errors are clustered at prefecture level. I also replace the  $Post_t$  dummy with a set of year dummies to check whether the effects took place right after the reform.

$$y_{i,t} = \alpha + \beta ITS_{i,2001} \times Post_t + X_{i,t} \mu + \gamma_i + \delta_t + \varepsilon_{i,t}. \quad (1)$$

Second, I examine the effect of the reform on discretionary charges. At county level, I use non-tax revenue to proxy for discretionary charges; at firm level, I use government levies to proxy for discretionary charges.<sup>15</sup> I use difference-in-differences estimates to show the effect of the reform on both county non-tax revenues and firm levies. The county-level regression follows specification (1) above and the dependent variable is log county non-tax revenues. The firm-level regression follows specification (2): The dependent variable is log levies, and  $X_{i,j,t}$  includes controls such as year dummies interacted with sector dummies, as well as other firm variables.<sup>16</sup> Quantitatively,  $\beta$  estimates the percent change in discretionary charges when there is a 1 percentage-point change in county's income tax share in 2001. Similar to specification

<sup>14</sup>If neither county revenues nor county income tax revenue in 2001 is normalized by county total revenue in 2001, the error term will be correlated with county size. The changes in unobservables are likely correlated with the main regressor, county income tax revenue in 2001, which is closely linked to county size. This correlation will cause bias in our estimates. For example, suppose all counties' non-tax revenue grew at the same speed before the reform, the error term would be positive correlated with county income tax revenue, which would cause an upward bias. Meanwhile, we can not normalize both sides by county current total revenue, which is itself affected by county's income taxes.

<sup>15</sup>There are several concerns using non-tax revenue as a proxy for discretionary charges. First, besides levies, fines, and confiscations, county non-tax revenue also includes net transfers from state-owned enterprises, which are revenues from state assets minus the government subsidies. Later, I show the results are not driven by changes in firm subsidies. I also show that non-tax revenue closely comoves with aggregate firm levies at the provincial level: A simple regression of log provincial total levies on log provincial total non-tax, after controlling for year fixed effects and provincial fixed effects, gives an estimate of 0.89 with standard error 0.50. Second, some of the discretionary charges on firms are undocumented in county non-tax revenue, since they are either extra-budgetary revenue or unrecorded. In this case, we might underestimate the effect of the reform on discretionary charges. I address this issue by using logs to show the percent change in discretionary charges.

<sup>16</sup>Since some firm levies are 0, I use  $\log(\text{levies} + 1)$  throughout the paper.

(1), the regressions control for country and year fixed effects. I also control for county pre-trends and a set of pre-reform county characteristics interacted with the  $Post_t$  dummy. The standard errors are clustered at prefecture level. Hypothesis 1 suggests that we would expect a positive estimate,  $\hat{\beta}$ . Suppose that firm levies are proportional to the total amount of non-tax revenue, we would expect similar  $\hat{\beta}$  for the county-level regression and firm-level regression.

$$y_{i,j,t} = \alpha + \beta ITS_{i,2001} \times Post_t + X_{i,j,t} \mu + \gamma_i + \delta_t + \varepsilon_{i,j,t}. \quad (2)$$

Third, I investigate the effect of the reform on discretionary charges on medium-sized firms compared with other firms. To define small, medium, and large firms, I use the 33<sup>rd</sup> and 67<sup>th</sup> percentiles of firm log sales in each province in 2001 as the baseline for the cutoffs.<sup>17</sup> I also use different cutoffs to check the robustness of the results. To estimate the effect, I use specification (3): The dependent variable is log firm levies, and the main independent variable is a triple interaction of the  $ITS_{i,2001}$ , a medium-firm dummy, and a  $Post_t$  dummy. Control variables,  $X_{i,j,t}$ , include year dummies interacted with sector dummies, as well as other firm controls. Quantitatively,  $\beta_1$  estimates the percent change in discretionary charges on medium firms compared with other firms, when there is a 1 percentage-point change in county's income tax share in 2001. I control for county-medium fixed effects and year fixed effects. I also control for county pre-trends and a set of pre-reform county characteristics interacted with the  $Post_t$  dummy. The standard errors are clustered at prefecture level. Hypothesis 2 suggests that we would expect a positive estimate,  $\hat{\beta}_1$ . I also investigate the effect of the reform on discretionary charges on small, medium, and large firms by running separate difference-in-differences regressions to see the driving force of the estimate from the triple-difference specification. According to Hypothesis 2, we would expect to see that the positive estimate,  $\hat{\beta}_1$ , is driven predominantly by increases in discretionary charges on medium-sized firms after the reform.

$$y_{i,j,t} = \alpha + \beta_1 ITS_{i,2001} \times Medium_{i,j,t} \times Post_t + \beta_2 ITS_{i,2001} \times Post_t + \beta_3 Medium_{i,j,t} \times Post_t + X_{i,j,t} \mu + \gamma_{i,m} + \delta_t + \varepsilon_{i,j,t}. \quad (3)$$

Fourth, I study the effect of the reform on firm size distribution and explore the causal channels of the effect. I use the 33<sup>rd</sup> and 67<sup>th</sup> percentiles of firm log sales in each province in 2001 as the baseline for size cutoffs and then calculate the share of small, medium, and large firms in each county in each year. Using a

<sup>17</sup>I use pre-reform provincial firm size distributions to proxy for the firm size distribution for counties within this province. Ideally, I would use pre-reform county firm size distribution to proxy for firm size distribution for each county. Due to small number of observations in each county, I use pre-reform provincial firm size distributions to have better approximates. These cutoffs are adjusted in each province across years, according to the changes in the medians of firm sales in each province each year. More specifically,  $Cutoff_{i,t} = Cutoff_{i,2001} + Median_{i,t} - Median_{i,2001}$ , where  $i$  refers to province  $i$  and  $t$  refers to year  $t$ . Meanwhile, firm size is a choice variable, which may bias the estimates if it is used as an independent variable. For instance, firms that chose to remain medium after reform may have better political connections. Thus, I use the firm log sales two years ago and these cutoffs to jointly define the firm size.

difference-in-differences strategy similar to specification (1), I examine whether counties with higher income tax shares prior to the reform experienced larger declines in the number of medium-sized firms as a share of total firms. The dependent variables are the share of small, medium, and large firms of total number of firms. The main independent variable is an interaction of the  $ITS_{i,2001}$  and a  $Post_t$  dummy. I control for a set of pre-reform county characteristics interacted with the  $Post_t$  dummy to alleviate the concern that  $ITS_{i,2001}$  may be correlated with changes in other county unobservables. County fixed effects and year fixed effects are added to control for the county-invariant and time-invariant unobservables. The standard errors are clustered at prefecture level. Hypothesis 3 suggests that we would expect a negative estimate,  $\hat{\beta}$ , for medium-sized firms, since the share of medium-sized firms is most likely to decline. We may also observe positive estimates for small and large firms, since the share of small firms and the share of large firms are likely to increase.

In addition, I explore the driving forces behind the change in firm size distributions. I test four plausible explanations. First, the effect could have been driven by slower growth rates of existing small firms. Because of the increases in discretionary charges on medium-sized firms, the small firms may have chosen slower growth rates to avoid excessive discretionary charges from the local government. This test follows specification (3): The dependent variable is firm growth in sales, and the main independent variable is a triple interaction of the  $ITS_{i,2001}$ , a small-firm dummy, and a  $Post_t$  dummy. Second, the effect could have been driven by an increase in the central government revenue—which naturally implies more central government spending—and a reduction in county expenditure had contributed to the change in firm size distributions. I check whether the results hold robust if we remove the western provinces from our sample, since these provinces most likely benefited from increased spending of the central government according to the government document of this reform. I study the effect of the reform on different county government expenditures and examine whether the decline in these expenditures could have affected firm production and contributed to the change in firm size distributions. Third, effect could have been driven by firm entry and exit. For instance, medium-sized firms might have exited counties where impacts were larger or relocated to counties where impacts were smaller. I use the newly registered firms in my sample to proxy for firm entry. Unfortunately, my firm sample does not allow me to directly study firm exit decisions. I use data from *Annual Survey of Manufacturers* to study the change in total number of above-scaled firms. If firms were closed down in response to greater amount of discretionary charges, we may also observe declines in the number of above-scaled firms. Last, the results could have been driven by higher incentives of firms under-report their sales. I use firm employment to check whether we could observe a similar change in the distributions of firm employment. I follow specification (2) check the validity of these possible channels.



## 4 Data

The county revenue and expenditure statistics are from the *Fiscal Yearbook of Chinese Prefectures and Counties* (1997-2007) and cover all counties in China from 1997 to 2007. The data are collected and published by the Office of State Budget in Ministry of Finance of China. This data set includes statistical information on counties' final account of (1) general budgetary revenue and expenditure by item, (2) inter-governmental transfers by item, and (3) "general fund revenue and expenditure." Statistics on extra-budgetary revenue and expenditure were not available until 2007. These data are generally believed to be accurate for the following three reasons: First, the Ministry of Finance audits counties' fiscal accounting books every year, after which the final account of these statistics are calculated and published. Second, over-reporting of government revenue reduces the inter-governmental transfers the county receives in coming years. Third, under-reporting is not consistent with the career incentives of local officials. I also check the accuracy of these statistics by using aggregated micro-level data.<sup>18</sup> It was not until 2007 that the non-tax revenue was further broken down into more detailed items in these yearbooks. At the provincial level, more than half of the non-tax revenue consists of administrative charges, fees, fines, and confiscations. The majority of the other half is labeled as "earmarked income." The "earmarked income" category was initiated in 1985 to increase local government revenue to finance local public projects. It typically includes levies, pollution fees, water resource fees, educational surcharges, natural resource compensation fees, etc. The local officials have considerable discretionary power—in particular compared with taxes—over this so-called earmarked income.

The firm-level data come from the *Chinese Private Enterprise Surveys* (1997-2006) that cover random samples of Chinese private firms biennially. Although the surveys also tracked a number of firms over years, these statistics are not publicly available. The survey was designed and implemented by the State Administration for Industry and Commerce of China, jointly with the All-China Federation of Industry and Commerce. The major contents of these surveys include (1) firm size, status of development, organization, and operation; (2) management system and decision-making style; (3) social-economic background of enterprise owners; (4) social mobility and network of owners; (5) source and composition of employees and employee-employer relations; (6) self-assessment by owners, political and social participation; (7) income, expenditure and asset of owner. To my knowledge, this survey provides the best publicly available data for this study. The surveys used a stratified systematic sampling method in each wave, which I discuss in detail in the Appendix. Each wave covered a randomly selected sample of counties and firms. On the one hand, because of the random selection, we can study the changes in firm size distributions over years. On the other hand, since we can not follow the same firm over years, we are not able to examine the dynamics of individual firms. To study

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<sup>18</sup>I add up both value-added tax and income tax reported by the manufacturing firms in each county. These numbers are highly consistent over years with the government statistics.

the impact of the reform on firms, I merge the county-level revenue statistics and the firm-level data to create a unique county-level panel data set. In total, 264 counties are matched between pre-reform waves and post-reform waves (Figure 8).

This paper also uses several supplementary data sets. I use data from the *Annual Survey of Manufacturing Firms* (2000-2003) to study the effect of the reform on the number of above-scaled firms, which is a proxy for total number of firms in each county. I use data from the *World Bank Enterprise Surveys* (2002-2012) to show similar inverted-U relationships exist in other developing countries.

## 5 Results

### 5.1 Main Results

The exogenous variation in the empirical exercise comes from the differences in the county income tax shares before the reform in 2001. It may be a concern that the county income tax share is correlated with other county variables, which may affect the post-reform discretionary charges and firm performance. In Table 1 (Panel A and B), I show that counties with high income tax shares tend to have larger firms (in terms of sales and profits), higher non-tax share in total revenue, higher GDP per capita, greater number of firms, and more export-oriented industries. But, as evidenced by the descriptive statistics I present in Table 1 (Panel C), county income tax share in 2001 is not correlated with the growth rates of these county variables in the pre-reform period. As such, we expect county fixed effects will eliminate the initial differences in dependent variables between counties with high and low income tax shares in 2001. And it is unlikely that the results of the difference-in-differences estimators will be driven by different county pre-trends.

