

The Effect of 1999 Agricultural Tax Reform on Chinese Farmers' Net Income Growth

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Abstract:

In 1999, the Chinese government implemented a tax reform that abolished a variety of local government fees levied on individuals and rural households in favor of a single agricultural tax. Although the aim of the reform was to increase farmers' income, it also led to an immediate decrease in local governments' fiscal revenue, which could give them stronger incentive to extract money from the farmers. This paper first discusses the evolution of Chinese farmers' economic burden, the aim and unintentional consequences of the reform. After that, this paper uses a large dataset containing China's 501 counties in 6 provinces from 1999 to 2003 to conduct empirical analyses of the causal effects of 1999 China's rural tax reform on farmers' net income growth. Two-way fixed effects are employed in this paper. The major finding is that the effects of the reform on the growth rate of farmers' net income is statistically significant: on average, a county which adopted the tax reform saw its farmers' annual net income growth rise 1.89% higher than a county without reform in the same province. The basic result is robust to a number of checks. However, the long term impact of the reform is uncertain.

Key words: Rural tax reform, farmers' net income growth, Fixed Effects

I: Introduction

Oppressive taxation and farmers' uprisings have marred Chinese dynastic rule and many empires have crumbled as a result of popular rebellions. In the 1990s, while the Chinese economy was experiencing rapid development, excessive taxation reemerged as an issue in the countryside. In addition to formal taxes, local governments charged exorbitant levies and fees on rural households, which exacerbated the economic burden on farmers. According to one study, the percentage of direct and indirect tax of farmers' total income increased from 15.4% in 1995 to 19.7% in 1999.¹ In addition, a vicious cycle developed in many regions: to secure tax collection, local governments recruited more staff members, and an enlarged local bureaucracy in turn required more revenues and collections. Rural taxation became a major source of grievance and social instability and many of China's agriculture-based localities witnessed escalating levels of conflict and protest against local governments (Chen, 2003).

In response to farmers' growing frustration and protests, in 1999 the Chinese central government initiated a rural tax reform, which incorporated the fees farmers paid to the village into a single reduced agricultural tax, abolished the slaughter tax, and abolished all unauthorized fees for administrative and institutional purposes for local governments. Along with the tax reform, the central government has begun streamlining local bureaucracy by

¹ See Appendix A

downsizing local governments and cutting personnel expenditure.

This paper aims to provide theoretical and analytical analyses of the effect of the 1999 tax reform. The evolution and explanations of the economic burden on farmers is outlined in Section II. The aim of the reform policy and its unintentional consequences are discussed in Section III. Section IV uses a large dataset that covers 501 counties in 6 provinces across China from 1999 to 2003 to measure the impact of rural tax reform on farmers' net income growth through a two-way Fixed Effects model. In Section V the meaning of the results is discussed and conclusions are drawn.

II: Evolution and Explanations of the Economic Burden on Farmers

The farmers' tax burden is not a problem that appeared recently. The Chinese government began to extract resources from agriculture to fund industrial development soon after 1949. Rural taxation, which accounted for nearly 40% of total taxation in China in the early 1950s, was an effective mean to achieve this goal. The central government relied on two instruments to collect agricultural tax. The first was the monetary agricultural tax. The tax rate was set to about 15% of grain output revenue in China's first five-year-plan period, with slight variation among different provinces. The second taxation tool was indirect but turned out to be more powerful. Farmers were mandated to sell their grain products through collectives to the state at below-market prices, which enabled the state to provide cheap food to feed industrial workers in cities. In addition, communes relied largely on farmers for other sources of income, including levying a collective accumulation fund, a welfare fund, and cadres' compensation funds. This income compensated for the low prices communes received from the state and the agricultural tax that had been deducted from these sales. Only afterwards did farmers receive their share of grain and cash based on their work-points². However, at that time, farmers did not feel the tax burden, since they were members of the communes and therefore did not bear the tax expenditure directly.

After the economic reform began in 1978, the status of farmers was fundamentally changed. Instead of being commune members, they became independent agricultural producers. This change largely inspired their motivation for production. Hence, despite the fact that they directly bore the tax burden, they didn't necessarily feel the burden was unbearable. This is because of the rapid increase in their income during this period. The farmers' burden became an issue during the mid 1980s, when communes were replaced by township governments as a result of rural political reform. In order to give these newly-founded governments more incentive for promoting local economic growth, central government allowed them to manage their extra-budgetary accounts in addition to formal budgetary revenues. Township governments, for example, were authorized to collect five pooling funds (Wu Tong) for local public services such as education, militia training, road construction and maintenance, welfare for veterans, and birth control. Villages were authorized to collect three deductions (San Ti) for collective capital accumulation, welfare funds, and cadres' salaries. All of these fees were classified as extra-budgetary and they added to the economic burden on farmers.

