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The Effects of Democratization on Economic Policy: Evidence from China*

(Preliminary Draft)

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Abstract

This study investigates the effect of the introduction of elections on public goods and redistribution in the context of rural China. Our study collects a unique survey to document the history of political reforms and economic policies in 217 villages for the years 1980-2005. To establish causality, we exploit the staggered timing of the introduction of elections. Our results show that elections increase public goods expenditure by 27%, and farmland by 20-27% for median village households. The increase in public goods is paralleled by an *increase* in local taxes and the change in land allocation is paralleled by a reduction in income inequality. In addition, elections reduce the enforcement of unpopular upper-government policies such as family planning and the expropriation of village land. We argue that these empirical findings provide strong support for the characterization of democracy in recent theories of democratization.

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1 Introduction

How democracy affects economic policies such as taxation and public goods provision is a central question for policy makers as well as researchers in political economy, development economics and political science. Recently, several prominent theoretical studies have emphasized democracy's proclivity to implement majoritarian policies and argue that relative to autocracies, democracies provide higher levels of public goods (e.g., de Mesquita et al., 2003; Lizzeri and Persico, 2004; Besley and Kudamatsu, 2008), and are more likely to engage in redistribution (e.g., Acemoglu and Robinson 2000, 2001, 2006; Boix, 2003).[1] However, a much larger body of theoretical studies highlight the shortcomings of democracy, which could lead to failures in public goods provision or redistribution.[2] The empirical evidence, which mostly comes from cross-country studies, is 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. We argue that this reform provides a uniquely advantageous context for studying the effects of democratization on public goods and redistribution for the following reasons. First, the reform was stark and well-defined. Village leaders were appointed by the Communist Party prior to the reform, and switched to being elected by villagers. Importantly, elections were introduced without changing *de jure* constraints on executives. Thus,

[1] Recent studies such as Acemoglu and Robinson (2000, 2001, 2006); 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. Studies based on accountability theories, such as de Mesquita et al. (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. In a related study, Lizzeri and Persico (2004) proposes a theory in which democracy solves a commitment problem for the elite and also results in more public goods provision.

[2] For 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).

[3] In 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 et al., 1998; Tavares and Wacziarg, 2001; Reuveny and Li, 2003), higher human capital (Tavares and Wacziarg, 2001) and better health indicators (Besley, 2006; Kudamatsu, 2011). However, in a large study looking at several socio-economic policy dimensions, Gil et al. (2004) find that democracy is associated with no difference on the outcomes they examine. Also, democracy seems to have a weakly negative relation relationship with GDP growth in the cross-section (Barro, 1996; Tavares and Wacziarg, 2001), and a weakly positive relationship using other data and techniques (e.g., Rodrik and Wacziarg, 2005; Persson and Tabellini, 2007; Papaioannou and Siourounis, 2008). These studies are well-aware of the difficulty of omitted variables and use strategies such as controlling for country fixed effects to address it. See studies the latter set of studies for strategies for addressing the crudeness of country-level measures of democracy.

we can interpret our results as the effect of a change in only one of the two key components of democracy – elections, which increase representation, and checks and balances, which constrain the executive.[4] Second, the timing of the introduction of elections varied across regions and villages. Village elections were typically initiated at the behest of the province level and introduced in villages of each province in a quasi-random fashion.[5] Thus, the introduction of elections was unlikely to be correlated to factors that could affect economic policy, such as changes in culture or human capital, enabling the causal identification of the impact of elections.[6] Third, the omitted variables problem is further minimized in two ways. First, relative to most democratic transition episodes, China was politically, socially and economically stable during this period; second, relative to cross-country comparisons, Chinese villages are much more similar to each other.[7] Finally, Chinese villages have substantial fiscal autonomy. Therefore, changes in village government can plausibly affect public good provision and redistribution.[8]

Our empirical analysis proceeds as follows: 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 of217 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 are the longest and broadest panel ever constructed to describe the political economy of Chinese villages, and the first to systematically document the fiscal and political structure of village governments.[9]

Second, we estimate the causal impact of the introduction of elections. The main difficulty is 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 villager preferences. To address this, we

[4]Elections and checks and balances on the executive are commonly considered to be the two fundamental institutions that characterize democracies (e.g., Tavares and Wacziarg, 2001; Besley, 2006).

[5]There are few exceptions. Please see section 3 for a detailed discussion.

[6]Our analysis does not take this as given, and carefully considers the correlates of the introduction of elections.

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

[8]Relative to cross-country comparisons, our context is not very suitable for directly examining economic growth. Many of the most relevant policy instruments for that outcome that are available to policymakers at the national level such as introducing better protection of property rights or economic and trade liberalization are not relevant to villages.

[9]See discussion below for a review of earlier studies on Chinese elections.

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, between villages that have already introduced elections to those that have not. 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 the macroeconomic changes in China during this period. 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 not correlated to factors that could affect the outcomes of interest through channels other than the reform. Section 3 provides a detailed discussion of the qualitative evidence on electoral reforms to motivate this assumption. However, we do not take this as given and conduct a large number of exercises to check the robustness of our identification strategy after presenting the main results.

The first set of outcomes of interest are public goods expenditure and provision. First, we estimate whether, on average, the provision of public goods increased as a result of the introduction of elections. Second, to investigate whether changes in public goods provision correspond to demand from villagers, we predict demand for specific public goods and estimate the interaction effect of the introduction of elections and a proxy for demand on the specific public goods. Furthermore, to investigate whether changes in expenditure reflect reallocations of government funds or changes in village governments' ability to raise revenues, we examine the effect of elections on different sources of public goods funding, paying particular attention to within village funds.

The second main outcome of interest is redistribution. Since village governments do not have the power to impose regular taxes and therefore can not use taxes and transfers to redistribute income, we examine household land allocation, which is the main determinant of income and wealth in rural China and is determined by the village government. We estimate the effect of elections on household farmland for households on each decile of the within-village distribution of farmland, and also on land not allocated to households. We also examine how changes in land allocation affect income distribution by estimating the effect of elections on income from different sources for households on different deciles of the within-village income distribution.

We conduct several exercises in addition to the main analysis. First, we consider and provide

evidence against the interpretation that results reflect changes in upper-government preferences. Specifically, we examine the effect of elections on important upper-government policies that are unpopular amongst villagers such as the One Child Policy and upper-government expropriation of village land. Second, motivated by the recent literature on re-election incentives, we explore the mechanisms driving the effects of elections (e.g., Besley and Case, 1995; Besley and Coate, 2003; Dal-Bó and Rossi; 2008; Ferraz and Finan, 2011). In particular, we investigate the extent to which the main results reflect increased incentives for leaders or the villagers' ability to select different leaders relative to the Communist Party. Finally, we conduct a large number of exercises to check the validity of our empirical strategy and the sensitivity of our main results to controlling for factors that can affect the effectiveness of elections.

This study contributes to the existing literature in several ways. First, by comparing mostly fiscally autonomous units, it adds to the cross-country evidence on the effect of democratization on public goods and redistribution that was discussed at the beginning of the introduction. Our analysis is novel in being able to better identify the causal impact of elections and in directly examining taxation, for which existing studies have provided indirect evidence inferred from examining public goods. Second, it adds to a smaller number of within-country studies of the effects of changes in aspects of democracy (e.g., Besley and Case, 1995; Besley and Coate, 1995; Foster and Rosenzweig, 2005; Fujiwara, 2011).[10] 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.[11] Third, in identifying the effects of elections, our study contrasts and complements recent studies that emphasize the importance of the 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 and, in particular, in China (e.g., Lorentzen, 2010; , 2011).

Finally, our study is the first to systematically document the history of electoral reforms and

[10]For example, Besley and Case (1995) find that binding term limits affect the policy choices of U.S. governors, and Fujiwara (2011) shows that extending the effective franchise increases public goods provision. However, these studies 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.