First, as shown in specification (1), I run difference-in-differences regressions to investigate the effect of the reform on various county revenues. The main regressor is county income tax share in total revenue in 2001,  $ITS_{i,2001}$ , interacted with a  $Post_t$  dummy, which equals 0 before 2001 and 1 after 2002. The dependent variables, which include various county revenues, are normalized by total revenue of counties in 2001.<sup>19</sup> The study period, 1998 to 2005, covers from 4 years before the reform to 4 years after the reform. To address concerns caused by time-invariant and county-invariant unobservables, I control for both county and year fixed effects. I also control for linear pre-reform trends as well as a set of county variables interacted with the  $Post_t$  dummy. To address the concern that counties with higher income tax shares were richer and might grow faster after 2002 for reasons other than the reform, I control for county GDP per capita, log county average firm output, and log county total firm numbers in 2001. I control for county export intensity

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<sup>19</sup>Normalizing dependent variables by current year county total revenue will not be valid, since current year county total revenue include income taxes and will be mechanically affected by the reform.

in 2001 to address the concern that the effect of the reform on discretionary charges might be driven by China's accession to the World Trade Organization in 2001, and counties with higher income tax shares in 2001 might have experienced different impacts from the trade liberalization. All regressions control for these county variables to reduce the bias caused by unobserved confounders.

In Table 2, column (1) shows that a 1 percentage-point increase in income tax share in 2001 predicted a 0.59 percentage-point drop in income tax revenue (as a fraction of 2001 county total revenue) after the reform. This result is consistent with government documents on the reform that the reform cut local government by 50 percent in 2002 and 60 percent since 2003. Column (2) suggests that the reform had no significant impact on the other tax revenues. These other tax revenues include value-added taxes, business taxes, agricultural taxes, etc. Column (3) presents evidence that the reform increased the demand for non-tax revenue—a proxy for county-level discretionary charges—after the reform. These increases were greater in counties that were more dependent on income taxes prior to the reform. A 1 percentage-point increase of income tax share in 2001 resulted in a 0.18 percentage-point increase in non-tax revenue (as a fraction of 2001 county total revenue) after the reform. I also study the impact of the reform on inter-governmental transfers to counties, which include general transfers, earmarked transfers, tax returns, and other types of transfers from upper government. The estimate in column (4) shows that the inter-governmental transfers decreased more, though not significantly, in counties with higher income tax share in 2001. This decline in inter-governmental transfers was driven predominantly by a reduction in general transfers. Column (5) shows the effect of the reform on county total revenue. In total, a 1 percentage-point increase in income tax share in 2001 resulted in a 0.64 percentage-point decline in county tax revenue (from columns (1)-(2)) and a 0.59 percentage-point decline in county total revenue after the reform.

Second, I investigate the effect of the reform on discretionary charges by using a difference-in-differences estimator as shown in specification (2). At county level, I use non-tax revenue as a proxy for discretionary charges; at firm level, I use levies as a proxy for discretionary charges.<sup>20</sup> The main regressor is the county income tax share in 2001,  $ITS_{i,2001}$ , interacted with the  $Post_t$  dummy. All regressions control for county fixed effects and year fixed effects to reduce the bias caused by time-invariant and county-invariant unobservables. For the firm level regressions, I also control for sector-year fixed effects, province-year fixed effects, as well as a set of firm controls, which include firm age, owner's educational level, firm sales, owner's political affiliation. I also control for linear pre-reform trends as well as a set of county variables interacted with the  $Post_t$  dummy to reduce bias from unobserved confounders.

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<sup>20</sup>One implicit assumption here is that firm numbers in each county had not changed over the study period (or they could be accounted for by province-year fixed effects). Thus, county fixed effects will control for the firm numbers in each county. I use the number of above-scaled firms as a proxy for total number of firms and find no significant change. The dependent variable is log number of above-scaled firms and the independent variable is county ITS in 2001 interacted with the  $Post_t$  dummy. The point estimate is 0.063 and the standard error is 0.183.

In Table 3, columns (1)-(2) show that a 1 percentage-point increase in county income tax share led to a 1.2 to 1.3 percent increase in county non-tax revenue. Columns (3)-(4) show that a 1 percentage-point increase in county income tax share led to a 1.3 to 1.4 percent increase in firm levies. These estimates are quantitatively comparable.<sup>21</sup> The evidence shows that (i) the reform has increased the demand for discretionary charges, more so in counties where the reform hit harder in terms of tax revenue loss, and (ii) levies may be proportional to other types of discretionary charges. Together with the estimates in Table 2, these results suggest that a 1 percentage-point drop in county tax revenue led to a 1.9 to 2.0 percent increase in county non-tax revenue and a 2.1 to 2.2 percent increase in firm levies.<sup>22</sup>

Third, I study the heterogeneous effects of the reform on levies for firms of different size. I use a triple-difference strategy in this study as shown in specification (3). I divide firms into different size categories according to cutoffs of firm sales. I use the 33<sup>rd</sup> and 67<sup>th</sup> percentiles of the distribution of firm sales in each province in 2001 as the baseline cutoffs. And I adjust these cutoffs according to the changes in medians of firm sales in each province each year. Since current sales is a choice variable, I define each firm's size according to its sales two years ago. For example, for a firm we observe in the 2003 sample, I use its size in 2001 and the cutoffs in 2001 to determine its size category. The surveys only report firm sales two years ago for the 2001 and 2003 sample and, thus, I can only use these two years for this study. All regressions control for county fixed effects and year fixed effects to reduce biases from time-invariant and county-invariant unobservables. For the firm-level regressions, I also control for sector-year fixed effects, as well as a set of firm controls, which include firm age, owner's educational level, firm sales, and owner's political affiliation.

In Table 4, columns (1)-(2) show that a 1 percentage-point increase in county income tax share led to a 3 percent increase in levies on medium-sized firms compared with other firms.<sup>23</sup> I use a difference-in-differences strategy to explore the driving force of this result. I find that a 1 percentage-point increase in county income tax share led to an insignificant 0.65 percent increase in levies on small firms (standard error is 0.96), a significant 3.81 increase in levies on medium-sized firms (standard error is 1.51), and an insignificant 0.61 percent decrease in levies on large firms (standard error is 2.22).<sup>24</sup> Thus, the positive estimate from the triple difference strategy is driven by the disproportionate increases in levies on medium-sized firms after the reform. Together with the estimates in Table 2, these results suggest that a 1 percentage-point drop in county tax revenue (as a fraction of 2001 county revenue) led to a 4.7 percent increase in levies on medium-sized

<sup>21</sup>A simple regression of log provincial aggregate firm levies on log provincial aggregate non-tax revenue suggests that non-taxes and levies highly comoved. The estimate is 0.89 and the standard error is 0.41.

<sup>22</sup>A 1 percentage-point increase in county income tax share in 2001 led to a 0.64 percentage-point drop in county tax revenue after reform. The number 0.64 is the sum of 0.59 and 0.05 from the estimates in Table 2 columns (1) – (2).

<sup>23</sup>I use data from 2001 and 2003 for this exercise because these are the only years the surveys asked for firm sales two years ago.

<sup>24</sup>Due to a relatively small number of observations, I did not control for province-year fixed effects in the difference-in-differences regressions. The discrepancy between the results from individual regressions and the triple difference regression can be partly attributed to it.

firms.

Fourth, I investigate the effect of the reform on firm size distribution. Figure 10 compares the changes in firm size distributions between low-impact regions and high-impact regions. If the income tax share of a county is above (below) 21 percent—the national median ITS—this county will be grouped into the high(low)-impact regions. Thus, high-impact regions experienced larger drops in revenue from income taxes after the reform. This figure shows that, compared with low-impact regions, the high-impact regions had discernible less medium-sized firms but more small and large firms after the reform. To quantify the effect of the reform on firm size distribution, I use a difference-in-differences specification. The main regressor is county income tax share in total revenue in 2001,  $ITS_{i,2001}$ , interacted with a  $Post_t$  dummy, which equals 0 before 2001 and 1 after 2002. The dependent variable is the share of firms of various sizes in total number of firms in each county each year. The cutoffs are defined by the 33<sup>rd</sup> and 67<sup>th</sup> percentiles of the distribution of firm sales in each province in 2001, adjusted by the provincial medians in each year. All regressions control for county fixed effects, year fixed effects, as well as a set of county variables interacted with the  $Post_t$  dummy.

Table 5 shows the effect of the reform on the number of small, medium, and large firms as shares of county total number of firms. Columns (1)-(3) show that a 1 percentage-point increase in county income tax share led to (i) a 0.40 percentage-point, yet statistically insignificant, increase in the share of small firms, (ii) a significant 0.75 percentage-point decrease in the share of medium firms, and (iii) an statistically insignificantly 0.36 percentage-point increase in the share of large firms. Together with the estimates in Table 2, these results suggest that a 1 percentage-point drop in county tax revenue led to a 1.2 percent decrease in the share of medium-sized firms.

There are two potential explanations for the change in firm size distributions. First, small firms anticipated the large increases in discretionary charges on medium-sized firms and they chose to produce less. Second, the reform may have impacted firm entry and exit decisions. For example, if medium-sized firms relocated from counties where the reform hit more to counties where the reform hit less, we may observe the same change in firm size distributions.

I examine the first explanation by studying growth rates of firms of different size across regions, before and after the reform. I use a triple difference strategy and the dependent variable is firm sales growth in the last two years. The sample of this study only includes firms that existed two years ago. My main regressor is a triple interaction between county income tax share in 2001, a small-firm dummy, and a  $Post_t$  dummy. In Table 6, columns (1)-(2) show that a 1 percentage-point increase in county income tax share in 2001 led to a 2.6 percent decrease in growth rates of small firms compared with the growth rates of other firms after the reform. I use individual difference-in-difference regressions to explore the driving force of this finding. I find that a 1 percentage-point increase in county income tax share led to a significant -2.65 percent lower growth

rates in sales for small firms (standard error is 1.18), an insignificant 1.24 percent lower growth rates in sales for medium-sized firms (standard error is 0.84), and a marginally significant 1.93 percent higher growth rates in sales for large firms (standard error is 1.03). Therefore, the reform negatively affected the growth rates of small firms and likely contributed to the change in firm size distributions.

I study whether the change in firm size distribution was driven by firm entry. Entry is calculated as the number of firms registered within the last two years. In Table 7, I show the effect of the reform on firm entry by firm size. The results show that there was an insignificant decline, if anything, in the share of newly entered small firms in counties where impacts from the reform were greater. There was no effect of the reform on the share of newly entered medium-sized firms. There was a marginally significant increase in the share of newly entered large firms. The results suggest that firm entry can not explain the decline of medium-sized firms. It is unlikely that firm entry and relocation have contributed to the change in firm size distributions. I use data from *Annual Survey of Manufacturers* to study the change in total number of above-scaled firms. If firms were closed down in response to greater amount of discretionary charges, we may also observe declines in the number of above-scaled firms. I do not observe significant change in these numbers. The dependent variable is log number of above-scaled firms and the independent variable is county income tax shares in 2001 interacted with a  $Post_t$  dummy. The point estimate is 0.063 and the standard error is 0.183. Still, the definition of medium-sized firms overlaps with the definition of above-scaled firms in some counties (usually richer counties) but not in others. We can not completely rule out the possibility that medium-sized firms were closed down more in those high-impact regions. If medium-firms, however, were shut down in response to excessive discretionary charges, we may under-estimate the negative impact of discretionary charges on economic growth.

In sum, the empirical findings suggest that the reform has reduced county revenue from income taxes. Counties with larger income tax shares in total revenue in 2001 experienced larger increases in average firm levies. In particular, medium-sized firms had disproportionate increases in levies compared with other firms. We also observe a decline in the number of medium-sized firms. It is most likely driven by slower growth rates of small firms, rather than by firm entry and exit.