In addition to these fiscal incentives, several other factors also exacerbated the farmers' economic burden. First, submitted profits from Township and Village Enterprises (TVEs)

² Work-point indicated how much time a farmer worked during a certain period of time.

constituted a major source of local governments' extra-budgetary revenues in the 1980s. When the Chinese economy became more marketized in the early 1990s, many TVEs went bankrupt or were privatized (Naughton 2007). Once this cushion eroded, local governments had to turn to farmers for revenues. Second, in 1994, the central government introduced a separate tax reform with the ultimate goal of recentralizing fiscal revenues. As a result, local governments found their shares of the fiscal pie shrinking. At the same time, central leadership intensified cadre evaluation. Local officials were required to fulfill their targets in areas such as school enrollment rate, telephone coverage, running water, and road connections in order to avoid punishment. Therefore, local governments felt the financial squeeze from both sides of revenue collection and public spending. To make ends meet, they started to issue fee-collecting regulations and raise extra funds from farmers. These extractions usually took the form of fees and levies for things like school renovations, environmental protection, cooperative health services, etc. Local governments fabricated a variety of excuses to extract resources. As farmers found it harder to bear the ever-increasing economic burden, many began to fight back. In some cases, the resistance escalated into violent clashes involving police, local cadres, and the masses, seriously undermining social stability and the Party's legitimacy in the countryside (Li 2004, Chen 2003).

III: 1999 Tax Reform

The 1999 tax reform (Shuifei Gaige) was enacted by the central government to relieve farmers of their economic burden that had been eroding rural incomes throughout the 1990s and especially after the 1994 tax reform. The aim of the tax reform was to reduce farmers' burden by eliminating local fees altogether and utilizing a single agricultural tax. It also served to streamline local revenue collection and establish a more transparent and efficient provision of services. The reform was first introduced in Anhui province in 1999, and then broadly introduced to the other 20 provinces in 2002.

The content of the reform can be summarized below: five pooling funds (Wu Tong) and three levies (San Ti)—extra-budgetary items which were previously extracted by township governments and villages—were abolished by combining them into a formal budgetary system. The agricultural tax rate was set at 7% of grain output revenue. All other fees and levies were labeled as “unauthorized” and were to be eradicated.

Theoretically, the reform should increase the net income of farmers by reducing the numerous fees that they were required to pay. In addition, the fact that the pooling funds and levies were classified as extra-budgetary allowed local officials to operate under the radar screen. By incorporating these fees into a formal budgetary system, the management of those funds would become more transparent. Further, from an institutional perspective, the reform was part of the central government reconstruction and centralizing program that can be traced back to 1998. The logic behind the reconstruction was to improve bureaucratic efficiency by reducing personnel and administrative units. The reform was a policy that suited this goal because the tax reduction would dramatically cut payrolls and thus lead to the downsizing of local governments. The central government hoped to improve local bureaucratic efficiency as a result of cutting down excessive personnel.

However, the reform may also raise some unintentional consequences. First of all, the reform undoubtedly decreased local governments' fiscal revenue. At the same time, these

governments were still responsible for providing common goods such as compulsory education and medical care to farmers. Without increasing other channels for fundraising, governments either could not afford the provision of public services, or they would go to farmers to extract funds.

In addition, like any policy changes in China, implementation itself can be a major challenge. The fact that government can not legally collect money from farmers does not mean they will obey the rules. There are three main reasons for this. First, formal institutions of accountability are often weak in China, in that it lacks strong bureaucratic institutions to control corruption and ensure that lower level officials are doing their jobs. Democratic institutions such as elections that allow citizens to hold local officials accountable are unreliable. Therefore, for local bureaucrats, the risk of not obeying the law is low. Second, because higher-level governments cannot monitor at all times the enforcement of regulations by local governments, there is an asymmetric information problem. Local governments may choose to expand their bureaucracy or impose more charges in the name of implementing higher-level government regulations. As the expansion of local governments crowds out private investment and reduces farmers' consumption, it will aggravate rural tax burdens and slow farmers' income growth. Finally, the challenge of implementation is also reflected in terms of "Heterogeneous Enforcement". The amount of tax for every province, county, and township is determined by the upper-level government according to a set of rules taking into account natural conditions, historical factors and political concerns. This leads to significant differences in policy enforcement across regions. These unintentional consequences of the reform may incentivize local government officials to extract money in other ways from the farmers, resulting in no improvement, or even a reduction in farmers' net income growth.

With the above considerations in mind, whether this reform effectively increased farmers' net income growth becomes an empirical question of interest. Because the influences of these unintentional consequences are hard to separate out, I try to measure in the following section the overall effect of the tax reform on farmers' net income growth.

IV: Empirical Analysis

While the purpose of tax reduction is to increase income of the targeted group, the desired effect is hard to achieve. It is argued that developing countries in general have not been able to use tax and transfer policies effectively increase farmers' income (Chu, et al 2000). Further, inappropriate policy design often results in greater income gaps. In the case of rural taxation, poorer farmers were paying more taxes and fees than richer farmers (Tao, 2005). In terms of China's 1999 agriculture tax reform, there has been a growing amount of literature discussing its effect on farmers' income. However, the findings are mixed. Optimists believe that the reform is an effective way to increase farmers' income. Wang Suping (2004) argues that the reform had reduced farmers' burden by 28 billion yuan by 2004. However, some scholars believe that the key to improve farmers' income is not through tax reform, although tax reform did have some positive effect. For instance, Cai Jiming (2003) argues that increasing the efficiency of county government is the most effective way to improve farmers' income. There are other scholars who believe tax reform actually increased farmers' economic burden; because reducing the tax lowered the fiscal revenue for county governments, without auxiliary policies on political institutional reform, they tend to collect

more money illegally from farmers.