[11]Besley and Coate(2003) find that elected regulators are more responsive to consumer demands and lower prices relative to appointed regulators.

the political and economic structure of Chinese villages in such detail. This allows us to add to previous studies that have provided important evidence on the effect of elections on public goods and inequality using small panels or large cross-sectional data.[12] Our results show that these effects can be generalized to almost all of China. More importantly, the richness of our data allows us to examine a much broader set of outcomes (e.g., the sources of public goods funding, local taxes, land allocation, income by source, enforcement of unpopular upper government policies) and to estimate the interaction effect of the introduction of elections and leadership change, which are important for understanding the mechanisms of why elections matter.

This paper is organized as follows. Section 2 briefly describes the data. Section 3 discusses the background. Section 4 presents the empirical strategy. Section 5 presents the main results. Section 6 examines the mechanisms behind the effect of elections. Section 7 tests the robustness of the main results. Section 8 summarizes and discusses the results and offers concluding remarks.

2 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 in two waves.[13] The first wave, conducted 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. The VDS forms a balanced panel of 217 villages for the years 1982-2005. The second survey is the *National Fixed-Point Survey* (NFS), a detailed village-level and household-level economic survey collected and maintained by a research centre 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 panel is not balanced since the NFS introduced villages over time to maintain representativeness.

The NFS villages were chosen in 1987 to be nationally representative for rural China at the time the survey began. The VDS surveys the same villages as the NFS so that the policy data could be

[12] For example, see Gan et al., 2007; Luo et al., 2007, 2010; Shen and Yao, 2008; Zhang et al., 2004; Birney, Rozelle and Boisvert, 1994, 1995; Rozelle and Li, 1998; Jacoby et al., 2001; Oi and Rozelle, 2000; Kennedy et al., 2004; Brandt and Turner, 2007; Mu and Zhang, 2011.

[13] The questionnaires are available at http://www.econ.yale.edu/~nq3/NANCYS_Yale_Website/Surveys.html

matched to the economic data. The data used in this paper include villages from 29 provinces.[14] From the NFS, we were able to obtain village-level data for all villages, but household-level data for only a third of the villages.

To avoid recall bias, 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., teacher, traditional doctor) in each village. This applies to very few of our variables and we will note it in the text.

Our data have several advantages. First, these are probably the most comprehensive data on village-level reforms and village-level outcomes 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 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, it means that 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 within-village distribution of economic outcomes in addition to their means.

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.[15] The second drawback is that the NFS, which is mainly an agricultural labor and production survey, did not collect detailed demographic data. Therefore, we can only proxy for variables such as fertility and schooling with crude measures

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

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

of the number of children age 0-6 or the fraction of children age 7-13 that are in school. 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 in the study.

3 Background

3.1 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.[16] Since all levels of government above the village are dominated by the 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 extremely 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 public goods provision and land allocation (see Rozelle and Boisvert, 1994; Whiting, 1996; Oi and Rozelle, 2000; Brandt and Turner, 2007).

Village governments do not have legal authority to impose regular taxes. For example, it is illegal for a village government to impose recurrent taxes. Therefore, village governments must raise revenues by imposing *ad hoc* fees and levies. In our paper, we refer to these *ad hoc* fees as *taxes* for simplicity. It follows that it is difficult for village leaders to credibly commit to redistribute

[16]The Chinese government, led by the Chinese Communist Party (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.

income since *ad hoc* taxes are by construction one-time events. This is the main motivation for our empirical examination of redistribution to focus instead on household farmland, the allocation of which is within the discretion of the village government.

Note that village 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 village 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. Moreover, many believe that the ban was never completely enforced.[17] In any case, we will explicitly control for this reform in the section on robustness.

3.2 Electoral Reforms

Motivation The first local elections were introduced in the early 1980s soon after the dismantling of the commune system. Proponents of the reform used two main arguments to defend this introduction.[18] 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 conception 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 hierarchical monitoring structure not only observed the actions of local leaders imperfectly, but also faced the difficulty of knowing the preferences and needs of each locality.[19] The introduction of local elections was seen as a potential solution to this problem because it shifted the monitoring responsibilities onto villagers. Proponents argued that making local leaders accountable to villagers would impose checks on the VC's behavior and would also allow villagers to select the most competent candidates (Kelliher, 1997).

> "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

[17] See studies such as Zhang et al. (2004) and Luo et al. (2010) for studies of the Tax and Fee Reform.

[18] See O'Brien, 1994; Kelliher, 1997; O'Brien and Li, 1999 for descriptions of the policy debates that led to the official introduction of local elections.

[19] See Meng et al. (2010) for a study of the role that information problems can play in a centrally planned regime in the context of China's Great Famine.

the chairmanship meeting of the Standing Committee of the Sixth NPC, April 6, 1987 (O'Brien and Li, 1999).

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 these 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.

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 needed to 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.[20] Villagers may abstain from voting. There was no change in the selection method of the members of the village party branch and PS positions, who continued to be appointed. The party also maintained control over the villages by allowing the local party branch to appoint candidates. Thus, the main change that the reform effected 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.

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.

The elections were implemented top-down. Each level of government would pilot the reform in a few select villages, and once the procedures and logistics were tested, then the reform would be rolled out (O'Brien and Li, 1999). Anecdotal evidence from interviews that the authors conducted with county and province-level officials and the speed in which elections were implemented within

[20]The last requirement ensured that the elected VC had sufficient mandate. For 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.

provinces suggest that the roll-out was orthogonal to village characteristics in most cases. By all accounts, villages had little discretion over the timing of introduction of elections, which is characteristic of reforms in rural China.

> "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 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." — Unger (2002, p. 222).[21]

There are two notable exceptions. First, the model villages that piloted the reform conducted elections earlier. Second, elections were sometimes delayed for "problematic" villages that had a history of non-compliance to unpopular central government policies (e.g., Oi and Rozelle, 2000; Li, 2009) or had a large kinship clan that could dominate other villagers in a majoritairan regime.[22]

There are several additional facts to keep in mind. 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 from the same village. As a consequence, candidates typically run on well-understood issues and are probably selected for qualities that are observable on a daily basis.[23] Second, despite aberrations in electoral procedures, studies of Chinese elections have found that the introduction of elections improved village leadership accountability (see in particular Brandt and Turner, 2007).

3.3 Descriptive Evidence

In this section, we briefly describe our data on electoral reforms and the village government.[24] The data present several interesting facts. First, when we compare the VC to the PS, we find that the VC's tenure is typically shorter. Second, consistent with the fact that most of the candidates were

[21] Unger (2002) also 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.

[22] The 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).

[23] There 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).

[24] The key descriptive statistics are also shown in Table 1 Panel C.

appointed by the party during the early reform period, we find that 77% of VCs are party members and 46% served as village cadres before being VC. Third, the mean village in our sample had held its first election by 1989 and its first election with open nominations by 1997. Note that 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.[25]

To examine whether the elections were *de facto* implemented 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-level governments. The timing of the first election in each county, excluding a respondent's own village, is based on respondent recall. To maximize accuracy, our surveyors only record a date if all respondents from a village agree. If there is no consensus, then this variable is recorded as missing. Since provinces are large and respondents could not confidently recall the year of the first election within a province, this is inferred as the first election of a village in each province according to our survey.

Our data indicate that 16% of villages held their first elections prior to the introduction of elections by the county government, 66% held their first elections the year that the county introduced elections, and 18% held its first election afterwards. 60% of villages within a province introduce elections within three years of the first election in that province. 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 support the qualitative literature discussed earlier. First, the fact that most villages introduce elections at the same time 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. More specifically, 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. 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 the introduction being mostly quasi-random and orthogonal

[25] See Appendix Table A1 for a more detailed timing of the introduction of these reforms.