## 5.2 Robustness Check

This section provides exercises to check robustness of the main results. I will address five concerns. First, local officials may have anticipated the reform and started to collect discretionary charges before the reform. In this case, we may have underestimated the effect of the reform. Second, small, medium, and large firms may have different pretrends. The heterogeneous impacts on firm levies and the change in firm size distri-

butions might both reflect different pretrends. Even though I could not control for these pretrends due to the significant loss of observations, I could still conduct a placebo test by assuming the reform had happened one year earlier to check the robustness of the results. Third, the result could be sensitive to the choice of the cutoffs for small, medium, and large firms. I divide firms into five groups according to size quintiles and check whether the results are still robust and consistent. Fourth, the reform, on the one hand, increased tax revenue for the central government and, on the other hand, reduced public expenditure from the local government. Both could have affected firms differentially over this period. Last but not the least, we may worry that, after reform, firms had higher incentives to under-report sales in order to avoid additional discretionary charges. As such, I examine whether the impact of the reform on the firm size distribution is real by using firm employment and firm costs as measure of firm size.

To address the first concern, I replace the  $Post_t$  dummy with a set of year dummies. The dependent variables are log firm levies and log county non-tax revenues. The main independent variables are county income tax share in total revenue in 2001,  $ITS_{i,2001}$ , interacted with the year dummies. This test shows which year the effects actually took place. In Figure 9, the solid line plots the estimates of coefficients and the dash lines describes the 95 percent confidence intervals. Figure 9A plots the effect of the reform on county non-tax revenue over years, using 1998 as the base year; Figure 9B plots the effect of the reform on firm levies over years, using 1997 as the base year. In both figures, the effect began right after the reform in 2002. It shows evidence that the officials did not start collecting more discretionary charges beforehand. Thus, it is unlikely that we underestimate the effect of the reform on discretionary charges.

Meanwhile, in my main results, I could not control for the pre-reform trends for small, medium, large firms in each county. I would lose a significant number of observations if I control for these trends. One concern is that the results may be driven by different pre-reform trends, and we may detect effects that are actually non-existent. To address it, I conduct a placebo test that investigates whether we could observe similar changes assuming the reform had happened in 2000. I replace the main regressor,  $ITS_{i,2001}$ , with a new regressor,  $ITS_{i,2000}$ . I examine whether counties with larger income tax shares in total revenue in 2000 had experienced i) larger increases in discretionary charges and ii) disproportional increases in levies on medium-sized firms after 2000. If the results in the previous section are driven by different pre-trends, we expect to find an effect in this placebo test. Otherwise, the results in the previous session should be largely attributable to the 2002 tax reform. Unfortunately, we do not observe firm sales two years ago for the 1999 firm sample and, thus, we could not conduct a placebo test for firm growth.

In Table 8, I show the results from a placebo test using data from year 1999 and 2001 to examine whether discretionary charges increased more in regions with high income tax shares in 2000. I do not find statistically significant results and the magnitude is far smaller than the effects I showed in the main results. In Table 9 I

show the results from this placebo test to examine whether levies increased more on medium-sized firms in regions with high income tax shares in 2000. I do not find statistically significant results and the estimates are negative. Therefore, the results from the previous section are not likely driven by pre-reform trends and the findings are attributable to the 2002 reform.

Next, I examine whether the main results are sensitive to the choice of the size cutoffs. I divide firms into quintiles and run separate regressions for each quintile. The cutoffs for quintiles were defined by the 1999 and 2001 firm size distribution in each province. Firm size is defined by log firm sales two years ago and these cutoffs. I study firms in 2001 (before the reform) and firms in 2003 (after the reform) and investigate firms in which quintile had experienced the largest increases in firm levies and growth rates of firms in which quintile. The dependent variable is log value of firm levies and firm sales growth rates. The main independent variable is county income tax share in total revenue in 2001,  $ITS_{i,2001}$ , interacted with the  $Post_t$  dummy. I control for the county fixed effects and year fixed effects. I also control for county GDP per capita interacted with the post dummy, county labor productivity interacted with the post dummy, number of firms interacted with the post dummy, export intensity interacted with the post dummy, firm sector dummies, firm owner's education and political affiliations. Standard errors are clustered at the county level due to small number of prefectures in each regression.

In Table 10, I show the effect of the reform on firm levies by size quintiles. The quintiles are defined by the 20<sup>th</sup>, 40<sup>th</sup>, 60<sup>th</sup>, and 80<sup>th</sup> percentiles of provincial firm size distribution in 1999 and 2001 (pre-reform period). S, S/M, M, M/L, L refer to small, small/medium, medium, medium/large, and large firms respectively. Columns (1)-(4) show that firm levies on small firms and small/medium-sized firms had not increased over the reform period. Columns (5)-(8) show that firm levies on medium-sized and medium/large-sized firms had remarkable increases, although these increases are only marginally significant. The small number of observation may have undermined the statistical inference. Columns (9)-(10) show that there were no meaningful changes in firm levies on large firms over this period. There are more small firms and large firms but less small/medium, medium, and medium/large firms exactly because of the change in firm size distributions. In Table 11, I show the effect of the reform on firm growth by size quintiles. Columns (1)-(4) show that small firms and small/medium-sized firms had slower growth rates in those regions where the reform hit harder. Columns (5)-(6) show that there was no significant impact on the growth rates of medium-sized firms. Columns (7)-(10) show that the growth rates were higher for medium/large firms and large firms in those regions where the reform hit harder. The evidence of growth rate is consistent with the findings in the main results. Again, it is likely that small firms grew at slower rates to avoid the excessive discretionary charges to be placed on medium-sized firms.

Official government document states that the additional revenue to the central government from the reform



will be used to support the western provinces, where the income is generally lower compared with the eastern provinces. One may worry that the increased spending of the central government after the reform may have driven our results. To alleviate this concern, I drop the firms from 12 western provinces in my sample.<sup>25</sup> In Table 12, I show the point estimates and standard errors. These estimates are generally consistent with the main results from the previous section. Thus, it is unlikely that the increased spending from the central government has contributed to our empirical findings in a significant way. I then study the changes in local expenditure over the reform period. Counties also experienced decline in public expenditures after the reform. One might argue that these expenditures have changed in such a way that they affected medium-sized firms disproportionately. I examine the effect of the reform on local government expenditure. The results in Table 13 show that the reform had a large and negative impact on local government total spending. Educational expenditure and “unlabeled” expenditure underwent the greatest declines. The results in column (2) and column (5) imply that a 1 percentage-point reduction in county tax revenue caused a 0.21 percentage-point reduction in county expenditure on education and a 0.54 percentage-point reduction in county “unlabeled” expenditure. There was no significant change in county expenditure in infrastructure, which may arguably have impacts on firms. It is unlikely that the decline in educational spending and “unlabeled” spending had impacted the firm size distribution significantly.

Last, I use employment as an alternative measure of firm size to check whether the impact on the firm size distribution is real. In Figure 11, I show the changes in firm size distributions in terms of firm employment in high-impact and low-impact regions. The declines in medium-sized firms are visible in both Figure 11A and Figure 11B. We see a larger decline in medium-sized firms in high-impact regions; we also see a larger increase in the number of small and large firms in high-impact regions. In Table 14, I show that the decrease in number of medium-sized firms—defined by firm employment—as a share of total firms is statistically significant. It is unlikely that our results were driven by higher incentives to under-report firm sales.

## 6 Discussion

The previous results show that discretionary charges create distortions and affect firm growth in the Chinese context. This section will discuss why the key insights of this study may be generalizable to other developing countries and why discretionary charges are relevant to economic development.

First, I study the relationship between discretionary charges and firm size in a large set of countries. I use firm-level data from the *World Bank Enterprise Surveys*, which covers 145119 firms in 165 countries over

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<sup>25</sup>These 12 provinces include Chongqing, Sichuan, Yunnan, Guizhou, Shaanxi, Qinghai, Gansu, Ningxia, Xinjiang, Tibet, Guangxi, and Inner Mongolia.

the period of 2002 to 2012. I use informal payment—the amount of gifts and payments to public officials to “get things done” with regard to customs, taxes, licenses, services etc.—to proxy for discretionary charges. Although, by definition, informal payment is different from government levy, the proxy for discretionary charges in my study, these two measures are not distinctively different. Informal payment can be collected at the discretion of officials and a failure to pay may bring consequences including suspension of business. In this regard, informal payment is one kind of discretionary charges. In Figure 12A, I show that a similar inverted-U relationship exists in these countries by plotting informal payment rate against firm size.<sup>26</sup> Informal payment rate is the firm-level amount of informal payments as a percentage of firm sales; firm size is measured by log sales in thousands of local currency units and it is normalized by country and year. The key insight in the paper can also explain this relationship: small firms are less likely requested informal payments by officials and large firms likely receive protection against these payment requests. As such, medium-sized firms face disproportionately more informal payments.

Second, I explore the relationship between average discretionary charges and countries’ income. In Figure 12B, I plot informal payment rate against country-level log GDP per capita, measured in constant 2005 U.S. dollars. Informal payment rate is the average firm-level informal payments as a percentage of firm sales in each country. It shows that countries with lower income tend to have higher average informal payment rates. To give one example, India had a GDP per capita of 797 U.S. dollars in 2006 and the average informal payment rate was 5 percent of firm sales. In comparison, in the same year, Chile’s GDP per capita was 7870 U.S. dollars—almost ten times of India’s—but the average informal payment rate was 0.5 percent of firm sales, one tenth of India’s. This negative association is robust to different firm characteristic controls.

One way to interpret the negative association, between informal payment and income, is that countries with lower income tend to have lower tax revenue and a lack of legal protection on firms. For instance, India’s total tax revenue in 2006 was 11 percent in GDP, whereas the number in Chile was almost 20 percent. [Gordon and Li \(2009\)](#) found that poorest countries collect two-thirds or less of the revenue collected in the richer countries, as a fraction of GDP. And low income countries stay persistently at the bottom of global ranking on property right protection, such as *The International Property Rights Index*. Low tax revenue increases incentives for local officials to seek revenue from firms. In addition, because of lack of legal protection on firms, even when imposing discretionary charges is illegal, it may be hard for prosecutors to indict officials who are involved. As a result, discretionary charges may have posed a more pronounced problem in low-income countries and have affected medium-sized firms disproportionately.

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<sup>26</sup>In Figure 13, I present evidence that the relationships follow similar inverted-U curves in individual countries.

## 7 Conclusion

This paper first documents a novel fact: firm size and discretionary charges imposed on firms have an inverted-U relationship. I explain this fact with a simple model to capture the idea that medium-sized firms are most vulnerable to these charges since they are large enough to be targeted for expropriation but not yet large enough to receive protection against expropriation. Using a natural experiment and data from China, I provide rigorous empirical evidence that is consistent with predictions from the model. I show that disproportional higher charges for medium-sized firms can affect firm production and, thus, impact firm size distribution by reducing the number of medium-sized firms.

My findings make progress on understanding the determinants and consequences of size-dependent firm output distortions in low-income countries. These distortions have impacts on firm production decisions, affect firm size distribution, and reduce aggregate efficiency. My study also highlights the importance of understanding the heterogeneous effects on firms of fiscal decentralization in low-income countries, where legal protection on firms, such as protection on property right, could be weak.

In addition to the main results, the evidence in this paper suggests that the reduction in local government tax revenues significantly reduced local government spending on education and unlabeled categories. These results highlight the ambiguity of the welfare effects of reducing local government tax revenues. The welfare effects will depend on the extent to which educational and unlabeled expenditure reflects inefficiencies or stealing, as well as on the relative efficiency of central provision of public goods versus local provision of public goods. These are all important questions for future research. In the meantime, the results caution policymakers that it is important to consider increasing central provision of public goods to compensate for the reduction of expenditure from local governments.

