Village Democracy: The Effects of Increased Accountability on Inequality and Production (Preliminary and Incomplete)

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Abstract

This paper uses a unique nationally representative data set of political reforms during 1980-2005 and a fixed effects strategy to study the effects of increased leadership accountability on economic and social outcomes in rural China. The results show that elections moderately reduced within village income inequality, increased taxes for the rich, had little effect on delivery to the upper levels of government, reduced the size of the government but increased wages of government personel, and reduced village disputes by more than 50%. However, elections also decreased gross income levels, with larger proportional decreases for richer households. It decreased gross income of the top 90th percentile by approximately 10%; and that of the median household by approximately 5%. Similarly, elections decreased consumption expenditure, with larger percentage reductions for richer households.

1 Introduction

This study investigates the degree to which accountability of leaders matters by studying the effects of local democratization in post-Mao China, 1980-2005;

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and the tradeoff between decreased economic production and increased income equality, expenditure in public goods and improvement in social contentment that resulted from democratization. The central government is ruled by the China Communist Party (CCP), which is considered by most observers an authoritarian regime. Wishing to avoid political unrest and increasing income inequality, the CCP introduced elections into Chinese villages during the 1980s. The first phase of the reform allowed controlled elections where candidates were nominated by the local branch of the CCP. This shifted accountability of the elected officials from being only accountable to the CCP to both the CCP and villagers. The second phase of the reform, called *haixuan* which literally means "an ocean of choices", opened nominations to voters. This further shifted the accountability of the elected officials so that they responded directly only to the villagers. Of course, the continued presence of the village party committee and the CCP's control over upper levels of government meant that accountability was never completely removed from the CCP.

The CCP faced several trade-offs. On the one hand, elections could help resolve information problems. There are well over 100,000 villages in China. Over 800 million individuals of more than 50 ethnicities reside in them. At least 30 major and mutually incomprehensible dialects are spoken. The level of economic development varied widely. The CCP lacked information on the policies each region required to reduce inequality, and the characteristics that make up an effective leader for each region. The first problem exacerbates the second in that the CCP cannot monitor the performance of local leaders if they don't know what the measurable goals should be. On the other hand, shifting the accountability of village leaders from the CCP to the villagers may also cause villages to implement policies against the interests of the center. For example, elected leaders may feel pressure to reduce the delivery of taxes to upper levels of government or to relax the enforcement of unpopular central policies such as the One Child Policy. Moreover, any reduction in inequality may distort incentives to produce and slow down economic growth.

This study attempts to estimate the effects of the electoral reforms on inequality, social and fiscal outcomes since the introduction of elections, and then of haixuan, gradually shifted accountability from the CCP to villages. The controlled nature of these reforms allows us to narrow down the number of potential channels that democracy typically operate through to one main mechanism: accountability. In addition to examining the reduced form effects, we will present evidence on the mechanisms through which inequality is reduced within villages.

Existing empirical evidence on the relationship between political institutions and economic outcomes mostly come from cross-country evidence. While these studies directly address the questions that we are interested in, the nature of cross-country comparisons also creates many difficulties of interpretation. First, it is not clear which institutions (e.g., elections, political competition, protection of property rights, constraints on the government, etc.) are responsible for any given effect. Second, in a cross-section of heterogeneous countries, political institutions cannot possibly be considered exogenous to economic circumstances and policies. The occurrence of elections and economic outcomes may be jointly de-

termined by larger political or social changes. During periods of political reform, for example, governments may simultaneously introduce elections and welfare programs to relieve poverty. In this case, the effect of electoral reforms will be confounded with the effect of other programs. Reverse causality is also a likely problem since economic outcomes can determine which political institutions are adopted. For example,countries that experience faster economic growth may be more likely to choose democratic institutions. If this is the case, then a correlation between economic growth and democratic institutions will not be able to disentangle the impact of the institutional change on economic outcomes from the effect of economic outcomes on the adoption of good institutions. Finally, countries are heterogeneous social and economic objects. And the small sample sizes caused by most country level data sets make it difficult to implement fixed effects strategies.

There are very few within-country studies that directly analyze the effects of electoral accountability on economic outcomes and policies. For example, studies in the U.S. have gauged the effect of accountability by comparing elected officials who face term limits with those who do not (Besley and Case, 1995; Daniel and Lott, 1997). In developing countries, Olken (2007) compares the effect of bottom-up versus top-down accountability on corruption in Indonesia. And more recently, a growing number of studies have examined the effects of the village reforms in China which produce somewhat mixed findings on the effects of electoral reforms. Zhang et al. (2004) uses a differences-in-differences strategy with panel data of sixty villages from two provinces and finds that elections have little effects on village government revenues but shift the distribution of taxation from individuals to enterprises; and that elections and power sharing are conducive to improve the allocation of public expenditures. Alternatively, other studies have used a panel of 48 village in eight provinces that are a part of the Ministry of Agriculture's National Fixed Point Surveys (NFS) in combination with a household level retrospective survey conducted by the authors. Wang and Yao (2007) finds that elections are found to substantially increase the share of public expenditures in the village budget, but reduce the shares of administrative costs and income handed to the township government. They found no effects on tax revenues. Nor did they find differential effects for close (competitive) elections. Shen and Yao (2008) finds that elections reduce the Gini coefficient by 0.04, or 14.3% of the sample average and increase the income shares of poorer portions of the population. Li, Xu and Yao (2006) finds that villages are more likely to establish a health care plan after the election is introduced. In addition, village elections reduce the probability of a household to borrow by 16.7% when one of its working adults is seriously sick. In a follow-up study, Li, Xu and Yao (2007) find that consumption insurance for poor and middle income households is more complete when the households live in villages with elected village leaders.

All of these studies, with the exception of Olken (2007), face the empirical difficulty of establishing causality because term limits and electoral reforms were not randomly assigned. This makes them susceptible to omitted variable bias problem (OVB). For example, in China, the introduction of elections and

decrease in inequality may both be outcomes of changes in village income levels. A few previous studies have devised creative ways of addressing this problem. Shen and Yao (2008) and Li, Xu and Yao (2007) instrument for the timing of elections in villages with the timing of elections in provinces. However, causal interpretation of these estimates is made difficult by the fact that there are only eight provinces in their sample, which is unlikely to satisfy the asymptotic properties of 2SLS. They address this by also instrumenting with the interaction of provincial implementation and measures of heterogeneity in surnames within each village. They find that higher heterogeneity leads to earlier elections conditional on the province having allowed any elections. However, this strategy is potentially problematic since the heterogeneity of surnames will most likely affect income inequality through other channels. In addition to instrumental variables, these studies and Wang and Yao (2007) also use a simpler village fixed effects specification which they find to produce similar estimates as the instrumental variables strategies.

In our study, we use the more transparent fixed effects strategy. We compare outcomes before and after their first election (or haixuan) between villages that have had the reform and those that have not. This is similar in spirit to a differences-in-differences strategy. Village fixed effects control for all time invariant differences across villages and year fixed effects control for all changes over time that affect villages similarly. Identification relies on the trend break in the coefficients we estimate around the time of each reform. We are able to address some concerns of the omitted variables bias and reverse causality problems by showing that there were no pretrends for the years leading up to the first election in income levels or inequality. Our strategy would be called into question if a reform is introduced at exactly the year when the outcome of interest changes for reasons other than the reform. This is by no means a perfect strategy for resolving identification issues. We choose this strategy over alternative ones because there are no ideal instruments, and the caveats for interpretation are most transparent with this one.

We conducted a survey of 266 nationally representative villages in 26 provinces on the timing and implementation of electoral reforms of each village. This data allows us to be the first to carefully document the reforms for a broad cross section over a long time horizon. Amongst other variables, our data includes the personal characteristics of the village chief and the village party secretary, and powers held by each office. The reform happened in two phases. On average, the first haixuan was implemented nine years after the first election. Every village has a village chief and a village party secretary. Elections were only implemented for the position of the village chief. Party secretaries have always been and still are appointed by the upper level CCP. Each reform was implemented in a top-down fashion and the timing of each varied across villages. We match our survey data to contemporaneously collected data for the same villages from the NFS for 1987-2005 that contains data on economic and social outcomes.