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, we find that most of the elections with too few candidates were first elections, and 85% 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 provided by 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 addresses the concern that village records report elections as they are supposed to occur and not what actually occurs. Finally, we find that there was a 38% VC turnover for the first election, which is almost twice as high as the average turnover rate in the sample (17%).

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 in spirit 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 differences between villages, such as geographic characteristics (e.g., hilliness or distance from a city). 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 DD estimates. First, we allow time effects to vary flexibly rather than assuming that they are constant in the pre-reform and post-reform periods (i.e., control for a post dummy instead of year fixed effects). Second, we include province-time trends, which allows our estimates to control for the widening differences across regions brought about by unequal economic growth during the long time horizon of our study.[26] Controlling for province-specific time trends

[26]Note that we control for province-time trends instead of the more flexible *province* × *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 there are many province-year cells within which there is no variation in election.

means that our estimates mostly rely on within-province variation in the timing of the introduction of elections. This captures the fact that election timing was mainly determined at the province level, which we argue is unrelated to the characteristics of the average village in each province. This is important for our identification strategy, which assumes that the timing of the introduction of elections is uncorrelated with factors that determine the outcomes of interest, conditional on the baseline controls. We do not take this as given and check the validity of our assumption later in the section on robustness.

Our baseline specification also controls for the second wave of reforms that made *open nominations* of candidates mandatory. This controls for potential heterogeneity in the effect of elections and improves the precision of our estimates.[27] The effect of elections can be characterized as the following equation:

$$Y_{vpt} = \beta Election_{vpt} + \lambda OpenNom_{vpt} + \gamma_p t + \delta_v + \rho_t + \varepsilon_{vpt}. \quad (1)$$

Y_{vpt} is the policy outcome of village v in province p during calendar year t. It is a function of: whether the first election, $Election_{vpt}$, and the first open nomination, $OpenNom_{vpt}$, has taken place; province-year trends, $\gamma_p t$; village fixed effects, δ_v; and calendar year fixed effects, ρ_t. All standard errors are clustered at the village level. The main coefficient of interest is β. It will be statistically different from zero, $\hat{\beta} \lessgtr 0$, if elections had an effect on a particular policy outcome.

There are several important points to keep in mind for our strategy. First, elections were implemented in heterogeneous ways and there were many procedural aberrations. For example, some of the initial elections did not have as many candidates as required by law, and elections vary substantially in dimensions such as the anonymity of ballots or whether the ballot box was in a fixed location during election day. While these differences are interesting, we do not control for them in the baseline because they are outcomes of the reform. We return to this in the section on robustness.

Second, despite the qualitative evidence, one may be concerned that the timing of elections was not random within provinces. For example, if the upper government timed elections to coincide with other reforms or policy changes at the village level, then the interpretation of our estimates will

[27] We find that omitting this control does not affect the magnitude of our estimates of the introduction of elections. For brevity, these results are no reported in the paper, but are available upon request.

be confounded. Similarly, it is possible that a history of poor policy enforcement, which delayed the introduction of elections in some places according to the qualitative evidence, is correlated with factors that would generate a change in the outcomes of interest through channels other than elections. This seems unlikely *a priori*. However, to be cautious, we return to discuss these potential problems in detail after we present the main results.

5 Main Results

5.1 Public Goods and Taxation

5.1.1 Expenditure

Table 2 presents the results on 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 allows us to separately examine expenditures according to the source of the funds, which we categorize as funds from villagers and funds for non-village sources.[28] Consistent with the assertion from the descriptive literature 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 2 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. 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 zero and statistically insignificant.

[28] Villages began recording public goods expenditures in 1986 at the request of the Ministry of Agriculture and follow ministry guidelines in the categorization of the source of funding.

5.1.2 Appropriate Public Goods Provision

The increase in public goods investment can reflect an improvement in public goods provision and quality of life for citizens. However, this may not be the case if the public goods are not needed by villagers or if village officials are able to embezzle or misallocate village funds.[29] We investigate these possibilities in two ways.

First, we examine whether the increase in public goods investment corresponds to the needs of villagers – i.e. whether these investments are *appropriate*. We are able to proxy for the villagers' demands for two public goods: irrigation and schooling.[30] We assume that villagers living in villages that rely more on household farming have higher demand for irrigation and those who live in villages with more school-age children have higher demand for schools. To find the effect of elections on appropriate public goods investment, we estimate equation (1) with log public expenditure on irrigation and primary schools as dependent variables.[31] As explanatory variables, we add the interaction effect of the introduction of elections (and open nominations) and the average log amount of village land used for household farming in the irrigation equation; and the interaction effect of the introduction of elections (and open nominations) and the average number of children of ages 7-13 in a village in the primary schools equation. We interact elections with the average of each characteristic for each village to address the possibility that the year-to-year measures can be outcomes of the introduction of elections.[32]

Panel B in Table 2 shows the effect of elections on irrigation investment. The negative estimate for the main effect of the introduction of elections in column (1) shows that elections reduce public expenditures in irrigation for villages with no household farmland, while the positive interaction

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

[30] The remaining public funds are spent on sanitation, roads (within villages), electricity, environment (e.g., planting trees).

[31] Primary schools are the only schools in Chinese villages.

[32] The estimating equation can be written as:

$$\begin{aligned} Y_{vpt} = {} & \theta Election_{vpt} + \zeta OpenNom_{vpt} \\ & + \beta(Election_{vpt} \times X_{vp}) + \lambda(OpenNom_{vpt} \times X_{vp}) + \gamma_p t + \delta_v + \rho_t + \varepsilon_{vpt}, \end{aligned} \tag{2}$$

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

effect between elections and average log household farmland shows that elections increase irrigation for villages with more farmland. The estimates are statistically significant at the 5% and 1% levels, respectively.

In Panel C, we examine public expenditures on schooling. The main effect in column (1) is negative, but this estimate is statistically insignificant. 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. These results show that the increases in public expenditures correspond to demand. Moreover, a comparison of the interaction effects in columns (2) and (3) show that the increase in appropriate public goods investment is also driven by funding by villagers.

The second way to address the concern that public funds are misallocated is to examine 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 data are reported by the NFS.[33] The sample mean presented in Table 2 panel B column (5) shows that approximately half of village land is arable on average. The mean in panel C column (6) show that approximately 96% of children age 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 reduce arable land for villages with no farmland and increase arable land for villages with farmland. The main effect of elections and 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 strongly suggest that elections improve appropriate public goods provision.

Our findings that elections not only increase public goods expenditure, but that such increases correspond to villagers' need, and that elections also increase actual public goods provision show that elections improve public goods provision.

[33]Data for arable land and total land are available for the years 1987-2005 (excluding 1992 and 94) 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.

5.1.3 Taxes

The finding that the changes in public expenditures are entirely driven by changes in financing by villagers seems inconsistent with the notion that democratization reduces the government's ability to tax due to the consumption demands of constituents (e.g., Huntington, 1968). However, it is possible that elected officials increase public goods expenditures by reallocating funds from other purposes. In that case, public expenditures can increase without an increase in taxation.

To investigate this, we directly examine the effect of elections on 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, there was no policy change that increased such taxes when reforms were introduced.

Data on household taxes are only available for approximately a third of the full sample of villages for the years 1986-2005 (excluding 1992 and 94).[34] In this sample, households pay 320 RMB per year in local taxes on average, which is approximately 3% of gross income. In Table 3, we present the estimated effects of the introduction of elections on taxes paid by households. To investigate whether the change in taxes differed for households across the village, we divide the data according to the household's position on the within-village distribution of taxes paid for each year.[35] The estimates show that elections increased local tax payments for households on the 10th-80th quantiles by 0.64 to 0.91 log points, which equals approximately 70-160%. The estimates are statistically significant at the 10% or higher level for all households except those on the 20th percentile. Note that the estimate for households on the 90th percentile is smaller in magnitude than the other estimates. Given that there are only approximately 73 villages in the sample used for these estimates, this is most likely an aberration driven by outlier observations.

