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EVIDENCE FROM CHINA**

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ABSTRACT

The Effects of Democratization on Public Goods and Redistribution: Evidence from China*

This study investigates the effects of introducing elections on public goods and redistribution in rural China. We collect a large and unique survey to document the history of political reforms and economic policies and exploit the staggered timing of the introduction of elections for causal identification. We find that elections significantly increase public goods expenditure, the increase corresponds to demand and is paralleled by an increase in public goods provision and local taxes. We also find that elections cause significant income redistribution within villages. The results support the basic assumptions of recent theories of democratization (Acemoglu and Robinson, 2000; Lizzeri and Persico, 2004). In addition, we show that the main mechanism underlying the effect of elections is increased leader incentives.

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1 Introduction

Whether democracy affects public goods provision and redistribution is a central question in political economy. Several recent theoretical studies of democratic transition are based on the premise that democracies implement policies that favor a majority of citizens. In these theories, democracies provide higher levels of public goods (e.g., Bueno de Mesquita, Smith, Siverson, and Morrow, 2003; Lizzeri and Persico, 2004; Besley and Kudamatsu, 2008) and are more likely to redistribute wealth (e.g., Acemoglu and Robinson 2000, 2001, 2006; Boix, 2003) relative to autocracies.¹ However, a large body of theoretical studies in political economy argue that in practice democracy suffers from several difficulties that can obstruct public goods provision or redistribution.² The empirical evidence, which mostly comes from cross-country studies, is similarly inconclusive.³

The objective of this paper is to test whether recent theories correctly characterize the policy consequences of democratization by taking advantage of the introduction of village-level elections in rural China, which began in the late 1980s. This reform provides a uniquely advantageous context for studying the effects of democratization on public goods and redistribution for the following reasons. First, it was a stark and well-defined reform. Changes were confined to the village level. Village leaders were appointed by the Communist Party prior to the reform, and switched to being elected by villagers. These electoral reforms were not accompanied by *de jure* constraints on executives.⁴

¹Recent studies such as Acemoglu and Robinson (2000, 2001, 2006) and Boix (2003) characterize democracy as reflecting the preferences of the median voter, which leads to redistribution since the median voter is poorer than the elites by construction. The basic mechanics in Lizzeri and Persico (2004) are also based on the median voter, whose preferences result in more public goods provision. Studies based on accountability theories, such as Bueno de Mesquita, Smith, Siverson, and Morrow (2003) and Besley and Kudamatsu (2008) argue that democratic governments provide more public goods because it is the most economic way of satisfying a majority of the population.

²For instance, the literature on special interest politics and on political capture highlights that policies can fail to satisfy a majority in equilibrium (Bardhan and Mookherjee, 2000; Grossman and Helpman, 2001). Also, democracy can suffer from dynamic commitment problems which generate political failures (Besley and Coate, 1998). Since these ailments can also affect autocracies, the relevant question is which political regime suffers the most from them. In addition, older theories postulate that voters want immediate consumption and hence will refuse to pay higher taxes or to invest in education or physical capital, which can hinder public goods provision (Galenson 1959; Huntington 1968).

³In the cross-section, democracy has been found to be positively associated with government size (Tavares and Wacziarg, 2001), higher wages (Rodrik, 1999), lower inequality (Li, Squire, and Zou, 1998; Reuveny and Li, 2003; Tavares and Wacziarg, 2001), higher human capital (Tavares and Wacziarg, 2001), and better health indicators (Besley and Kudamatsu, 2006; Kudamatsu, 2011). However, in a large study looking at several socioeconomic policy dimensions, Gil, Casey, and SalaiMartin (2004) find that democracy is associated with no difference in the outcomes they examine. Also, democracy seems to have a weakly negative relationship with GDP growth in the cross-section (Barro, 1996; Tavares and Wacziarg, 2001), and a weakly positive relationship using other data and techniques such as matching and fixed effects (e.g., Papaioannou and Siourounis, 2008; Persson and Tabellini, 2007; Rodrik and Wacziarg, 2005). See the latter set of studies for strategies used to address the crudeness of country-level measures of democracy.

⁴The two fundamental institutions that characterize democracies are elections, and checks and balances (e.g.,

Second, the implementation and general environment of the reform are conducive for identifying the effect of elections. There is substantial variation in the timing of the reform for empirical analysis. By all accounts, villages had little say in the timing, which was decided by upper levels of government and quasi-random within provinces.⁵ China was politically, socially and economically stable during the period of the reform and unlike most other historical episodes of democratic transition, electoral reforms did not coincide with other turmoil. Relative to cross-country comparisons, Chinese villages are much more comparable, which helps mitigate the confounding influences of omitted variables such as culture or human capital.⁶ Finally, relative to other within-country comparisons, Chinese villages have substantially more fiscal autonomy, and therefore changes in village government can plausibly affect public goods and redistribution.⁷

Our empirical analysis proceeds in two steps. First, we construct a large new dataset to allow us to study the political economy of Chinese villages in detail. These data are a panel of 217 randomly selected villages from 29 provinces for the years 1982-2005. The variables include the history of political reforms and economic policies that we obtain by surveying village administrative records, and economic data at the household and village levels collected contemporaneously by China's Ministry of Agriculture. These data comprise the longest and broadest panel ever constructed to describe Chinese villages and is the first to systematically document the changes in the fiscal and political structure of village governments.

Second, we estimate the causal impact of the introduction of elections. As with all studies of the effect of institutional change, the fundamental difficulty arises from the potential presence of omitted variables. For example, both the introduction of elections and economic policy may be outcomes of a third factor such as villagers' preferences. To address this, we exploit variation in the timing of the introduction of elections across villages while controlling for village and calendar year fixed effects. Our strategy compares outcomes in villages before and after the introduction of elections,

Besley, 2006; Tavares and Wacziarg, 2001). Since the electoral reforms we study in this paper did not change the *de jure* constraints on executives, our analysis isolates the effects of elections.

⁵There are few exceptions. Please see Section 2 for a detailed discussion.

⁶For example, several studies have argued that culture can play important roles in determining economic policy and the effectiveness of democracy (e.g. Guiso, Sapienza, and Zingales, 2006; Guiso, Sapienza, and Zingales, 2007; Guiso, Sapienza, and Zingales, 2010). Similarly, since Lipset(1959) many studies have argued that human capital plays an important role in the effectiveness of democracy.

⁷Note that our context is not suitable for examining the effect of democratization on growth because many of the most relevant policy instruments for promoting growth such as property rights, or market or trade liberalization are not relevant at the village level.

between villages that have already introduced elections to those that have not yet. Village fixed effects control for all time-invariant differences across villages such as culture or geography. Year fixed effects control for all time-varying factors that affect villages similarly such as macroeconomic growth. Our baseline estimates also include province-time trends to control for the growing economic divergence across regions during the reform era. Interpreting our estimates as causal relies on the assumption that conditional on our baseline controls, the timing of the introduction of elections is uncorrelated to factors that could affect the outcomes of interest through channels other than the reform. We do not take this assumption as given. Section 2 provides a detailed discussion of the qualitative evidence on electoral reforms to motivate this assumption and Section 6 tests these assumptions with a large number of robustness exercises.

The first outcome we examine is village government expenditure on public goods. The estimates show that elections increased total government expenditure on public goods by approximately 27%. While this result is consistent with the recent theoretical characterization of democracy, it can also be consistent with other explanations. In particular, the increase in expenditure could be misallocated towards public goods that are not needed by villagers, or simply be stolen by corrupt officials. We provide evidence against these hypotheses by showing that the increase in expenditure corresponded to village-specific demand for particular public goods, and was paralleled by an increase in actual public goods provision. We also examine the sources of funding for the increase in public goods expenditure. We find that it was entirely financed by villagers, and was paralleled by an increase in local taxes for almost all village households. The fact that all funds were raised locally and that elections had no effect on transfers from the upper-levels of government is important because it supports interpreting the increase in expenditure and provision as the effects of elections on village leaders, who become more accountable to villagers, against the alternative interpretation that our results reflect a change in the preferences of the upper government that coincided with the introduction of results.⁸ The results on public goods funding are also interesting because they show that democratization does not necessarily diminish a governments' ability to raise taxes.⁹

Our second main outcome is income redistribution. We find that the introduction of elections

⁸Also, see our companion paper Martinez-Bravo, Padró-Miquel, Qian, and Yao (2011) for additional evidence that the introduction of elections resulted in a shift in leader accountability from being only towards the upper-levels of government to being to both to the upper-levels of government and to villagers.

⁹Huntington (1968) suggests that constituents' strong demand for short-term consumption impairs a democratic government's ability to raise taxes and hence fund public goods and investment.

caused significant redistribution from households that were rich prior to the reform to those that were poor. For instance, elections increased the ratio of the income of the households that were in the poorest ten percent over the households that were in the top ten percent by approximately 21 percentage-points. Given that village leaders are legally prohibited from imposing recurring taxes on income or production, it is interesting to examine the policy mechanism used to achieve such redistribution.¹⁰ We find that elections caused significant redistribution of household farmland, which was controlled by the village government. This was paralleled by the redistribution of income from agriculture, the main source of income for the households we study. We also provide evidence consistent with elected leaders using their powers over the management of village enterprises to redistribute wage income, and show that elections reduced land that was directly controlled by the village government, which had disproportionately benefitted elites.

We conduct several additional exercises. First, using information on the signatures of official documents, we show that elected leaders had *de facto* powers over policies and that their powers were not reduced when elections were introduced. Second, motivated by the recent literature on re-election incentives (e.g., Besley and Case, 1995; Besley and Coate, 2003; Dal-Bó and Rossi, 2008; Ferraz and Finan, 2011), we explore the mechanisms driving the effects of elections. We provide suggestive evidence that the increase in leader incentives is an important driver of the effect of elections, whereas the ability for villagers to select better candidates appears unimportant. Finally, we conduct a large number of exercises to check the validity of our empirical strategy.

This study contributes to the existing literature in several ways. First, it adds to the cross-country evidence on the effects of democratization on public goods and redistribution (e.g., Besley and Kudamatsu, 2006; Kudamatsu, 2011; Rodrik, 1999; Tavares and Wacziarg, 2001) by providing rigorous empirical evidence in support of recent theoretical characterizations of democracy (e.g., Acemoglu and Robinson 2000, 2001, 2006; Besley and Kudamatsu, 2008; Lizzeri and Persico, 2004). Relative to these studies, our analysis improves the casual identification of the impact of elections and is novel in directly examining redistribution and taxation, for which existing studies have provided indirect evidence inferred from income inequality and public goods expenditure. Similarly, our study is novel in providing evidence that the increase in public goods provision due to democratization correlates to citizens' demand. Second, it adds to a smaller number of within-country

¹⁰See sections 2 and 5 for more detailed discussions.

studies that have focused on various aspects of democracy (e.g., Besley and Case, 1995; Besley and Coate, 2003; Dal-Bó and Rossi, 2008; Ferraz and Finan, 2011; Foster and Rosenzweig, 2005; Fujiwara, 2011; Tyrefors and Pettersson-Lidbom, 2012).¹¹ In terms of the mechanism, our study is most closely related to Besley and Coate’s (2003) comparison of elected versus appointed electricity regulators in the United States. They find that elected regulators are more responsive to consumer demands and lower prices relative to appointed regulators. However, the limited scope of the powers of U.S. regulators prevents them from examining public goods and redistribution, which are more central to the cross-country and theoretical literature on democracy.¹² Third, in highlighting the effects of elections, our study contrasts and complements recent studies that emphasize the importance of constraints on the executive in determining economic outcomes (e.g. Acemoglu and Robinson, 2001; Besley and Persson, 2011). Fourth, our study adds to the nascent literature on governance in autocracies (e.g. Martinez-Bravo, 2011) and, in particular, in China (e.g., Lorentzen, 2010; Martinez-Bravo, Padróimiquel, Qian, and Yao, 2011; Persson and Zhuravskaya, 2011).

Finally, we are the first to systematically document the history of electoral reforms and the political and economic structure of Chinese villages in such detail. Relative to the few, but growing number of studies on the effects of rural Chinese elections, our data are significantly richer and larger in sample size. This allows us to add to the existing evidence by examining a broader set of outcomes, which sheds light on the mechanisms underlying the effects of elections and provides evidence against alternative explanations.¹³ This study complements a companion paper, Martinez-Bravo, Padróimiquel, Qian, and Yao (2011), which provides evidence that the introduction of

¹¹For example, Besley and Case (1995), Dal-Bó and Rossi (2008) and Ferraz and Finan (2011) provide evidence for the role of re-election incentives in the United States, Argentina and Brazil. Fujiwara (2011) finds that extending the effective franchise increases public goods provision in Brazil. Tyrefors and Pettersson-Lidbom (2012) finds that representative democracy leads to higher redistribution than direct democracy. These earlier studies differ from ours in that they do not identify the effect of elections *per se*. Foster and Rosenzweig (2005) examines the effect of Party competition and the introduction of rural elections on appropriate public good provision in India. Our results on public goods are consistent with theirs. However, the mechanisms underlying elections in the Chinese and India contexts are very different because Party competition is unlikely to apply in China’s one-party context. Our study also differs from theirs in examining a broader set of outcomes.

¹²There is also a related literature examining the differences between elected and appointed judges. For a recent example see Lim (2012).

¹³Existing studies have used either panel data of fewer villages or a cross-section of many villages. Shen and Yao (2008) examines the effect of elections on inequality and infer redistribution from changes inequality. Our analysis provides much more direct and precise estimates of redistribution by examining the effect of elections on income for different parts of the pre-election income distribution. Several studies have related elections to public goods (e.g., Zhang, Fan, Zhang, and Huang, 2004; Luo, Zhang, Huang, and Rozelle, 2007, 2010; Mu and Zhang, 2011). Also, Gan, Xu, and Yao (2006) examines the relationship between elections and villager health shocks.

elections were successful in shifting the accountability of the village government towards villagers.¹⁴ It is worth noting that the extensive longitudinal data we have constructed and the detailed analysis we provide will open Chinese villages as a context for rigorous empirical study by researchers in the same way that the ICRISAT and REDS data have facilitated the research of the Indian economy, and the Penn World Tables have facilitated cross-country studies.