Our study improves upon past studies in having much more breadth of data. This allows us more statistical power for examining the effect of elections on village level outcomes such as inequality; and exploring the mechanisms underlying

the effects of elections. Moreover, our data is the first to be nationally representative. Therefore, the results will be able to speak to the average effect of elections for China.

The main results of this paper show that controlled elections, which shifted accountability from only to the CCP to both the CCP and voters, had moderate effects in reducing within-village income inequality. For example, elections increased gross income for the median household relative to that of households on the 90th percentile of the income distribution by approximately two percentage-points. For net income (gross income minus taxes, fees, levies, and production costs), elections increased the ratio by 1.4 percentage points. These results can be explained by a model where the median voter values income redistribution or a model where the income generating activities of the rich impose negative externalities on the median voter. For example, the enterprises of the rich pollute the agricultural lands of the rest of the village, or if it takes up land that could otherwise be used for grazing or recreation or some other activity valued by the median voter.

To investigate this, we examine the effect of elections on income levels across the income distribution. Interestingly, we find that elections reduce income for all households, but the reduction is larger for the rich. For households in the top 90th percentile of the gross income distribution within villages, elections decreased gross income by approximately 10% and net income by approximately 7.5%. This is approximately twice and three times the size of the reduction for households on the 50th and 10th percentile of the income distribution. Note that income is increasing at approximately 10% per year for households across China on average during this period. Therefore, our results say that elections cause income for households on the 50th and 10th percentile of the income distribution to increase less. And for the top 90th percentile, elections on average cause incomes to stop growing. These results suggest that redistribution is not an important motivation for the median voter in our context. This is consistent with the finding that elections have no effect on the amount of taxes and fees paid by rich households, which would be necessary for redistribution.

There is an important caveat to these results. If rich villagers think that their neighbors wish to tax their incomes, they may hide their incomes and under-report income to the survey. In this case, the estimated effects of elections in a reduction in inequality and income for the rich would be due to systematic under-reporting rather than the election. Of course, it is harder to explain the result that elections also decrease income levels for households on the bottom 10th percentile of the distribution with this reasoning. To be cautious, we explore this possibility by estimating the effect of elections on consumption. We find that elections cause a decrease in consumption that parallels the decrease in income. There is a large and statistically significant decrease for households on the top 90th percentile. A much smaller but statistically significant decrease for households on the 50th percentile. And an even smaller but insignificant effect on households on the bottom 10th percentile. These results go against the hypothesis that are results are driven by systematic under-reporting of income.

We find that the introduction of open nominations had no effect in addition

to the effects of controlled elections. This suggests that the continued presence of the communist party in village and upper level governments meant that accountability was never completely shifted away from the party; and/or that the party nominated candidates before the introduction of haixuan were not drastically from who the villagers would have nominated themselves.

These results can be explained by a model where the central planner, the CCP, disproportionately emphasized income growth as targets for local leaders. This was a period of rapid growth an average. Moreover, income is easy to measure and observe. Before elections, village leaders prioritized policies that promoted economic growth over policies that promoted other factors that the median voter valued; and the growth generating opportunities both provided income for villagers and imposed negative externalities. Therefore, elections caused policies to shift away from the complete focus on economic growth towards the preferences of the median voters. An example that often arises from anecdotal evidence is about village enterprises that increase incomes for the whole village but also cause environmental pollution. The results imply that elections caused the closing of the polluting enterprise at the cost of incomes for the whole village, but more for the rich. We are currently collecting quantitative and qualitative data to investigate such channels. Interestingly, the hypothesis that voters are not less happy with the reduction in income is consistent with the finding that elections decrease the number of disputes within a village by over 50%.

These preliminary results show that accountability of the leadership has significant effects on economic outcomes. To judge whether this was beneficial for the CCP, one must weigh the disadvantage of slowing down rural economic growth against a reduction in inequality and an increase in the content of villagers. The results show that elections had no effect on the delivery of taxes to the central government. This is presumably in part because the CCP continue to exert direct influence in village politics through the village party secretary. Without knowing the cost of achieving the same re-distribution and decrease in village conflicts through other policies, we cannot conduct any cost benefit analysis. Assuming that the only loss to the CCP is in revenues in terms of GDP, we can calculate the cost of this reform. In 2000, 40% of Chinese GDP and 15% of GDP growth came from rural areas. Our estimates suggest that elections on average decrease total village incomes by up to 5%, 2% of Chinese GDP.

This study contributes to several branches of the existing literature. First, as a study of political economy, it is novel in exploring the role of elections in an authoritarian regime. While there are many institutional factors specific to the context of post-Mao China, the results provide evidence for several generalizable insights: 1) democratically elected leaders favor redistribution; and 2) even in non-democratic regimes, lower level elections can be meaningful and they have a significant impact on both economic and political outcomes. Finally, it adds to our understanding of the process of democratic deepening in China, presumably the largest grass-roots electoral reform in history. Unlike previous studies, we consider the different phases of the electoral reforms separately. And the breadth

of our data allows us to shed some light on the mechanisms underlying the reduced form results and directly investigate the effects of the elections on the CCP.

The paper is organized as follows. Section two describes Chinese villages and the electoral reforms. Section three presents a simple model of the village. Section four presents the empirical strategy. Section five describes the data. Section six presents the empirical results. Section seven offers conclusions.

2 Village Elections

In this section, we describe the powers of the village government and provide a brief history of the electoral reforms. The village government comprises of a committee, the head of which is the village chief (often called chairman or chief); and the CCP party committee, the head of which is the secretary. The electoral reforms only apply to the committee.

Villages are not considered an official level of government. Nor are they fiscal accounting units. However, in practice, villages are fiscally autonomous and village leaders control the revenue and expenditure decisions at the local level. Villagers pay their agricultural taxes directly to a higher level of government, whether the township or the county level. Villages are not allocated any tax revenues, but obtain their resources from collectively owned property and enterprises and from ad hoc surcharges know as *tiliu*. Almost all public goods in rural villages must be financed by the villagers themselves from these sources. Thus, when we compare results in tax receipts or in public goods provision across villages there are no fiscal spillovers that can confound the comparison. Village officials' salaries are also paid from these funds. Very few transfers are made from upper levels of government or across regions. According to our data, only 3.2% of total village revenue is from upper levels of government. See Oi (1999) for a description of the fiscal structure of villages. In this way, even though we are comparing units within a country, the results we obtain can shed light on relative performance across countries.

To the best of our knowledge, there is little documentation of the fiscal arrangements between the village and the upper levels of government which affects a few public goods such as schools and roads, and more generally in the power village leaders had. Such arrangements will affect the extent to which elections can affect outcomes. To address this, we conducted focus groups in villages in Gansu province during the summer of 2006. The focus groups comprised of all present and past village leaders and village accountants in the village. They discussed the roles that leaders played and their experiences with elections. We summarize the relevant findings below.

Village governments were first organized by the communist government during the land reforms of the early 1950s. Each village has two groups of leaders. The village chief (who is often also called the village chairman) leads a village committee typically comprised of three to five members. This group is supposed to be democratically elected by the village. However, with the exception of the

early 1950s, there were no real elections until the reforms described in this study. Leaders were appointed by the party. The second group of leaders comprise of the village party members (cadres). They are led by the village party secretary, who is appointed by the county level party. Villages do not have police or official judicial systems for solving disputes. The village committee and party are the only source of law enforcement and problem resolution between villagers.

Village governments may not be in the position of making large fiscal investments (either because of the lack of funds or political constraints from upper levels of government). But a democratically elected leader with a popular mandate may have a different effect on coordinating villager support for a project relative to an appointed leader. Anecdotally, all village leaders are supposed to work together for a variety of tasks. This includes solving disputes amongst villagers and coordinating villagers for public projects. One common public project undertaken during the period of our study is road construction. The village government is responsible for all funding of roads within a village. They are also responsible for a part of the construction of roads that connects the village to the main through-ways constructed by upper levels of government. The village must contribute labor and sometimes even money for materials. All the villages we interviewed had chosen to construct the road. The decisions are made in meetings that are open to all villagers. The villagers did not have a formal process of voting on decisions. Rather, all decisions were achieved by discussion both in the meeting and outside until a consensus is achieved. In each case, the village leaders were responsible for coordinating meetings and mediating between differing opinions outside of meetings. In the case of schools, through most of the period of this study, villagers typically had to provide labor for constructing and repairing the school, as well as for raising funds to pay the teacher. Only the construction materials were provided by the upper levels of government. In cases of limited public goods such as irrigation, the village leaders need not only to coordinate their construction, but also the distribution of resources across households.