These results are consistent with elections having increased villager-funding of public goods by increasing taxes. To assess the plausibility of the magnitude of the effect, it is important to keep in

[34] Villages in the subsample have similar median incomes, growth and income inequality as the full sample of villages. See Table 1.

[35] We can alternatively divide the households according to their position on a time-invariant distribution of household taxes within village, or according to their position on a time-varying or time-invariant distribution of within-village income distribution. As the different methods all produce similar results, we show only one for brevity. The other results are available upon request.

mind that average local taxes are very low, only approximately 3% of total gross income. Therefore, a 70-160% increase does not result in an implausibly high level of local taxes.

5.2 Land

5.2.1 Household Land Distribution

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). By law, the amount of land per household is supposed to depend on need and the ability to farm the land, and all rural households are entitled to enough farmland to guarantee subsistence. Households cannot rent out or sell their land rights.

Data for household farmland allocation is reported by the NFS for the years 1986-2005 (excluding 1992 and 94). We were able to obtain this data for a third of the full sample of villages. The means presented in Table 4 columns (1)-(9) for households on each decile of the within village household land distribution. They show that households have two to nine mu of land on average, where 1 mu is equal to 1/15th of a hectare. Since average land allocations are so small, our study retains this indigenous unit of measurement for convenience. Note that there is substantial inequality in land allocation across households.

Next, we examine the effects on each decile of the time-varying within-village distribution of land allocation to investigate whether elections induce redistribution.[36] The estimates for households on the 30th-70th percentile are very similar in magnitude in columns (3)-(7). The estimates for the 40th-60th percentiles in columns (4)-(6) are statistically significant at the 1% level. These estimates show that elections increase household farmland by approximately 0.28 to 0.25 log-points, or 15-29% for approximately 30-50% of households in the village.

These results are consistent with elections reducing inequality. However, we find no evidence of land redistribution since elections do not reduce land holdings for any household. This is most likely

[36] As with the tax results, the estimates are similar if we divide households according to their position on a time-invariant within-village distribution of land holding, or according to their positions on within-village income distributions. Thus, for brevity, we only show one set of results. The others are available upon request.

due to the fact that all households are legally entitled to certain levels of farmland and it would be very difficult for village leaders to reduce land for any household. This raises two questions. First, where does the additional land come from? Second, does the reduction in land inequality result in a reduction of income inequality? We address these two questions below.

5.2.2 Government Controlled Land

The results on public goods provide evidence that the introduction of elections increases irrigation and arable land. Therefore, the increase in household farmland may partly be due to the increase in total arable land. Here, we investigate whether part of the increase in household land is also due to a reduction in the amount of (pre-existing) arable land that was directly controlled by the village government prior to the introduction of elections.

A small fraction of village land is retained under the direct control of the village government 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 to villagers for farming or non-farm activities, or leased to entities from outside of the village.[37] The profits generated from these activities are typically reinvested or paid out to the villagers. Villagers who work in the enterprises also benefit from earning wages. It is generally believed that not all villagers benefit from these enterprises because they are either unprofitable or because the rents are not distributed equitably.[38]

Data for village land use is reported by the NFS for all villages for the years 1987-2005 (excluding 1992 and 94). The villages in our sample dedicate approximately 96% of arable land (which is approximately 51% of total village land) to households for farming. Approximately 75% of the remaining arable land is leased out to "enterprises", a term which we use to include firms run by collectives or villagers (see Table 1).

In columns (10)-(11) of table 4, we examine the effect of elections on the amount of village land not used for household farming. We measure this as either the log of the amount of land, or a dummy variable indicating whether any arable land is not used for household farming. Since elections can only reduce the amount of land not used for farming if this occurred prior to the first election, we

[37] For a study that describes land use and contracts in rural China, see for example, Rozelle and Li, 1998.

[38] For example, corrupt village leaders have been known to extract personal rents from land controlled by the village governments (Rozelle and Boisvert, 1994; Oi and Rozelle, 2000; Brandt and Turner, 2007).

restrict our analysis to villages that ever used any arable land for non-household farming prior to the first election. The estimates show that elections reduced the amount of arable land not used for farming by 0.71 log-points, which is approximately 51%, and the probability that any arable land was not used for farming declined by fourteen percentage-points. These estimates are statistically significant at the 1% level. In columns (12)-(13), we present analogous estimates for land that is leased out to enterprises using a sample of villages that ever leased any land to enterprises prior to elections. The estimates show the same pattern. The introduction of elections reduced land leased out by 0.64 log-points, which is approximately 47%, and the probability that any land is leased out by fifteen percentage-points. These estimates are statistically significant at the 1% level.

These results suggest that elected village leaders increase household farm land by reducing the amount of arable land that was previously not used for household farming. If the rents (e.g., profits, wages) generated from land held by the village government are distributed inequitably, then our results also indicate that elections could be reducing rent skimming by the village elites.

5.2.3 Income

Since land is the main determinant of income, we investigate whether the effect of elections on household land allocation is reflected in income changes. Data for household income is reported by the NFS for the years 1996-2005 (excluding 1992 and 94). We were able to obtain data for total household income for all villages and household income by source for a third of the villages in the full sample. In our data, the average median household earns approximately 10,513 RMB of gross income per year.[39] 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 growth than urban areas. Consistent with the fact that land is a key determinant of income, Table 1 shows that measures of income inequality are similar to measures of inequality in land holdings.[40] In the sub-sample for which we have income data by source, households earn

[39]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 earn approximately 11,391 RMB per year. During most of our study period, one RMB was approximately equal to eight 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.

[40]We calculate inequality as the quotient of household land for the 50th (10th) percentile households divided by land for the 90th (50th) percentile households according to the distribution of within-village land holding for a given year. Appendix Table A1 shows that the number of observations for these inequality measures are slightly smaller than the other sub-sample measures because the there are a few observations where the households in the denominator

approximately 67% of their total income from agriculture and home production (see Appendix Table A2).

In Table 5, we present the sample means and estimated effects of elections on income for each decile of the within-village distribution of household income. In panel A, we examine income from the profits of enterprises. These include village enterprises as well as private and collective enterprises. Consistent with the belief that not all households benefit from enterprises, we find that the amount of enterprise income is disproportionately higher for the richest households, and the poorest 30% of village households do not receive any such income. The estimated coefficients are negative in sign for all households, and increasing in magnitude with the total income of the households. The estimate for the richest 90th percentile households are statistically significant at the 1% level. The reduction of income from enterprises is consistent with the reduction in land that is leased out to enterprises. These results also show that the reduction is disproportionately larger for the richest households that benefited the most from enterprise income prior to the reform.

In panel B, we examine wage income, which most households in our rural sample earn from working at village enterprises. Panel B shows that estimates for the effect of elections on wage income are positive in sign for households with incomes below the median, but negative for households above the median of total household income. They are statistically significant at the 1% level for households at the 20th, 70th, 80th and 90th percentiles. Elections reduced wage income of rich households by approximately 0.59 to 1.3 log-points, or 35-72%. In contrast, for poor households, it increased wage income by up to 0.9 log points, which is equivalent to a 148% increase. The fact that elections reduce the wage income of the rich could simply be an artifact of rich households being more likely to have worked for enterprises prior to elections. However, the increase in wage income for the poor suggests that elected leaders may also be using their influence to favor hiring the poor.

Next, we examine income from agriculture. The estimates in panel C show that the effect of elections on agricultural income is positive in sign for households on the 50th percentile of the village income distribution and below, but negative for richer households. The estimates are statistically insignificant for all households.[41]

Finally, we examine the effect of elections on total household income. We were able to obtain

of these calculations have zero land holdings.