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Table 1: Descriptive Statistics

	Counties with high ITS (1)	Counties with low ITS (2)	<i>p</i> -value (3)
<i>Panel A: Firm Means (1999, 2001)</i>			
Firm age	6.68	6.79	0.69
Owner's education	2.01	2.06	0.56
People's congress (PC) membership	0.16	0.18	0.49
People's political consultative conference (PPCC) membership	0.32	0.43	0.00***
Log employment (persons)	4.02	4.02	0.46
Log sales (in 10,000RMB)	6.27	5.94	0.00***
Log profit (in 10,000RMB)	3.35	3.06	0.00***
Log levy (in 10,000RMB)	0.79	0.80	0.92
<i>Panel B: County Means (2001)</i>			
Income tax share	0.32	0.13	0.00***
Non-tax share	0.20	0.30	0.00***
Log GDP per capita	8.64	8.24	0.00***
Log county average above-scale firm output	4.40	4.05	0.00***
Log county number of above-scale firms	3.61	3.06	0.00***
County export intensity (export/sales)	0.10	0.06	0.00***
<i>Panel C: County pre-reform trends (1999-2001)</i>			
Growth: county mean firm levy	0.14	0.23	0.67
Growth: county mean firm sales	0.48	0.55	0.70
Growth: county mean firm profit	0.10	0.24	0.62
Growth: county mean PPCC membership	-0.01	-0.11	0.17
Growth: county non-tax share	-0.01	-0.01	0.37
Growth: county GDP per capita	0.17	0.19	0.80
Growth: county above-scale firm number	-0.08	-0.02	0.53
Growth: county average firm output	0.22	0.23	0.35
Growth: county export intensity	0.00	0.00	0.27

*Notes:* This table shows descriptive statistics of firms and counties in the pre-reform period. A county is grouped into "counties with high ITS" if the income tax share (ITS) in total revenue of the county in 2001 is above the national median. Panel A suggests that firms are larger in sales and profits in counties with high ITS. Panel B shows that counties with high ITS are of higher income, and these counties have lower share of nontax revenue in their total revenue. Panel C presents evidence that the pre-reform trends are similar in these two types of counties. This evidence motivates the difference-in-differences specification used in the paper.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 2: The Effect of the Reform on County Revenues

	Dependent variable: $Revenue_{i,k,t}/Revenue_{i,2001}$				
	Income taxes (1)	Other taxes (2)	Non-Tax (3)	Transfer (4)	Overall (5)
County ITS in 2001 $\times$ Post	-0.589*** (0.062)	-0.047 (0.082)	0.176** (0.084)	-0.129 (0.204)	-0.589*** (0.286)
County controls in 2001 $\times$ Post	Y	Y	Y	Y	Y
Linear trends	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y
Obs	12986	12986	12986	12986	12986
$R^2$	0.787	0.829	0.787	0.981	0.978

*Notes:* This table shows the effects of the reform on various types of county revenue.  $Revenue_{i,k,t}$  is the government revenue type  $k$  in county  $i$  in year  $t$ ;  $Revenue_{i,2001}$  is the total government revenue in county  $i$  in year 2001. The results suggest that a 1 percentage-point increase in county income tax share (ITS) in total revenue in 2001 predicted a 0.59 percentage-point decrease in income tax revenue, a 0.18 percentage-point increase in non-tax revenue, and a 0.59 percentage-point decrease in overall county revenue. The study period, 1998 to 2005, covers 4 years before the reform to 4 years after the reform. County controls include log county GDP per capita, log average firm output, county export intensity, and log county total firm numbers. All regressions control for prefecture-year fixed effects. Standard errors are in parentheses and are clustered at prefecture level.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 3: The Effect of the Reform on Discretionary Charges

	Dependent variables: (in <i>log</i> )			
	county non-tax		firm levies	
	(1)	(2)	(3)	(4)
County ITS in 2001 $\times$ Post	1.185*** (0.160)	1.279*** (0.179)	1.326** (0.565)	1.395*** (0.425)
Firm controls	-	-	Y	Y
County controls in 2001 $\times$ Post	N	Y	N	Y
Year Trend	N	Y	N	Y
Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y
Obs	13561	13561	2305	2305
$R^2$	0.948	0.951	0.328	0.331

*Notes:* This table shows that i) counties with higher income tax share (ITS) in total revenue in 2001 experienced larger increases in both county nontax revenue and firm levies after the reform, and ii) the magnitude of the effects of the reform on these two measures of discretionary charges are quantitatively comparable. The county sample covers year 1998-2005, ranging from 4 years before the reform to 4 years after the reform. The firm sample covers 1999, 2001, 2003, and 2005 (these are biannual surveys). Firm controls include log sales, firm age, owner's educational level, owner's political affiliation. County controls include log county GDP per capita, log average firm output, county export intensity, and log county total firm numbers. I control for sector-year fixed effects for the firm-level regressions (columns (3)-(4)). All regressions control for province-year fixed effects. Standard errors are in parentheses and are clustered at prefecture level.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent



Table 4: The Effect of the Reform on Medium-sized Firms' Levies

	Dep. var.: log levies	
	(1)	(2)
County ITS in 2001 $\times$ Medium $\times$ Post	3.027** (1.355)	2.984** (1.404)
County ITS in 2001 $\times$ Post	0.802 (0.659)	0.556 (0.736)
Medium $\times$ Post	-0.863** (0.343)	-0.860** (0.344)
Log sales	0.174*** (0.032)	0.175*** (0.031)
Firm age	0.002 (0.009)	0.001 (0.010)
Owner's education	0.020 (0.072)	0.017 (0.070)
People's Congress	0.100 (0.131)	0.111 (0.133)
People's Political Consultative Conference	0.047 (0.075)	0.043 (0.074)
Log county GDP p.c. in 2001 $\times$ Post		0.221 (0.267)
Log county avg. firm output in 2001 $\times$ Post		-0.285 (0.213)
Log county total firm number in 2001 $\times$ Post		0.094 (0.196)
County export intensity in 2001 $\times$ Post		-1.332* (0.689)
Year FE	Y	Y
County FE $\times$ Medium FE	Y	Y
Obs	900	900
$R^2$	0.390	0.395

*Notes:* The table shows that counties with higher income tax share (ITS) in total revenue in 2001 experienced larger increase in medium-sized firms' levies after 2002. I control for the province-year fixed effects and sector-year fixed effects. Standard errors are in parentheses and are clustered at prefecture level.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 5: The Effect of the Reform on Firm Shares

	Dependent variables:		
	$N_{small}/N_{total}$ (1)	$N_{medium}/N_{total}$ (2)	$N_{large}/N_{total}$ (3)
County ITS in 2001 $\times$ Post	0.395 (0.250)	-0.753*** (0.201)	0.358 (0.256)
County controls in 2001 $\times$ Post	Y	Y	Y
Year FE	Y	Y	Y
County FE	Y	Y	Y
Obs	352	352	352
$R^2$	0.511	0.441	0.563

*Notes:* The table shows that counties with higher income tax share (ITS) in total revenue in 2001 experienced larger decline in medium-sized firms' share after 2002. I keep counties with at least 3 observations. Standard errors are in parentheses and are clustered at prefecture level.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 6: The Effect of the Reform on Firms' Growth

	Dep. var.: firm growth	
	(1)	(2)
County ITS in 2001 $\times$ Small $\times$ Post	-2.580*** (0.877)	-2.586*** (0.881)
County ITS in 2001 $\times$ Post	1.090** (0.430)	0.880** (0.410)
Small $\times$ Post	0.569*** (0.216)	0.565** (0.223)
Log sales (2 years ago)	-0.122*** (0.042)	-0.127*** (0.042)
Firm age	-0.010 (0.008)	-0.011 (0.008)
Owner's education	0.039 (0.077)	0.035 (0.077)
People's Congress	0.071* (0.035)	0.071* (0.036)
People's Political Consultative Conference	0.041 (0.038)	0.039 (0.039)
Log county GDP p.c. in 2001 $\times$ Post	-0.051 (0.053)	0.014 (0.086)
Log county avg. firm output in 2001 $\times$ Post		0.118 (0.128)
Log county total firm number in 2001 $\times$ Post		-0.090 (0.072)
County export intensity in 2001 $\times$ Post		-0.232 (0.357)
Year FE	Y	Y
County FE $\times$ Medium FE	Y	Y
Obs	1320	1320
$R^2$	0.294	0.295

*Notes:* The table shows that small firms in those counties with higher income tax share (ITS) in total revenue in 2001 grew at slower rates in sales after 2002. I control for the province-year fixed effects and sector-year fixed effects. Standard errors are in parentheses and are clustered at prefecture level.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 7: The Effect of the Reform on Firm Entry

	Dependent variables:		
	$N_{entry}^S/N_{total}$ (1)	$N_{entry}^M/N_{total}$ (2)	$N_{entry}^L/N_{total}$ (3)
County ITS in 2001 $\times$ Post	-0.320 (0.215)	0.312 (0.235)	0.363* (0.211)
County controls in 2001 $\times$ Post	Y	Y	Y
Year FE	Y	Y	Y
County FE	Y	Y	Y
Obs	108	108	108
$R^2$	0.575	0.430	0.605

*Notes:* The table shows the effect of the reform on firm entry by firm size.  $N_{entry}^S$ ,  $N_{entry}^M$ , and  $N_{entry}^L$  respectively refer to the number of firm entry of small, medium, and large firms. Entry is calculated as the number of firms registered in current year and last year. I keep counties with at least 3 observations. Standard errors are in parentheses and are clustered at prefecture level. There is no evidence that there was more entry of small firms in counties where the reform reduced tax revenue more.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 8: Placebo Test: The Effect of a 2000 Reform on Discretionary Charges

	Dependent variables: (in <i>log</i> )			
	county non-tax		firm levies	
	(1)	(2)	(3)	(4)
County ITS in 2000 $\times$ Post	0.207 (0.183)	0.234 (0.178)	0.188 (1.071)	0.373 (1.085)
Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y
Section-Year FE	N.A.	N.A.	N	Y
Obs	4746	4746	1269	1269
$R^2$	0.97	0.97	0.26	0.29

*Notes:* The table shows the results from a placebo test. It assumes a similar reform happened in 2000 and examines whether discretionary charges increased after "reform." I show that this hypothetical reform has no impact on both county non-tax revenue and firm levies. The estimates are small and statistically insignificant. Standard errors are in parentheses and are clustered at prefecture level.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 9: Placebo Test: The Effect of a 2000 Reform on Medium-sized Firms' Levies

	Dependent variable: log levies	
	(1)	(2)
County ITS in 2000 × Medium × Post	-1.044 (1.178)	-1.151 (1.374)
County ITS in 2000 × Post	0.034 (0.963)	1.360 (1.479)
Medium × Post	0.426 (0.282)	0.427 (0.324)
Log sales	0.168*** (0.029)	0.149*** (0.027)
Firm age	0.002 (0.008)	0.003 (0.008)
Owner's education	0.131*** (0.041)	0.128*** (0.046)
People's Congress	0.170* (0.096)	0.173* (0.093)
People's Political Consultative Conference	0.064 (0.061)	0.072 (0.063)
Year FE	Y	Y
County FE * Medium FE	Y	Y
Province-Year FE	N	Y
Sector-Year FE	N	Y
Obs	1130	1130
R <sup>2</sup>	0.404	0.395

*Notes:* The table shows the results from a placebo test. It assumes a similar reform happened in 2000 and examines whether medium-sized firms experienced larger increases in levies after "reform." I show that the hypothetical reform has no disproportional impact on levies on medium-sized firms. The estimates are negative and statistically insignificant. Standard errors are in parentheses and are clustered at prefecture level.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 10: The Effect of the Reform on Firms' Levies by Quintiles

	Dependent Variable: log levies									
	S	S/M			M		M/L		L	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
County ITS in 2001 × Post	0.749 (1.683)	0.212 (0.994)	0.747 (1.176)	0.792 (1.869)	4.362* (2.585)	5.618 (4.575)	3.126* (1.764)	3.779 (2.756)	-0.740 (2.548)	-1.542 (3.329)
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Other Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Obs	250	250	145	145	144	144	139	139	167	167
R <sup>2</sup>	0.37	0.48	0.49	0.59	0.51	0.60	0.45	0.58	0.37	0.53