Disputes are typically solved by mediation by the village chief and party secretary. If a villager is dissatisfied with the outcome of the policies of his village leaders, his only official venue of appeal is to county-level party cadres.

2.1 Electoral Reforms

Studies of village reforms typically focus on the *Organic Law on Village Committees* (OLVC) which was nationally introduced in 1987. However, as our data will show, elections occured as early as 1982, at the very beginning of the post-Mao reform. As with almost all reforms in Post-Mao China, elections spread slowly across China, until its reasonable "success" caused it to be recognized by the central government.

In deciding whether or not to implement the OLVC, the CCP faced a trade-off. Village leaders were responsible for providing public services within the village. These services were typically under-provided or non-existent, either because the lack of accountability to villagers decreased village leaders' incentives

to respond to local needs, or because the appointed village chiefs lacked the mandate necessary to raise the necessary revenues from villagers. On the one hand, elections, by improving leaders' accountability to and mandate from villagers, were expected to increase the revenues raised within the village to meet such public good expenses. Elections also had the benefit of partly relieving the CCP from the burden of choosing the correct leader or understanding the specific needs of each of the hundreds of thousands of villages in China. On the other hand, many were concerned that without the power of appointing village leaders, the CCP would have little leverage with these leaders to implement and ensure compliance with centrally mandated policies. Some of these policies, such as the One-Child Policy, were quite unpopular among villagers.

In 1987, the CCP decided to implement the OLVS. It established a democratically elected village committee as the governing body of the village. The entire adult population obtained the right to vote for the committee, which consisted of a chairman (village head), a vice-chairman, and three to five other members. Unlike Maoist period elections, the number of candidates in the post-OLVC elections were supposed to exceed the number of seats; and the candidates were supposed to be nominated by the villagers themselves. To supervise the village committee, villagers were required to set up a village assembly. The candidates were typically appointed by village, county and township level party branches. As long as the number of candidates exceeded the number of positions, the OLVC was satisfied.

All villages which had not already held elections were supposed to implement them eventually. Provincial governments were given a large window of time to ensure that their villages complied. The reform was implemented gradually through the late 1980s and 1990s. By 1998, the Ministry of Civil Affairs (MoCA) reported that over half of the villages had conducted competitive elections with more candidates than posts, and more than 70% had at least some kind of elections.

The next phase of the reform occured in 1998, when the OLVC was revised and reinforced to specifically address the importance of open primaries, commonly called *haixuan*. Before this law was passed, very few villages had open nominations. For example, O'Brien and Li (2000) find that in 2000, only 17% of the villages in their survey had open nominations. After the revised law was passed in 1998, open nominations were rapidly introduced. Pressure from the central government and from villagers helped the expansion of procedurally correct elections with open primaries (Pastor and Tan, 2000). Some subsequent evaluations argue that the reforms were successful in introducing democratic elections (see for instance Xiang, 2000).

The local Communist Party branches persisted despite electoral reforms. The 1998 Law still defines the Party Branch as the "Leadership Core" of the village. However, the OLVC has weakened their influence in two ways. First, the decisions are now formally in the hands of the elected village committee. Second, in cases where party branch heads and elected village leaders disagree, public opinion makes it harder for the former to overrule democratically elected leaders, relative to leaders appointed by upper levels of government. Guo and

Bernstein (2004) and Oi and Rozelle (2000) provide analyses of these power struggles.

2.2 How Would Elections Matter?

In the context of rural China, increased electoral accountability can affect policies and outcomes in two basic ways. First, accountable leaders can be replaced by their constituencies. This typically means that constituents will choose leaders who are more compatible or more competent. If villages have better information on these attributes than party officials, they should be better at spotting the relevant types. This is the selection effect. Second, increased accountability changes the incentives of the leader by constraining his actions if he wants to remain in power. This is true even if the actual leader is not replaced when elections are introduced since he might want to work to ensure his re-election. A typical outcome of this channel would be a reduction in corruption or bureaucratic "slack" and an increase in responsiveness to villagers' preferences. This is the incentive effect.

The effects of having a democratically elected leader on the ability to coordinate villagers are ambiguous ex ante. On the one hand, leaders have a popular mandate. On the other hand, they may be swayed by electoral pressures, or they may be strongly influenced by voting blocks. Our qualitative research suggested that this is potentially a problem in villages with a dominant clan. The leader is either from that clan or influenced by that clan which can result in bullying of other villagers. In cases like this, it is the job of the party secretary to resolve the dispute or to remove the village chief. But the party secretaries we spoke were reluctant to remove a democratically elected leader for fear of damaging the popularity of the party amongst villagers. This example illustrates both the potential problems of democracy and the fact that these elections seemed to have been effective in "checking" the party. Note that this is why we do not follow existing studies in using heterogeneity of surnames as instrumental variables.

The effect of elections on dispute resolution is also ambiguous. On the one hand, a democratically elected leader has a popular mandate and could potentially mediate more effectively. On the other hand, villagers may view the elected leader as being more detached from party officials, and therefore having fewer means to exert authority over them. Hence, they may be more likely to ignore the opinions of the village leader and appeal to the county-level party.

3 A Simple Model of Village Leadership

A village is a continuum of individuals with measure 1. Each individual i is characterized by a skill level, θ_i. Individuals care about their income y_i and about public goods in the village, g. We can understand g in a very general way (good environment, health insurance, good dispute-management, etc.). Note

that g impacts individual's utilities directly, and not through income. Individual's utility function is simply

$$u_i(y_i, g) = y_i + g$$

The village leadership, denoted by L is modelled as a unitary agent even though it is an amalgam of the party secretary, the party cadres and the (previously not elected, afterwards elected) village leader and village committee members.

L has 1 unit of effort to put in two tasks. He devotes e to generate opportunities for growth in the village, and devotes $1-e$ to generate public goods g. For simplicity, let us assume the following production functions:

$$\begin{aligned} y_i &= \theta_i e \\ g &= G(1-e) \end{aligned}$$

where $G(.)$ is increasing and concave.

The village leader, L, has the mandate to implement growth enhancing policies in order to maximize growth. This reduced form specification could be capturing a number of things.

1. The villager leader is an agent of the CCP, which values growth per se
2. Growth is easier to measure relative to other things that the CCP may also care about such as the happiness of villagers.
3. Local leaders may like growth because it could increase their scope for skimming funds.[1]

Hence, we can model L's decision as follows

$$\max_{e\in[0,1]} \mu\bar{y} + (1-\mu)\, u_m$$

where $\bar{y}$ is average income (whether it is average or total or L's own income is not really important) and u_m is the utility of the median voter.

We know that the process of instituting elections has been very complex, and the final level of accountability varies across villages and provinces. However, it seems difficult to contest that the elections should result in an increase of the voice of the median voter. Hence, in the formulation above, initiating elections should be equivalent to a drop in μ (i.e. the L gives more weight to the median voter).

Solving the model:

$$\begin{aligned} \max_{e\in[0,1]} \mu\bar{y} + (1-\mu)\, u_m &= \\ & \max_{e\in[0,1]} \mu e\bar{\theta} + (1-\mu)\left[e\theta_m + G(1-e)\right] \\ G'(1-e) &= \frac{\mu}{1-\mu}\bar{\theta} + \theta_m \end{aligned}$$

[1] In the context of the model, it would probably be equivalent if L has a very high θ_L.

hence clearly e is increasing in μ.

In this (extremely simple) model, when villages democratize we should observe:

1. A reduction in effort devoted to growth
2. An increase in effort devoted to public goods

This very simple model, highlights that when there is a shift in the village leader accountability, away from the CCP and towards the median voter, the policies implemented tend to appease more the median voter. Finally, notice that, to the extent that the median voters has a preference over inequality, the level of inequality in the village could be also included in the parameter g. Therefore our result of the reduction of inequality could be also interpreted along the lines of this model.