[41] Note that we can alternatively examine agricultural income according to household farm land allocation. These estimates are similarly imprecise. They are not presented in the paper, but are available upon request.

this measure for fewer quantiles, but for all 217 villages. The estimates in Panel D show that the introduction of elections reduced total household income for the households on the 75th and 90th percentiles by approximately 5.7% and 8.3%. These estimates are statistically significant at the 1% level. The estimates for poorer households are smaller in magnitude and statistically insignificant. Given these results, it is not surprising that we also find that elections reduce income inequality by increasing the ratio of the median to the 90th percentile household income by 1.7 percentage-points (see column 9). This estimate is statistically significant at the 10% level.[42]

To summarize, these results are consistent with elected leaders redistributing through land allocations. Our findings suggest that the introduction of elections reduces the incomes of the rich by reducing the amount of arable land not used for household farming, the proceeds of which disproportionately benefit richer households. At the same time, the introduction of elections increases the amount of farmland allocated to households in the middle of the village distribution of land allocation. As a consequence of the changes in land allocation, elections reduce within village income inequality.

6 Mechanisms

The main results presented so far are consistent with recent theories of democratization that emphasize that policy should favor the views of the median voter in democracies (e.g. Acemoglu and Robinson, 2001, 2006; Boix, 2003) or that elected leaders are accountable to the majority (e.g. de Mesquita et al., 2003). However, one might be concerned that the authoritarian context in which we obtain these results complicates this interpretation. Specifically, recall that the party maintained tight control of electoral implementation and that elections did not directly change the power of the village party branch. These constraints could limit the ability of elected village leaders to affect policies. In this case our main results would reflect a change in upper-government policy preferences that occur when elections are implemented, rather than the effect of electoral pressure on policies.

[42]To check that the results on income are not a result of rich households under-reporting income to avoid taxation, we also examine consumption. The estimated coefficients for the introduction of elections are more negative and larger in magnitude for richer households. Therefore, it seems unlikely that our income results are driven by strategic under-reporting of income. These results are statistically insignificant. We do not show them for brevity. They are available upon request.

We also examine the effect of elections on income growth. We use both our main specification as well as the method developed by Arellano and Bond (1991). We find no effect: the estimates are very small in magnitude and statistically insignificant. For brevity, we do not report these estimates. They are available upon request.

This seems rather unlikely given that we find that the increase in village public goods is entirely driven by an increase in within-village financing (as opposed to upper-government transfers). Similarly, it seems unlikely that the upper-government would involve itself with the administrative burden of the details of within-village land allocation, especially for land allocations of such a small scale.

Nonetheless, this section addresses the concern that the elections were not meaningful by providing direct evidence that: *i*) elected VCs had *de facto* power to affect policies; *ii*) elections lead to an increase in the turnover of village leadership, and *iii*) elections caused village leaders to be more accountable to villagers. In addition, we investigate whether the effects of elections are driven by re-election incentives or by the citizens' ability to select better candidates.

6.1 VC Power

To establish that VCs have some *de facto* control over policies, we document the powers of the VC and PS. We identified the most important policies under the jurisdiction of the village government through focus groups and interviews with local government officials and villagers. Then, in the VDS we asked whether the VC or the PS have the unilateral power (i.e. "signature rights") to take these important decisions or if consent from both is necessary to reach a decision. These means are reported in Table 6 panels A-C. They show that VCs had substantial power over policies. Our data show that the VC's approval is required over the appointment of managers, employment of workers at village enterprises, land reallocation and public goods expenditures in 69-86% of observations (the sum of unilateral and joint powers shown in panels A and B). It follows that the VC has *de facto* power over important village decisions, and in particular over land allocation and public goods, which focus our analysis above.

Next, we estimate the effect of elections on the *de facto* powers of the villager leaders to check whether those opposed to the introduction of elections subverted the reform by decreasing the power of the elected leader. Note that the number of observations changes because not all policies are relevant for every village. The estimates in Table 6 panel A show that elections increased the *de facto* powers of the VC. The estimated effect of elections on the unilateral power of the VC for each of the powers measured by our data is positive. The estimate for the power to fund public expenditures is statistically significant at the 1% level (see column 5). As reported in Panel C,

this is paralleled by a reduction in the unilateral power of the PS across all powers. The estimates in panel C are statistically significant at the 1% level for the power to appoint village enterprise managers, the power to reimburse public expenditures and the power to reallocate land. These results show that elections shifted *de facto* power from the PS to the VC. Note that the cause of the increase in VC power after elections are introduced is not important for our study. The key point of these findings is to establish that elected VCs had the power to affect policy.[43]

6.2 VC Turnover

In panel D, we present the effects of elections on leader turnover and leader characteristics. Note that the observations change across columns because not all villages recorded all characteristics of village leaders for the entire period. The results show that the introduction of elections increased the probability that the VC in office is not the same person as the VC from the previous term by 4.5 percentage-points (column 1), reduced the age of VCs at the time of entering the office by approximately two years (column 2) and increased the educational attainment of VCs by almost one year (column 4). These estimates are statistically significant at the 1% level and are consistent with the belief that the introduction of elections increased the power of villagers over candidate selection.

In panel D, we also report the effect of the introduction of open nominations to note that the latter reform does not cause further changes in leader characteristics, with the exception of the likelihood of the VC being a party member before he enters office.[44] This suggests that for the most party, the party chooses candidates that are similar to the preferences of villagers or the party is able to circumvent the open nomination reform and manipulate its own candidates into office.

6.3 A Shift in Accountability

For our findings to speak to theories of democratization, the results on public goods and land should reflect the fact that elections change the VCs position from being only accountable to the party to being accountable, at least partially, to a majority of villagers. In this section, we provide direct

[43]The increase in power could be caused by several different mechanisms. For example, it could be due to the democratic mandate that elections give the VC. Alternatively, it is also consistent with the upper government providing more support to the elected VCs.

[44]The introduction of open nominations reduces the likelihood of a party member entering office by 8.7 percentage points.

evidence that elections shifted accountability. We exploit the existence of policies that the VC can influence and the villagers and the party are known to have opposing and strong preferences. If elections successfully make VCs more accountable to villagers (and therefore less accountable to the party), it is straightforward that these policies should move in favor of villagers insofar as the VC has discretion. A formal model of leadership accountability, which demonstrates this effect, is presented in a companion paper (Martinez-Bravo et al., 2011).

In the context of rural China, such policies are the One Child Policy (henceforth OCP) and upper-government land expropriation. The VCs are ultimate enforcers of each policy, which are pushed by upper governments. While the VC is unlikely to openly resist the upper government, he can reduce or delay the enforcement of a policy by not exerting effort to enforce them. In the case of the OCP, this means that the VC can simply not monitor pregnancies, cajole parents to abort, or impose fines. In addition, the VC can exert more effort to apply for legal exemptions from the upper government.[45] In the case of land expropriation, it means that the VC does not block villagers' attempts to protest.[46]

We create a dummy variable that equals one if the village records indicate that any household in a village had more than one child in a given year.[47] The data show that at least one household has more than one child in half of the village-year observations.[48] Table 7 column (1) shows that the introduction of elections increased the probability that any household in a village gives birth to a second child by 8.6 percentage-points. The estimate is statistically significant at the 1% level. We

[45]For example, *State Council Document No. 7*, which was introduced in 1984, lists causes for obtaining legal exemptions. These include cases in which the first child is a girl or if the parents are handicapped.

[46]Upper-government land expropriation are always highly contentious issues as they result in the permanent loss of farmland to villagers (O'Brien and Li, 1999; Guo, 2001; Bernstein and Lu, 2003; Cai, 2003). Village leaders are needed for reallocating remaining village land so that dispossessed households have farmland and occupation, as well as other activities to minimize the dissatisfaction of villagers with the central government. Therefore, while village leaders will rarely directly oppose land allocation, they can resist expropriation through non-cooperation. O'Brien and Li (2006: Ch 3) provides many examples of how village governments coordinate "rightful" resistance in protest against land expropriation.