Notes: The table shows the effect of the reform on firms' levies by size quintiles. The quintiles are defined by the 20th, 40th, 60th, and 80th percentiles of provincial firm size distribution in 1999 and 2001 (pre-reform period). Firm size is defined by firm sales 2 years ago. For example, suppose we observe a firm in 2003 and the firm sales 2 years ago (in 2001) were between 20th and 40th percentile cutoffs, this firm will be categorized as S/M (small/medium) size. S, S/M, M, M/L, L refer to small, small/medium, medium, medium/large, and large firms. "Other controls" include firm sector dummies, county GDP per capita interacted with the post dummy, county labor productivity interacted with the post dummy, number of firms interacted with the post dummy, export intensity interacted with the post dummy, firm owner's education and political affiliation, firm size two years ago. We do not have enough observations for more controls such as sector-year specific shocks. Standard errors are clustered at the county level due to small number of prefectures in each regression. We find that medium and medium/large firms experienced largest increases in firm levies after the reform. We have more small firms and large firms but fewer firms with size in between.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 11: The Effect of the Reform on Firms' Growth by Quintiles

	Dependent Variable: firm growth									
	S		S/M		M		M/L		L	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
County ITS in 2001 × Post	-2.568 (2.003)	-1.312 (1.145)	-2.383** (1.029)	-2.108 (1.383)	-0.108 (1.345)	-0.907 (1.772)	1.956** (0.928)	2.296** (1.091)	1.022 (1.431)	0.174 (1.911)
Log firm sales (2 years ago)	-0.544*** (0.155)	-0.598*** (0.157)	0.100 (0.198)	0.152 (0.276)	-0.005 (0.187)	-0.034 (0.234)	-0.179 (0.186)	-0.155 (0.225)	0.084 (0.095)	0.059 (0.098)
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Other Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Obs	247	247	145	145	144	144	139	139	166	166
R <sup>2</sup>	0.48	0.54	0.48	0.55	0.46	0.57	0.53	0.60	0.38	0.51

Notes: The table shows the effect of the reform on firms' growth rates by size quintiles. The growth rates is calculated as log sales minus log sales 2 years ago. The quintiles are defined by the 20th, 40th, 60th, and 80th percentiles of provincial firm size distribution in 1999 and 2001 (pre-reform period). Firm size is defined by firm sales 2 years ago. For example, suppose we observe a firm in 2003 and the firm sales 2 years ago (in 2001) were between 20th and 40th percentile cutoffs, this firm will be categorized as S/M (small/medium) size. S, S/M, M, M/L, L refer to small, small/medium, medium, medium/large, and large firms. "Other controls" include firm sector dummies, county GDP per capita interacted with the post dummy, county labor productivity interacted with the post dummy, number of firms interacted with the post dummy, export intensity interacted with the post dummy, firm owner's education and political affiliation. We do not have enough observations for more controls such as sector-year or province-year specific shocks. Standard errors are clustered at the county level due to small number of prefectures in each regression. We find that small and small/medium firms experienced largest reduction in firm growth rates after the reform. This finding is consistent with the previous results that medium and medium/large firms had the largest increases in government levies. Meanwhile, medium/large firms and large firms had higher growth rates after the reform.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent



Table 12: The Effect of the Reform on Firm Levies and Growth by Firm Size (Sample Excluding Western Provinces)

	Dependent variables:					
	log levies			firm growth		
	S	M	L	S	M	L
	(1)	(2)	(3)	(4)	(5)	(6)
County ITS in 2001 $\times$ Post	-0.288 (0.499)	3.514** (1.539)	1.289 (1.793)	-3.039** (1.146)	-0.931 (0.840)	2.338** (1.028)
Other controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Obs	280	201	225	280	201	225
$R^2$	0.47	0.52	0.43	0.47	0.42	0.47

*Notes:* This table shows the effect of the reform on firm levies and firm growth using a subsample. It is to address the concern that the central government may have spent more in the western provinces and our results were driven by the spending of the central government. The evidence, however, shows that even if we exclude all the western provinces, the results are still consistent and robust. "Other controls" include firm sector dummies, county GDP per capita interacted with the post dummy, county labor productivity interacted with the post dummy, number of firms interacted with the post dummy, export intensity interacted with the post dummy, firm owner's education and political affiliation, firm size two years ago. We do not have enough observations for more controls such as sector-year specific shocks. Standard errors are clustered at the county level due to small number of prefectures in each regression.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 13: The Effect of the Reform on County Expenditures

	Dependent Variables: $Expenditure_{i,k,t} / Revenue_{i,2001}$					
	Construction (1)	Education (2)	Social security (3)	Administration (4)	Unlabelled (5)	Total (6)
County ITS in 2001 $\times$ Post	0.120 (0.101)	-0.135*** (0.065)	-0.040 (0.031)	-0.112 (0.084)	-0.346*** (0.181)	-0.548 (0.246)
County controls in 2001 $\times$ Post	Y	Y	Y	Y	Y	Y
Linear trend	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Obs	12986	12986	12986	12986	12986	12986
$R^2$	0.761	0.975	0.830	0.984	0.956	0.984

Notes: The table shows the effect of the reform on county expenditure. Educational expenses were negatively affected by the reform, only second to the "Unlabelled expenditure." It is unlikely that the changes in these two types of expenditure would have affected firms differentially. Standard errors are in parentheses and are clustered at prefecture level.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

Table 14: The Effect of the Reform on Firm Shares: Using Firm Employment

	Dependent variables:		
	$N_{small}/N_{total}$ (1)	$N_{medium}/N_{total}$ (2)	$N_{large}/N_{total}$ (3)
County ITS in 2001 $\times$ Post	0.174 (0.250)	-0.373* (0.201)	0.198 (0.259)
County controls in 2001 $\times$ Post	Y	Y	Y
Year FE	Y	Y	Y
County FE	Y	Y	Y
Obs	355	355	355
$R^2$	0.471	0.406	0.479

*Notes:* The table shows that counties with higher income tax share (ITS) in total revenue in 2001 experienced larger decline in medium-sized firms' share after 2002. I keep counties with at least 3 observations. Standard errors are in parentheses and are clustered at prefecture level.

\* Significant at 10 percent

\*\* Significant at 5 percent

\*\*\* Significant at 1 percent

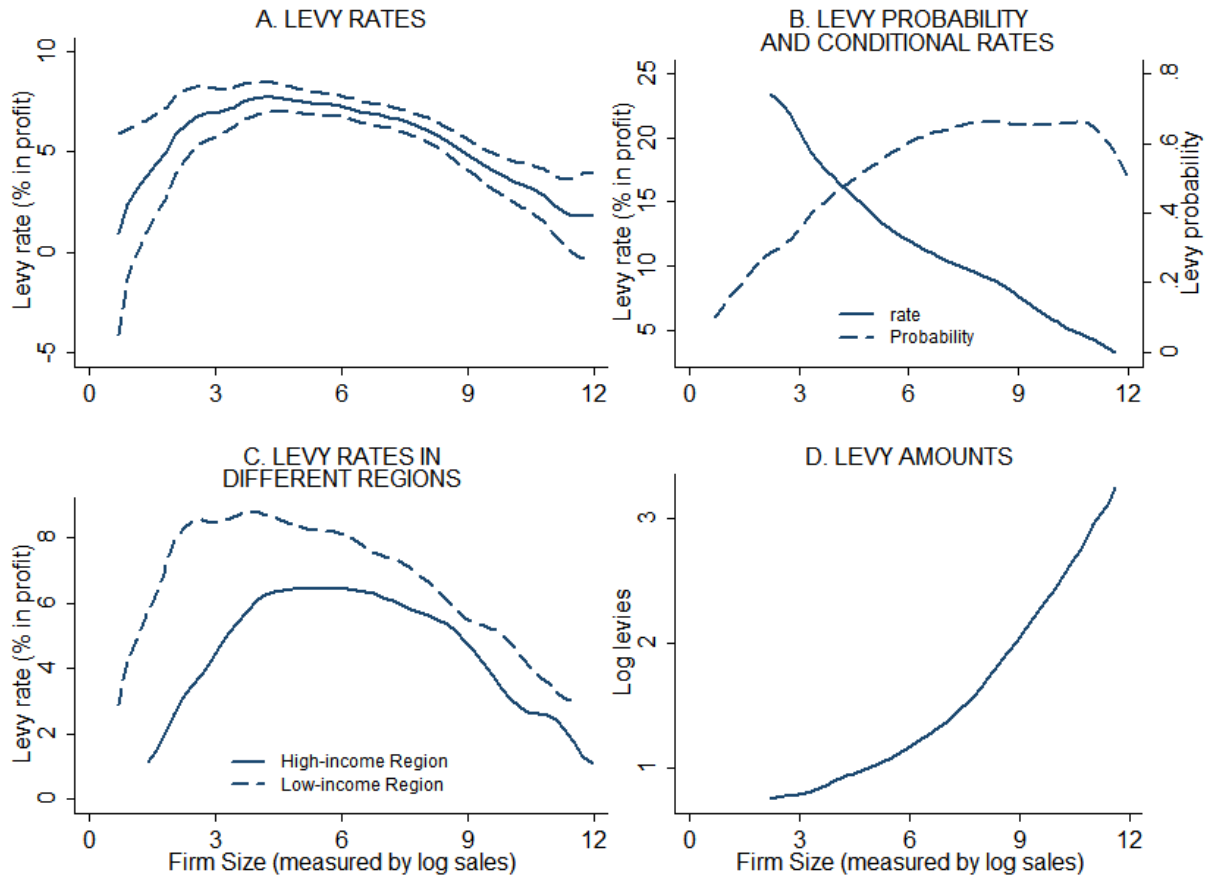


Figure 1: Firm Levy and Size of Firms

Source: I use firm-level data from the *Private Enterprise Surveys* (1999, 2001). See Section 4 for more details about these data.

Notes: The table presents four stylized facts about firm levies. These figures are all local polynomial plots. Figure 1A shows that there is an inverted-U relationship between levy rate and firm size. Levy rate measured as the percentage of firm levies in firm profit; firm size is measured by log firm sales (in 10,000 current RMB). The solid line is the local polynomial estimate; the two dashed lines describe the 95 percent confidence intervals. Figure 1B shows the two driving forces of the inverted-U relationship: i) there is a positive relationship between firm size and probability of being charged levies, and ii) conditional on being levied, a negative relationship between firm size and levy rate. Figure 1C demonstrates that regions with lower income tend to have higher levy rates. High-income regions include municipalities and coastal provinces (mean GDP per capita in 2001 was 15,415 yuan); Low-income regions include inner and western provinces (mean GDP per capita in 2001 was 6,425 yuan). Figure 1D shows a positive relationship between firm size and amount of levies, once the firm is imposed levies.