This paper is organized as follows. Section 2 discusses the background. Section 3 briefly describes the data. Section 4 presents the empirical strategy. Section 5 presents the main results. Section 6 tests the robustness of the main results. Section 7 concludes.

2 Background

2.1 The Village Government

Villages are the lowest level of administration in rural China. Village governments were first organized by the communist government during the early 1950s, with two groups of leaders in each village. First, there is the village committee. It typically comprises three to five members and is led by the village chairman, henceforth VC. Second, there is the Chinese Communist Party branch in the village. It is similar in size to the village committee and is led by the village Party secretary, henceforth PS. Before elections were introduced, all of these positions were filled by appointment by the county government and village Party branch.¹⁵ Since all levels of government above the village are dominated by the Communist Party, we will sometimes use the term *Party* to refer to the village Party branch and all the upper levels of government as one body for simplicity.

The village government is very important for the well-being of its citizens because it implements policies mandated by the central government within the village and takes many important village-level decisions, such as local public goods provision, the management of village enterprises and land allocation (e.g., Oi and Rozelle, 2000; Rozelle and Boisvert, 1994; Whiting, 1996). We will discuss public goods, enterprises and land allocation further when we present the results.

Village governments collect taxes on behalf of the central government, but do not have legal

¹⁴In that paper, we present a model of leader accountability and show that consistent with the predictions of our model for when elections shift accountability towards villagers, elections reduced the enforcement of unpopular upper-government policies such as the One Child Policy and the upper-government expropriation of land.

¹⁵The Chinese government, led by the Chinese Communist Party, is broadly ordered in a vertical hierarchy, from the central government in Beijing down to the rural levels that comprise counties and townships. According to the *National Statistical Yearbooks*, rural population decreased from approximately 83% of total population in 1980 to approximately 75% by 2000.

authority to impose additional regular and recurring taxes. Instead, they raise revenues by imposing *ad hoc* fees and levies, which for simplicity, we will henceforth refer to as *local taxes*. Since such taxes are supposed to be one-time events devoted to specific uses, it is difficult for village leaders to credibly commit to income redistribution via progressive taxation. Therefore, even though our analysis will examine the effect of elections on local taxes, our investigation of redistribution mechanisms will focus on land allocation and income from village enterprises, both of which are under the discretion of the village government.¹⁶

2.2 Electoral Reforms

Motivation The first local elections were introduced in the early 1980s as collectives were being dismantled. Proponents of the reform made two main arguments.¹⁷ First, village elections would reduce the need for the central government to closely monitor local officials, which was difficult in a geographically vast and heterogeneous country. This concern had been endemic in the centrally planned regime since its inception in 1949, and was exacerbated by the widening regional differences caused by post-Mao market reforms. Imperfect monitoring meant that many local cadres were suspected of corruption and shirking, which generated intense discontent and discredited the regime in rural China. The introduction of local elections was seen as a potential solution to this problem because it shifted monitoring responsibilities onto villagers. Proponents argued that making local leaders accountable to villagers would impose checks on the VC's behavior and also allow villagers to select the most competent candidates (Kelliher, 1997).¹⁸

The second argument for introducing local elections was to improve the enforcement of centrally mandated policies at the village level. Proponents of reform claimed that elected village leaders would have more legitimacy and would better distribute the burden of unpopular policies, which would increase overall compliance. It was also hoped that local leaders with a democratic mandate would better determine which public goods investments were necessary and would better facilitate the local coordination necessary for providing them.

¹⁶Note that local taxes can be controversial when villagers believe them to be extortionary or to be misused by corrupt village governments. This led the central government to explicitly ban local taxes in the *Tax and Fee Reform* in 2003. For our study, this ban will have little effect as it occurred towards the end of the period we examine. In the Section on Robustness, we show that controlling for the introduction of this reform has no effect on our main results.

¹⁷For example, see Kelliher (1997), O'Brien (1994, 1999) for descriptions of the policy debates that led to the official introduction of local elections.

¹⁸“Who supervises rural cadres? Can we supervise them? No, not even if we had 48 hours a day...” – Peng Zhen, vice-chairman of the NPC Standing Committee, said at the chairmanship meeting of the Standing Committee of the Sixth NPC, April 6, 1987 (O'Brien and Li, 1999).

The initial introduction of elections changed the VC's position from being appointed by the Party to being elected by villagers. The main legal requirements were that: *i*) the number of candidates must exceed the number of positions; *ii*) term lengths were to be three years; and *iii*) the VC must obtain 50% of votes in the last round of voting.¹⁹ Villagers could abstain from voting. There was no change in the selection method of the members of the village Party branch and the PS, all of whom continued to be appointed. The Party also maintained control over villages by allowing the local Party branch to nominate the candidates. Thus, the main change that the reform affected was to give villagers the power to vote an unsatisfactory VC out of office. In a second reform, villagers were allowed to nominate the candidates. Open nominations became national law in 1998.

Note that all adult villagers have the right to vote and that there is little permanent migration in rural China in the period of our study.

Timing Several innovative provincial governments began to experiment with elections in the early 1980s. They were formally codified by the central government in the *Organizational Law on Village Committees* (OLVC) in 1987. From this point onwards, all provinces were pushed to introduce elections in all rural areas. A revision of the OLVC in 1998 required candidate nominations to be open to all villagers.

Elections were implemented in a top-down manner. Each level of government would pilot the reform in a few select villages, and the reform would be rolled out once the procedures and logistics were tested (O'Brien and Li, 1999). Anecdotal evidence from interviews that the authors conducted with county- and province-level officials suggests that the pattern of the roll-out was orthogonal to village characteristics in most cases. This is consistent with the rapidity of roll-outs within provinces. By all accounts, villages had little discretion over the timing of introduction of elections, which is characteristic of reforms in rural China.²⁰ In his detailed study of elections, Sinologist Unger (2002, p. 222) writes that “These [elections] should not be interpreted as bottom-up initiatives by the villagers themselves; they are not in a position to play any precedent-setting part in the initiation of new electoral reforms. There is a mistaken belief among some people outside China regarding

¹⁹For example, elections with multiple candidates could have many rounds of votes. Each round removes the candidates with the least number of votes. This is done until one candidate has fifty percent or more of the votes.

²⁰Unger (2002) notes the general passivity of villages in implementing rural reforms such as land reforms and the adoption of the *Household Responsibility Reform* earlier in the reform era.

this... elections are quietly being instituted at levels above the village, engineered first in selected districts at a distance from Beijing, through the connivance of the [central] Ministry of Civil Affairs and middle-ranking officials out in the regions”.

There are two notable exceptions to the quasi-random timing of the introduction of elections. First, the model villages that piloted the reform obviously received elections earlier. Second, elections were sometimes delayed for “problematic” villages that had a history of non-compliance to unpopular central government policies, such as the One Child Policy or the permanent expropriation of village land by the upper-levels of government (e.g., Li, 2009; Oi and Rozelle, 2000) or had a large kinship clan that could dominate other villagers in a majoritarian regime.²¹

There are several additional facts to note. First, there are no political parties and no slates of candidates with common platforms. Candidates are typically well-known by the villagers as they are all from the same village. As a consequence, candidates typically run on well-understood issues and are probably selected for qualities that have been long observed by their fellow villagers.²² Second, despite aberrations in electoral procedures, studies of Chinese elections have found that elections improved village leadership accountability (e.g., Birney, 2007; Brandt and Turner, 2007). Furthermore, in a companion paper (Martinez-Bravo, Padróimiquel, Qian, and Yao, 2011), we present a formal model of leadership accountability and present empirical evidence showing that elections effectively shifted the position of village leaders from being solely accountable to the Party to being accountable both to the Party and to villagers.²³

3 Data

3.1 The VDS and NFS Surveys

This study uses data from two surveys. The first is *The Village Democracy Survey* (VDS), a unique retrospective survey conducted by the authors of this paper.²⁴ The first wave, conducted

²¹The concern was that the elected position would be captured by the dominant clan, which would then implement policies for the benefit of its clan members at the cost of other villagers (O’Brien and Li, 2006: Ch. 3).

²²There are very few accounts of actual electoral campaigning. In many cases, elections were set up with only a few days’ notice (Unger, 2002: p. 221).

²³Specifically, we show that if elections make VCs accountable to villagers, then after the introduction of elections, policies should move in favor of villagers in cases where the upper government and villagers disagree and the VC has discretion (e.g. family planning, upper government land expropriation), and experience little change in cases where the upper government and villagers disagree, but the VC has little discretion (e.g., distance to high schools, the amount of upper government special aid transfers). We present empirical evidence that this is the case.

²⁴See http://www.econ.yale.edu/~nq3/NANCYS_Yale_Website/Village_Democracy_Survey.html.

in 2006, records the history of electoral reforms, *de facto* leader power, public goods expenditures and the enforcement of central government policies. The second wave, conducted in 2011, records the characteristics of village leaders. To ensure accuracy of the historical data, the retrospective VDS relies on administrative records for each village when possible. When village records are not available, we relied on the recall of survey respondents, which include all current and former living village leaders and elders (e.g., teachers) in each village. This applies to very few of our variables and we will note them in the text as they become relevant. The VDS forms a balanced panel of 217 villages for the years 1982-2005. The villages we survey are the same villages surveyed by the *National Fixed-Point Survey* (NFS), our second source of data.

The NFS is a detailed village- and household-level economic survey collected and maintained by a research center of the Ministry of Agriculture of China. It is collected each year beginning in 1986, with the exception of 1992 and 1994 due to administrative issues. The NFS villages were chosen in 1986 to be nationally representative for rural China and villages have been introduced over time to maintain representativeness. Similarly, approximately 25% of households within villages were randomly selected in 1986 and followed over time, and new households are introduced to maintain representativeness.²⁵ The panel is not balanced in terms of villages or households. From the NFS, we were able to obtain village-level data for all villages. However, we were only given access to the household-level data for a third of the villages (approximately seventy villages in eleven provinces). Given our interest in redistribution across households within villages, we chose the villages with the largest number of households. Henceforth, we call this sample the “household subsample”.

We aggregate the NFS data to the village and year level and match it to the VDS at that level. The full merged sample includes 217 villages from 29 provinces.²⁶ The descriptive statistics of the main variables for both samples are shown in Table 1. Note that they are very similar between the full sample (Panel A) and household subsample (Panel B).

Our data have several advantages. First, these are probably the most comprehensive data on village-level reforms ever constructed. Our data cover a larger and more nationally representative sample and span a longer time horizon than any other existing data. In addition to recording

²⁵According to the Ministry of Agriculture, there is very little attrition and households and villages are mainly added to adjust for gradual demographic changes.

²⁶Tibet and Xinjiang are excluded because these autonomous regions are dominated by ethnic minorities and are subject to different political and economic policies.

the history of electoral reforms, we also recorded the timing of the implementation of other major rural reforms and the occurrence of village mergers. This allows us to control for heterogeneity across villages more comprehensively than past studies, which is particularly important in a study of China during a period of large and widening disparity between regions. The richness of the data also allows us to provide a detailed analysis of the effect of elections on several policies and to assess the mechanisms driving the reduced-form effects. Second, the NFS economic data and the village administrative records that we surveyed in the VDS were collected contemporaneously. Since the majority of our data comes from these sources, most of our variables avoid recall bias. Third, the panel structure of the survey allows us to control for village fixed effects and province-year trends. Finally, the fact that the NFS samples approximately 100 households per village means that we are able to examine the redistribution across households within villages.

The main drawback is that the variables included in the NFS change over time to meet the needs of the Ministry of Agriculture. To maximize the accuracy and precision of our study, we focus on variables that are collected consistently for most years.²⁷ The second drawback is that the NFS, which is mainly an agricultural labor and production survey, did not collect detailed demographic data. Finally, because we have household-level data for only a third of the total number of villages, our estimates for these outcomes will sometimes be less precise.

All observations in the empirical analysis are at the village-year level. Table 1 lists the main variables, their sources and indicates whether or not a variable relies on recalled information. We describe the variables as they become relevant.

3.2 Descriptive Evidence

Most of the descriptive statistics for our study will be discussed as they become relevant to the analysis. In this section, we briefly describe our data on electoral reforms and the village government. The mean village in our sample had held its first election by 1989 and its first election with open nominations by 1997. By the end of our study period, all of the villages in our sample had introduced elections, but many had still not introduced open nominations.²⁸

To examine whether the elections were introduced in a top-down fashion, we compare the year of the first election in each village to the official introduction of elections by the county- and province-

²⁷As a consequence, some interesting variables that are only in the survey for very few years (e.g., obligated working days, roads) are not examined.

²⁸See Appendix Table A.1.

level governments. The timing of the official introduction of elections in each county is based on respondent recall.²⁹ Since provinces are large and respondents could not confidently recall the year of the first election within a province, the date of province-level introduction is inferred as the year of the first election in each province according to our survey.

Table 1 Panel C shows that the average village implemented its first election five years after the first election in the same province and within the same year as the official introduction of elections in the same county. In addition, our data indicate that 16% of villages held their first elections prior to the official introduction of elections by the county government, 66% held their first elections the year that the county introduced elections, and 18% held their first election afterwards. Similarly, 60% of villages within a province introduce elections within three years of the first election in that province. The rapid roll-out at the province-level, which we calculate using administrative records on the dates of introduction, is reassuring since one may be concerned that the recalled dates of the official introduction at the county level may be biased. Since the 29 provinces of our sample include approximately 2,885 counties and 623,669 rural villages (as defined by the number of village governments, *cunming weiyuanhui*), these statistics imply that the average province was able to introduce reforms in 13,859 villages within three years and the average county was able to introduce elections in 143 villages within one year.