[47]Since village leaders are supposed to enforce the OCP by law, we de-sensitized our survey question by asking two factual questions "Did any household in your village have a second child because the first child is a girl?" and "Did any household in your village have a second child?". The answers are recorded from village records. Since there are legitimate exemptions to the OCP, we believe that the indicator variables that we record are unlikely to be intentionally mis-recorded. Specifically, the first question refers to an exemption that is permitted by the central government for curbing female infanticide. The second question naturally follows from the first.

[48]The data also show that there is substantial time and regional variation in the enforcement of the One Child Policy. For example, the average standard deviation of whether any household had two children in a year is 0.37 within a province (and year). In addition, we document from another data source, the 1989 wave of the *China Nutrition and Health Survey,* that there is substantial variation in the policies implemented across villages within a county. These facts are all consistent with the hypothesis that local governments have significant discretion over the implementation of family planning policies.

can alternatively measure the stock of children. The NFS reports the stock of children age 7-13 for the years 1993, and 1995-2005. Thus, we are able to construct a measure of the stock of children age 0-6 as the number of children 7-13 in year $t + 7$ for the years 1986, and 1988-98.[49] Column (2) shows that the estimated effect on the stock of young children is positive in sign but statistically insignificant. The lack of precision is most likely due to the small sample size. Nevertheless, the two results are consistent and suggest that elections reduced the enforcement of the OCP.

Next, we examine land expropriation. Upper-government land expropriation occurs infrequently in our data: only in 4% of the village-year observations. In the regression analysis, we will also examine the outcome of this policy, which is the amount of total village land. In our sample, the average village has 8.4 log mu of total village land. Column (3) suggests that elections reduce the incidence of land expropriation. However, the estimate is statistically insignificant, which is not surprising given the rarity of the event. In Column (4), we examine total village land. This variable is reported by the NFS for the years 1986-92, 93, 95-2005. If elections reduce the incidence of land expropriation, we should observe that the introduction of elections increases total village land. We find that this is indeed the case: elections increase village land by 0.1 log mu. The small magnitude of the average effect is consistent with the rarity of the event. However, the estimate is precisely estimated and statistically significant at the 1% level.

These results provide strong support for the view that elections were successful in shifting VC accountability in favor of villagers. The competing hypothesis that these results might reflect a change in upper-government policies towards villages when elections are introduced can be further weakened by examining policies for which villagers have strong preferences but the VC has no way of influencing. If the results on the OCP and land expropriation are exclusively driven by party favoritism, it should not make any difference whether the implementation required the cooperation of the VC. In contrast, if the results are driven by a shift in accountability, we should observe a larger effect on policies that require the VC's cooperation.

In columns (7) and (8), we examine the effects of elections on the distance to the nearest high school and the amount of special aid transfers from the upper government. High schools are typically located in townships and cities. The locations are determined with no participation

[49] Since there is very little migration during our period of study, especially amongst young children, who are typically left at home even if the parents migrate away to work, this should be a reliable measure. See West and Zhao (2000) for a survey of studies on internal migration patterns during the period of our study.

from the village government. Similarly, special aid transfers are automatically made to villages and households below the poverty line. The assignment of special aid is determined by the provincial and central government. We find that elections have no effect on these outcomes. These results directly contradict the hypothesis that our results are driven by changes in upper-government preferences or directives.

6.4 Incentives versus Selection

The political agency literature proposes two main mechanisms that voters use to hold elected politicians accountable.[50] First, elections can help voters address moral hazard problems by rewarding good performance with re-election. In this way, 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 our results are mainly driven by incentives rather than by better candidate selection.

The first piece of evidence comes from the fact that the introduction of open nominations for candidates had little effect in changing the characteristics of leaders in addition to the introduction of elections (Table 6 panel D). Table 8 panel A presents the estimates from the baseline equation for our main outcomes of interest and shows that open nominations also have little effects on these outcomes. Unless the party was already selecting the candidates that villagers wanted before the introduction of open nominations, these results suggest that candidate selection is not an important driver of the results.

The second piece of evidence comes from examining the effect of elections on villages where the first election did not cause leader turnover. In our sample, approximately 64% of the villages retained the same VC after the introduction of elections, while 36% of first elections caused leader turnover. Since the selection effect does not operate if there is no turnover, finding that elections have similar effects on these two categories of villages provides evidence for the incentive effect. 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

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

election changed the VC in office, while controlling for its interaction with the introduction of open nominations and the same baseline controls as before.[51] The estimated main effect of elections reveals the effect of the introduction elections for villages that do not experience VC turnover in the first election (i.e. the *incentive* effect), and the interaction effect reveals the additional effect of elections when there is VC turnover (i.e. the *incentive* and *selection* effects).

The estimates are presented in Table 8 panel B. The estimated main effect is very similar to the main effect in panel A and the interaction term is always small in magnitude and statistically insignificant. This means that elections have very similar results regardless of whether there is VC turnover and that the effect of elections are mainly driven by incentive effects.

7 Robustness

The main concern for our empirical strategy is that the timing of the introduction of elections may be endogenous such that the determinants of timing are correlated with factors that could affect the outcomes of interest through channels other than elections. Our estimation strategy compares outcomes before and after elections are introduced, between villages that have already introduced elections to those that have not. If the villages that adopted elections earlier are also more responsive to the introduction of elections, then our estimates of the average effect of elections would overstate the true effect. For example, if villages with higher latent demand for public goods provision introduce elections earlier, our main results would be biased upwards**.** Our prior is that this is an unlikely possibility. Given that our baseline estimates control for village and year fixed effects and province-year trends, a confounding omitted variable would need to be village-specific *and* time-varying, as well as vary within provinces and over time in a way that is not captured by the linear time trends. Nevertheless, to be cautious, this section carefully considers possible confounding factors and provides evidence that our main results are unaffected by them.

Early Movers Since the national OLVC law to introduce elections was enacted in 1987 and the qualitative evidence suggests that villages that introduced elections prior to this law were likely to have been experimental or pilot programs, one obvious concern is that the first villages to implement

[51] This equation is similar to equation (2), except that we interact elections and open nominations with the indicator of VC turnover rather than predictors for the demand of public goods.

the reform were chosen to be the villages that would be most responsive. To address this, we re-estimate our baseline regressions on a sample restricted to villages that held their first election after the national law was passed in 1987. Table 9 compares the results on the restricted sample with our baseline estimates. These estimates show that the coefficients and standard errors for the main effects in columns (1)-(2) and (7)-(10) and for the interaction effects in columns (3)-(6) are very similar for the two samples. The magnitudes of the estimates are if anything, larger, in the restricted sample. Thus, our main results are not driven by the endogenous timing of the introduction of elections prior to the national reform.[52]

Instrumenting for the Introduction of Elections The descriptive statistics in Table 1 panel C show that not all villages introduce elections at the same time as the official introduction by their county- or province-level governments. This raises the concern that the main results are driven by the potential endogeneity in timing for the villages that led or lagged the official introduction by higher levels of government. To address this concern, we instrument for the introduction of elections at the village level with the official introduction at the county or the province level. The identification assumption for the instrumental variables estimates is that the timing of the first election within a county or province is uncorrelated with factors that can affect the outcomes of interest through channels outside of village elections.

Table 10 presents the estimates. Panel A re-states the baseline OLS estimates for comparison. Panel B presents the 2SLS estimates where the instrument is the introduction at the county level and panel C presents the 2SLS estimates where the instrument is the introduction at the province level. The F-statistics for the first stage are reported at the bottom of each panel. Note that we focus on the main variables that are available for a large sample because the instrumental variables estimates require that there be substantial variation across counties or across provinces.