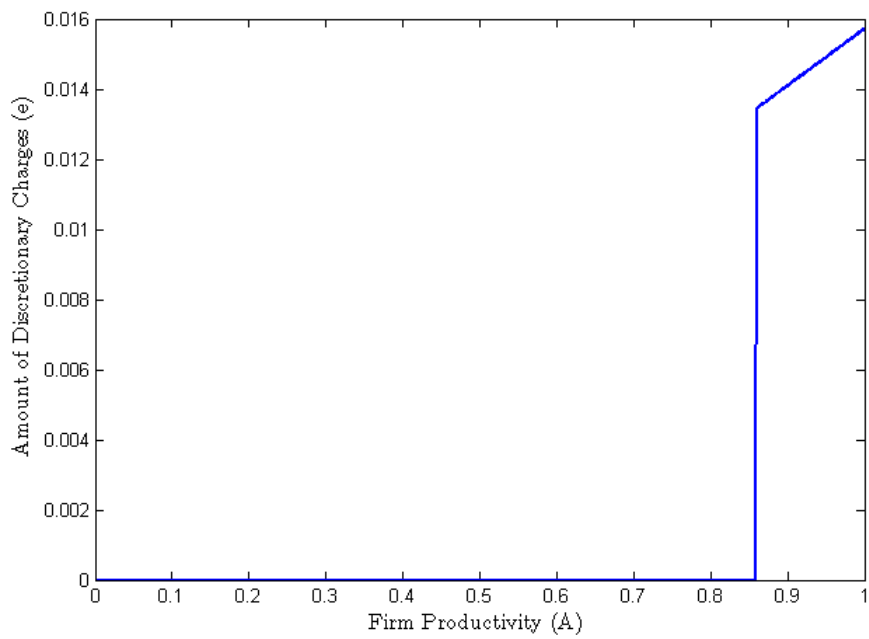


Figure 2: Firm Productivity and Discretionary Charges

*Notes:* This figure shows the relationship between firm productivity and the allocation of firm-level discretionary charges in equilibrium. Firms with productivity below 0.85 will receive no discretionary charges; firms with productivity above 0.85 will receive positive amounts of discretionary charges and these amounts increase with firm productivity. The assumed endowed revenue,  $T$ , is 20.94. In equilibrium, the total revenue from discretionary charges is 2.06.

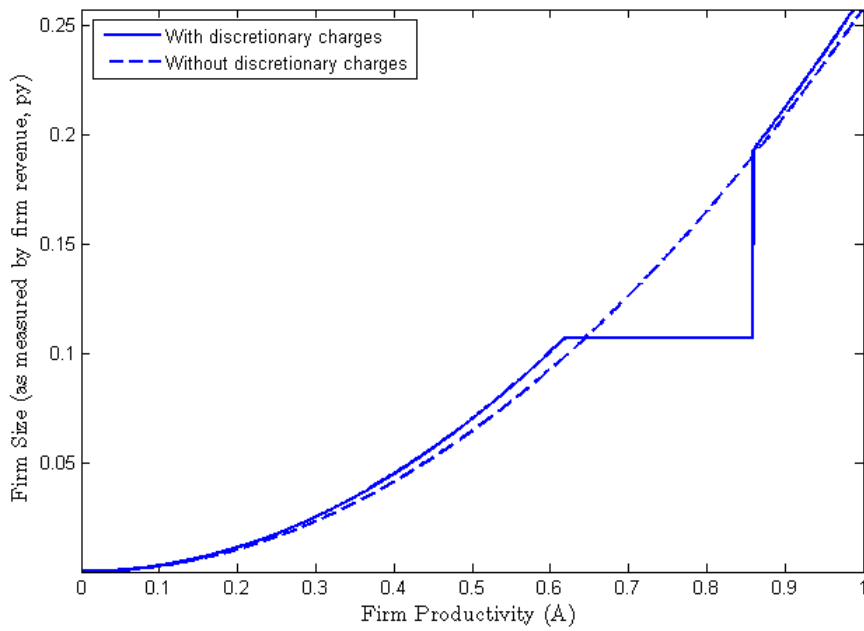


Figure 3: Firm Productivity and Firm Size

*Notes:* This figure shows the relationship between firm productivity and firm revenue in equilibrium. The dashed line serves as a benchmark for this relationship, when we completely shut down the channel of distortions from officials collecting discretionary charges. The solid line describes this relationship, when firms are faced with distortions from officials collecting discretionary charges. The assumed endowed revenue,  $T$ , is 20.94. Firms stay smaller to minimize the costs from these charges. The aggregate output drops by 7 percent.

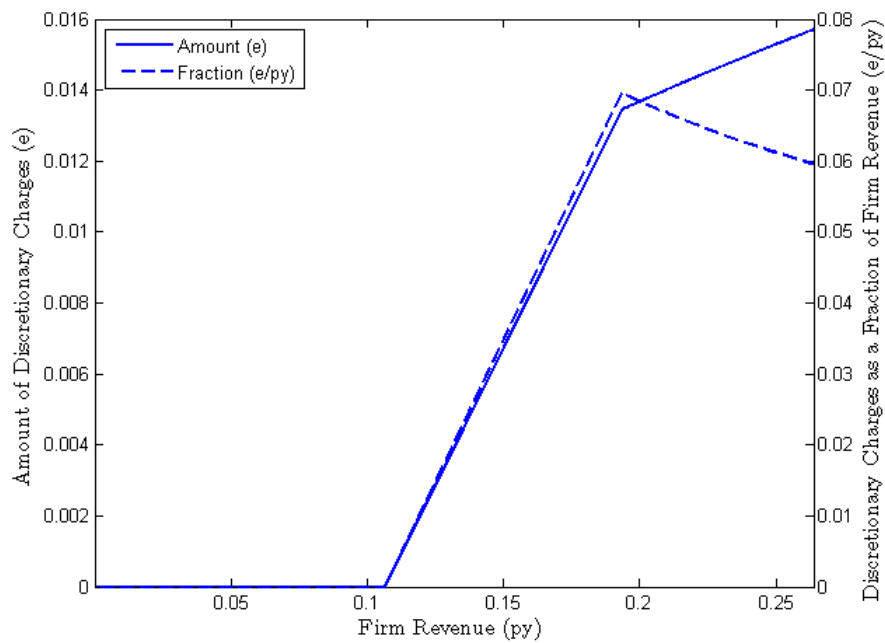


Figure 4: Firm Size and Discretionary Charges in Equilibrium

*Notes:* This figure presents the relationship between firm size (measured by  $py$ ) and firm-level discretionary charges in equilibrium predicted by the model. On the one hand, the absolute amount of firm-level discretionary charges increases with firm size. On the other hand, the fraction of firm revenue taken as discretionary charges exhibits an inverted-U shape. The assumed endowed revenue,  $T$ , is 20.94. In equilibrium, the total revenue from discretionary charges is 2.06.

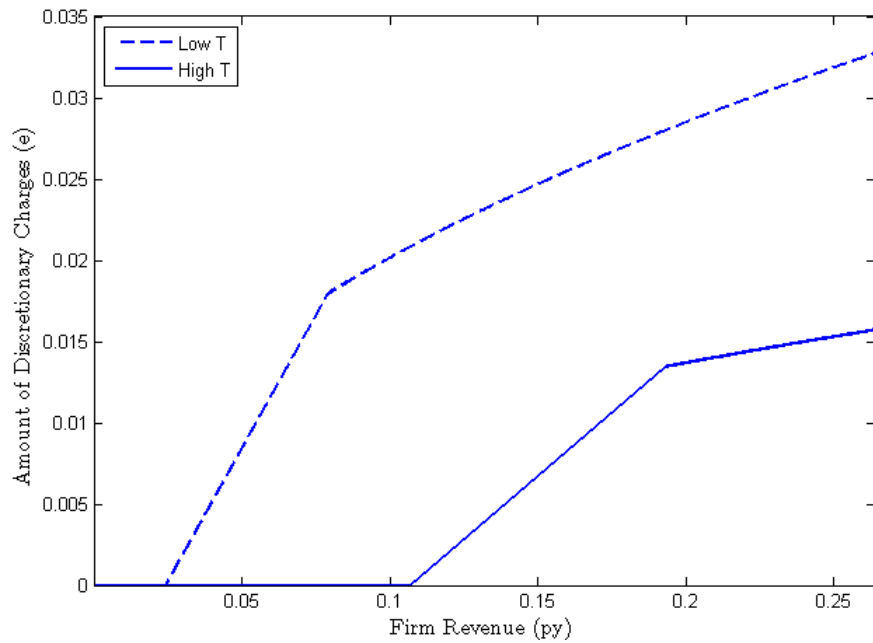


Figure 5: The Effect of a Reduction in  $T$  on Discretionary Charges

*Notes:* This figure shows the effect of a reduction in endowed revenue,  $T$ , on the allocation of discretionary charges, as predicted by the model. After a reduction in  $T$ , firms will on average experience increases in discretionary charges. How these increases varied by firm size depends on the initial condition and the magnitude of the reduction in  $T$ . In this case, where  $T_{low}$  is 1.14 and  $T_{high}$  is 20.94, firms of medium size will experience the largest relative increases in discretionary charges.



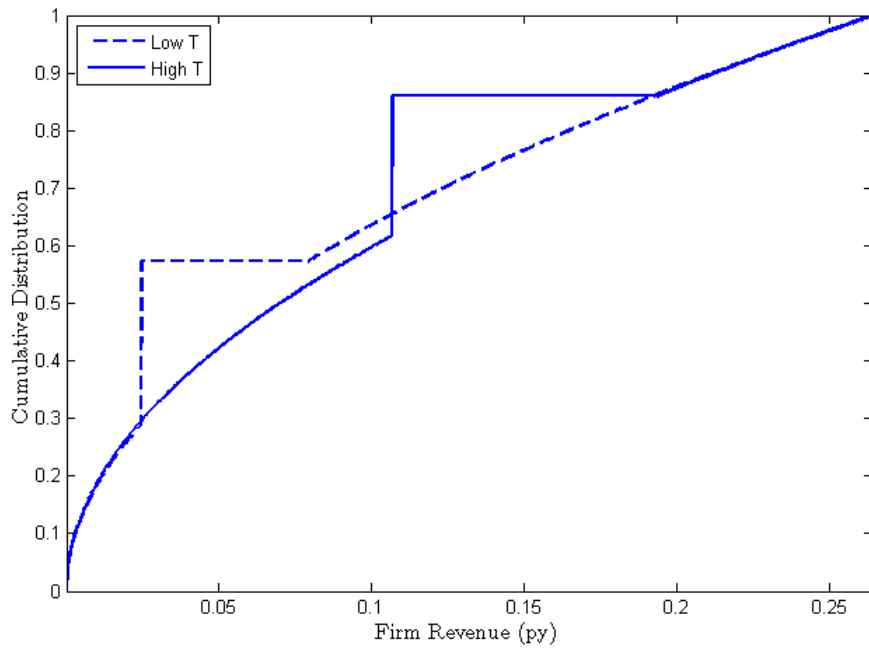


Figure 6: The Effect of a Reduction in  $T$  on Firm Size Distribution

*Notes:* This figure shows the effect of a reduction in endowed revenue,  $T$ , on firm size distribution, as predicted by the model. After a reduction in  $T$ , there will be more firms of smaller size. In this case, where  $T_{low}$  is 1.14 and  $T_{high}$  is 20.94, there will be more firms of small size and less firms of medium size. The revenue cutoff points for small, medium, and large firms here are 0.029 and 0.114. The share of medium-sized firms drops from 54 percent to 11 percent, whereas the share of small-sized firms increases from 32 percent to 57 percent. The share of the large firms increase from 14 percent to 32 percent.



Figure 7: The Effect of the Reform on County Income Tax Revenue

*Source:* Statistics on county revenue are from the *Fiscal Yearbooks of Chinese Prefectures and Counties* (2001, 2002). See Section 4 for a further discussion of the data.

*Notes:* This figure presents evidence that (i) the reform had reduced counties' income tax revenue, and (ii) this reduction is correlated with the share of income taxes in total county revenue before the reform. Both axes are normalized by each county's total revenue in 2001. The x-axis indicates the share of income taxes in total county revenue in 2001; the y-axis indicates the change in county income tax revenues from 2001 (pre-reform year) to 2002 (post-reform year), as a fraction of county total revenue in 2001. This figure suggests that counties that were more dependent on income taxes experienced larger declines in total revenue. The slope of the predicted line is -0.56 (the standard error is 0.01). The evidence is consistent with the reform policy that the local government's revenue from income taxes was to be cut by half.