These statistics are in line with the qualitative literature discussed earlier. First, the fact that most villages introduce elections in the same year as the rest of the county and very soon after the first election in the same province is consistent with the patterns expected from a top-down reform. The fact that a small number of villages implemented elections before and after the official introduction in each county is consistent with the anecdotal evidence that each administrative division typically piloted the reform before it officially introduced it and also delayed elections in a few villages. This is also consistent with our data being a random sample of villages. Second, it is important to note the speed in the roll-out of the reform. Such rapid roll-out is conducive to the timing of the introduction being quasi-random and orthogonal to village characteristics.

The data also shed light on the implementation of the reforms. We find that 79% of elections had more candidates than positions, as the law required. When we examine the data more closely,

²⁹To maximize accuracy, our surveyors only record a date if all respondents surveyed in a given village agree. If there is no consensus, then this variable is recorded as missing.

we find that most of the elections with too few candidates were first elections, all of which were immediately followed by new elections in the subsequent year. This is consistent with the belief that opponents to the electoral reform were unable to fully derail the introduction of elections, and with qualitative accounts of dissatisfied villagers demanding and obtaining recalls (O’Brien and Li, 2006). We also find that, as legally required, elections occur every three years on average. However, note that there is variation in this variable (the standard deviation is approximately one year), which mitigates the concern that village records report elections as they are supposed to occur and rather than when they actually occur. Finally, we find that there was a 38% VC turnover rate for the first election, which is almost twice as high as the average turnover rate in the sample (17%).³⁰

4 Empirical Strategy

Elections were introduced at different times across villages. We exploit the variation in this timing to estimate the causal effect of elections. Our strategy is similar to a *differences-in-differences* (DD) strategy, where we compare the outcomes of villages that have had their first election to villages that have not yet implemented their first election. Our baseline estimates always control for village and year fixed effects, and province-specific time trends. Village fixed effects control for all time-invariant or slow-moving differences between villages, such as geographic characteristics (e.g., hilliness or distance from a city) or culture. Year fixed effects control for changes over time that affect all villages similarly (e.g., national policy changes, macroeconomic growth). There are two main differences between our estimates and standard DD estimates. First, controlling for year fixed effects instead of a post-reform dummy variable allows time effects to vary flexibly rather than assume that they are constant across years in the pre- and post-reform periods. Second, we include province-time trends, which allow our estimates to control for the widening differences across regions brought about by unequal economic growth during the long time horizon of our study.³¹

Our baseline specification also controls for the second wave of reforms that opened the nomination

³⁰The data also show that there was significant variation in electoral procedures. See the section on robustness.

³¹Note that we control for province-time trends instead of the more flexible *province* \times *year* fixed effects because we do not have enough variation to estimate the latter. The closeness in timing of the introduction of elections for villages within the same province means that for the majority of province-year cells, there is no variation in election. We can also control for province-specific quadratic trends. The results are similar and not reported for brevity. They are available upon request.

of candidates to villagers to control for potential heterogeneity in the effect of elections.³² The effect of elections is estimated as follows:

$$Y_{vpt} = \beta E_{vpt} + \lambda O_{vpt} + \gamma_p t + \delta_v + \rho_t + \varepsilon_{vpt}, \quad (1)$$

where the policy outcome of village v in province p during calendar year t , Y_{vpt} , is a function of: dummy variables that take the value of one after the first election, E_{vpt} , and the first open nomination, O_{vpt} , have taken place; province-year trends, $\gamma_p t$; village fixed effects, δ_v ; and calendar year fixed effects, ρ_t . Since the introduction of the reform was initiated by province-level governments, all standard errors are clustered at the province level.³³ The main coefficient of interest is β . It will be statistically different from zero, $\hat{\beta} \lesssim 0$, if elections have an effect on a particular policy outcome.

Based on the qualitative and anecdotal evidence in Section 2, our identification strategy relies on the assumption that conditional on the baseline controls, the introduction of elections is not correlated with time-varying village characteristics that affect the outcomes of interest through channels other than elections. We do not take this as given and provide a large body of evidence in support of our strategy after we present the main results in Section 6.

5 Main Results

5.1 VC Power and Turnover

To motivate our analysis of the effect of elections, which we interpret as an increase in the accountability of the VC to villagers, we first establish that the VC had power over policies and that his power was not subverted when elections were introduced. We also provide evidence that the elections effectively changed the political game by showing that elections increased VC turnover rates and changed the profiles of VCs.

³²This improves the precision of our estimates, but does not affect the magnitude of estimated effects of the introduction of elections. For brevity, these results are not reported in the paper, but are available upon request. Note that we do not control for other procedural differences in elections because they are much more likely to be endogenous (recall that open nominations was introduced in a top-down manner much as the introduction of elections, and the timing within provinces was also quasi-random). However, we address these other differences in the section on robustness.

³³Recall that there are 29 provinces in the full sample and 11 provinces in the household subsample. To check that the standard errors are not biased towards zero by the small number of provinces, we also estimate standard errors that are clustered at the village level. The results are very similar and available upon request.

Power The VDS examines village records and documents whether the VC, the PS, or both sign their approval for important village policies. Commonly referred to as “signature rights”, these provide objective and quantifiable proxies of the *de facto* power of each village leader. For brevity, we focus on the policies most relevant to our main analysis: the appointment of a village enterprise manager, the employment of enterprise workers, land reallocation and public goods expenditure. The means for the frequency of signature rights are reported in Table 2 Panels A-C. Adding up the means from Panels A and B shows that the VC’s approval is required in 69-86% of observations. A possible concern is that local Party officials reduced the power of VCs in order to avoid potentially disruptive effects of elections. To investigate this possibility, we estimate the effect of elections on the *de facto* relative powers of village leaders.³⁴ The estimates of the effect of elections on the VC having unilateral power are all positive and those of the effect on the PS having unilateral power are all negative. Only a few of the estimates are statistically significant by conventional standards. Thus, we interpret elections as having zero or weakly positive effects on the power of the VC and rule out the concern that his power was diminished when elections were introduced.

Turnover In Table 2 Panel D, we present the effects of elections on leader turnover. The results show that the introduction of elections increased the probability that the VC in office was not the same person as the VC from the previous term by 4.5 percentage-points. The estimate is statistically significant at the 1% level and is consistent with the belief that elections effectively changed the political game. We also examine the characteristics of the VC.³⁵ We find that elections reduced the age of VCs at the time of entering office by approximately two years and increased the educational attainment of VCs by almost one year. These estimates are statistically significant at the 1% level.

In addition to the estimated effects of the introduction of elections, we report those of the introduction of open nominations. The estimates show that the latter reform reduced the probability of turnover (column (1)), which is consistent with open nominations improving the villagers’ ability to select their preferred candidates, and reduced the probability that a VC is a Party member (column (4)), which suggests that villagers have weaker preferences for Party members than the Party itself. These estimates are statistically significant at the 1% level. For brevity, we do not

³⁴Note that the number of observations changes because not all policies are relevant for every village.

³⁵Note that the observations change across columns because not all villages recorded all characteristics of village leaders for the entire period.

present the estimates of the effects of open nominations in the following sections that examine public goods and redistribution. However, we will present their estimates and discuss the interpretation afterwards when we discuss the mechanisms driving the main election results in Section 5.4.

5.2 Public Goods

Expenditure We first examine public goods expenditure at the village level, which are recorded by villages as the sum of expenditure on irrigation, primary schools, sanitation, within-village roads, electricity, the environment (e.g., planting trees) and other.³⁶ Table 3 presents the effect of elections on public expenditures from estimating equation (1) together with the sample means of these variables. These data are recorded in the VDS from village administrative records and are available for all years and villages during 1986-2005. Our data allow us to separately examine expenditures according to the source of the funds, which we categorize into funds from village and non-village sources. Consistent with the belief that village leaders are responsible for raising most of the funds required for village public goods, the means show that approximately 69% of total funding for village public goods comes from village sources.

Panel A of Table 3 shows the results for total public expenditures across all village public goods. Column (1) shows that elections increase total public expenditures from all sources by approximately 27.2%. The estimate is significant at the 1% level. This is consistent with the recent theories of democracy that we discussed in the Introduction. Specifically, it is consistent with the belief that elections increase accountability of village leaders towards villagers. Thus, leaders exert more effort in raising the funds to finance public goods expenditure (and/or in determining the correct object for investment).

Furthermore, a comparison of the magnitude of the coefficients in column (1) and those in columns (2)-(3) shows that the aggregate increase is entirely driven by an increase in funding from villagers. The estimate for village financing in column (2) is similar in magnitude to the estimate for total financing and statistically significant at the 1% level, while the estimate for non-village financing in column (3) is near zero in magnitude and statistically insignificant. This further contradicts the notion that democratically elected leaders cannot raise revenues for public goods

³⁶The villages in our began recording public goods expenditures in 1986 at the request of the Ministry of Agriculture. The accounting methods, the categories for public goods and the sources of financing are all determined by the ministry. In addition to public goods, village government expenditures also cover other items such as salaries of local cadres and expenditure on festivals and celebrations. In our data, public goods expenditures account for approximately 27% of total village government expenditures.

(in our context). It is also interesting because it suggests that our estimates of the effect of the introduction of elections is very unlikely to reflect changes in upper-government preferences that coincided with the electoral reforms. If that were the case, we would be more likely to observe that the effect of elections on public goods expenditure is financed by funds from the upper government.

Public Goods Provision and Demand Given the widely held belief that public goods were under-provided in rural China prior to election (e.g., Luo, Zhang, Huang, and Rozelle, 2007; Luo, Zhang, Huang, and Rozelle, 2010), the increase in public goods expenditure is consistent with elections improving public goods provision. However, this interpretation is subject to the assumptions that the increase in funds are not entirely embezzled or misallocated.³⁷ We provide evidence in support of these assumptions with two exercises.

First, we try to determine whether the increase in public goods investment corresponds to the demands of villagers – i.e., whether these investments are *desired*. We are able to find proxies for demand for two of the public goods: We assume that villagers living in villages that rely more on household farming have higher demand for irrigation; and villagers who live in villages with more school-age children have higher demand for schools. The dependent variables are the log of public expenditure on irrigation and on primary schools, which mainly refers to variable costs such as teacher salaries in our context.³⁸ As explanatory variables, we add the interaction effect of the introduction of elections (and open nominations) and the average log amount of all village land that is used for household farming; or the interaction effect of the introduction of elections (and open nominations) and the average number of children of ages 7-13 in a village. Note that we use time-invariant measures of village characteristics to construct our interaction terms to address the possibility that time-varying measures may be outcomes of elections, and also to maximize the number of observations.³⁹

³⁷See Olken (2007) for an example of local corruption in public goods provision and Bardhan and Mookherjee (2000) for a study on capture.

³⁸Primary schools are the only schools in Chinese villages.

³⁹The variables for village land used for household farming and the number of children age 7-13 are reported by the NFS. The land variable is available for 1986-91, 93, 95-2005, and the children variable is available for 1993, 95-2005. Thus, by calculating the village mean, we create a time-invariant variable that is available for all of the years for which our public expenditure data are available (1982-2005).

The estimating equation can be written as

$$Y_{vpt} = \theta E_{vpt} + \zeta O_{vpt} + \beta(E_{vpt} \times X_{vp}) + \lambda(O_{vpt} \times X_{vp}) + \gamma_{pt} + \delta_v + \rho_t + \varepsilon_{vpt}, \quad (2)$$

where X_{vp} is a measure of either the average log amount of village land used for household farming or a time-invariant measure of the average of the number of children age 7-13 in a village for the years 1987-2005. Since these

Panel B in Table 3 shows the effect of elections on irrigation investment. The positive interaction effect between elections and average log village farmland shows that elections increase irrigation expenditure more for villages with more farmland. The estimate is statistically significant at the 1% level. To interpret the magnitude of our estimates, we consider the implied effects for the villages in our sample with the highest and lowest amount of average log village farmland. For the village with the most farmland (9.73 log mu), the estimates imply that elections increased public investment in irrigation by approximately 26% ($9.73 \times 0.055 - 0.275 = 0.26$). In contrast, for the village with the least land (-2.3 log mu), the estimates imply that elections reduced public investment in agriculture by approximately 40% ($-2.3 \times 0.055 - 0.275 = -0.4$).

In Panel C, we examine public expenditures on schooling. The interaction term is positive, which shows that the effect of elections on public expenditures for schooling is increasing with the number of school-age children. This estimate is statistically significant at the 1% level. Because the effect is small in magnitude, we multiply the dependent variable by 1,000 for presentation purposes. For the village with the highest number of children ages 7-13 (9,013 kids), the estimates imply that elections increased public spending on primary schools by approximately 10% ($(9,013 \times 0.013 - 12.68)/1,000 = 0.1$). For the village with the least number of school-age children (42 kids), elections reduced the amount of public expenditure towards primary schools by approximately 1.2% ($(42 \times 0.013 - 12.678)/1,000 = -0.012$).

These results show that the increases in public expenditures correspond to our (admittedly crude) proxies for demand and are consistent with the notion that elections forced village leaders to provide what villagers wanted. Moreover, a comparison of the interaction effects in columns (2) and (3) shows that the increase in desired public goods investment is also driven by funding by villagers.