The first-stage F-statistics for both instruments are large, which implies that our 2SLS estimates are not biased by weak instruments. The 2SLS estimates for the interaction terms in columns (1)-(2) and the main effects of elections in columns (3)-(5) are very similar in magnitude to the OLS estimates in panel A. Therefore, our main results are not driven by selection in the timing of

[52]In addition, we also test whether our results are driven by underlying trends by randomly generating the year of the first election and estimating the effects of this random election. We find that the randomly generated elections have no effect. These results are not reported in the paper for the sake of brevity. They are available upon request.

introducing elections within counties or within provinces.[53]

Pre-Trend Analysis The 2SLS estimates leave open the concern that the main results are driven by selection across provinces, which would be consistent with the qualitative evidence that the main initiative for reform was taken at the province level. We approach this difficulty by directly examining the presence of pre-trends in the outcomes of interest for the years leading up to the first election. For example, a positive trend in public goods expenditure in the years leading up to the introduction of elections would be consistent with elections being introduced earlier in villages where they could have the largest impact on public goods expenditure (e.g., because there is great demand for public goods). If there is no pre-trend, then one would be more convinced that the main results are not driven by endogenous timing.

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

$$Y_{vpt} = \sum_{\zeta=-3}^{6} \beta_{\zeta} \chi_{vpt} + \lambda OpenNom_{vpt} + \gamma_p t + \delta_v + \rho_t + \varepsilon_{vpt}, \tag{3}$$

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 number of years prior to the first election should be similar for all the years prior to the first election, $\hat{\beta}_{\varsigma} \approx \hat{\beta}_{\varsigma+1}$ for $\varsigma < 0$. 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, $\hat{\beta}_{\varsigma} >$ for $\varsigma \geq 0$. 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.

For brevity, we only present the results for the main outcomes.[54] The coefficients and the

[53]Note that we can additionally instrument for the introduction of open nominations at the village level with the introduction at the county- and province-levels. Doing so does not affect the magnitude of our estimates. Therefore, we do not present them in the paper for brevity. They are available upon request.

[54]The estimates for the other outcomes exhibit similar patterns. They are available upon request.

standard errors for the estimated effects of the number of years since the first election on log total public goods investment, log median household farmland, log median household taxes, a dummy indicating if any households had two or more children and the log of total village land are presented in Appendix Table A3 columns (1)-(10). The coefficients are plotted in Figures 1-7. There is no evidence of pre-trends in our main outcomes of interest. 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 trend break in the coefficients when the first election is introduced. The point estimates are statistically significant for the majority of the coefficients for all of the outcomes except for household taxes.

To examine whether there are pre-trends for the log of arable land and school enrollment rate, we estimate a similar equation, but interact the dummies for the number of years since the first election with the average log village farmland or the average number of school-age children age 7-13, and also include interactions of these variables for a dummy variable indicating the introduction of open nominations. For these estimates, we focus on the interactions with the number of years since the first election. They are presented in Appendix Table A3 columns (11)-(14), and plotted in Figures 7-8. They also exhibit no evidence of pre-trends, as well as a trend break when elections are introduced. The estimates for the interaction terms of village farmland and the election year dummies are almost all statistically significant. Most of the estimates for the interaction terms of the average number of children and the election year dummies are statistically significant for the post-election years.

These results provide no evidence of pre-trends. They also show that elections affect the main outcomes of interest beginning in the year that elections are introduced. Hence, our main estimates of the post-election dummy variable are not driven by spurious trends during the post-election years.

Controlling for Pre-Conditions Although there are no pre-trends in the outcomes of interest, one can argue that election timing is correlated with factors that influence the outcome, but are not exhibited in the pre-trends. For example, elections may be introduced earlier in villages where unelected officials are corrupt and there is high demand for land redistribution. But the same power that allows officials to capture rents also allows officials to prevent redistribution. Thus, elections could have a larger effect on land allocation in such a village, but we may not observe a pre-trend

in the years leading up to the first election.

To address this possibility, we control for the pre-election average level of median household land allocation in each village. Since this is a time-invariant village-level characteristic and collinear with village fixed effects, we control for the interaction of this variable with the full set of year dummy variables. This allows the influence of the pre-election average of land allocation to vary fully flexibly over time. In addition, it controls for any correlates with the pre-election average of land allocation (e.g., pre-election income, political capture) and allows these effects to vary fully flexibly over time.

In Table 11, we present the robustness checks for all of our main outcomes. We repeat the procedure above for each outcome so that we control for the interaction of the pre-election average of that outcome with the full set of year dummy variables. In the cases where the first election takes place before data become available, we use the average for the variable over all the years in the data and interact it with year dummy variables. Column (1) restates the baseline estimates for comparison purposes. Columns (2)-(5) gradually introduce each vector of control for the controls that are available from the full sample of villages. Column (6) controls for all of these controls simultaneously. This rigorous specification produces estimates that are nearly identical to the baseline. In columns (7)-(8), we introduce the controls for variables that are only available for a third of the villages. In column (9), we control for all of the controls in columns (2)-(5) and (7)-(8) 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 in Table 11 show that our main results are extremely robust to controlling for pre-conditions. The results presented in this section thus far provide strong evidence that our main results are very unlikely to be confounded by omitted variables.

Additional Sensitivity Tests The final robustness exercise is to examine the sensitivity of our main results to controlling for factors that may influence the effectiveness of elections. The results are presented in Table 12. As before, we present the baseline estimates for comparison in column (1) and focus on the main policy outcomes.

First, we examine the influence of election procedures. These data are recorded from village records by the VDS and show that there is significant heterogeneity. Approximately 84% of elections

have anonymous voting, 72% allow voting by proxy and 65% use a roving ballot box. Despite the mandate of holding competitive elections, only 79% of elections follow this requirement (see Table 1 panel C).[55] We test the sensitivity of the main estimates by controlling directly for election procedures, which are time varying. The estimates are very similar to the baseline for all outcomes. Note that these estimates should be interpreted cautiously because procedures can potentially be outcomes of the electoral reforms.

Another factor that can influence the effect of elections is the share of the largest clan. When elections were introduced, many officials feared that a large extended family would dominate elections and then use its power in the elected office to implement policies which may not be beneficial for the rest of the villagers. Alternatively, having a large dominant clan could facilitate coordination in making decisions for policies such as public goods. In column (3), we control for the interaction of the fraction of the village that comprises the largest clan, a time-invariant variable, with year fixed effects. Our estimates are robust to these controls.[56]

Next, we consider 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. In column (4), we control for the interactions of each of these variables with year fixed effects. The estimates are very similar to the baseline.

In column (5), we control for the introduction of the *Tax and Fee Reform*, which restricted the collection of fees by village governments. Since such fees were the main source of funding for village public goods, especially in poor villages, this could curb the village government's ability to provide public goods. The VDS records the implementation date for each village from village records. As with the electoral reforms, the timing of this reform varied across villages although the national law was passed in 2003. In column (5), we investigate whether our baseline results, particularly

[55]Several past studies have observed that the quality of the electoral procedures is highly uneven (Brandt et al., 2004; Pang and Rozelle, 2006; Birney, 2007). 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.

[56]We also used alternative measures, such as the share of the largest two clans and a dummy for whether the largest clan was more than half of the village population, as controls. The estimates are very similar to the baseline. They are not reported for the sake of brevity and are available upon request.

those on public goods investment, are robust to controlling for the village-specific introduction of this reform. Note that the year fixed effects in the baseline already control for the national law.

In column (6), we control for whether a village had experienced a merger with another village.[57] The VDS records whether a village ever experienced a merger and the year of the merger. Approximately 23% of our sample has experienced a merger since 1980. We created a dummy variable indicating whether a village ever experienced a merger and control for the interaction of this dummy variable with year fixed effects. Column (6) shows that the estimated effect of elections is very similar in magnitude to the baseline.[58]

Finally, in column (7), we control for all of the factors described above simultaneously. This rigorous specification produces estimates that are very similar in magnitude to the baseline. Therefore, we conclude that our main estimates of the effect of elections are very robust and not sensitive to controlling for the factors that are the mostly likely to influence the effect of elections.