However, most of the literature on measuring the impact of this agriculture tax reform does not provide any empirical evidence. For the most part, the impact of the reform has been evaluated by surveys, which simply compare farmers' pre- and post-reform income in some regions (Tian Xiujuan and Zhou Feizhou, 2003; Xiang Jiquan, 2004). Zhou Li-An and Chen Ye (2005) were the first to use econometric methods to evaluate the impact of rural taxation and fee reform in China. Their paper, however, is flawed in two major ways. First, the authors used Pooled OLS to estimate the impact of tax reform. The critical assumption of POLS is that the residual should not correlate with any of the independent variable. However, counties in different provinces have large variation in terms of geography and political and economic development. Those factors are captured in the residual when using POLS. Yet these factors are correlated with both dependent and independent variables. As strict exogeneity fails, POLS will provide biased coefficients. Second, they did not take into account autocorrelation issues. If the residual is correlated with itself across time, GM theorem is violated and errors are not independent. This also leads to the biased coefficient estimates. Therefore, I will attempt to correct these mistakes and determine whether the tax reform has had a real impact on farmers' net income growth.

I collected county level data of Anhui, Fujian, Hebei, Hunan, Jiangsu and Shandong provinces from 1999 to 2003. Data on annual farmers' income, fiscal revenue and expenditure, grain output, and designation as coastal and mountainous counties were collected from Chinese county statistical yearbooks. GDP per capita in 1998 and 1999 were collected from provincial statistical yearbooks. Within the 501 counties that are included in the sample, 4 counties began reform in 1999, 69 in 2000, 63 in 2001, 310 in 2002 and 55 in 2003.

The dependent variable, growth rate of farmers' net income³, was calculated from farmers' net income per capita (collected from provincial statistical yearbooks) from 1998 to 2003. As the key independent variable, tax reform was treated as a dummy, with counties that had adopted the reform labeled one. It is worth noting that the name list of the counties that will experience tax reform is announced at the beginning of every year, but data of farmers' income is collected at the end of the year. Thus I assume the effect of the reform could be reflected in the same year.⁴

Econometric Models⁵: Two-way fixed effects were used for the baseline model. Fixed Effects knock out all unobserved observation-specific characteristics that don't change over time, and therefore solve the problem of omitted variables which will bias the coefficient estimates. If I were to use Random Effects—which results in a more efficient estimator—I would be assuming that there is no correlation between explanatory variables and unobservable county or province variation. To see whether I should use an efficient or an unbiased estimator, I conducted a Hausman test on the base model. The test indicated a systematic difference in coefficients, which suggests I should use Fixed Effects.⁶

First, I used Fixed Effects on county level to estimate the impact of reform. The model is:

³ "Income growth" in later analysis means "net income growth"

⁴ See Appendix B

⁵ See Appendix C for all models used

⁶ See Appendix D

$$Netinc_{it} = \beta_0 + \beta_1 Reform_{it} + \sum_{1999}^t \delta year_t + \beta_4 Grain_{it} + \alpha_i + u_{it}$$

“i” stands for county level variation and “t” stands for the variation in time. Grain output serves as a control variable. A dummy for each year except the first year was included in the model because there seems to be unobservable trends that all counties follow closely.

However, in this case, I have county level data for a policy intervention that occurs at the province level.⁷ This hierarchical data structure allows me to use Fixed Effects at the province level, which provides two advantages. First, this model allows me to control for some time-invariant variables at the county level; second, I am worried about differences such as economic development at the province level that would influence farmers’ income growth. By using province-level Fixed Effects, I removed all heterogeneity at the province level. Now my question is: if a county has adopted the reform, is it likely to have a more rapid income growth for farmers compared to the other counties in the same province? My model is:

$$Netinc_{igt} = \beta_0 + \beta_1 Reform_{igt} + \sum_{1999}^t \delta year_t + \beta_2 Coast_{ig} + \beta_3 Mount_{ig} + \beta_4 Grain_{igt} + \alpha_g + u_{igt}$$

Other things remaining equal, “g” stands for province level variation. α_g is province level Fixed Effects and u_{igt} stands for errors that vary across time. *Coast* and *Mount* are two dummies that control for geographical differences across counties.

Results⁸:

County Level FE: First I ran a Fixed Effects model on reform dummy without any control. It showed that the reform increased annual income growth by 2.93%, significant at the 99% level. After this, I added year dummies and grain output for control. The effect of the reform on income growth fell to 2.0% but still is highly significant.⁹

Province Level FE: I ran the FE regression only on the reform dummy as well. Now the coefficient of reform was 1.89, meaning counties adopted the reform saw their farmers’ incomes grow by 1.89% higher than counties in the same province that had not adopted the reform. After that I added some time-invariant county level variables to this model. Some literatures show that counties along the coast have well-developed TVEs and more opportunities for agricultural product export. This implies that farmers’ income should grow faster. Others suggest that some mountainous counties see less farmers’ income growth due to the tough environment. After adding the geographical dummies *Coast* and *Mount*, the results showed that, all else equal, mountainous counties’ farmer saw their income growth 0.7% lower than other counties’ within the same province. However, no evidence showed that a coastal county’s farmers would experience faster income growth. Finally, I clustered my standard errors at the province level to account for the fact that my observations are considered nested in 6 provinces. The results showed that the reform was still significant at the 99% level, supporting the idea that the reform had a positive impact on farmers’ income growth.¹⁰

⁷ There is some variation in the timing of the reform within each province, but most counties in a given province adopted the reform at the same time.