Second, we address the concern that public funds are misallocated by examining public goods *provision* directly. We examine the effect of elections on arable land, which should increase if irrigation increases, and school enrollment rates, which should increase if schools receive more investment. These are the only two proxies of public goods provision for which we have sufficient

variables are time-invariant, we do not control for their main effects, which are absorbed by village fixed effects. $\hat{\theta}$ is the effect of elections on villages where no land is used for household farming or villages where there are no school-age children. $\hat{\beta} + \hat{\theta}x$ is the effect of elections for villages where the average log amount of land dedicated to household farming (or the average number of school-age children) equal x .

sample size for analysis. They are reported by the NFS.⁴⁰ The sample mean presented in Table 3 Panel B column (5) shows that approximately half of village land is arable on average. The mean in Panel C column (6) shows that approximately 96% of children ages 7-13 are enrolled in school.

In Panel B, columns (4)-(5), we note that the estimates for the log of total arable land and the fraction of land that is arable show similar patterns to those for expenditure on irrigation. Elections increase arable land for villages with more household farmland. The interaction terms are statistically significant at the 1% level. In Panel C, column (6), the estimates for primary school enrollment rate show that elections increase school enrollment rates for villages with school-age children. The estimated interaction term is statistically significant at the 1% level. These results show that the effect of elections on provision parallel the effects of elections on public goods expenditure.

Local Taxes The main results show that the increase in public goods expenditure due to elections is financed by villagers. Here, we investigate the policy mechanism used by elected officials to raise funds from villagers by examining the effect of elections on local taxes. The NFS reports taxes, levies and fees paid to local governments by households. Unfortunately, this measure does not distinguish payments to the village government from payments to other local governments (e.g., county, township). Therefore, interpreting this result requires the assumption that elections did not change the taxes paid to local governments outside of the village. To the best of our knowledge, non-village local taxes did not change with electoral reforms.⁴¹

Data on local taxes paid by households are only available for the household subsample.⁴² In this sample, households pay 320 RMB per year in local taxes on average, which is approximately 3% of gross income. To examine the distribution of taxes across village households, we examine household payment of taxes according to households' positions in the within-village distribution of taxes paid for each year. In Table 4, we present the mean level of local taxes paid by households

⁴⁰Data for arable land and total land are available for the years 1987-2005 (excluding 1992 and 1994) and the data for school enrollment rate is available for 1993, 1995-2005. Both variables are reported for all villages, however there are a few observations with missing values.

⁴¹The main reform affecting local taxes was the *Tax and Fee Reform*. The VDS documents the introduction of this reform and show that it occurred towards the very end of our sample. Later, in the section on robustness, we show that controlling for the introduction of this reform has no effect on our results.

⁴²Recall that it comprises approximately seventy villages for the years 1986-2005 (excluding 1992 and 1994). Villages in the subsample have similar median incomes, growth and income inequality as the full sample of villages. See Table 1.

and the estimated effects of the introduction of elections on taxes for each decile. The estimates show that elections caused a large increase in the amount of local taxes paid by households and the magnitude of the increase (approximately 150-290%) does not systematically vary across deciles. The estimates are statistically significant for households at the 10th and 30th-70th percentiles.

These results show that elected officials are able to increase local taxes. Together with the results on the appropriateness of public goods, they suggest that elected officials can increase taxes because voters trust that elected officials are more likely to allocate public funds according to voter preferences. To assess the plausibility of the magnitude of the effect, it is important to keep in mind that average local taxes are very low. Thus, the large percentage increase implied by our estimates does not result in an implausibly high level of local taxes.

5.3 Redistribution

Total Household Income We begin our analysis on redistribution by examining the effect of elections on income. Data for household income is reported by the NFS for the years 1986-2005 (excluding 1992 and 1994). For the full sample of 217 villages, we have *total household income* for households on the 10th, 25th, 50th, 75th and 90th percentiles of the time-varying income distribution within villages.⁴³ For the household subsample, we can also compute *income by source*. The main analysis of the effects of elections uses this latter data and we use the full sample to show that the household subsample is similar in terms of median income and income inequality. Table 1 Panel A shows that the median household in the full sample earns approximately 10,513 RMB per year, and Panel B shows that the median household in the sub-sample of villages for which we have household level data earns approximately 11,386 RMB per year. During most of our study period, one RMB was approximately equal to 0.125 USD. The incomes we report are not adjusted for inflation. In the regression analysis, price changes are largely absorbed in the year fixed effects and province-time trends. Panels A and B show that the median household income of each village grows at 7% per annum on average, which is slightly lower than the national growth rate for this period and reflects the fact that rural areas had lower average growth than urban areas.

To examine redistribution, we divide households according to their average pre-election positions on the within-village distribution of total household income. This requires us to use the household

⁴³For example, the income of the 10th percentile household refers to the income of the household that is on the 10th percentile of the income distribution within a village during a given year.

subsample (because we need to link households over time) and restricts our sample to villages that introduced elections after 1986, when our income data become available (because we need to observe pre-election income for each household). Table 5 Panel A presents the mean income for households and the estimates of the effect of the introduction of elections for each pre-election income group. To illustrate the effect visually, we plot the estimated coefficients and mean incomes for each pre-election income group in Figure 1. The figure shows that there are substantial differences in income across households and that elections caused income redistribution. The effect of elections is large and positive for the poorest households, and becomes successively more negative for richer households. In other words, elections caused the households that were the poorest before the election to gain the most income, 28% (0.25 log-points), and the households that were the richest before the election to lose the most income, 29% (-0.34 log-points). The estimates are statistically significant at the 1% level for the poorest and richest households in Table 5 columns (1) and (10). The estimates for the other households are less precise, but the coefficients present a systematic decreasing pattern.

Table 5 columns (11)-(13) provide estimates for more parsimonious measures of redistribution. The dependent variable in column (13) is the ratio of the income of households in the first pre-election decile to the income of households in the tenth pre-election decile. Similarly, the dependent variables in columns (12) and (13) are the income ratios of the sixth pre-election decile to the tenth pre-election decile, and of the first pre-election decile to the sixth pre-election decile. The estimated coefficients in all three columns are positive and statistically significant at the 5% and 1% levels. To interpret the magnitude, consider the estimate in column (11). It shows that the introduction of elections increased the incomes of the poorest 10% of households relative to the richest 10% by 20.9 percentage-points. This is a large effect since the sample mean of this measure is 0.56.

In Panel B, we examine the effect of elections on income from agriculture (and home production), which is the most important source of income for households in our sample and accounts for approximately 67% of total household income. The estimates show a similar pattern to the estimates of the effects of elections on total income. The estimates and the mean agricultural incomes for each pre-election agricultural income group are plotted in Figure 1. The plot shows that elections increase agricultural income for households that had below median agricultural income prior to the first election, had little effect on households around the median, and reduce agricultural income of the 20% of households with the highest agricultural income. The gain is decreasing in pre-election

agricultural income for poor households, and the loss is increasing in pre-election income for rich households. The estimates are statistically significant at the 1% level in Table 5 columns (1)-(4) and (10). The estimates in columns (11)-(13) show that the effects of elections on our parsimonious measures of agricultural income redistribution are all statistically significant at the 1% level.

In Panel C, we examine wage income, which is the second most important source of income. For the households in our sample, wage income comprises approximately 16% of total household income. The villages in our sample are very rural and wages are mainly earned by working at village enterprises. Since elected leaders have substantial control over the appointment of enterprise managers and other employees (see Table 2), they can redistribute part of total income by redistributing jobs, and thus, wage income. The estimated coefficients support this and show a strong and consistent pattern of redistribution. The estimates and the mean wage income for each pre-election wage income group are plotted in Figure 2. Elections increase wage income for households that had below median wage income prior to the first election, have little effect for households around the median, and reduce the wage income of the 30% of households with the highest wage income. The gain is decreasing in pre-election income for poor households, and the loss is increasing in pre-election income for rich households. The estimates in Table 5 columns (11)-(13) show that the effects of elections on our parsimonious measures of wage income redistribution are all statistically significant at the 1% level.

The results on income show that the introduction of elections caused significant income redistribution, and in particular, the redistribution of agricultural and wage incomes.

Land Given the village government's inability to impose recurring taxes, the most obvious mechanisms for redistribution are through the government's control over land and village enterprises.⁴⁴ This is consistent with our finding that elections cause the redistribution of agricultural income (as land is a key production factor) and wage income (which is mainly earned from village enterprises). In this section, we provide further evidence on the mechanisms of redistribution by directly examining household land allocations and by providing suggestive evidence that elections also caused

⁴⁴Recall from our earlier discussion and results that the village government cannot impose recurring taxes and this makes it difficult for candidates to commit to use taxes to redistribute. Consistent with this belief, we find no systematic pattern when we examine the effect of elections on local taxes according to a household's pre-election position on within-village distribution of income or land. These results are not shown for brevity and are available upon request.

leaders to redistribute enterprise profits by showing that elections reduced the amount of village land leased to enterprises, which disproportionately benefitted the rich.

Land is the main productive asset and indicator of wealth in rural China. All land is publicly owned in China and granted to households for farming in long-term land contracts. The allocation of such contracts is one of the main responsibilities of village leaders. Since average households enjoy very small land allocations, a small increase in land can be extremely valuable (Unger, 2002; p.145). Note that rural households cannot sell their land rights in China, and during the period of our study, were also prohibited from renting out their land.⁴⁵

Data for household farmland allocation are reported by the NFS for the years 1986-2005 (excluding 1992 and 1994). We were able to obtain these data for the household subsample. To observe redistribution, we divide households according to their average position on the pre-election within-village distribution of household farmland allocation. The means for each group of households are presented in Table 6 columns (1)-(10). They show that households have very little land and there is substantial inequality within villages. Since average land allocations are so small, our study retains the indigenous unit of measurement for convenience (one mu is equal to 1/15th of a hectare).

To examine the effect of elections on land redistribution, we estimate equation (1) with the log of household farmland as the dependent variable. The estimates are positive for households below the median (columns (1)-(5)) and negative for households above the median (columns (6)-(10)). The estimates and the mean household farmland for each pre-election land group are plotted in Figure 3. The magnitude of the estimates shows a consistent pattern of redistribution across households with the most land-rich households losing the most land, 23% (0.26 log-points), and the most land-poor households gaining the most land, 59% (0.46 log-points). The estimates are statistically significant at the 1% level for households in from the first, second, ninth and tenth deciles, and at the 10% level for households in the fourth decile.

In columns (11)-(13), we present more parsimonious measures of land redistribution. The estimates are all positive and statistically significant at the 1% level, showing that elections increased the land allocations of the land poor relative to the land rich. To assess the magnitude, consider the estimate in column (11). It shows that elections increased the land allocated to the households

⁴⁵In most cases, rural households were also restricted from hiring laborers because households that did not farm their own land would lose land rights. See Jacoby, Li, and Rozelle (2001) for a related study about tenure security in rural China.

with the least land (0-10th group) relative to households with the most land (90-100th group) by 24.5 percentage points. Given that on average, the ratio of land for these two groups is 0.56, the estimates imply that elections had a large effect.

Profits from village enterprises are supposed to be shared equitably among villagers since village enterprises are typically founded with collective assets. However, a common complaint from villagers is that the rents from these enterprises disproportionately benefits elites.⁴⁶ This is consistent with our earlier finding that elections redistributed wage income, which is mostly earned from enterprises, and for which there was substantial inequality across households. This is also consistent with the fact that in our data, the poorest 30% of households in each village at any given point in time do not earn any income from enterprise profit sharing. Unfortunately, the sample of households that earn income from enterprise profits is too small to allow us to directly examine the effect of elections on its redistribution.⁴⁷ Instead, we attempt to provide indirect evidence by examining land that is leased out to enterprises.

When communes were dismantled and agriculture was de-collectivized during the early post-Mao reforms, village governments retained a fraction of arable land under their direct control so that small adjustments in household land allocation can be made for demographic changes such as marriages or deaths without village-wide disruptions. This land can be leased out to enterprises conducting farming or non-farming activities.⁴⁸ Data for the use of village land is reported by the NFS for all villages for the years 1987-2005 (excluding 1992 and 1994). The villages in our sample dedicate approximately 96% of arable land (approximately 51% of total village land) to households for farming. Approximately 75% of the little remaining arable land is leased out to “enterprises”, a term which we use for firms run by collectives or villagers (see Table 1). Since elections can only reduce the amount of land not used for farming if such land existed prior to the first election, we restrict our analysis to villages that ever used any arable land for non-household farming prior to the first election. In columns (14)-(15) of Table 6, the estimates show that the introduction of elections

⁴⁶For instance, corrupt village leaders have been known to extract personal rents from land controlled by the village governments. For example, see Brandt and Turner (2007), Kennedy, Rozelle, and Shi (2004), Oi and Rozelle (2000), Rozelle and Boisvert (1994) and Rozelle and Li (1998). Moreover, Brandt and Turner (2007) find that redistributing collective land to villagers is positively correlated with re-election probabilities.

⁴⁷Recall that we only have household-level data on income for approximately seventy villages and we further lose observations when we condition on households’ pre-election position on the within-village income distribution. Thus, the fact that only 70% of households ever earn any enterprise income causes our sample size to be too small for meaningful regression analysis.

⁴⁸For a study that describes land use and contracts in rural China, see Rozelle and Li (1998).

reduced the amount of land that is leased out to enterprises by approximately 41% (0.52 log-points), and the probability that any land is leased out by eleven percentage-points. These estimates are statistically significant at the 1% level.

The finding that elections reduced the amount of land leased out to enterprises, the profits of which disproportionately benefit the rich, is further evidence that elections cause village leaders to be accountable to a broader group of villagers.

5.4 Re-election Incentives and Leader Selection

The main results are consistent with recent theories of democratization that emphasize that policy should favor the views of the median voter in democracies and that elected leaders are accountable to the majority. Within these broad notions, the political agency literature proposes two main mechanisms that voters use to hold elected politicians accountable.⁴⁹ First, elections can help voters address moral hazard problems by rewarding good performance with re-election – i.e., elections serve as means to provide the correct *incentives* to office holders. Second, voters can use elections to *select* the politicians that are more competent or whose preferences are better aligned with citizens’ preferences. In this section, we provide two pieces of evidence to suggest that the change in leader incentives is important in driving our main results.