4 Empirical Strategy

A village's exposure to electoral reforms is determined by whether it has ever experienced a reform, and the year of its implementation. The estimation will control for village fixed effects and year fixed effects. This strategy is similar to simple differences-in-differences (DD) in that all differences between villages that do not change over time are controlled for by the between-village comparison, and all changes over time that do not differ across villages are controlled for by the across-year comparison. Unlike DD estimates, these estimates allow the effect of electoral reforms to vary by the years since implementation. Hence, to the extent that the effects of electoral reforms are not equally realized in the years after implementation, we will be able to identify this. Finally, in the regression estimates, we can add province*year fixed effects, which will further control for changes over time that differ across provinces.

The identification relies on a break in the trend of outcomes for villages on average at the time when elections are introduced. Therefore, we first estimate the effect of electoral reforms for each year before and after the first election. We do this separately for the introduction of elections and the implementation of haixuan.

$$Y_{vpt} = \sum_{\tau=-3}^{T} \beta_\tau yrs\_to\_reform_{vp\tau} + \gamma_v + \rho_t + \varepsilon_{vpt} \tag{1}$$

The outcome in village v of province p in year t is a function of: the dummy variable for the number of years since the reform, $yrs\_to\_reform_{vp\tau}$; village fixed effects, γ_v; and calendar year fixed effects, ρ_t . The reference group comprises of observations for four or more years before the first reform. T is the maximum number of years after the first election for any village in our sample. To control for serial correlation of the residuals within villages, we cluster the standard errors at the village level. β_τ is the effect if the reform τ years since

the reform. If the reform had an effect, then β_τ should be constant prior to the reform, $\tau < 0$, and then different from zero after the reform, $\tau \geq 0$.

The yearly estimation allows us to verify that the effects on outcomes occur during the election and that we are not just capturing spurious changes during the pre and post periods. It also allows us to examine pre-trends. One concern for the identification strategy is that elections were implemented in villages where the elites were loosing power for other reasons. In that case, the effect of elections on reducing inequality will reflect the impact of these other factors rather than a causal effect of elections on inequality. To the extent that these omitted variables are reflected in pre-trends in the years leading up to the first election, we can assess their significance.

To assess the magnitude of the effect and the average statistical significance, we estimate a simpler differences-in-differences specification.

$$\begin{aligned} Y_{vpt} &= \beta post\_election_{vp\tau} + \theta post\_haix_{vpt} && (2) \\ &\quad + \mathbf{X}_{vt}\alpha + \delta_{pt} + \gamma_v + \rho_t + \varepsilon_{vpt} \ / && (3) \end{aligned}$$

This is similar to equation (1). The only differences are that we are now estimating the effects of both reforms in one equation and have grouped all the years prior to the reform into a pre group and all the years after the reform into a post group. The reference group comprises of all villages before the first election. We continue to control for individual year and village fixed effects. $post\_election_{vp\tau}$ takes on a value of one for all the years after a village has implemented its first election. $post\_haix_{vpt}$ takes on a value of one for all the years after a village has implemented its first haixuan. Note that villages only implement haixuan after their first election. For robustness, we can also control for a vector of time-varying village characteristics such as village income, $\mathbf{X}_{vt}$; or province $\times$ year fixed effects, δ_{pt}. β is the effect of the elections relative to when there are no elections, and θ is the effect of haixuan and elections relative to when neither exists since haixuan can only occur together with elections. If haixuan has effects beyond the effects of the elections, then $\hat{\theta} > \hat{\beta}$.

The main caveat for interpreting the estimates as causal is that implementation of the reforms at the village level is potentially endogenous to unobserved characteristics that are correlated with the outcomes of interest. For example, if villages in need of raising large tax revenues chose to democratize earlier than other villages, then simple fixed effects estimation will overestimate the positive effect of democratization on tax revenues. To address these concerns, we examine the pretrends in these characteristics for the years leading up to the first election. For robustness, we also control for *province* $\times$ *year* fixed effects. Since they do not affect our estimates, we do not report them in the paper for the sake of brevity.

5 Data

This study uses data from two sources. The first one is a unique survey collected by the authors. We collected a retrospective survey of the political reform histories of 266 villages from 1980-2005. The survey asked present and former village leaders to meet in a local school room. Together, with the help of professional surveyors, they filled out a questionnaire of the years of when elections and haixuan were first implemented, the years when elections were held, the number of candidates for each election, personal characteristics of the village leaders and the powers of each office.[2] In most cases, recalling these data was not a problem. Most villages were able to retrieve village records for documentation. The sample of the villages were chosen to match the second source of data, the National Fixed-Point Survey (NFS).

The NFS is collected and maintained by the RCRE, a research division of the Ministry of Agriculture. It is a longitudinal survey of about 320 villages and 24,000 households distributed across all continental Chinese provinces. The NFS began in the mid-1980s. The villages were chosen in the early 1980s to be nationally representative. According to the RCRE, there has been no attrition except in the cases of administrative mergers at the village level and deaths at the household level. Villages and households are surveyed every year. The survey used a stratified sampling approach. For each province, it first randomly selects a number of counties, and then randomly selects a number of villages within each county. Households are then randomly selected from each village. For this study, they shared with us 30% of the variables from their village-level data for 26 provinces for all of the available years, 1987-2005. We did not apply for the earlier years of the survey because changes in survey techniques made the data difficult to compare over time. Within the 26 provinces, we use all 266 villages in the NFS. The number of surveyed households per village ranges from approximately 7 to 90. The RCRE village-level survey contains eight sections: 1) population, households, and local organizations; 2) the labor force; 3) land; 4) fixed-capital assets; 5) agricultural production and sales; 6) total income and expenses; 7) village fiscal revenues and expenditures; and 8) other social indicators (e.g., crime, religious participation, etc.).[3] Figure 1 maps the counties for which we have NFS data.

There are several key advantages of this data. First, the RCRE panel data is reported contemporaneously. This avoids measurement error that would arise

[2]For personal characteristics of the village chief, the village party secretary and the village accountant, we asked for age, sex, level of education, whether he/she belonged to a family that owned land before the communist land reforms in the early 1950s, whether that individual was persecuted during the Cultural Revolution, *pidou*. For power, we asked them to check a box indicating if the village chief, secretary or accountant's signature was necessary for employing village personnel, or spending money from village funds. We also ask the villagers to recall the method of the election (e.g. anonymous ballot). Documentation for this data can be seen at

http://www.econ.brown.edu/fac/Nancy_Qian/Papers/Village%20Democracy.htm

[3]Samples from four provinces of the NFS have been used in studies by Benjamin et al. (2005), de Brauw and Giles (2006), Giles (2005), Giles and Yoo (2006) and Shen and Yao (2008).

from using retrospectively recalled data. Second, the panel structure of the survey allows us to control for village fixed effects. There is very little attrition of households. Changes in the composition of the data over time are mainly due to village mergers that took place during the late 1980s and early 1990s. Villages that experienced administrative mergers are not in our survey sample or the sample we obtain from the NFS. Third, the long time horizon allows us to examine long run outcomes. Finally, the richness of the RCRE data allows us to explore mechanisms that underlie our reduced form effects.

We our survey data to the NSF data at the village and year level. Thirteen villages are dropped because of data entry mistakes. Our final sample comprise of 217 villages. The political data spans 1980-2005 and the economic and social outcome data from the NFS span 1987-2005. The NFS was not collected in 1992 or 1994. For those years, we imputed values that were the averages of 1991 and 1993, or 1993 and 1995. In addition to the village level data, we obtained yearly household level data on gross and net incomes. We use this to calculate mean income and Gini coefficients, as well as the incomes on different parts of the village income distribution. Comparisons of the net and gross incomes also allow us to compute the amount of taxes that households paid.

Table 1 shows how many villages had already held their first election or haixuan by year. As we can see, most villages implemented elections during the late 1980s. On average, the first elections with haixuan were implemented during the late 1990s. By 2005, all 217 villages in the sample had implemented elections and 132 of the villages had haixuan. On average, the first haixuan follows the first election by approximately nine years.

Table 2 shows the descriptive statistics. Panel A shows the demographic composition of the villages. On average, there are approximately 420 households per village. Each household has approximately young child and two laborers (working age adults). Approximately 20% of the villages are high school graduates. 50% of households are engaged solely in agriculture. On average, each village has approximately nine disputes per year. We define disputes as non-criminal safety violations. This includes fights and any disturbances of public peace. This is our only measure of social stability in the village.