⁸ FE results on both levels are presented, but the following analysis focused on FE on province level.

⁹ See Appendix E Model 1 and Model 2

¹⁰ See Appendix F Model 1 to 3

Robustness Check: The validity of the above regression results depends on whether those counties that did not adopt the reform in a certain year serve as a good control group. Thus, the question is: “Did government purposefully pick particular counties to reform first?” To see whether this is the case, I would like to test whether the time of treatment is related to certain baseline factors, or to the change in income growth before the reform.

First I ran a cross sectional regression to see what factors would determine the time a county adopted reform. I used 1999 data and excluded the 4 counties that were already adopting the reform at that time. The regression results showed that the coefficients of grain output and income growth are both significant at the 95% level. It seems counties with smaller grain output and faster income growth will adopt reform later. However, I am not too worried about these results because the cross-sectional relationship did not claim causality and it is not the identifying assumption of Fixed Effects. All differences presented in this cross-sectional regression will be eliminated by the two-way Fixed Effects model.¹¹

The next step is to test the identifying assumption of Fixed Effects, which is that the treatment and control groups would grow at the same rate in absence of the reform. In this case, I need to see whether the pre-reform change in income growth was similar across counties. Because this analysis requires at least two periods of pre-treatment data, I split up the sample into counties that began enforcing reform in 2001 and those that began reform in 2002. I used data from 1999 and 2000 to test the 63 counties that were treated in 2001. I used data from 2000 and 2001 to test the 310 counties that were treated in 2002. The 428 counties (include 55 counties that were not treated in 2002) comprise more than 85% of my total observations. The regression results¹² showed that coefficients of “change in reform” in all cases are insignificant; meaning that entry into the treatment is not correlated with the pre-treatment change in farmers’ income growth. Therefore, I can safely treat the dataset as a “quasi-natural experiment” and estimate the reform’s impact on farmers’ income growth.

I also created a “tau” variable which collapsed the staggered entry and reconciled counties’ first reform year by using leads and lags. I plotted income growth against this variable.¹³ The pictures presented the following information: first, there was no Ashenfelter’s dip¹⁴ before the counties’ entry into reform. Second, from the picture with two years before and after the reform, I found the reform did accelerate farmers’ income growth. However, if I looked at the picture with four years before and after the reform, I found that from the third year after the treatment, income growth actually decreased, and at the end of the fourth year, it was even less than growth before reform! I got the same results by creating reform dummies indicating zero to four years after the reform. The purpose was to see the effect of the reform on a year-to-year base. The regression¹⁵ showed that the effect of the reform was significant within the first two years after the county adopted the reform. In the second year after the reform, farmers’ income growth increased a maximum of 3.5%. However, after that, the effect became insignificant. Did these results indicate the tax reform reproduced the “Huang Zongxi Law”¹⁶ which says all important tax reforms in China have no long-term impact? It’s hard to

¹¹ See Appendix G

¹² See Appendix H

¹³ See Appendix I

¹⁴ Ashenfelter’s dip refers to the fact that the selection for treatment is influenced by individual-transitory shocks on past outcomes.

¹⁵ See Appendix J

¹⁶ “Huang Zongxi Law” is the name given to the phenomenon that the results of agricultural tax reforms in Tang, Ming and Qing dynasty were without exception an initial reduction of tax burdens on farmers, but followed by resurging fees.

say for two reasons. First, it's not accurate to simply compare the four years before with four years after the reform. Recall that I've found that higher pre-treatment income growth means that a county will implement the reform later. So it's reasonable to see that those counties treated in 2003 (lead=4) have higher income growth rate than counties treated in 1999 (lag=4). Second, I have few observations three or four years after the reform. Thus the long-term impact of the reform could not be accurately concluded with current data.

To further test whether the increase in growth rate of farmers' income really comes from the tax reform, I introduced another comparison group: urban citizens. Because the purpose of the reform was to increase farmers' income, it is reasonable to assume that urban citizens' income growth would not be affected by this reform. If the regression results show that urban income growth was also greatly affected by this reform, I would suspect that the causal effect of farmers' income growth was due to some unobserved variables. Unfortunately this data was not available for Shandong province. Thus, I used the other five provinces' data to run the same regression using urban income growth in place of farmers' income growth. The results showed that the reform dummy was not significant at 95%. It supported the initial hypothesis that the increase in farmers' income growth was a result of the tax reform.¹⁷

Another threat of this kind of quasi-natural experiment is that the experiment itself would cause endogenous reaction. However, in this particular reform, this is not a big problem. Because of the strict Hukou system¹⁸ in China, there were very few cases in which farmers will migrate due to tax reform.