The first piece of evidence is based on a comparison of the effects of introducing elections and the effects of interdicting open nominations, which allow villagers to nominate their own candidates. If the selection of better politicians is important, then the introduction of open nominations should have a significant effect on the outcomes of interest.⁵⁰ Table 7 Panel A presents the baseline estimates for our public goods and redistribution outcomes. It re-states the main results for the post-election dummy, but also presents the estimates for the post-open nomination dummy, which captures the marginal effect of open nominations in addition to elections.⁵¹ The latter estimates are all small in magnitude relative to the post-election dummy and are statistically insignificant. Thus, the introduction of open nominations has no marginal effect on the outcomes of interest. This

⁴⁹This literature is large, starting with the seminal contribution of Barro (1973). For textbook treatments, see Besley (2006), Besley and Persson (2011) and Persson and Tabellini (2000) .

⁵⁰This interpretation assumes that the Party does select the same candidates as those preferred by villagers. Recall that consistent with this, we have shown that the introduction of open nominations caused leaders to experience lower turnover rates and be less likely to be a Party member at the time of entering office. See earlier discussion and the results in Table 2 Panel D.

⁵¹Recall that the introduction of open nominations (as with the introduction of elections) was irreversible. Once it was introduced, all subsequent elections had open nominations.

suggests that the ability to select candidates is not important in our context.

The second piece of evidence is based on examining the effect of the introduction of elections in villages that experienced no turnover after the first election (i.e., 64% of the villages in our sample). Since the selection effect is not applicable when there is no turnover, finding that the effect of elections in villages with no turnover is similar to our main results will be consistent with leader incentives playing an important role. To maximize precision and sample size, we make this comparison by estimating the interaction effect of the introduction of elections (and open nominations) with a dummy for whether the first election changed the VC in office, while controlling for its interaction with the introduction of open nominations and the same baseline controls as before.⁵² Note that we examine villages where there is no turnover after the first election because of our focus on the effect of introducing elections.⁵³ This means that to interpret our estimates, we must assume that the effect of elections is immediate. If the effect gradually evolves over time after the first election, it will be difficult to interpret the interaction effect of elections and whether there is turnover in the first election. Later in the paper, we show that only the effects on public goods is immediate. Thus, although we present the interaction effects for the same outcomes as in the previous exercise for consistency, we only interpret the interaction effect for the outcomes related to public goods.

The estimates are shown in Table 7 Panel B. The estimated main effect of elections reveals the effect of the introduction of elections for villages that do not experience VC turnover in the first election (i.e. the *incentive* effect). The main effect estimates in columns (1)-(3) are very similar to the baseline estimates shown on Panel A. This is consistent with the incentive effect driving our main results. Also consistent with the lack of importance of selection effects, the interaction terms that capture the additional effect of elections when there is VC turnover (i.e., the *selection* effect) are much smaller in magnitude relative to the main effect, have the opposite sign, and are statistically insignificant.

In addition to the estimates presented in this section, we also find that public goods expenditure and redistribution are positively correlated with re-election probabilities (e.g., the probability that

⁵²This equation is similar to equation (2), except that we interact the introductions of elections and open nominations with a dummy variable that indicates whether there was VC turnover in the first election (instead of predictors for the demand of public goods).

⁵³Note that there is only one village that experiences no turnover in any elections. Thus, the sample size is too small for us to examine villages that never experience turnover.

the same leader stayed in office). These results are not shown for brevity and are available upon request. They are also consistent with the presence of re-election incentives in our context.

6 Robustness

The main concern for our empirical strategy is that the timing of the introduction of elections may be endogenous. Specifically, there may be village-specific and time-varying determinants of the introduction of elections that are not controlled for by the baseline controls and affect the outcomes of interest through channels other than elections. To investigate this possibility, we examine whether the timing of the introduction of elections at the village level is correlated with pre-election measures of potentially confounding factors. Consistent with our identification assumption, we find that, conditional on the timing of the first election in a province, there is no systematic relationship between the timing of the introduction of elections in villages with village demographic characteristics, pre-election income, pre-election land, and a large number of other variables.⁵⁴ Nevertheless, this section provides additional evidence to support our empirical strategy and show that our estimates are very robust to additional controls. For brevity, we focus on the main outcomes of interest: log total public goods expenditure, and the parsimonious measures of income and land redistribution (e.g., the ratio of incomes for households that were in the first pre-election decile to households that were in the tenth pre-election decile, and the analogous measure of land redistribution).

Pre-Trend Analysis One way to investigate the validity of our identification strategy is to recall that the key assumption for our strategy (as with all DD strategies) is that of “parallel trends” – i.e., our strategy assumes that absent elections, the outcomes of villages that introduced elections earlier would have moved along parallel trends with villages that introduced elections later. Thus, we can check the validity of our strategy by investigating the presence of pre-trends. A positive trend in public goods expenditure in the years leading up to the introduction of elections would be a sign

⁵⁴The factors that we examine include: village demographics (e.g., village size, proximity to urban area, size of the largest kinship group and presence of a temple), income (e.g., household income mean and inequality), land (e.g., household farmland mean and inequality, village arable land not used for farming), local taxes (e.g., mean and inequality), and the enforcement of unpopular policies (e.g., One Child Policy, land expropriation). For each variable, we calculate the pre-election mean for each village and regress the year of the introduction of elections in villages on pre-election means and the year of the first election in the province. Since there is no theoretical motivation for the functional form of this relationship, we experiment with many different functional forms. The results are similar across specifications and we find no correlation between the explanatory variables and the timing of the introduction of elections. These estimates are not reported for brevity and are available upon request.

that public good provision was evolving differently in villages that were about to receive elections. This would raise the suspicion that the timing of elections was endogenous and cast doubt over our interpretation of the results as the causal effect of elections.

To investigate the presence of pre-trends, we estimate the following equation:

$$Y_{vpt} = \sum_{\zeta=-3}^6 \beta_{\zeta} \chi_{vpt} + \lambda O_{vpt} + \gamma_p t + \delta_v + \rho_t + \varepsilon_{vpt}, \quad (3)$$

where the outcome of interest is a function of the number of years since the introduction of elections, χ_{vpt} . The other explanatory variables are the same as the baseline specification, equation (1). Since elections begin early in our sample and many observations would be lost by estimating the effects of many lead years, the earliest lead we estimate is four years prior to the election. For this estimate, we group all observations that are four or more years prior to the first election together. This is the reference group that is omitted. We also group all observations that are six or more years after the first election together. If there are no pre-trends, then the estimated dummies for the years prior to the first election should be similar, $\hat{\beta}_{\zeta} \approx \hat{\beta}_{\zeta+1}$ for $\zeta < 0$. Moreover, if the main results reflect changes that begin when elections are introduced, one should find that the estimated dummies begin to differ from zero starting the first year of the election, $\hat{\beta}_{\zeta} > 0$ for $\zeta \geq 0$. This allows us to examine whether our main results reflect the introduction of elections or whether it captures spurious effects that occur after elections are introduced.

The coefficients and the standard errors for the estimated effects of the number of years since the first election are presented in Appendix Table A.2. The coefficients are plotted in Figures 4-6. There is no evidence of pre-trends in our main outcomes of interest. Consistent with the parallel trends assumption, these figures show that the correlations between the number of years before the first election and the outcome of interest are roughly constant over time. Moreover, there is a level shift upwards in the coefficients when the first election is introduced for public goods expenditure, which implies that the introduction of elections immediately increased public goods expenditure. The estimates for income and land redistribution show that redistribution gradually increased after the first election. However, there is still a trend break in the first year of the election. The point estimates are statistically significant for the majority of the coefficients for public goods expenditure. They are less precise for income and land redistribution, which is most likely due to the smaller

sample sizes.

Controlling for Pre-Conditions Although there is no evidence of pre-trends, one may still be concerned that election timing is correlated with pre-conditions that affect our outcomes of interest through channels other than elections. The most blunt and straightforward method to address omitted variables bias is to directly control for the interaction terms of the pre-election averages of each of the outcomes variables and the full vector of year fixed effects. We control for the pre-election averages of public goods expenditure and redistribution instead of time-varying measures to avoid the difficulty that the latter are outcomes of elections. The interaction with year fixed effects controls for the influence of these variables over time in a fully flexible manner. Note that these interaction terms will also control for the influences of all correlates of the pre-election outcome variable averages over time.⁵⁵ For example, controlling for pre-election income inequality controls for both the influences of the level of pre-election income inequality and the influences of pre-election factors such as human capital inequality, which may be correlated with pre-election income inequality.

In addition, we control for the pre-election averages of the probability that any household was permitted to have two or more children and the incidence of upper-government land expropriation, each interacted with the full set of year fixed effects. Although we found that the pre-election averages of these variables are uncorrelated with election timing, we include them to address any remaining concerns that elections were delayed where there was a history of non-compliance with unpopular central government policies and our pre-election measures of unpopular policies are too crude to show a negative correlation with election timing.

In Table 8, we present the robustness checks for our main outcomes. Column (1) re-states the baseline estimates for comparison purposes. Columns (2)-(4) gradually introduce each vector of controls for the controls that are available from the full sample of villages. Column (5) controls for all of these controls simultaneously. This rigorous specification produces estimates that are nearly identical to the baseline. In columns (6)-(7), we introduce the controls for variables that are only

⁵⁵For villages that introduced the first election very early, before we have outcome data, we use the sample average of the outcome variables in place of the pre-election average. This only affects the outcomes related to public goods since our income redistribution measures are already restricted to villages for which we have pre-election data. Our results are very similar if we restrict the sample to villages for which we have pre-election data. These estimates are available upon request.

available for a third of the villages. In column (8), we control for all of the controls in columns (2)-(4) and (6)-(7) simultaneously. The sample size is significantly smaller. However, the estimated coefficients have the same sign, and their magnitudes are either similar or larger than the baseline in column (1).

The estimates show that our main results are extremely robust and are unlikely to be driven by village-specific pre-reform conditions.⁵⁶

Additional Sensitivity Tests In Table 9, we control for a set of factors that could potentially influence the effectiveness of elections. These include proximity to a city (interacted with year fixed effects), electoral procedures, the size of the largest kinship clan (interacted with year fixed effects), proxies for social capital (interacted with year fixed effects), the implementation of the *Tax and Fee Reform*, and a dummy for whether a village had ever experienced an administrative merger (interacted with year fixed effects). Electoral procedures are measured as three dummy variables that indicate whether the previous election used roving ballot boxes, anonymous ballots or allowed proxy votes.⁵⁷ Since voting procedures can be an outcome of elections, these estimates should be interpreted very cautiously. Our control for social capital is motivated by Tsai (2007)'s argument that strong informal institutions (e.g., social capital) are major determinants of policy outcomes, which could weaken the effect of elections. We follow her work in using the presence of a lineage group, which is measured as the presence of a household with a family tree or an ancestral temple, or the presence of a village temple to proxy for informal institutions. Since these variables are time-invariant, we control for their interactions with the full set of year fixed effects to allow their influences to vary fully flexibly over time. The motivation for controlling for the *Tax and Fee Reform* is based on the fact that this reform could have curbed the village government's ability to provide public goods. Our estimates are extremely robust to controlling for any or all of these controls.

We conduct several additional robustness checks that are not presented in the paper.⁵⁸ First, we

⁵⁶We obtain similarly robust estimates when we control for the pre-election average annual growth of the outcomes of interest, each interacted with year fixed effects. These estimates are not presented for brevity and are available upon request.

⁵⁷The data also show that there was significant variation in electoral procedures. A roving ballot can decrease the ability of citizens to monitor the ballot box and facilitate ballot stuffing. Similarly, the lack of anonymous ballots could increase the pressure on villagers to vote for a particular candidate. Allowing villagers to vote in proxy of family members that are away can be important in the context of villages where many workers work away from the village part of the year. Several past studies have observed that the quality of electoral procedures is highly uneven (e.g., Birney, 2007; Brandt, Rozelle, and Turner, 2004; Pang and Rozelle, 2006).

⁵⁸They are available upon request.

check that our estimates are robust to controls for province-time varying factors such as per capita GDP, per capita agricultural GDP and government expenditure for public goods.⁵⁹ Second, we address the fact that several villages introduced elections prior to when our outcome data begin.⁶⁰ To check that our estimates are not driven by villages for which there is no variation in elections, we re-estimate the effect of elections on each outcome where each regression is restricted to a sample of villages for which we observe the outcome of interest before and after the first election. Third, we check that our estimates are not driven by lead villages that piloted the reform or lag villages for which the reform was delayed by re-estimating our baseline equation on a sample of villages that were not the first or last villages to introduce elections within a province, and on a sample of villages that introduced elections the same year as their counties. Fourth, we check that our estimates are not driven by especially motivated villages that implemented their first election before the national law by re-estimating our main equation using a sample restricted to villages that held their first elections after 1987. Finally, we check that our estimates are not driven by selection within counties by instrumenting for the introduction of elections at the village level with the introduction at the county level or with the introduction at the province level.⁶¹ All of these robustness exercises produce estimates that are very similar in magnitude to our baseline results.

7 Conclusion

Several recent theoretical studies on democratization characterize democratic regimes as providing high levels of public goods and engaging in redistribution. However, whether this characterization is true in practice is far from obvious given the larger body of theoretical work that points to the numerous ways in which democracies can fail to deliver the policies preferred by the majority of their citizens and the mixed empirical evidence. Our study addresses this important question by taking advantage of the Chinese electoral reforms, which relative to cross-country contexts, provide a well-identified natural experiment for studying the effects of democratization on public goods and redistribution.