The NFS reports gross and net income per household. Gross income includes income from all activities, including remittance payments from household members that have migrated away. Net income is income net of taxes and fees paid out. Panel B shows the household net income distribution in villages. On average, mean village income is growing at 13% per year.[4] The average household on the bottom 10th percentile of the village income distribution is approximately 3,044 RMB. It is less approximately 45% of the median income (6,853 RMB), which is approximately 53% of the top 90th percentile income (14,157 RMB). We calculate total taxes paid by households as the difference between gross and net incomes divided by gross income. This includes taxes paid to the central government (collected by the village government) and fees paid to the village

[4]Inflation is extremely low during this period in China so we report all income in nominal terms.

government for village expenditures. Households on average pay 36% of their gross income as taxes.

Panel C shows the characteristics of the village government. Villages have on average five members on the administrative committee (including the village chief), and four members on the party committee (including the party secretary). The Village chief is on average 42 years of age, has nine years of education (equivalent to a middle school graduate), and is in office for seven years. Approximately 20% are from former land-owning families. Our definition of a land-owning family is a family "middle-rich" farmer who farmed his own land during the initial land reforms during the 1950s. Party secretaries are on average 45 years old, in office for approximately ten years, and have nine years of education. Approximately 17% of party secretaries come from land-owning families.

Table 3 reports average village government revenues and expenditures. On average, village governments have revenues of approximately 490,677 RMB. The majority of revenues, approximately 55%, come from collective production, and spproximately 21% of this comes from households. A similar proportion come from other sources. Expenditures are on average 470,056 RMB. The biggest expenditure is on collective production. Approximately 10% is delivered to upper levels of government in the forms of levies and taxes. And 7% is spent on village administrative expenditures. This mostly comprises of salaries to the government personnel (e.g. administrative and party committees and accountant). On average, a village has 11 members on the party and administrative committees and one village accountant. Therefore, the average salaries for these 12 members of the village government is approximately 2,762 RMB, approximately 40% of the median gross household income.

6 Results

6.1 The Effects on Income and Consumption

First, we estimate the yearly effect of elections on the difference in income between the top 90th percentile households and the median households, $HHInc_{vpt}^{50p} - HHInc_{vpt}^{90p}$, and the top 90th and the bottom 10th percentile households, HHInc_{vpt}^{10p}-HHInc_{vpt}^{90p}, for each year. For brevity, we only report the estimates for these outcomes. The estimated coefficients for the vector of $\hat{\beta}s$ from equation (1) are plotted in Figures 2A and 2B. The coefficients are reported in Appendix Table A1. The figures show that there is a clear trend break at the time of the first election and no evidence of a pre-trend. Since HHInc_{vpt}^{50p}-HHInc_{vpt}^{90p}<0 and $HHInc_{vpt}^{10p} - HHInc_{vpt}^{90p} < 0$, the positive coefficients for the years after the first election means that elections reduce inequality. The finding that the magnitude of the coefficients increase over time suggests that successive elections further reduce inequality (at least for the first two or three elections).

Table 4 shows the main estimates for the effects of elections and haixuan on inequality. Panel A shows that elections reduced the Gini coefficient of

gross incomes by approximately 0.01. Columns (2), (4) and (6) show that in RMB terms, elections reduced the gross income distance between the 10th and 90th percentiles by 5,752 RMB, between the 50th and 90th percentiles by 4,999 RMB, and between the 10th and 50th percentiles by 754 RMB. In terms of ratios, columns (3), (5) and (7) show that elections increased the ratio of 10th to 50th percentile incomes by 1.4 percentage-point and the ratio of 50th to 90th percentile incomes by 2.2 percentage-points. These estimates are statistically significant at the 5% and 10% levels. The estimates for haixuan are similar in magnitude but typically not statistically significant. Panel B shows that elections also decreased net income inequality within villages. But the reductions are smaller in magnitude both in terms of levels and ratios than the reduction in gross income inequality.

Elections could decrease inequality because the median voter values income re-distribution or because she values public goods. To explore this further we estimate the effect on taxes and income levels across the distribution.

Villagers pay taxes and fees. Villages do not officially have the power to change taxes, which are paid to upper levels of government. Typically, to raise revenue, village governments impose fees. This was made illegal by the *Tax and Fee Reform* in 2003. However, anecdotal evidence suggests that their collection was continued in practice. Some production costs (e.g. use of collectively owned assets such as tractors or other machinery) are also paid to the village. Our anlaysis will use three different measures to proxy for the effective tax rate. First, we use the measures of taxes and fees as reported in the NFS survey. Fees are the sum of those delivered to the township, village, and production groups.[5] Second, we use a measure of imputed taxes, fees, and production costs. This is the difference between a households gross and net incomes divided by gross income. This measure has the benefit that does not rely on accurate reporting of taxes and fees paid (which could potentially be systematically under-reported after fees are nominally abolished). For this exercise, we estimate the effect of elections for the village mean, and the mean for households with gross incomes below the 25th percentile of the village income distribution, between the 25th and the 50th, the 50th and 75th, and above the 75th. The results are shown in Table 5. There is no evidence that elections affected taxes, fees or production costs. The estimates are all small in magnitude and statistically insignificant. Therefore, we conclude that the reduction in inequality is not due to redistributive tax policies.

Table 6 shows the effect of elections on gross and net income levels. Columns (1)-(3) shows that elections decreased gross income more for richer households. The estimate for the 10th percentile is negative, small in magnitude and insignificant. For the 50th and 90th percentile households, elections decreased gross income by 4.7% and 10%. These estimates are statistically significant at the 10% and 1% level. Columns (4)-(6) show that elections also decreased net incomes for all households. Only the estimate for households on the 90th

[5]These include fees paid to as collective levies, miscellaneous fees and fines, paid to contracted businesses.

percentile is statistically significant (at the 5% level). It shows that elections decreased net income by 7.5%. Columns (7)-(9) show the estimates for the effects on annual growth. Despite the statistical insignificance of the estimates, the strong pattern of the coefficients suggests that growth is declining for the higher income households. The neglible effect of elections on tax rates suggests that this decrease in gross income is not caused by under-reporting.

One concern over the intrepretation of the results is that households, in fear of progressive taxation, under-report income proportional to income level; the more they earn, the more they under-report. If this is the case, then we will not be able to distinguish whether the elections decreased inequality or if elections simply increased proportional under-reporting. To address this possibility, we investigate whether elections decreased consumption proportional to the decrease in reported income. If elections have no effect on consumption, then it would be hard to believe that the decrease in income is completely genuine. However, if consumption also decreases, and decreases more for richer households, then we are more likely to believe that at least part of the income fall due to elections were real. For these estimates, we currentlty only have data from 48 villages. We are in the process of collecting data for the other villages. Table 7 columns (1)-(6) shows that for this subsample of villages, elections have a larger effect in reducing income that for the main sample on average. The coefficients are twice the size in magnitude as those from the main sample in Table 6. The estimates are negative for all percentiles. For the 50th, 75th and 90th percentiles are statistically significant at the 1% level. Like the main estimates, they show that the reduction of income caused by the elections are twice as large for households on the 90th percentile than the median household. Next, we estimate the effect of elections on the mean for households with incomes below the 25th percentile of their villages, between the 25th and 50th percentiles, the 50th and 75th percentiles, and for those above the 75th percentiles. Columns (7)-(10) show that elections reduce income more for richer households. Columns (11)-(14) show that elections also reduce consumption more. The estimates are statistically significant at the 1% level for households in the top three quartiles. In fact, the relative effect for households in the top quartile to households in the second quartiles is the same for the two outcomes. On average, elections reduced the incomes and consumption expenditures of the average households in the top quartile of the village income distribution by twice as much as the household in the second quartile. These results are very suggestive that the reduction income from elections is not entirely due to under-reporting.

6.2 The Effect on Labor and Agricultural Prices

How are incomes being reduced? Using the subsample of 48 villages, we estimate the effects of elections on labor input and agricultural prices. In Table 8, columns (1)-(4), the estimates suggest that elections reduce labor input and the reduction is more for richer households. However, the estimates are not statistically significant in this small subsample. In columns (5)-(9), the estimates show that elections increase the grain procurement prices. The estimate are larger in

magnitude and statistically significant for households on the second and third quartile of the income distribution. They say that elections increase grain procurement prices for these households by approximately 60-90%. Interestingly, one of the most common responses to the question of what villagers expected elected leaders to do during the focus groups was that the latter were expected to find better markets for the agricultural production of villagers.