Upon running residuals on their lags, autocorrelation was found in the error terms.¹⁹ To deal with this issue, I first ran the model on bi-annual data (99, 01, 03), after which the reform dummy remained significant at 99% and the marginal effect even became larger. Then I ignored the staggered entry data structure and used 1999 and 2003 data to run Fixed Effects. In this case, the reform dummy still remained significant and large.²⁰ This indicated that the real effect of the reform on farmers' income growth could have been higher. My last attempt to deal with autocorrelation is to apply Newey-West Standard Errors. The significance and marginal effect of reform remained. Now I'm confident that the reform did contribute to the increase of farmers' income growth.²¹

Impact of Governments' Behaviors on Reform: The aim of the tax reform was to stop unauthorized fee extraction and reduce farmers' economic burden, thus the behaviors of local governments before the reform should be related to the effect of the reform. My hypothesis was that the reform should have larger impact on governments with higher incentives to extract money from farmers in the pre-reform period. To test this hypothesis, I constructed the following variables to measure government behavior before reform and to examine the relationship between these variables and reform implementation.

- Ratio of administrative expenditure over total fiscal expenditure in 1998: This variable measures the size of the government. Counties with a high ratio before reform may have excessive government bureaucracy.
- Ratio of fiscal revenue over fiscal expenditure in 1998: This variable measures the

¹⁷ See Appendix K

¹⁸ Hukou is the registered residency status of a particular individual in this country. In China,

¹⁹ See Appendix L

²⁰ See Appendix M

²¹ See Appendix F Model 4

government's ability to self-fund its own operations.

- The distance from the county to the provincial capital: I use this variable to measure the supervision county government received from upper level. I assume the farther the county government is away from the provincial capital, the less supervision it received.

I created interaction terms of these variables with reform and added those terms in the model. The result showed if the counties did enforce the reform, farmers' income growth for counties that are farther away from the provincial capital (relative to median) was 1.67% higher. Farmers' income growth for counties with higher percentage of administrative expenses and lower ability to fund themselves (relative to median) before the reform was 1.62% and 1.32% higher, respectively.²² To see the exact percentage of reform impact, I divided my sample into two sub-groups by using the median of those variables as cutoff points, running Fixed Effects model I had before. In each case, I found that the difference in the coefficients in the two sub-groups matched the coefficient in interaction terms in general, meaning those numbers are reliable.²³

V: Further Discussions and Conclusion

After estimating the impact of tax reform, I found that within a province, this tax-reduction increased the growth rate of farmers' income by an average of 1.89%. This result was supported by a series of robustness checks and empirical tests. Theoretically, if the tax reform is successfully implemented, farmers can surely expect benefits, especially in the long term. The first level of these benefits is that the tax reduction itself increases farmers' net income directly. On a second level, farmers' average income growth rate should increase by 1.89%.

However, the regression results also showed that the reform's long run effect was vague. One possible explanation for this long term ineffectiveness lies in the tension between a liberalized economy and a still-centralized political system. Market economy saw the bankruptcy of many TVEs, whose profit constituted a major source of local governments' fiscal revenues. In addition, farmers' grievances increased as income gaps between urban and rural inhabitants widened. Thus the central government had to implement policies to reduce farmers' economic burdens and accelerate the socioeconomic development in the country to secure its governing legitimacy. Now all the pressure fell on local governments, which were required to stop extracting fees from farmers while still promoting local economic development despite decreasing fiscal revenues. Under these circumstances, institutional constraints often determine how officials order their preferences or even compel them to choose between competing preferences.

Institutions in China are hierarchical in that top leaders of the party appoint subordinate officials. Although the officials appointed by the center are expected to articulate the interests of their regions, they always look upward to the party leaders in Beijing because they know their careers depend on satisfying these leaders. As Shirk puts it, although the lines of accountability run in both directions, power is not equally shared. Because top-down authority is much stronger than bottom-up authority, China's political system pressures lower-rank officials to be more responsive to their superiors. Under the cadre evaluation system, the overriding criterion for performance and hence promotion is the speed of local socioeconomic

²² See Appendix N

²³ See Appendix O

development, which in turn required abundant funds. The tax reform cut the revenues to local governments, but the performance criterion remained unchanged. In addition, the central government had difficulty supervising the local implementation of the tax reform. Therefore, the pressure for performance under tighter fiscal constraints drove desperate local officials to try to balance their budgets at the cost of farmers. Although the increased grievance of farmers in their areas might drag down the overall performance evaluation of local officials, their competing preferences required them to rank this preference lower if not give it up altogether. This led to the “selective policy implementation.” As O’Brien and Li (1999) said, knowing how to distinguish between “hard” and “soft” targets, these officials invariably place stop fee collection in the soft, nonbinding category and regard performance criterion as hard targets that must be met. Given its relatively low risk, extracting money from farmers to construct facilities that satisfy the performance criterion of central government conferred maximum benefits on local officials. Fiscal burdens thereby ultimately fell upon farmers, whose interests were least likely to be represented in the political process.