We obtain several novel results. First, we find that the introduction of elections increased

⁵⁹These data are reported by *China Statistical Yearbooks*.

⁶⁰This is relevant for the analysis of public goods expenditure and provision and local taxes (the redistribution outcomes are already restricted to villages for which there is pre-election data).

⁶¹When we instrument with province-level introductions, we only examine public goods outcomes because income or land redistribution data are available for too few provinces.

overall total public goods investment by 27 percent. We also find that this increase in public goods expenditure is biased in favor of goods that presumably have higher demand. In addition, we find that the increase in expenditures is funded by villagers and that they are accompanied by an increase in the amount of local taxes paid by villagers.

Second, we find that elections caused significant redistribution of income from households that were rich before the election to households that were poor. In terms of the policy mechanisms used to achieve redistribution, we find that the redistribution of household farmland and wages paid by village enterprises played important roles. We also provide evidence that part of the redistribution was likely to have resulted from the reduction of land that was leased to enterprises, which had disproportionately benefitted elites. While our inability to estimate the household-level gains from the increase in public goods prevents us from being conclusive on the total redistribution caused by elections, it is interesting to note that one of the major increases in public expenditure went to irrigation, which is likely to disproportionately benefit poorer households since elections increased their farmland and agricultural income.

Our results on redistribution support recent theories of democratic transition which characterize democracies as regimes that are likely to engage in redistribution (e.g. Acemoglu and Robinson, 2001, 2006; Boix, 2003). Given the widely held belief that the level of public goods provision is far below the demands of villagers, our results on public goods are consistent with theories which predict that democracy and majoritarian rule increase public goods provision (e.g., Besley and Kudamatsu, 2008; Bueno de Mesquita, Smith, Siverson, and Morrow, 2003). Moreover, they are also consistent with cross-country evidence which suggests that democracy can improve public goods provision such as health and education and reduce inequality (e.g., Barro, 1996; Besley and Kudamatsu, 2006; Kudamatsu, 2011; Tavares and Wacziarg, 2001). In contrast, the results on public goods are inconsistent with the more traditional argument that democracy reduces the government's ability to provide public good investments because of short-term consumption demands from voters (e.g., Huntington, 1968). Interestingly, our finding that the increase in public goods provision is aligned with demand suggests that one of the reasons that elected leaders can increase taxes may be that they choose investments that correspond better to citizen preferences. These results are important as they provide rigorous evidence that democracy need not inhibit a governments' ability to raise

revenues for public goods.⁶²

Our findings also shed light on the mechanisms underlying democratization. First, since Chinese electoral reforms introduced elections without changing the constraints on the executive, our results provide empirical evidence for the impact of elections. They complement recent studies that emphasize the importance of constraints on the executive in determining economic outcomes (e.g. Acemoglu and Robinson, 2001; Besley and Persson, 2011). Second, we find that the introduction of open nominations caused no additional change after the initial introduction of elections and that the introduction of elections had similar effects on public goods regardless of whether it caused leadership turnover. This implies that an important driver of the effect of elections is the increased incentives of leaders rather than the villagers' ability to select different leaders.⁶³

In addition to providing evidence for the effects of democratization, our study speaks to the effectiveness of local elections in autocratic regimes, which has only very recently begun to receive serious attention from scholars (e.g. Martinez-Bravo, 2011; Martinez-Bravo, Padróimiquel, Qian, and Yao, 2011).⁶⁴ *A priori*, there are many reasons to be skeptical that local democracy would produce any sizable effects. For example, Acemoglu and Robinson (2008) argue that nothing but a drastic institutional reform that overhauls the power structure in society will have significant effects because existing elites can circumvent marginal democratic reforms. Our results provide a stark example where this need not be true by showing that local democracy has a significant impact on public goods and redistribution in China.

To conclude, we emphasize that this study, which focuses on estimating the *average* effects of elections in rural China, is a first take on the effects of Chinese elections and that there are many interesting and complementary questions that require further research. First, an equally important question regards the presence of heterogeneous effects. Due to space limitations, we pursue this in

⁶²Moreover, recall that to curb village-government corruption, the central government banned these *ad hoc* taxes with the *Tax and Fee Reform* (2003). Our results are consistent with the grave concern of some Chinese policy makers that in addition to curbing corruption, this reform could severely cripple the village governments ability to fund public goods. This is consistent with recent analyses which provide evidence that in many cases, the rural fiscal reform has reduced public good provision (Bird, Brandt, Rozelle, and Zhang, 2009; Zhang, Yan, Brandt, and Rozelle, 2005).

⁶³These findings support recent studies that argue for the importance of re-election incentives (e.g., Besley and Case, 1995; Besley and Coate, 2003; Dal-Bó and Rossi, 2008; Ferraz and Finan, 2011).

⁶⁴Many non-democratic regimes have introduced democratic reforms at the local level. Examples include Indonesia under Suharto (1968-1998), Brazil during the military dictatorship (1964-1985), Mexico under the PRI (1929-2000), rural China starting in the mid-1980s, and, more recently, in Vietnam (starting in 1998), Yemen (starting in 2001) and Saudi Arabia (starting in 2005). Similarly, many autocracies gradually open by increasing the power and independence of the legislative assembly (Myanmar and Morocco are very recent cases).

a companion paper (PadróiMiquel, Qian, Xu, and Yao, 2012), which investigates the interaction between social capital and the introduction of elections. Second, the findings that elections increase redistribution begs the question of whether elected leaders are more likely to redistribute to certain groups over others. For example, a recent study by Mu and Zhang (2011) finds that subgroups receive more public goods if a member of the subgroup is an elected village leader. Finally, another interesting question regards the motivation for autocratic regimes to introduce local democracy. This is important for understanding whether local elections are the first step to wider regime change, or whether they are used as an instrument for local governance that improves citizens' satisfaction of the autocratic central regime.⁶⁵

⁶⁵The latter hypothesis is consistent with the qualitative evidence discussed in Section 2. It is also consistent with Lorentzen's (2010) thesis that the Chinese government creates mechanisms for citizens to voice their preferences as a way to monitor cadres and improve governance, empirical evidence on the difficulty of central planning in the context of the Chinese Great Famine (Meng, Qian, and Yared, 2010) and current province-level economic performance (Persson and Zhuravskaya, 2011). In particular, Persson and Zhuravskaya (2011) argues that Beijing's recent strategy of rewarding regional leaders for measurable targets causes regional governments to under-invest in objects that are difficult to observe and objects that only yield returns in the long run.

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Table 1: Descriptive Statistics

Variable	Source	Obs	Mean	Std. Dev.	Variable	Source	Obs	Mean	Std. Dev.
A. Full Sample									
# of HH in Village	NFS	3,641	421.81	280.81	Median HH Gross Income (RMB)	NFS	1,300	11,386	10,508
Near City	NFS	5,208	0.30	0.46	Median HH Gross Income Growth	NFS	1,063	0.07	0.20
Village Expenditure in Public Goods (10,000 RMB)	VDS	4,340	14.28	135.47	HH Income 50th/90th	NFS	1,300	0.51	0.13
By Source of Financing:					HH Income 10th/50th	NFS	1,300	0.50	0.12
Funded by Villagers	VDS	4340	9.77	119.29	Median HH Farm Land (Mu)	NFS	1,300	5.23	6.05
Funded by Upper Governments	VDS	4340	4.42	64.21	HH Farm Land 50th/90th	NFS	1,267	0.57	0.14
By Object of Investment:					HH Farm Land 10th/50th	NFS	1,238	0.36	0.24
Schools	VDS	4,340	0.02	0.35	Median HH Fees & Levies to Village and County (100 RMB)	NFS	1,300	150.74	191.26
Irrigation	VDS	4,340	3.43	66.13	C. The Village Government and Elections				
Enrollment Rate	NFS	2,682	96.42	9.27	The Number of Village Committee Members	NFS	2,287	4.36	2.36
Total Village Land	NFS	3,612	9,245	14,719	The Number of Village Party Cadres	NFS	2,295	6.70	3.82
Arable Land (Mu)	NFS	3,612	2,295	2,329	Party Secretary Tenure	VDS	5,208	10.03	8.13
Share of Village Land that is Arable	NFS	3,612	0.51	0.32	Village Chief: Tenure	VDS	5,208	6.69	6.24
Used for HH Farming (Mu)	NFS	3,612	2,215	2,312	Has Election	VDS	5208	0.73	0.44
Not Used for HH Farming (Mu)	NFS	3,612	79.72	367.26	Has Open Nomination	VDS	5208	0.20	0.40
Leased Out to Enterprises (Mu)	NFS	3,612	60.46	347.61	Years between 1st Election in Village and Province	VDS	217	5.02	5.07
Median HH Gross Income (RMB)	NFS	3,778	10,513	8,366	Years between 1st Election in County and Province**	VDS	217	4.28	4.67
Median HH Annual Gross Income Growth	NFS	3,093	0.07	0.19	Years between 1st Election in Village and County**	VDS	217	0.74	2.28
HH Income 50th/90th	NFS	3,778	0.53	0.12	Years since last election	VDS	1,084	3.16	1.02
HH Income 10th/90th	NFS	3,778	0.51	0.12	VC different from previous term*	VDS	4,312	0.16	0.36
					1st Election Changed VC*	VDS	182	0.38	0.49
					Anonymous Voting	VDS	1,287	0.39	0.49
					Proxy Voting	VDS	1,263	0.72	0.45
					Roving Ballot Box	VDS	1,249	0.65	0.48
					>1 candidate per position	VDS	1,293	0.79	0.41

Notes: Each observation is at the village-year level. VDS indicates that the variable is reported by the *Village Democracy Survey*. NFS indicates that the variables are reported by the *National Fixed Point Survey*. *Not all villages retained records of VC's names from prior to the first election. **The year of the first election in a county is based on respondent recall.

Table 2: The Effect of Elections on Leader Powers and Characteristics

	Dependent Variables			
	Appt. Manager	Enterprise Workers	Land Reallocation	Public Goods Expenditure
	(1)	(2)	(3)	(4)
Panel A. VC has Unilateral Power				
<i>Dep. Var Mean</i>	0.32	0.27	0.33	0.18
Post Election	0.048 (0.033)	0.034 (0.028)	0.048 (0.031)	0.042 (0.024)
Obs	3336	4103	3936	4457
R ²	0.801	0.785	0.800	0.779
Panel B. VC and PS Share Power				
<i>Dep. Var Mean</i>	0.37	0.44	0.53	0.67
Post Election	0.001 (0.029)	-0.005 (0.027)	-0.008 (0.031)	-0.026 (0.026)
Obs	3336	4103	3936	4457
R-Square	0.797	0.769	0.804	0.808
Panel C. PS has Unilateral Power				
<i>Dep. Var Mean</i>	0.29	0.25	0.15	0.31
Post Election	-0.051 (0.029)	-0.031 (0.024)	-0.042 (0.028)	-0.017 (0.026)
Obs	3336	4103	3936	4457
R ²	0.832	0.819	0.798	0.774
Panel D. VC Characteristics				
	VC Turnover	Age	Years of Edu	Party Member
<i>Dep. Var Mean</i>	0.17	42.83	8.03	0.76
Post Election	0.045 (0.022)	-2.442 (0.817)	0.791 (0.257)	-0.034 (0.043)
Post Open Nomination	-0.052 (0.022)	0.762 (0.818)	0.085 (0.240)	-0.087 (0.042)
Obs	4312	4188	4194	4274
R ²	0.065	0.430	0.611	0.484

Notes: All regressions control for province-time trends, and village and year fixed effects. The regressions in panels A-C also control for post first open nomination. The number of observations vary across columns because observations are missing if the specified power is not relevant for that village-year. Standard errors are clustered at the village level.

Table 3: The Effect of Elections on Public Goods

	Dependent Variables					
	Ln Expenditure by Source			Public Goods Provision Measures		
	Total (1)	Villagers (2)	Non-Village (3)	Ln Arable Land (4)	Share of Arable Land (5)	School Enrollment Rate of Children Age 7- 13 (6)
<i>Dep. Var. Mean (not logged, 10,000 RMB)</i>	14.28	9.77	4.42	<i>A. Total Expenditure</i>		
Post 1st Election	0.272 (0.116)	0.309 (0.105)	0.002 (0.075)			
Observations	4340	4340	4340			
R ²	0.191	0.171	0.188			
<i>Dep. Var. Mean (not logged, 10,000 RMB)</i>	3.43	2.02	1.41	<i>B. Irrigation Expenditure</i>		
Post 1st Election	-0.275 (0.167)	-0.193 (0.156)	-0.075 (0.144)	-3.297 (1.653)	-0.15025 (0.051)	
Post 1st Election x Avg Ln Village Farm Land	0.055 (0.023)	0.041 (0.022)	0.012 (0.019)	0.444 (0.220)	0.01834 (0.007)	
Observations	4340	4340	4340	3291	3277	
R ²	0.120	0.123	0.106	0.876	0.917	
<i>Dep. Var. Mean (not logged, 10,000 RMB)**</i>	20.05	11.04	8.89	<i>C. Primary Schools Expenditure**</i>		
Post 1st Election	-12.678 (16.846)	-3.133 (15.346)	-8.904 (8.987)			-1.340 (1.837)
Post 1st Election x Avg. # Kids 7-13	0.013 (0.006)	0.014 (0.007)	-0.001 (0.002)			0.008 (0.004)
Observations	4220	4220	4220			2682
R ²	0.082	0.072	0.098			0.299

Notes: All regressions control for post first open nomination, province-time trends, village and year fixed effects. The regressions in Panels B and C also control for the interaction of the relevant village characteristic (e.g., the average number of kids age 7-13, average in household farm land) with post first open nominations. **In Panel C columns (1)-(3), the dependent variables are multiplied by 1,000 for presentation purposes. Standard errors are clustered at the village level.