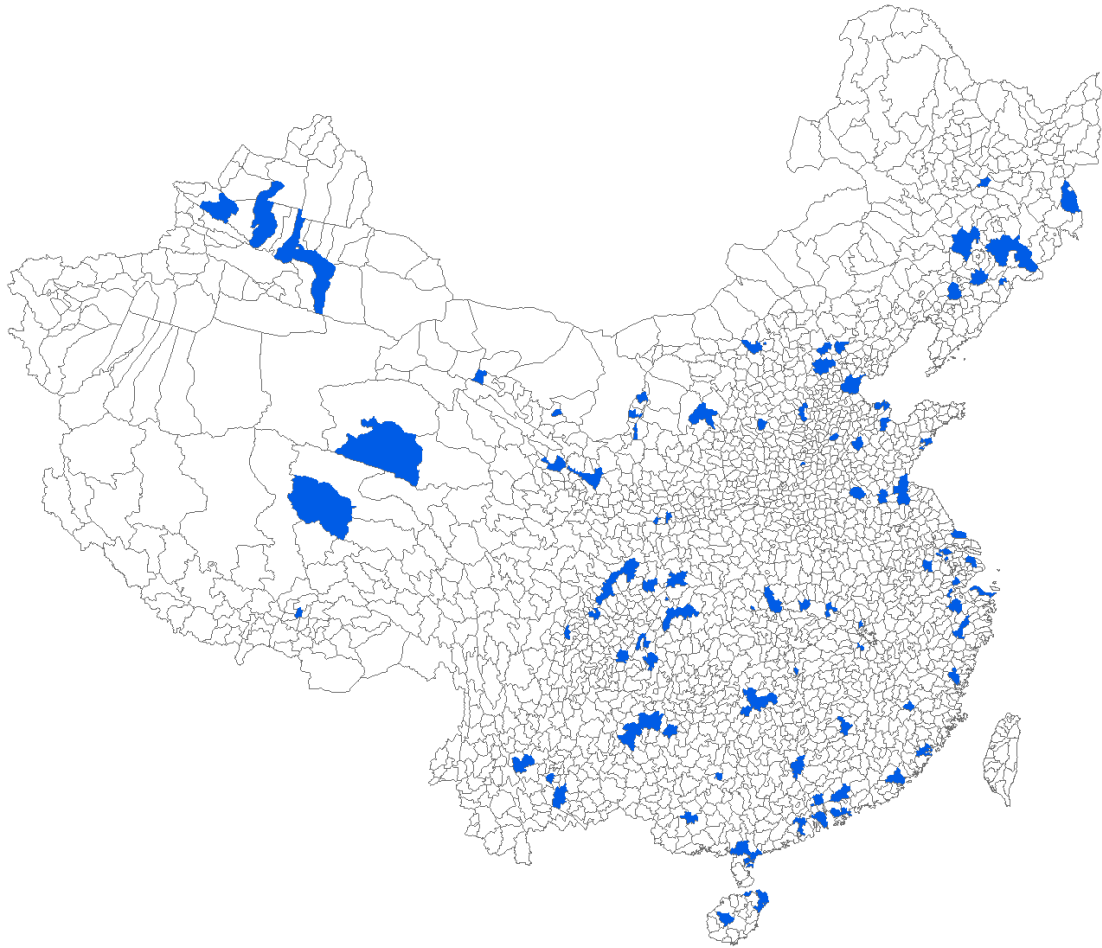


Figure 8: Counties Surveyed in both Pre-Reform and Post-Reform Periods

*Notes:* 264 counties were surveyed in both before and after reform periods.

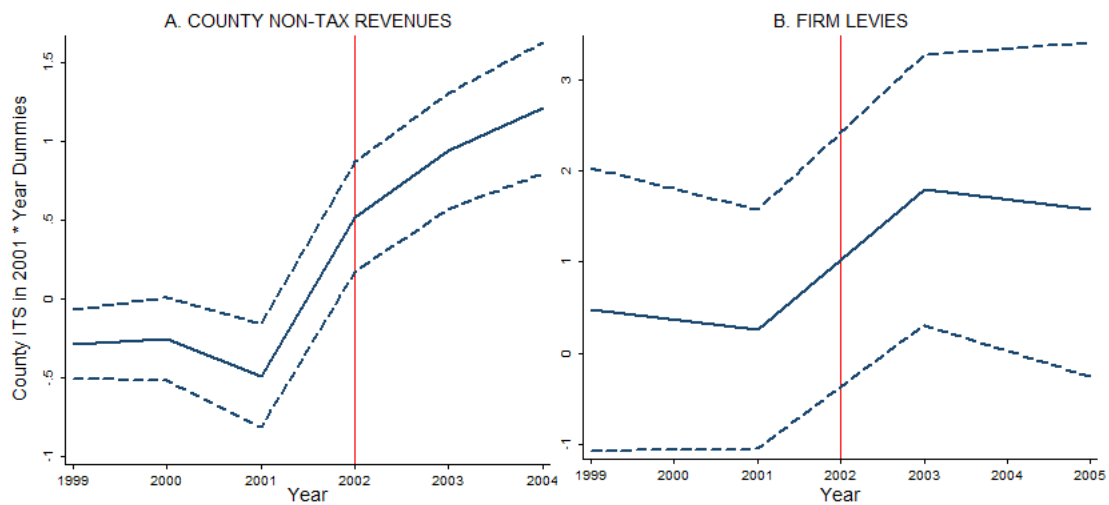


Figure 9: The Effect of the Reform on Discretionary Charges over Years

Source: I use i) firm-level data from the *Private Enterprise Surveys* (1996, 1999, 2001, 2003, and 2005), and ii) county-level data from the *Fiscal Yearbooks of Chinese Prefectures and Counties* (1998-2004). See Section 4 for a further discussion of the data.

Notes: This figure shows the effect of the reform on discretionary charges started in 2002. Figure 4A demonstrates that (i) the increase of county non-tax revenue (in log) began in 2002, and (ii) counties with higher income tax share (ITS) in total revenue in 2001, which also lost more revenue from income taxes, experienced larger growth in county non-tax revenue (normalized by county's total revenue in 2001). Figure 4B shows that (i) the increase of firm levies (in log) became more significant after 2002, and (ii) counties with higher income tax share (ITS) in total revenue in 2001 had experienced larger growth in firm levies (in log). The solid lines represent the estimated coefficients of the interaction terms, county's income tax share in 2001 times a set of year dummies; the two dashed lines are the 95 percent confidence intervals.

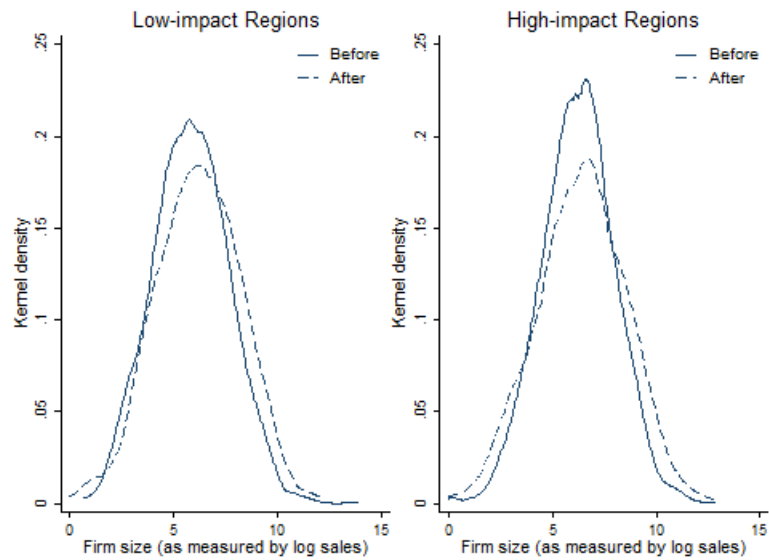


Figure 10: Changes in the Firm Size Distributions

*Source:* I use two data sets to compute this figure: (i) Firm-level data from the it:Private Enterprise Surveys (1999, 2001, 2003, and 2005), and (ii) County-level revenue data from the *Fiscal Yearbooks of Chinese Prefectures and Counties*. See Section 4 for a further discussion of the data.

*Notes:* This figure compares the changes in firm size distributions between “low-impact regions” and “high-impact regions.” If the income tax share of a county is above (below) 21 percent, the national median ITS, this county will be grouped into the “high(low)-impact regions.” Thus, high-impact regions experienced larger drops in revenue from income taxes after the reform in 2002. This figure shows that, compared with low-impact regions, the high-impact regions had less medium-sized firms and more small firms after the reform. Firm size is measured by log sales (in 10,000 current Renminbi).

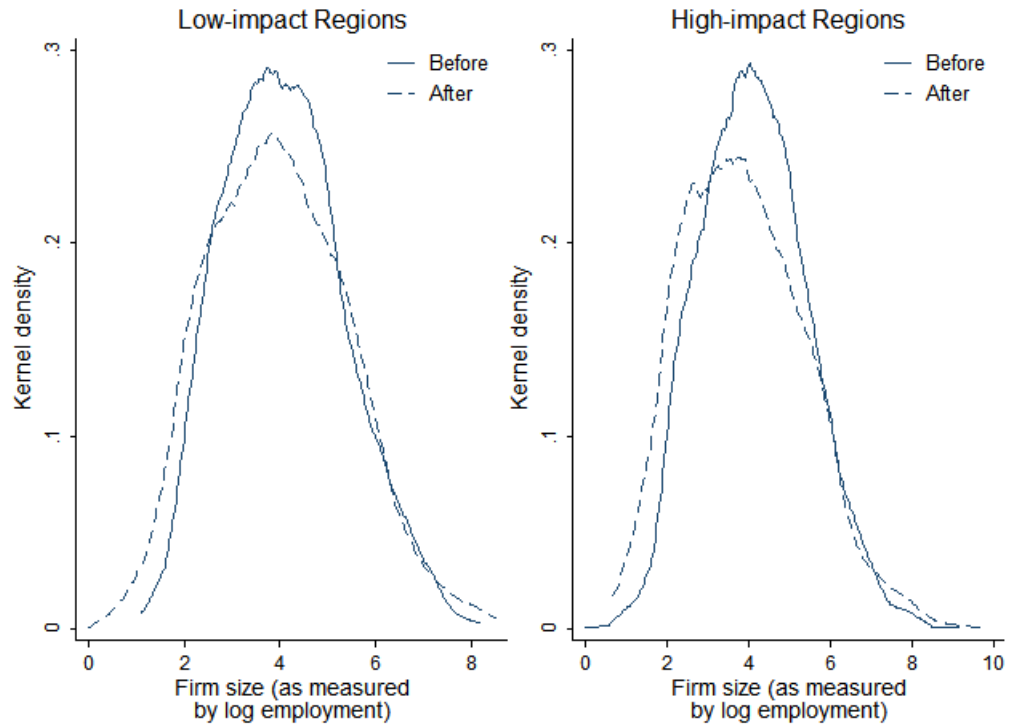


Figure 11: Changes in the Firm Size Distributions: Using Firm Employment

*Source:* I use two data sets to compute this figure: (i) Firm-level data from the *Private Enterprise Surveys* (1999, 2001, 2003, and 2005), and (ii) County-level revenue data from the *Fiscal Yearbooks of Chinese Prefectures and Counties*. See Section 4 for a further discussion of the data.

*Notes:* This figure compares the changes in firm size distributions between “low-impact regions” and “high-impact regions.” If the income tax share of a county is above (below) 21 percent, the national median ITS, this county will be grouped into the “high(low)-impact regions.” Thus, high-impact regions experienced larger drops in revenue from income taxes after the reform in 2002. This figure shows that, compared with low-impact regions, the high-impact regions had less medium-sized firms and more small firms after the reform. Firm size is measured by log employment (total number of workers).