Lastly, government behavior before the reform was found to have an impact on the effect of reform itself. In counties where government received less supervision, had lower self-funding ability and excessive staffs before the reform, their farmers’ income growth would be bigger if they did adopt the reform. These results are theoretically reasonable: large distances generally mean less supervision from upper level government; low rates of fiscal revenue over fiscal expenditure may indicate that the government lacks money to fund operations; and high proportions of administrative expenses suggest an inordinate amount of personnel. All three of these characteristics gave local governments incentive to extract money illegally from farmers. Thus, once those counties began the reform, I should expect higher farmers’ income growth than in other treated counties that did not display these characteristics. However, because of the existence of these initial factors, these counties are also more likely to deviate from the reform in the long run. In remote areas, local governments have a bigger say in public affairs as the asymmetric information problem is more severe than areas that are closer to provincial capitals. Therefore, governments in remote areas have ample flexibility in determining the extent of policy implementation. In places with large fiscal shortages, the governments are much less likely to see shrinkage of their already low fiscal revenues. In counties with excessive officials, it’s more difficult to downsize the government and cut personnel expenditures. Thus, even if the tax reform can induce larger initial effects in these counties, without corresponding political reform to improve local governance, the destiny of the tax reform is not yet certain.

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Appendix:

A. Rural Taxation as Percentage of Income Before Reform

Province	1999	1997	1995
National	19.4	17	15.4
Shanxi	18.1	17.5	16.7
Jilin	19.9	21.8	20.2
Jiangsu	17.3	16.3	18.8
Anhui	18.1	17.7	15.6
Hunan	22.7	20.7	20.9
Guangdong	18.1	17.5	14.3
Sichuan	19.7	17.1	19.5
Gansu	17.5	19.3	16.1

Data Sources: Yifu Lin, Ran Tao, Mingxing Liu and Qi Zhang, "Urban and Rural Household Taxation in China: Measurement and Stylized Facts", CCER Working Paper, Peking University, 2002, pp.18.

B. Description of Main Variable

Variables	Description
Year	Year (1999-2003)
Pid	Province ID (Start from 1 to 6)
Id	County ID (Start from 1 to 501)
Farinc99	Farmer's income growth rate in 1999 (%)
GDP99	County's GDP per capita in 1999 (yuan)
Grain	Grain output (ton)
Reform	Dummy, county adopted tax reform=1
Coast	Dummy, county along the coast=1
Mount	Dummy, county in mountainous area=1
Distance	The distance from county to provincial capital (cm / 1:3,000,000)
Urin	Growth rate of urban net income (%)
Netinc	Growth rate of farmers' net income (%)
Fisexp98	County's fiscal expenditure in 1998 (10000 yuan)
Admin98	County's administrative expenditure in 1998 (10000 yuan)
Treat	Dummy, County that is selected to be experimental county
Fisrev98	County's fiscal revenue in 1998 (10000 yuan)
Adpercent	% of fiscal revenue over fiscal income
Remote	Dummy, =1 when distance is larger than its median
Bigfis	Dummy, =1 when % of fiscal revenue over expenses is larger than its median
Admin	Dummy, =1 when adpercent is larger than its median

C. Regression Models

Two Way Fixed Effects, county level:

$$Netinc_{it} = \beta_0 + \beta_1 Reform_{it} + \sum_{1999}^t \delta year_t + \beta_4 Grain_{it} + \alpha_i + u_{it}$$

Two Way Fixed Effects, province level:

$$Netinc_{igt} = \beta_0 + \beta_1 Reform_{igt} + \sum_{1999}^t \delta year_t + \beta_2 Coast_{igt} + \beta_3 Mount_{igt} + \beta_4 Grain_{igt} + \alpha_g + u_{igt}$$

Cross-sectional Regression on Time of Treatment:

$$Time_{ig} = \beta_0 + \beta_1 netinc_{ig} + \beta_2 gdp99_{ig} + \beta_3 adpercent_{ig} + \beta_4 distance_{ig} + \varepsilon_{ig}$$

Examine Pre-treatment Changes:

$$Netinc_{ig00} - Netinc_{ig99} = \beta_0 + \beta_1 (grain_{ig00} - grain_{ig99}) + \delta (Reform_{ig01} - Reform_{ig00}) + \varepsilon_{igt}$$

Separate Policy Dummies on A Year-to-year Base:

$$Netinc_{igt} = \beta_0 + \sum_{j=0}^4 \gamma_j Reform(j)_{igt} + \sum_{1999}^t \delta year_t + \beta_2 Coast_{igt} + \beta_3 Mount_{igt} + \beta_4 Grain_{igt} + \alpha_g + u_{igt}$$

Initial Factors' Impact on Reform:

$$Netinc_{igt} = \beta_0 + \beta_1 Reform_{igt} + \sum_{1999}^t \delta year_t + \beta_4 Grain_{igt} + \beta_5 Remote_{ig} * Reform_{igt} + \beta_6 admin_{ig} * Reform_{igt} + \beta_7 bigfis_{ig} * Reform_{igt} + \beta_8 remote_{ig} + \beta_9 bigfis_{ig} + \beta_{10} admin_{ig} + \alpha_g + u_{igt}$$