6.3 The Effects on Fiscal Revenues, Expenditures, Administrative and Social Outcomes

Columns (1) and (2) in Table 9 show that elections increased fees paid by the average household by 0.2%, and had no effect on the average per household delivery to the upper levels of government. Columns (3) and (4) that show that administrative expenditures, which mostly comprise of salaries for the village government, increased as a share of total expenditure by 3 percentage-points. Since administrative expenditures are on average 17.7% of total village government expenditures, this is approximately an 18% increase relative to the sample mean. Interestingly, column (6) shows that elections decreased the number of village leaders from around five to four people per village. This suggests that elections increased the salaries of those in government. One explanation for this is that elections improved the quality of leaders and villagers were willing to pay an efficiency wage. To investigate whether elections changed leader characteristics, we examined the effect on the probability that village leaders come from land-owning backgrounds. Columns (7) and (8) show that election increased the fraction of village chiefs from land owning backgrounds by approximately 13 percentage-points, and had no effect on party secretaries. Finally, we examine whether elections decreased disputes. The estimates in column (9) show that elections decreased disputes by approximately four per year and elections with haixuan decreased it by approximately seven per year. This is more than 50% of the average number of disputes per year.

7 Conclusion

The preliminary results show that the introduction of democratic elections reduces inequality. But this occurs at a significant cost for production. Elections decrease gross income levels for all segments of the income distribution and decreases it more for richer households. If we apply a standard median voter theorem to the results, they suggest that the median voter values non-pecuniary factors to the extent that he is willing to give up 5% of his gross income. These non-pecuniary benefits could include a sense of equality, or public goods such as clean environment, more land devoted to recreation, etc. We are currently collecting household level data by source to investigate the channels in more detail.

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Figure 1: Map of the Counties where NFS Villages are Located.

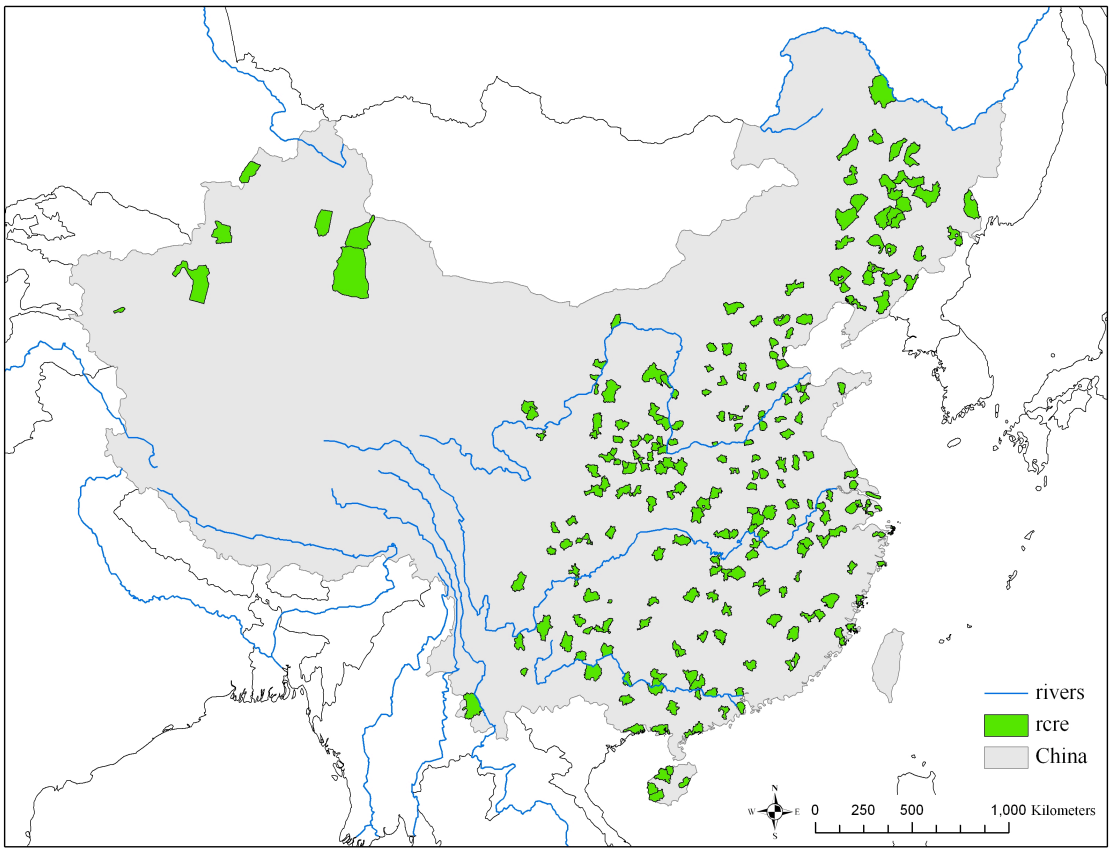

Figure 2A: The Effect of Starting Elections on The Difference between the Net Incomes of the 90th Percentile Household and the 50th Percentile Household

Coefficients of the dummy variables for the number of years before and after the first election in the village, controlling for village and calendar year fixed effects.

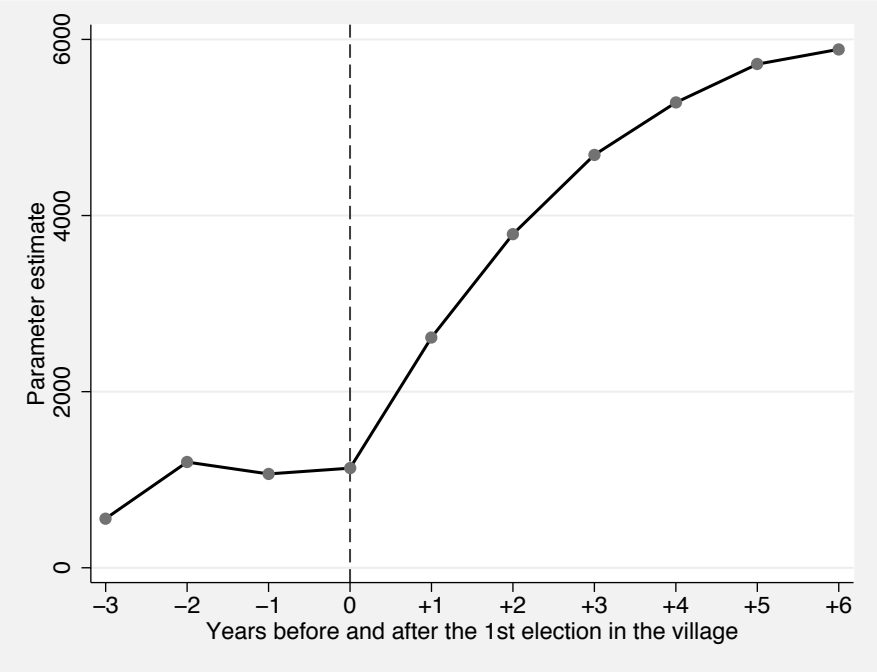

Figure 2B: The Effect of Starting Elections on The Difference between the Net Incomes of the 90th Percentile Household and the 10th Percentile Household

Coefficients of the dummy variables for the number of years before and after the first election in the village, controlling for village and calendar year fixed effects.