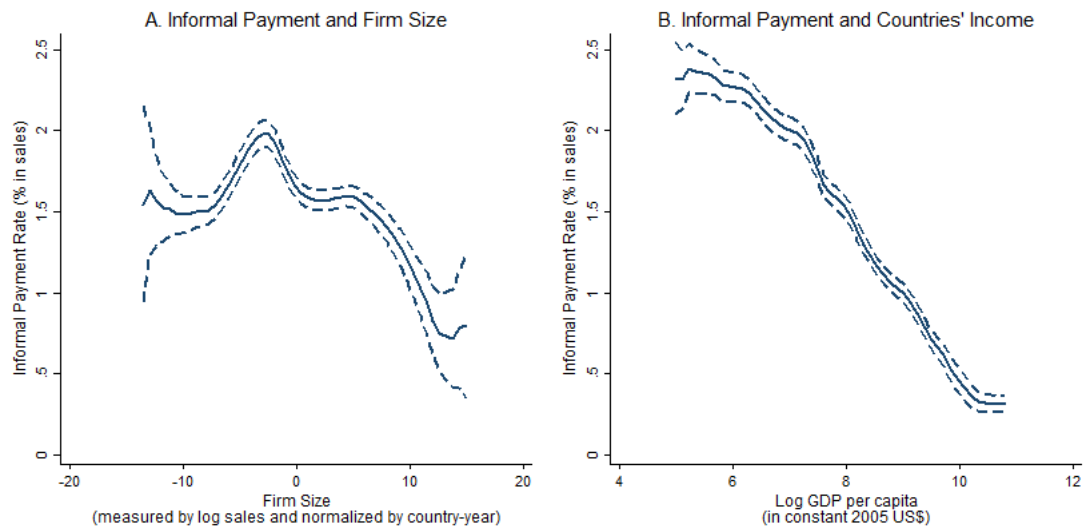


Figure 12: Informal Payment, Firm Size, and Countries' Income

*Source:* I use two data sets to compute this figure: (i) firm-level informal payment and firm size data from the World Bank Enterprise Surveys (2002-2012), and (ii) country-level GDP per capita data from the World Development Indicators.

*Notes:* This figure presents two facts. First, Figure 8.A shows that the relationship between informal payment rates and firm size follows a similar inverted U curve cross countries. The sample covers 145119 firms in 165 countries over the period of 2002 to 2012. Firm size is measured by log sales in thousands of local currency units and it is normalized by country and year. Second, Figure 8.B shows that countries with lower income, measured by log GDP per capita in constant 2005 U.S. dollars, have higher average informal payment rates.

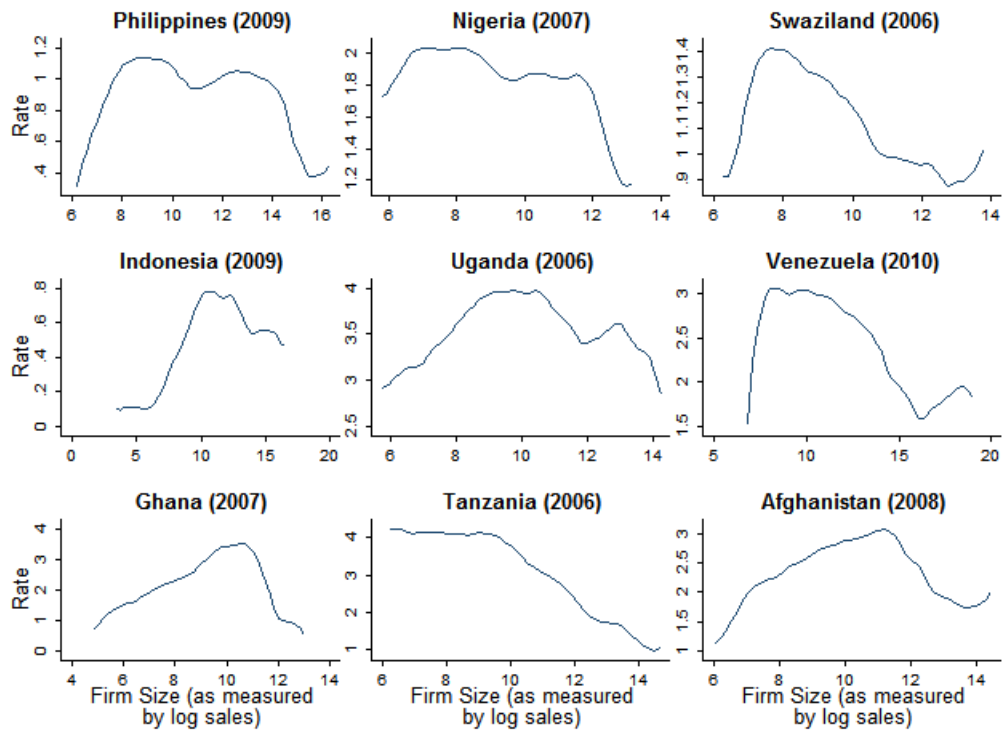


Figure 13: Firm Size and Informal Payment

Source: I use data from the *World Bank Enterprise Surveys* (2006 - 2013).

Notes: The rate on the vertical axis measures the percentage of total annual sales paid as informal payment. The firm size on the horizontal axis, as measured by log sales, is in 10,000 current U.S. dollars.



## A Appendix

### A.1 Firm Sampling

The firm sample is a nationally representative random sample of Chinese private firms in each wave. The sampling method is systematic sampling in each region-sector stratum. There are five stages in firm sampling:<sup>27</sup>

Stage 1: determine the total number of firms to be surveyed in each province, based on the actual representation in the population:

$$W_i = \# \text{ of firms surveyed in province } i.$$

Stage 2: for each province, select one capital city, one prefecture level city, one county level city, and three other counties with high, middle, and low incomes. These three other counties represent different stages of social-economic development:

$$W_{i,j} = W_i \times \frac{\text{total \# of firms in county } (i,j)}{\text{total \# of firms in province } i}.$$

Stage 3: within each city or county, determine the number of firms to be surveyed in urban areas and rural areas ( $k = 1, 2$ ):

$$W_{i,j,k} = W_{i,j} \times \frac{\text{total \# of firms in area } (i,j,k)}{\text{total \# of firms in county } (i,j)}.$$

Stage 4: within each area, determine the number of firms to be surveyed in each sector:

$$W_{i,j,k,s} = W_{i,j,k} \times \frac{\text{total \# of firms in sector } (i,j,k,s)}{\text{total \# of firms in area } (i,j,k)}.$$

Stage 5: use systematic sampling method to choose the specific firms. Take the registered firm list and randomly choose a firm on the it. Start with this specific firm and find the next firm with interval  $N_{i,j,k,s}$  and select firms with the same interval. The interval  $N_{i,j,k,s}$  is chosen in such a way that the total number of firms to be surveyed in this sector will be  $W_{i,j,k,s}$ .

### A.2 Government Budgetary Revenue in China

In China, the budgetary revenue, for both the central government and the local government, include revenue from taxes and non-taxes. There are three types of taxes: central taxes, local taxes, and shared taxes. Central taxes include tariff, excise duty, value-added tax on imports, corporation income tax of key state firms,

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<sup>27</sup>This subsection is based on the reference document in 2002, 2004, and 2006 surveys. The only reference document available for the pre-2002 surveys is the 1995 round, and the sampling method is consistent.

personal income tax of key state firms, business tax of key state firms, city maintenance and construction tax, vehicle sales tax, and export tax rebates. Local taxes include the majority of business tax, urban land using tax, fixed investment tax, city maintenance and construction tax, property tax, vehicle and vessel usage tax, stamp duty, agricultural tax, special duty on agriculture, farming land occupancy tax, tax on dividends, value-added tax on land, urban property tax, vehicle and vessel license plate tax. Two major shared taxes are value-added tax and income tax, and the latter one includes both corporation income tax and personal income tax. The central shares 75 percent value-added tax and 60 percent income tax after 2003. The central also shares resources tax and stamp duty on security exchange with the local government.

Both the central government and the local government collect non-tax revenue. For the central government, non-tax revenue includes net transfer from state-owned enterprises, surtax on education, earmarked revenue, administrative charges, fines and penalties, national treasury bond, independent revenue in ministries and departments, extra-budgetary revenue in ministries and departments. For the local government, non-tax revenue includes net transfer from state-owned enterprises, charges on pollution and urban water resources, surtax on education, administrative charges, fines and penalties, and public debt. However, the local government collects a major part of the non-tax revenue. See Table A.1 for more details.

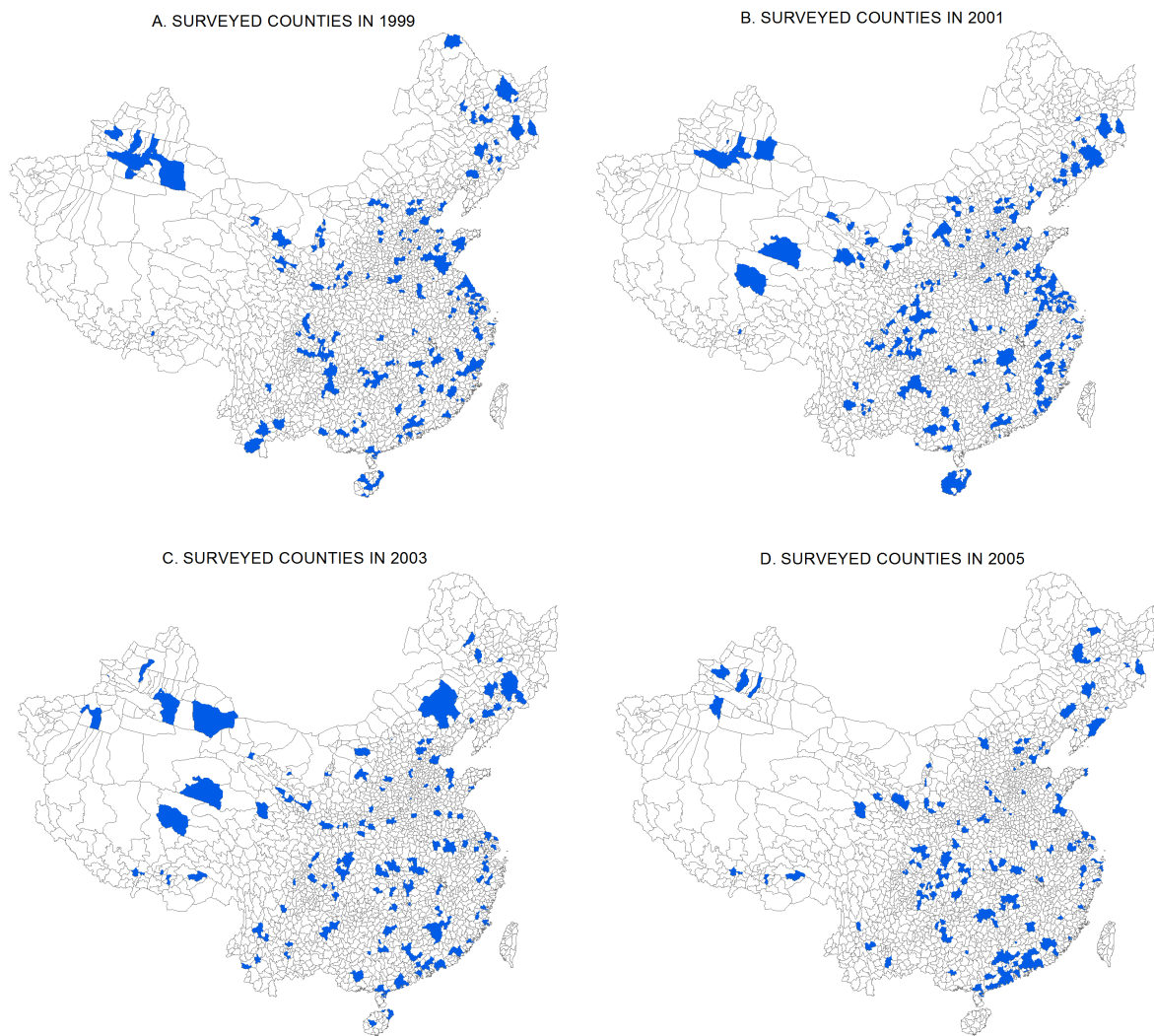


Figure A.1: Surveyed Counties (1999-2005)

*Notes:* In 1999, 419 counties were surveyed; in 2001, 437 counties were surveyed; in 2003, 361 counties were surveyed; in 2005, 320 counties were surveyed.