Note:

i: Variation in county level

g: Variation in province level

t: Variation in time

D. Hausman Tests: County Level and Province Level

County Level	FE	RE	Difference	S.E.
Reform	2.080395	-0.6223206	2.702716	0.3242053
Grain	9.78E-06	2.46E-06	7.32E-06	1.36E-06
Chi2=84.63, Prob>Chi2=0.0000				

Province Level	FE	RE	Difference	S.E.
Reform	1.885875	-0.5610348	2.4469098	0.2541043
Coast	-0.2057441	0.780421	-0.9861651	0.1737707
Mount	-0.7031512	-0.1900472	-0.513104	0.1017575
Grain	2.21E-06	2.20E-06	1.17E-08	2.52E-07
Chi2=109.50, Prob>Chi2=0.0000				

E. County Level Fixed Effect Regression Table

Variables	Net Income Growth	
	Model 1	Model 2
Reform	2.936***	2.080***
	(0.278)	(0.552)
Grain output		9.78e-06***
		(1.50E-06)
Constant	2.142***	-1.115**
	(0.181)	(0.566)
Observations	2450	2441
R-squared	0.054	0.077
Standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Note:

Model 1: Only reform as independent variable

Model 2: Add year dummy and grain output

F. Province Level Fixed Effect Regression Table

Variables	Net Income Growth			
	Model 1	Model 2	Model 3	Model 4
Reform	2.887***	1.886***	1.886***	1.886***
	(0.27)	(0.52)	(0.42)	(0.48)
Grain output		2.21e-06***	2.21E-06	2.21e-06***
		(0.00)	(0.00)	(0.00)
Coastal county		-0.206	-0.206	-0.206
		(0.37)	(0.42)	(0.22)
Mountainous county		-0.703**	-0.703	-0.703*
		(0.32)	(0.63)	(0.39)
Constant	2.165***	1.632***	1.632	-1.769**
	(0.17)	(0.41)	(0.90)	(0.72)
Observations	2450	2441	2441	2441
R-squared	0.046	0.057	0.057	
Standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Note:

Model1: Only reform as independent variable

Model2: Add year dummy, grain output, coast and mount as controls

Model3: Clustered standard error at province level

Model4: Newey-West standard error: Province ID dummy not reported

G. Determinants of Time to Treat

Variables	Year to Treat
Net income growth	0.0301***
	(0.01)
GDP in 1999	1.96e-05*
	(0.00)
Grain output	-1.98e-06***
	(0.00)
Admin expense/total expense	0.49
	(1.26)
Constant	3.990***
	(0.22)
Observations	400
R-squared	0.248
Standard errors in parentheses	
*** p<0.01, ** p<0.05, * p<0.1	

H. Test for Change in Pre-treatment Period

Variables	Difference of Net Income Growth	
	Model 1	Model 2
Adopted reform in 02	0.746	
	(1.603)	
Adopted reform in 01		-1.775
		(1.695)
Change in grain output	0.000136***	-1.11E-05
	(1.95E-05)	(1.25E-05)
Constant	0.596	-0.244
	(1.481)	(0.638)
Observations	360	407
R-squared	0.12	0.003
Standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Note:

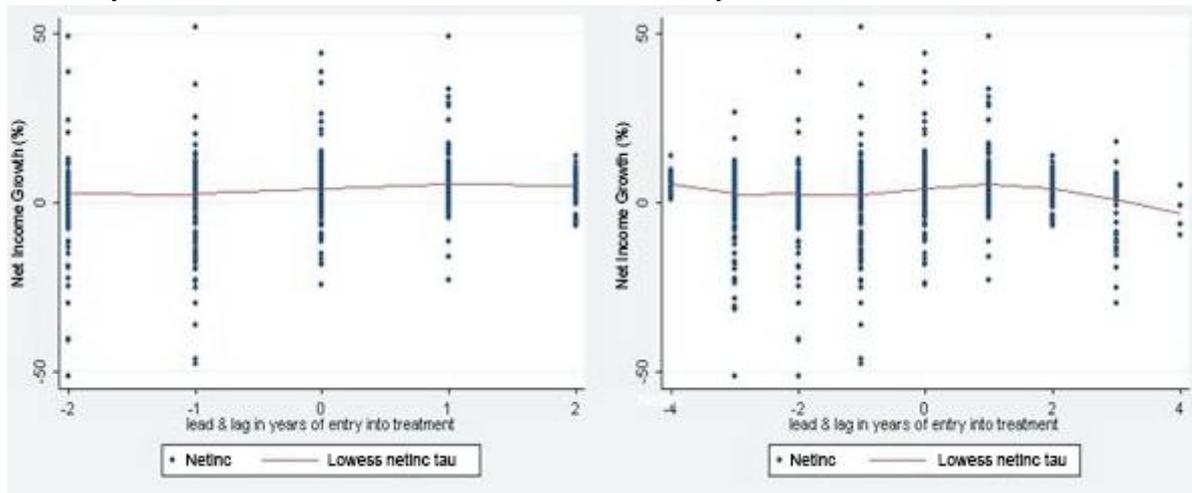
Model 1: Using 1999 and 2000 data to test whether there are differences in income growth between counties treated/not treated in 2001

Model 2: Using 2000 and 2001 data to test whether there are differences in income growth between counties treated/not treated in 2002

I. Lowess Net Income Growth with Leads and Lags of the Reform

Two years before and after the reform

Four years before and after the reform



Note: The y axis is measured in percentage term, thus a 2% change is not a clear trend.