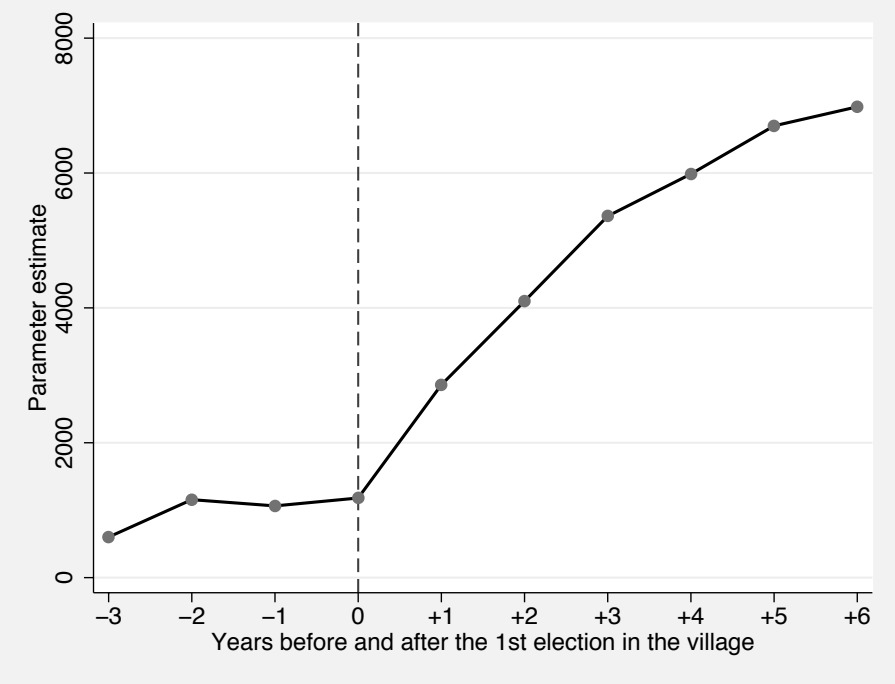

Table 1: Timing of Electoral Reforms

The year of the first election or the first election with haixuan

year	# villages that had 1st election	# villages that had 1st haixuan
1982	0	0
1983	13	1
1984	26	2
1985	68	9
1986	71	9
1987	106	13
1988	118	14
1989	125	15
1990	140	16
1991	165	17
1992	166	17
1993	169	18
1994	175	21
1995	177	24
1996	186	27
1997	190	45
1998	193	45
1999	199	51
2000	208	93
2001	215	105
2002	217	117
2003	217	128
2004	217	131
2005	217	132

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Table 2: Descriptive Statistics

	Mean	Standard Deviation
A. Village Characteristics		
Number of HH	419.7692	279.7648
# children between 7-13 years old per HH	0.7723	11.2886
# of laborers per HH	2.0346	0.4219
% of Primary Graduates	0.8523	0.6655
% of High School Graduates	0.2145	0.2313
% of HH Full-time Farming	49.4837	32.4397
# of Disputes (Non-criminal safety violations)	8.6003	23.1499
B. Income		
Mean annual growth (gross income)	0.1299	0.2524
10th Percentile Net Income	3043.9040	2579.8580
50th Percentile Net Income	6853.8430	5829.3120
90th Percentile Net Income	14156.9300	17517.9700
Ratio of 10th/90th Net Income	0.2512	0.1137
Ratio of 10th/50th Net Income	0.4587	0.2145
Ratio of 50th/90th Net Income	0.5303	0.1116
HH Taxes (Gross-net/Gross)	0.3611	0.1477
C. Village Government		
Number of Administrative Committee	5.4916	3.2263
Number of Party Committee	4.3708	2.2999
Age of Village Chief	42.3745	7.8153
Tenure of Village Chief (years in office)	7.2900	4.8587
Years of Education of Village Chief	9.0888	2.3334
Fraction of Village Chiefs from Landowning or Rich Families	0.2045	0.4034
Age of Party Secretary	44.6362	8.2145
Tenure of Party Secretary	9.7308	6.1528
Years of Education of Party Secretary	9.0292	2.3346
Fraction of Party Secretaries from Landowning or Rich Families	0.1721	0.3775

Table 3: Fiscal Revenues and Expenditures of Village Governments

Variable	Obs	Mean	Std. Dev.	Min	Max
Total Revenues (100 RMB)	**3687**	**4946.771**	**36755**	**0**	**1674285**
from collectives	3113	2763.654	30327.52	0	1421235
from HH	2886	1061.141	10558.87	0	480265
from obligated working days	1882	69.36185	217.579	0	3710
from firms	1673	440.2869	4262.417	0	127750
from upper levels of government	1882	157.8993	753.867	0	12868
from other sources	1673	1054.234	7999.234	0	176000
Total Expenditures (100 RMB)	**3693**	**4701.056**	**39060.8**	**0**	**1930056**
collective production	2886	1971.671	35441.05	0	1794526
HH production	2111	460.5604	2260.469	0	53100
delivery to upper levels of gov	2979	474.6455	2269.926	0	66120
public affairs	3189	418.1664	1455.833	0	26500
Administrative Expenditures	3291	331.4319	930.8903	0	22536

Table 4: The Effects Elections on Household Income Inequality Within Villages

	Dependent Variables: Income Inequality						
	A. Gross Incomes						
	(1) Gini	(2) inc10-inc90	(3) inc10/inc90	(4) inc50-inc90	(5) inc50/inc90	(6) inc10-inc50	(7) inc10/inc50
Sample Means	**0.280**	**-18629**	**0.277**	**-13371**	**0.530**	**-5258**	**0.512**
Election	-0.0106*	5,574.5022**	0.0145*	4,844.9278**	0.0215**	729.5744*	0.0109
	(0.0058)	(2,546.3687)	(0.0075)	(2,258.2526)	(0.0087)	(370.7059)	(0.0101)
Haixuan	-0.0077	6,130.6176	0.0196*	5,690.8740	0.0198	439.7436	0.0204
	(0.0089)	(5,318.1083)	(0.0112)	(4,838.2789)	(0.0138)	(640.2869)	(0.0134)
Observations	3968	4205	4205	4205	4205	4205	4205
F-test diff coeff (stat)	0.221	0.0209	0.373	0.0589	0.0329	0.335	0.863
F-test diff coeff (p-value)	0.639	0.885	0.542	0.809	0.856	0.563	0.354
	B. Net Income						
	Gini	inc10-inc90	inc10/inc90	inc50-inc90	inc50/inc90	inc10-inc50	inc10/inc50
Sample Means	**0.280**	**-11113**	**0.251**	**-7303**	**0.530**	**-3810**	**0.459**
Election	-0.0106*	2,041.3139*	0.0084	1,663.2647	0.0141*	378.0492	0.0110
	(0.0058)	(1,228.1713)	(0.0075)	(1,043.5273)	(0.0076)	(251.0326)	(0.0108)
Haixuan	-0.0077	2,387.8557	0.0134	2,240.9457	0.0147	146.9100	0.0164
	(0.0089)	(2,463.0905)	(0.0106)	(2,189.8869)	(0.0113)	(387.1709)	(0.0142)
Observations	3968	4193	4193	4193	4193	4193	4193
F-test diff coeff (stat)	0.221	0.0460	0.414	0.173	0.00549	0.516	0.235
F-test diff coeff (p-value)	0.639	0.830	0.521	0.678	0.941	0.473	0.629

All regressions include village and year fixed effects.
Standard errors are clustered at the village level.

Table 5: The Effects of Elections on Taxation of Households

	Dependent Variables														
	Ln Taxes Paid					Ln Fees and Levies Paid					Ln(Gross Inc - Net Income/Gross Income)				
	MEAN	< 25th	25th - 50th	50th - 75th	>75th	MEAN	< 25th	25th - 50th	50th - 75th	>75th	MEAN	< 25th	25th - 50th	50th - 75th	>75th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Sample Means	**4.141**	**3.496**	**4.021**	**4.248**	**4.862**	**3.712**	**3.312**	**3.749**	**3.878**	**3.935**	**0.361**	**0.332**	**0.312**	**0.315**	**0.362**
Election	0.1051	0.0840	0.1519	0.1208	-0.0029	-0.2236	-0.0635	-0.2410	-0.3753	-0.3452	-0.0034	-0.0019	0.0032	0.0023	-0.0068
	(0.2550)	(0.2753)	(0.2461)	(0.2878)	(0.4290)	(0.4412)	(0.4347)	(0.4885)	(0.4239)	(0.4684)	(0.0097)	(0.0078)	(0.0069)	(0.0077)	(0.0107)
Haixuan	-0.3591	-0.4461	-0.2705	-0.2621	-0.5064	0.0787	0.2949	0.0373	-0.0011	-0.0901	-0.0137	-0.0114	-0.0052	-0.0021	-0.0152
	(0.3928)	(0.4207)	(0.4088)	(0.4790)	(0.5338)	(0.6003)	(0.5769)	(0.6684)	(0.5876)	(0.6462)	(0.0133)	(0.0117)	(0.0096)	(0.0114)	(0.0145)
Observations	440	440	437	439	437	440	440	437	439	437	4172	3762	3762	3763	3762
F-test diff coeff (stat)	1.303	2.265	1.038	0.647	0.802	0.181	0.312	0.122	0.271	0.117	0.887	1.246	1.246	0.271	0.551
F-test diff coeff (p-value)	0.262	0.142	0.316	0.427	0.377	0.673	0.580	0.729	0.606	0.734	0.347	0.266	0.266	0.603	0.459

All regressions include village and year fixed effects.
Standard errors are clustered at the village level.