Table 4: The Effect of Elections on Local Taxes

	Dependent Variable: Ln Household Local Taxes									
	Deciles of Tax Distribution (within villages)									
	10th (1)	20th (2)	30th (3)	40th (4)	50th (5)	60th (6)	70th (7)	80th (8)	90th (9)	
<i>Dep. Var. Mean (not logged, RMB)</i>	64.36	93.48	113.15	132.91	150.74	169.49	191.01	220.89	275.07	
Post 1st Election	1.366 (0.587)	0.858 (0.596)	0.963 (0.590)	1.237 (0.601)	1.224 (0.643)	1.237 (0.638)	1.245 (0.656)	0.938 (0.587)	0.346 (0.463)	
Observations	1,300	1,300	1,300	1,300	1,300	1,300	1,300	1,300	1,300	
R ²	0.547	0.558	0.580	0.568	0.592	0.590	0.594	0.603	0.570	

Notes: All regressions control for province time trends, village and year fixed effects. Standard errors are clustered at the village level.

Table 5: The Effect of Elections on Income

	Dependent Variable: Ln Average Household Income												
	Pre-Election Income Distribution (within villages)											Income Ratios	
	0-10 (1)	10-20 (2)	20-30 (3)	30-40 (4)	40-50 (5)	50-60 (6)	60-70 (7)	70-80 (8)	80-90 (9)	90-100 (10)	10/90 (11)	50/90 (12)	10/50 (13)
A. Ln Total Household Income													
Dep. Var Mean (not logged)	10,005	10,390	11,005	12,158	12,299	13,107	13,249	16,056	16,575	26,637	0.56	0.73	0.74
Post Election	0.246 (0.066)	0.096 (0.078)	0.114 (0.064)	-0.014 (0.066)	0.046 (0.060)	0.044 (0.062)	-0.059 (0.068)	-0.11 (0.083)	-0.077 (0.082)	-0.338 (0.069)	0.209 (0.070)	0.203 (0.038)	0.17 (0.036)
Obs	605	605	605	605	605	605	605	605	605	605	605	605	605
R ²	0.843	0.863	0.864	0.870	0.847	0.863	0.838	0.846	0.846	0.801	0.424	0.480	0.446
B. Ln Income from Agriculture													
Dep. Var Mean (not logged)	5,454	7,285	6,308	7,355	6,963	7,893	8,594	10,779	12,538	21,191	0.53	0.69	0.74
Post Election	0.829 (0.349)	0.236 (0.078)	0.36 (0.065)	0.206 (0.089)	0.044 (0.083)	0.047 (0.125)	-0.005 (0.085)	-0.007 (0.108)	-0.142 (0.085)	-0.395 (0.072)	0.245 (0.089)	0.232 (0.090)	0.302 (0.062)
Obs	605	605	605	605	605	605	605	605	605	605	605	605	605
R ²	0.616	0.795	0.769	0.727	0.744	0.725	0.721	0.766	0.765	0.753	0.540	0.542	0.372
C. Ln Income from Wages													
Dep. Var Mean (not logged)	2,104	2,172	2,270	2,243	2,415	2,366	2,625	2,662	3,165	3,741	0.53	0.69	0.74
Post Election	9,009 (0.908)	6,906 (0.937)	5,416 (1.025)	2,926 (0.751)	1,862 (0.496)	0.753 (0.192)	-0.427 (0.717)	-0.864 (0.585)	-0.749 (0.320)	-0.956 (0.219)	-0.825 (0.779)	-0.664 (0.843)	1.178 (0.302)
Obs	605	605	605	605	604	605	605	605	605	604	605	605	605
R ²	0.868	0.777	0.679	0.600	0.557	0.531	0.498	0.516	0.521	0.457	0.124	0.117	0.227

Notes: All regressions control for province-time trends, village and year fixed effects. Standard errors are clustered at the province level. Panel A examines average household total income according to households' pre-election position on the distribution of total income within villages. Panel B examines average household income from agriculture and home production according to households' pre-election positions on the distribution of agricultural and home production income within villages. Panel C examines average household wage income according to households' pre-election positions on the distribution of wage income within villages.

Table 6: The Effect of Elections on Land

	Dependent Variables														
	Ln Household Land (According to the Pre-election Land Distribution within Villages)					Land Inequality					Ln Land Leased Out to Enterprises	Ln Land Leased Out to Enterprises Dummy			
	0-10	10-20	20-30	30-40	40-50	50-60	60-70	70-80	80-90	90-100	10/90	50/90	10/50	(14)	(15)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)		
<i>Dep. Var Mean (not logged)</i>	4.70	5.30	5.67	6.30	6.30	6.83	7.24	7.53	8.09	9.71	0.56	0.78	0.67	67.72	0.31
Post 1st Election	0.464 (0.118)	0.155 (0.069)	0.068 (0.051)	0.111 (0.061)	0.007 (0.048)	-0.036 (0.051)	-0.047 (0.040)	-0.041 (0.069)	-0.127 (0.058)	-0.260 (0.085)	0.222 (0.076)	0.112 (0.062)	0.228 (0.078)	-0.524 (0.251)	-0.114 (0.055)
Observations	605	605	605	605	605	605	605	605	605	605	605	605	605	3,438	3,438
R ²	0.880	0.948	0.954	0.949	0.954	0.941	0.949	0.916	0.944	0.923	0.421	0.374	0.565	0.517	0.528

Notes: All regressions control for the introduction of open nominations, province-time trends, village and year fixed effects. Standard errors are clustered at the province level. Columns (14)-(15) include villages that lease some arable land to enterprises prior to elections.

Table 7: The Effect of Elections, Open Nominations and Leader Turnover

	Dependent Variables				
	(1)	(2)	(3)	(4)	(5)
	Ln Public Investment	Ln Public Investment Financed by Villagers	Ln Median HH Tax	Income 10/90**	Land 10/90**
A. The Effects of Open Nominations					
Post Election	0.272 (0.150)	0.309 (0.119)	1.224 (0.657)	0.209 (0.070)	0.222 (0.076)
Post Open Nomination	-0.110 (0.150)	-0.059 (0.126)	0.254 (0.395)	-0.053 (0.053)	0.061 (0.103)
Observations	4340	4340	1300	605	605
R ²	0.191	0.171	0.592	0.424	0.421
B. The Effects of Elections x VC Turnover					
Post Election	0.301 (0.191)	0.349 (0.151)	1.359 (0.771)	0.262 (0.094)	0.241 (0.081)
Post Election x 1st Election VC Change	-0.133 (0.258)	-0.118 (0.279)	-0.278 (1.027)	-0.186 (0.086)	-0.085 (0.083)
Observations	3920	3920	1264	587	587
R ²	0.196	0.177	0.597	0.429	0.423

Notes: All regressions control for post first open nomination, province-time trends, village and year fixed effects. The regressions in Panel B also control for the interaction term of post first open nomination and whether the first election caused VC turnover. Standard errors are clustered at the province level. **The dependent variables in columns (4) (and column (5)) are the income (and land) ratios according to households' positions on the within-village income (and land) distributions prior to the first election.

Table 8: The Effect of Elections – Robustness to Controlling for Pre-Election Conditions

Controls:	Dependent Variables							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		<i>Pre Avg Share of Years where HH had 2+ kids x Year FE</i>	<i>Pre Avg Land Expropriation x Year FE</i>	<i>Pre Avg Ln Total Public Expenditure x Year FE</i>	<i>All Full Sample Controls x Year FE</i>	<i>Pre Avg Income 10/90 x Year FE</i>	<i>Pre Avg Land 10/90 x Year FE</i>	<i>All Controls</i>
Post 1st Election	0.272 (0.150)	0.272 (0.148)	0.281 (0.151)	0.231 (0.150)	0.296 (0.143)	0.383 (0.226)	0.343 (0.229)	0.317 (0.347)
Observations	4340	4340	4320	4340	4320	680	680	680
R ²	0.191	0.195	0.198	0.203	0.223	0.236	0.234	0.337
Post 1st Election	0.209 (0.070)	0.211 (0.067)	0.227 (0.078)	0.195 (0.081)	0.239 (0.084)	0.215 (0.064)	0.244 (0.098)	0.235 (0.061)
Observations	605	605	605	605	605	605	605	605
R ²	0.424	0.439	0.432	0.435	0.478	0.433	0.469	0.531
Post 1st Election	0.222 (0.076)	0.223 (0.084)	0.238 (0.081)	0.220 (0.089)	0.238 (0.105)	0.179 (0.061)	0.210 (0.051)	0.176 (0.062)
Observations	605	605	605	605	605	605	605	605
R ²	0.421	0.433	0.434	0.431	0.468	0.446	0.432	0.488

Notes: All regressions control for province-time trends, village and year fixed effects. Standard errors are clustered at the village level. **In Panel B, the dependent variable is the income ratio for households according to their positions on the within-village income distribution prior to the first election. The controls in column (5) are the pre-election average of this variable for each village interacted with year fixed effects. In Panel C, the dependent variable is the land ratio according to households' positions on the within-village land distribution prior to the first election. The controls in column (6) are the pre-election average of this variable interacted with year fixed effects.

Table 9: The Effect of Elections – Robustness to Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variables						
Controls:	Baseline	Election Procedures*	Size of Largest Clan x Year FE	Family Tree or Ancestral Temple x Year FE	Post Tax & Fee Reform	Ever Merged with Another Village x Year FE	All controls
Post 1st Election	0.272 (0.116)	0.319 (0.168)	0.267 (0.116)	0.268 (0.117)	0.272 (0.116)	0.270 (0.117)	0.312 (0.173)
Observations	4,340	4,340	4,340	4,340	4,340	4,340	4,340
R ²	0.191	0.191	0.194	0.200	0.191	0.194	0.206
	A. Ln Total Public Investment						
Post 1st Election	0.209 (0.102)	0.115 (0.072)	0.207 (0.108)	0.238 (0.112)	0.211 (0.102)	0.237 (0.111)	0.194 (0.097)
Observations	605	605	605	605	605	605	605
R ²	0.424	0.427	0.437	0.458	0.424	0.437	0.484
	B. Income 10/90**						
Post 1st Election	0.222 (0.076)	0.259 (0.128)	0.188 (0.059)	0.222 (0.092)	0.224 (0.075)	0.202 (0.088)	0.177 (0.154)
Observations	605	605	605	605	605	605	605
R ²	0.421	0.434	0.453	0.442	0.422	0.497	0.555
	C. Land 10/90**						

Notes: All regressions control for province-time trends, village, and year fixed effects. The standard errors are clustered at the village level. *In column (2), the controls are three time-varying dummy variables indicating whether the preceding election had a roving ballot box, allowed proxy voting, or had anonymous ballots. **In Panel B, the dependent variable is the income ratio for households according to their positions on the within-village income distribution prior to the first election. In Panel C, the dependent variable is the land ratio according to households' positions on the within-village land distribution prior to the first election.

Figure 1: The Effect of Elections on Total Household Income and Income from Agriculture by Pre-Election Total and Agricultural Income Decile

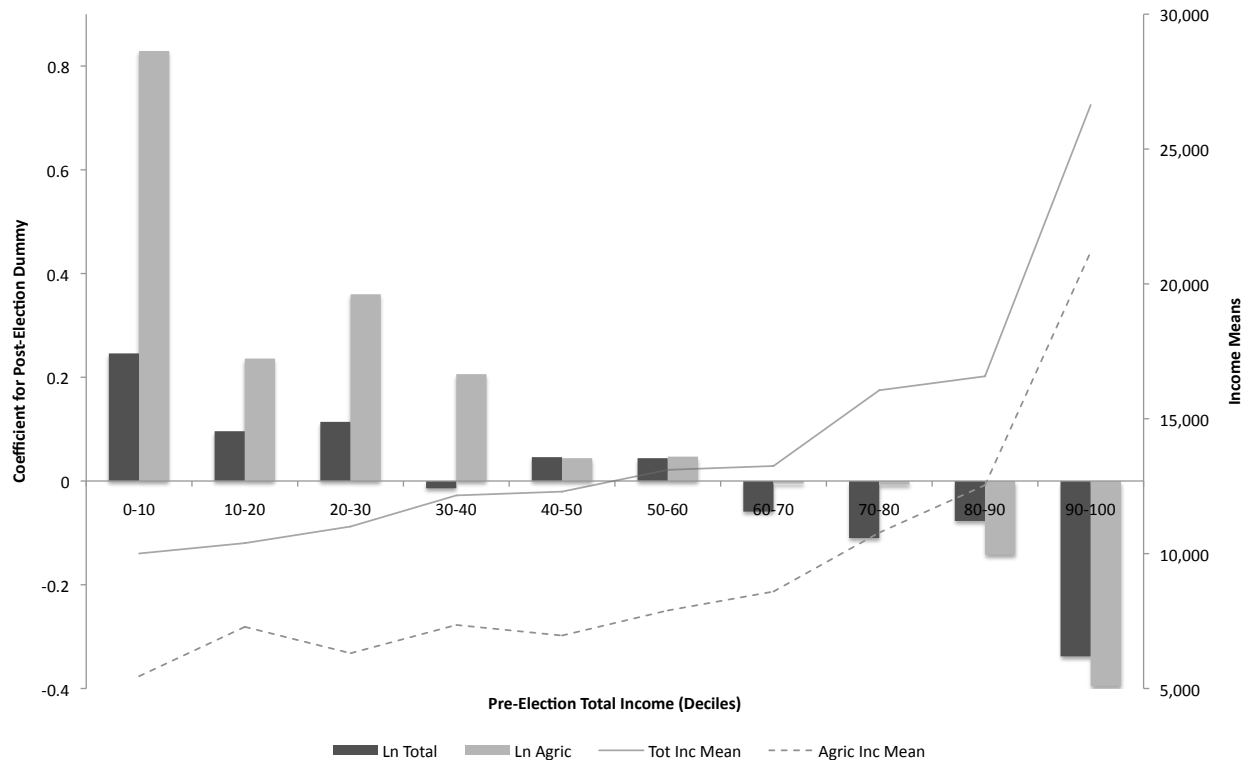


Figure 2: The Effect of Elections on Wage Income by Pre-Election Wage Income Decile