J. Long Term Effects of the Reform

Variables	Net Income Growth
Reform year	1.367**
	(0.55)
1 yrs after reform	3.379***
	(0.74)
2 yrs after reform	3.497***
	(1.01)
3 yrs after reform	0.89
	(1.32)
4 yrs after reform	
	(2.63)
	(3.30)
Grain output	2.21e-06***
	(0.00)
Coastal county	
	(0.19)
	(0.37)
Mountainous county	-0.719**
	(0.32)
Constant	1.644***
	(0.40)
Observations	2441.00
R-squared	0.07
*** p<0.01, ** p<0.05, * p<0.1	
Standard errors in parentheses	

K. Urban Net Income Growth Regression Table

Variables	Urban Income Growth		Rural Income Growth	
	Model 1	Model 2	Compare with Farmer	
Reform	-0.595	1.403*	2.887***	1.886***
	(0.43)	(0.74)	(0.27)	(0.52)
Grain output		2.77E-08		2.21e-06***
		(0.00)		(0.00)
Coastal county		-0.0783		-0.206
		(0.63)		(0.37)
Mountainous county		1.269**		-0.703**
		(0.51)		(0.32)
Constant	13.38***	14.76***	2.165***	1.632***
	(0.28)	(0.61)	(0.17)	(0.41)
Observations	2014	2005	2450	2441
R-squared	0.001	0.096	0.046	0.057
Standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Note:

Model 1: regression with only income growth and reform

Model 2: regression using FE province level model

L. Check for Autocorrelation in Residuals:

Variables	ehat
Ehatlag1	-0.140***
	(0.0247)
Ehatlag2	-0.00237
	(0.0218)
Constant	0.0466
	(0.147)
Observations	1442
R-squared	0.022
Standard errors in parentheses	
*** p<0.01, ** p<0.05, * p<0.1	

M. Correct for Autocorrelation: Three Years and Two Years Data Regression

Variables	Net Income Growth	
	Model 1	Model 2
Reform	3.151***	3.229***
	(0.779)	(0.408)
Grain output	2.81e-06***	2.01e-06**
	(8.94E-07)	(9.77E-07)
Coastal county	0.463	0.607
	(0.496)	(0.594)
Mountainous county	-0.0762	-0.671
	(0.426)	(0.508)
Constant	1.112**	1.565***
	(0.469)	(0.5)
Observations	1469	980
R-squared	0.06	0.066
Standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Note:

Model 1: Using data of 1999, 2001 and 2003

Model 2: Using data of 1999 and 2003

N. Fixed Effects with Interaction Effects

Variables	Net Income Growth
Reform	0.94
	(0.70)
Remote*reform	1.665***
	(0.50)
Bigfis*reform	-1.320**
	(0.52)
Admin*reform	1.616***
	(0.51)
Grain output	2.00e-06***
	(0.00)
Coastal county	-0.18
	(0.40)
Mountainous county	-0.552*
	(0.33)
Remote county	-1.436***
	(0.35)
Big fiscal deficit county	0.476
	(0.37)
Big admin expense county	-1.391***
	(0.35)
Constant	2.832***
	(0.51)
Observations	2441
R-squared	0.074
Standard errors in parentheses	
*** p<0.01, ** p<0.05, * p<0.1	

O. Fixed Effects Results in Different Sup-groups

Variables	Net Income Growth					
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Reform	1.026**	2.777***	2.652***	1.421***	1.459***	2.311***
	(0.46)	(0.93)	(1.01)	(0.47)	(0.53)	(0.89)
Grain output	-3.38E-07	5.62e-06***	6.66e-06***	8.91E-07	6.03E-07	4.74e-06***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Coastal county	-0.13	-0.119	-0.305	-0.412	-0.218	-0.204
	(0.43)	(0.60)	(0.87)	(0.36)	(0.37)	(0.70)
Mountainous county	-0.508	-0.0627	-1.299**	0.343	-0.851**	-0.248
	(0.32)	(0.56)	(0.55)	(0.34)	(0.33)	(0.57)
Constant	3.457***	-0.668	-0.146	2.753***	3.199***	-0.28
	(0.35)	(0.78)	(0.77)	(0.39)	(0.41)	(0.74)
Observations	1209	1232	1221	1220	1215	1226
R-squared	0.055	0.068	0.08	0.056	0.043	0.076
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Note:

Model 1: Effect of reform on counties that are closer to provincial capitals (relative to median)

Model 2: Effect of reform on counties that are farther away from provincial capitals (relative to median)

Model 3: Effect of reform on counties with a smaller share of fiscal revenue over expenditure (relative to median)

Model 4: Effect of reform on counties with a smaller share of fiscal revenue over expenditure (relative to median)

Model 5: Effect of reform on counties with smaller share of administrative expense over fiscal expenditure (relative to median)

Model 6: Effect of reform on counties with bigger share of administrative expense over fiscal expenditure (relative to median)