Table 6: The Effects of Elections on Household Income Levels and Yearly Growth Rates

	Dependent Variables								
	Ln (Gross Income) by Income Quantiles			Ln (Net Income) by Income Quantiles			Annual Growth*		
	(1) 10th	(2) 50th	(3) 90th	(4) 10th	(5) 50th	(6) 90th	(7) 10th	(8) 50th	(9) 90th
Sample Means	**8.273**	**8.987**	**9.655**	**7.634**	**8.540**	**9.203**	**0.130**	**0.123**	**0.138**
Election	-0.0177	-0.0437*	-0.1001***	-0.0334	-0.0336	-0.0714**	-0.0003	-0.0035	-0.0195
	(0.0401)	(0.0257)	(0.0355)	(0.0516)	(0.0319)	(0.0346)	(0.0191)	(0.0123)	(0.0150)
Haixuan	0.0072	-0.0568	-0.1157**	-0.0219	-0.0415	-0.0801	0.0233	0.0110	0.0018
	(0.0505)	(0.0364)	(0.0557)	(0.0707)	(0.0452)	(0.0524)	(0.0273)	(0.0170)	(0.0216)
Observations	4205	4205	4205	4185	4192	4193	3942	3947	3947
F-test diff coeff (stat)	0.454	0.262	0.167	0.0571	0.0535	0.0532	0.948	1.252	1.999
F-test diff coeff (p-value)	0.501	0.609	0.683	0.811	0.817	0.818	0.331	0.264	0.159

All regressions include village and year fixed effects.
Standard errors are clustered at the village level.
Growth is the ln(gross inc)_t+1 - ln(gross inc)_t

Table 7: The Effect of Elections on Income and Consumption for a 48 Village Subsample

	Dependent Variables													
	Ln Total Household Income										Ln Total Household Consumption			
		Percentiles					Income Brackets				Income Brackets			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Mean	10th	25th	50th	75th	90th	< 25th	25th - 50th	50th - 75th	>75th	< 25th	25th - 50th	50th - 75th	>75th
Sample Means	**9.132**	**8.381**	**8.762**	**9.118**	**9.494**	**9.919**	**8.411**	**8.958**	**9.313**	**9.927**	**7.969**	**8.346**	**8.584**	**8.900**
Election	-0.1787*	-0.0828	-0.1831*	-0.1314*	-0.1597*	-0.2636*	-0.1502	-0.1596**	-0.1649*	-0.2874*	-0.1732	-0.2063*	-0.2588**	-0.3344***
	(0.0893)	(0.1215)	(0.0926)	(0.0720)	(0.0938)	(0.1467)	(0.1165)	(0.0710)	(0.0833)	(0.1524)	(0.1225)	(0.1078)	(0.1022)	(0.1093)
Haixuan	-0.0236	-0.0741	-0.0004	0.0521	0.0175	-0.1007	-0.0285	0.0434	0.0518	-0.1638	-0.0839	-0.0146	-0.0668	0.0520
	(0.0887)	(0.1242)	(0.0862)	(0.0820)	(0.1123)	(0.1866)	(0.1094)	(0.0794)	(0.0934)	(0.1861)	(0.1176)	(0.1270)	(0.0979)	(0.1429)
Observations	440	440	440	440	440	440	440	437	439	437	440	437	439	437
F-test diff coeff (stat)	3.658	0.00365	3.977	6.207	3.304	1.341	1.029	6.483	6.075	0.718	0.945	3.483	4.413	4.817
F-test diff coeff (p-value)	0.0645	0.952	0.0544	0.0179	0.0782	0.255	0.318	0.0157	0.0191	0.403	0.338	0.0709	0.0434	0.0353

All regressions include village and year fixed effects.
Standard errors are clustered at the village level.

Table 8: The Effect of Elections on Labor Input and Agricultural Procurement Prices

	Dependent Variables								
	Total Labor Input					Farmer Grain Prices			
	Total Household Income Brackets					Total Household Income Brackets			
	< 25th	25th - 50th	50th - 75th	>75th	Ln Total	< 25th	25th - 50th	50th - 75th	>75th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample Means	**309.1**	**406.6**	**474.3**	**644.8**	**5.031**	**2.935**	**3.288**	**3.425**	**3.209**
Election	-10.8757	-19.5170	-15.8127	-39.3865	0.4961	0.4216	0.6346*	0.9007**	0.5482
	(27.8105)	(30.5558)	(28.4075)	(49.0164)	(0.3803)	(0.3396)	(0.3325)	(0.3945)	(0.4051)
Haixuan	-30.7300	-17.2599	-15.2972	-103.4080	-0.0745	0.2157	0.7573	1.0002*	0.5988
	(39.1771)	(46.8426)	(48.0323)	(62.7338)	(0.6948)	(0.5291)	(0.5156)	(0.5335)	(0.5854)
Observations	440	437	439	437	440	440	437	439	437
F-test diff coeff (stat)	0.391	0.00226	0.000186	2.099	1.057	0.160	0.0486	0.0330	0.00774
F-test diff coeff (p-value)	0.536	0.962	0.989	0.157	0.311	0.691	0.827	0.857	0.930

All regressions include village and year fixed effects.
Standard errors are clustered at the village level.

Table 9: The Effects of Elections on Village Government Fiscal Outcomes, Personnel, and Disputes within Villages

	Dependent Variables								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ln(Fees) per HH	Ln(Net Del) per HH	Ln (Admin Exp) per HH	Admin Exp/Vil Tot Exp	PartyComm	Administratives	Family vh	Family ps	Disputes*
Sample Means	**0.0168**	**0.0162**	**0.0139**	**0.177**	**4.371**	**5.492**	**0.204**	**0.172**	**8.600**
Election	0.0016*	0.0003	0.0009	0.0308**	-0.2399	-1.0860**	0.1262**	0.0319	-3.5567
	(0.0008)	(0.0005)	(0.0008)	(0.0140)	(0.2661)	(0.5324)	(0.0613)	(0.0337)	(2.9746)
Haixuan	0.0018	0.0001	0.0011	0.0296	-0.4626	-0.7876	0.1404**	0.0586	-7.2879*
	(0.0012)	(0.0009)	(0.0015)	(0.0227)	(0.3131)	(0.5903)	(0.0693)	(0.0412)	(4.0796)
Observations	3296	1738	3296	3249	2496	2499	3878	4497	2233
F-test diff coeff (stat)	0.0502	0.0503	0.0452	0.00524	1.580	2.187	0.178	0.834	3.087
F-test diff coeff (p-value)	0.823	0.823	0.832	0.942	0.210	0.141	0.673	0.362	0.0804

All regressions include village and year fixed effects.

Standard errors are clustered at the village level.

* Disputes are defined as non-criminal safety violations in a village.

APPENDIX Table A1: The effects of elections on Income Inequality by Year

Dummy variables for years to 1st elec	Dependent Variable: inc50-inc90
-3	511.9666
	(1,082.3916)
-2	1,136.0952
	(1,741.0206)
-1	1,006.9693
	(2,530.8654)
0	1,030.5468
	(3,074.0211)
+1	2,507.2036
	(3,923.2438)
+2	3,736.2949
	(4,567.5914)
+3	4,609.3318
	(5,149.7914)
+4	5,263.1330
	(5,803.4343)
+5	5,701.4228
	(6,368.1507)
+6	5,873.9945
	(7,011.6358)
+7	6,345.3968
	(7,745.6013)
+8	7,111.0049
	(8,355.5901)
+9	7,296.3605
	(9,129.0914)
+10	6,022.1925
	(9,988.6912)
Observations	2210
R-squared	0.741

All regressions include village and year fixed effects.

Standard errors are clustered at the village level.