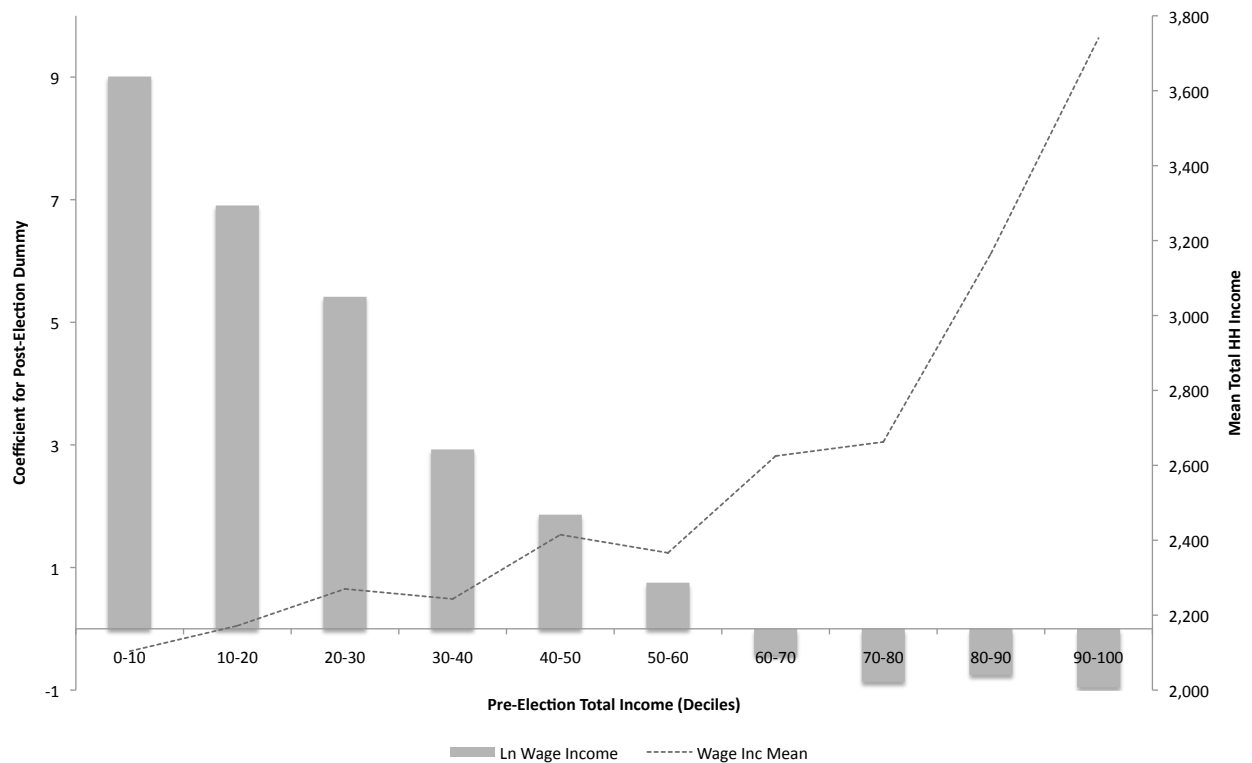


Figure 3: The Effect of Elections on Household Farmland Redistribution by Pre-Election Farmland Decile

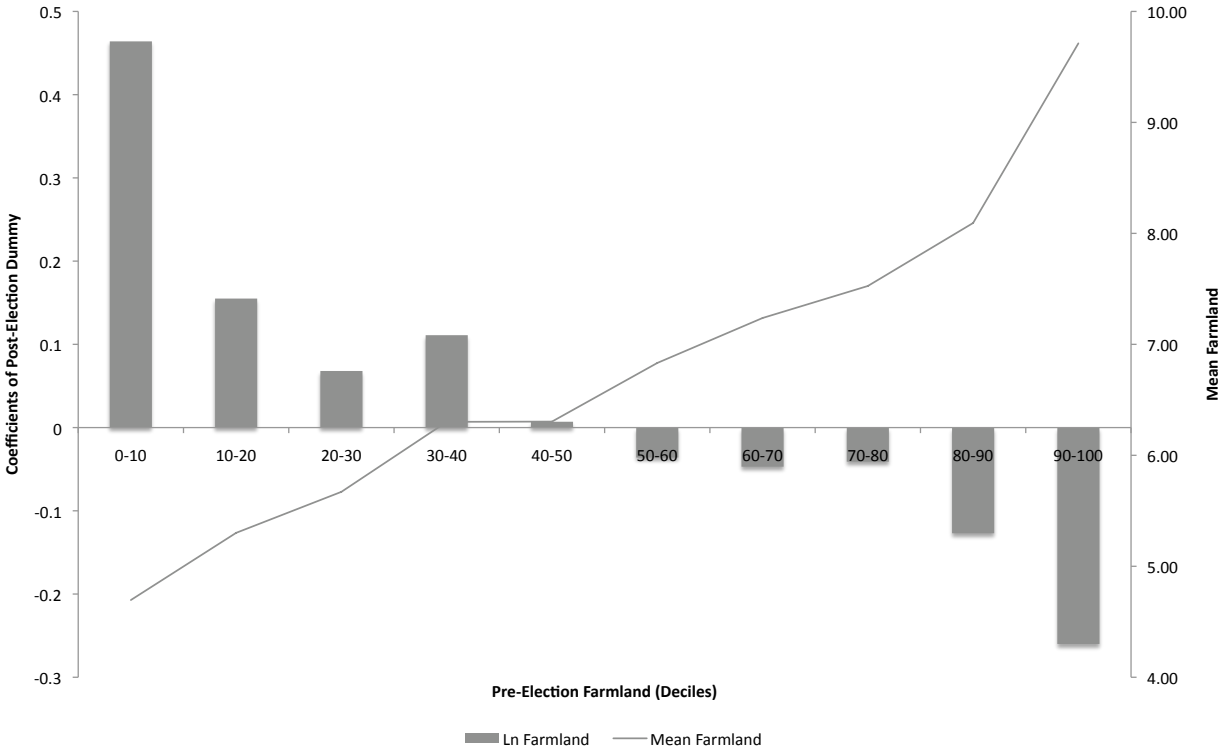


Figure 4: The Effect of Elections on Ln Total Public Goods Expenditure

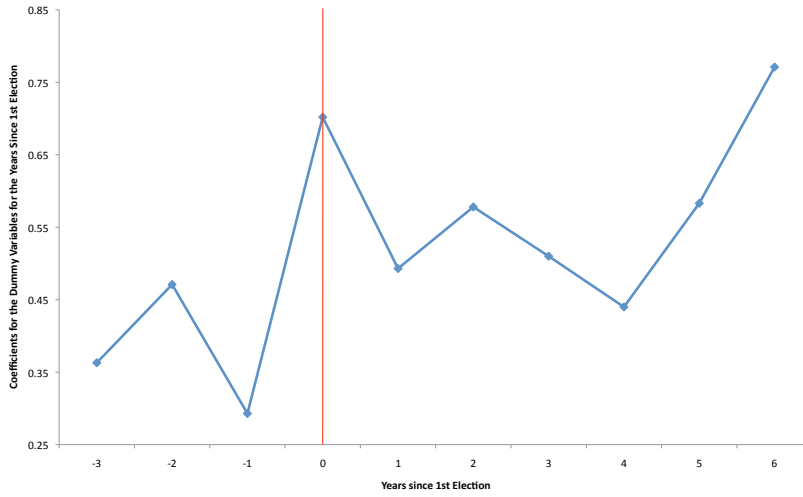


Figure 5: The Effect of Elections on Income Redistribution
(Ratio of incomes for households in the first pre-election income decile to households in the tenth pre-election income decile)

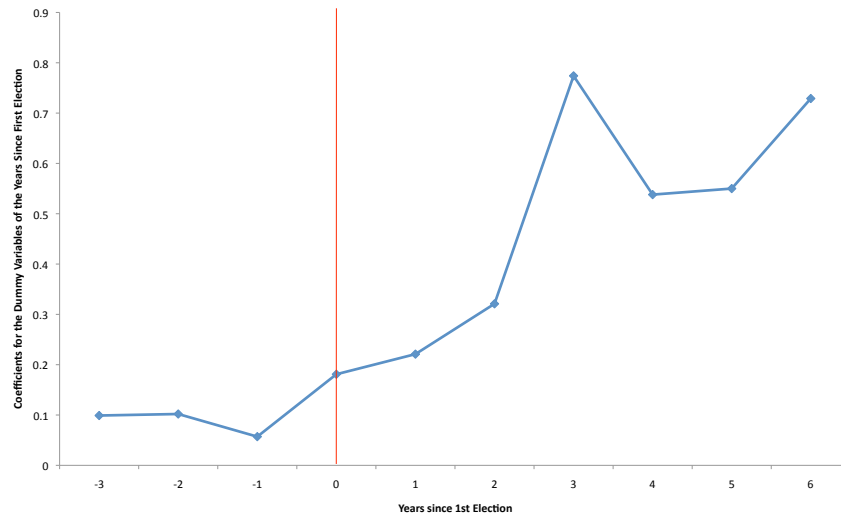


Figure 6: The Effect of Elections on Land Redistribution
(Ratio of farmland for households in the first pre-election farmland decile to households in the tenth pre-election farmland decile)

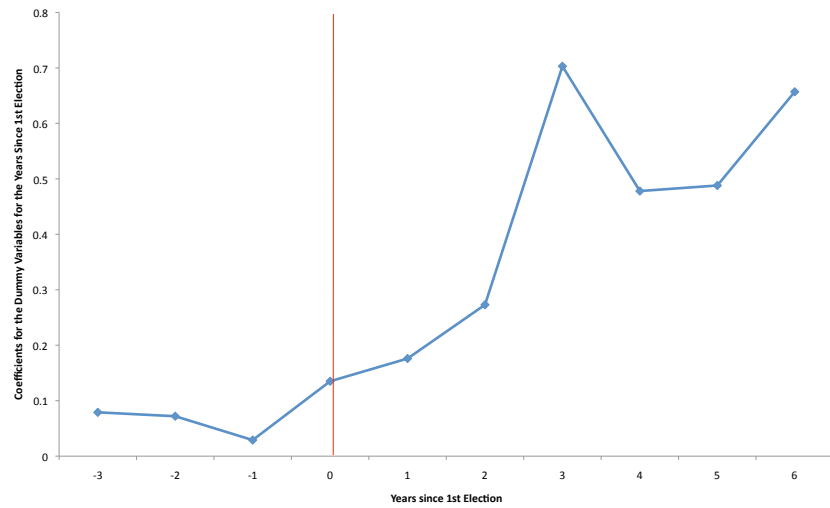


Table A.1: The Timing of Electoral Reforms

Year	First Election		First Open Nominations (Haixuan)	
	Number of Villages Introducing (1)	Cumulative % of Villages (2)	Number of Villages Introducing (3)	Cumulative % of Villages (4)
1982	13	5.99	1	0.72
1983	13	11.98	1	1.44
1984	42	31.34	7	6.47
1985	3	32.72	0	6.47
1986	35	48.85	4	9.35
1987	12	54.38	1	10.07
1988	7	57.6	1	10.79
1989	15	64.52	1	11.51
1990	25	76.04	1	12.23
1991	1	76.5	0	12.23
1992	3	77.88	1	12.95
1993	6	80.65	3	15.11
1994	2	81.57	3	17.27
1995	9	85.71	3	19.42
1996	4	87.56	18	32.37
1997	3	88.94	0	32.37
1998	6	91.71	6	36.69
1999	9	95.85	42	66.91
2000	7	99.08	12	75.54
2001	2	100	12	84.17
2002	0	100	11	92.09
2003	0	100	3	94.24
2004	0	100	1	94.96
2005	0	100	7	100
Total	217		139	

Notes: Each observation is a village.

Table A.2: The Effect of Elections for Each Year Since the First Election

	Dependent Variables					
	Ln Total Public Investment		Land 10/90*		Income 10/90*	
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy variables indicating the number of years since the first election:	coef	se	coef	se	coef	se
-3	0.363	(0.225)	0.079	(0.126)	0.099	(0.091)
-2	0.471	(0.248)	0.072	(0.133)	0.102	(0.100)
-1	0.293	(0.232)	0.029	(0.138)	0.057	(0.104)
0	0.702	(0.215)	0.135	(0.160)	0.181	(0.132)
1	0.493	(0.236)	0.176	(0.154)	0.221	(0.129)
2	0.578	(0.231)	0.273	(0.190)	0.321	(0.131)
3	0.51	(0.230)	0.703	(0.392)	0.774	(0.357)
4	0.44	(0.222)	0.478	(0.250)	0.538	(0.180)
5	0.583	(0.251)	0.488	(0.207)	0.55	(0.156)
6	0.771	(0.266)	0.657	(0.275)	0.729	(0.205)
Obs	4340		605		605	
R ²	0.193		0.46		0.433	

Notes: All regressions control for province-time trends, village, and year fixed effects. Standard errors are clustered at the province level. *In columns (3)-(4), the dependent variable is the income ratio for households according to their positions on the within-village income distribution prior to the first election. In columns (5)-(6), the dependent variable is the land ratio according to households' positions on the within-village land distribution prior to the first election.