8 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.[59] Our study addresses this important question by taking advantage of the Chinese electoral reforms, which we argue provide a better identified natural experiment for studying the effects of democratization on public goods and redistribution than what has existed thus far.

We obtain several novel and provocative results. First, we find that the introduction of elections increased overall total public goods investment by 27% percent. We also find that public goods

[57]This could be problematic for our data because the VDS allows villages to provide the electoral history of only one village. If a village was merged with another in the past, it is not clear which village is being represented in the historical survey. The NFS data faces similar problems. Another potential problem comes from the possibility that villages that have been merged with other villages can have very different electoral experiences. For one, merged villages may have more heterogeneity in their constituents, which could affect electoral outcomes. Alternatively, one may worry that villages which experienced mergers were either particularly problematic or prosperous villages that also introduced elections systematically earlier or later.

[58]The estimates are also similar in magnitude to the baseline if we drop villages that have ever experienced a merger, but they are less precise. We do not show these results for brevity. They are available upon request.

[59]See the introduction for references.

expenditure becomes more appropriate – i.e. elections reduce investment in public goods in villages where the demand for such goods is presumably low, while they increase investment in villages where demand is presumably high. 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.[60]

Given the widely held belief that the level of public good provision is far below the demands of villagers, our results are consistent with theories which predict that democracy and majoritarian rule increase public goods provision (e.g., de Mesquita et al., 2003; Besley and Kudamatsu, 2008).[61] At the same time, they 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 public goods provision becomes more appropriate suggests that one of the reasons that elected leaders can increase taxes may be that they choose investments that correspond better to citizens' needs. These results are important because they show that democracy need not inhibit a governments' ability to raise revenues for public goods.[62]

Second, we find that elections increase household farmland by twenty to 27 percent for approximately thirty to fifty percent of households in the middle of the village distribution of household land ownership. This is paralleled by a drastic reduction in the amount of arable land directly controlled by the village government, which is mostly leased to enterprises. We show that such enterprises disproportionately benefit richer households and that the reduction in land leased out to enterprises is paralleled by a reduction in income from enterprises, wage income and total income for the richest households of each village. This, in turn, reduces within-village income inequality.

These results are consistent with elected leaders redistributing towards median voters. The

[60]Recall that because we cannot measure the taxes paid to the village government separately from the taxes paid to other local governments, this interpretation assumes that elections did not increase taxes paid to other local governments.

[61]Luo et al. (2007) and Luo et al. (2010) show that, on average, village provision of public goods is very low and far from adequate.

[62]Moreover, recall that in China village governments are not allowed to impose regular taxes and resort to *ad hoc* fees and levies (which we call local taxes in our study) for financing public goods. 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 policymakers 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 (Zhang et al., 2005; Bird et al., 2009).

fact that land rather than income is redistributed is most likely caused by the village government's inability to impose regular taxes. That the increase in land for median households is partly achieved by shifting land that was previously used for other productive purposes towards household farming is similarly likely to be due to the fact that village governments cannot easily reduce existing household land allocations.[63] It is important to note that redistributing land may be inefficient and cause significant welfare losses relative to taxing and transferring income. These efficiency losses seem to be large since we find that elections reduce the incomes of rich households without significantly increasing the incomes of poor ones.

Nevertheless, these results provide strong evidence for recent theories of democracy which argue that democracies are likely to engage in redistribution (e.g. Acemoglu and Robinson, 2001, 2006; Boix, 2003). Given the legal requirement that a candidate obtain fifty percent of the votes to win an election, these results also suggest that an elected official distributes benefits to as many citizens as he needs votes for.[64] Our results also support the concerns of Chinese policy makers that the leasing of village land away from farming disproportionately benefits village elites, and suggest that democratically accountable leaders are less likely to allow these activities.[65]

It is important to keep in mind that these results should be cautiously interpreted as specific to the context of rural China. Nonetheless, the findings are consistent with cross-country evidence which suggests that democracy can improve public goods provision such as health and education and reduce inequality (Barro, 1996; Besley and Kudamatsu, 2006; Tavares and Wacziarg, 2001; Kudamatsu, 2011), and the within country evidence that increased representation improves health-care in Brazil (Fujiwara, 2011) and public goods provision in India (Foster and Rosenzweig, 2005). To the extent that investment in schooling increases human capital, which fuels economic growth according to conventional growth theory, our results are also consistent with panel data evidence that democratization increases economic growth (e.g., Papaioannou and Siourounis, 2008; Persson and Tabellini, 2007).[66]

[63] Recall that rural Chinese households are all legally entitled to a subsistence amount of farmland.

[64] Note that our results cannot conclusively measure the proportion of households that benefit from elections because we are unable to determine which households benefit the most from the improvements in public goods.

[65] Our results are consistent with the findings of Brandt and Turner (2007) using a different dataset and empirical techniques.

[66] Note that we find that elections have no effect on household income growth. These results are not presented in the paper for brevity and are available upon request. Our finding is difficult to interpret since many policy instruments for promoting growth, such as the improvement of property rights or economic liberalization, are not under the discretion of village governments.

Our study also provides evidence 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 elections has similar effects regardless of whether it caused leadership change. This implies that the main driver of the effect of elections is more likely to be the increased incentives of leaders rather than the villagers' ability to select different leaders.[67]

Our results also shed light on the growing interest in the effect of introducing local elections in autocratic governments, the effects of which have only very recently begun to receive serious attention from scholars (e.g. Martinez-Bravo, 2011; Martinez-Bravo et al., 2011).[68] *A priori*, there are many reasons to be skeptical that local democracy would produce the same effects as country-level democratization. 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 local democracy has a significant impact on public goods and redistribution in China.

A related question that is interesting to consider concerns the motivation behind local elections in autocracies. This question is especially important for those who wish to understand whether local elections are the first step to wider regime change, or whether it is used as an instrument for keeping the autocratic central government in power. The qualitative evidence that we discuss in section 3 suggests that the Chinese government mainly desired elections to improve local monitoring. This is consistent with Lorentzen (2010) thesis that the Chinese government creates mechanisms for citizens to voice their preferences as a way to monitor cadres and improve governance. It is also consistent with empirical evidence from studies such as Meng et al. (2010), which uses evidence from the Chinese Famine to argue that it is extremely difficult for a central government to directly

[67]These 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).

[68]Many 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 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).

monitor each locality directly; and (2011), which 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. Our study suggests that the autocratic government faces an important trade-off in introducing local democracy. On the one hand, it can increase average citizens' satisfaction with the regime by improving local public goods and redistribution. On the other hand, it reduces the locally elected leaders' incentives to enforce centrally mandated policies when they come in conflict with the preferences of citizens. Providing a more detailed analysis of the incentives of the autocratic central government regarding local governance is an important avenue for future research.

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Table 1: Descriptive Statistics

Variable	Source	Obs	Mean	Std. Dev.	Variable	Source	Obs	Mean	Std. Dev.
A. Full Sample					**B. Sub-sample with Household Variables**				
# of HH in Village	NFS	3,641	419.17	279.85	Median HH Gross Income (RMB)	NFS	1,297	11,391	10,520
Near City	NFS	5,208	0.09	0.28	Median HH Gross Income Growth	NFS	1061	0.06926	0.1968
					HH Income 50th/90th	NFS	1297	0.51168	0.1302
At least 1 HH had a 2nd Child	VDS	5,208	0.50	0.50	HH Income 10th/50th	NFS	1297	0.50285	0.12484
# Kids 0-6 per Household	NFS	1,995	0.57	0.34	Median HH Farm Land (Mu)	NFS	1297	5.22417	6.0545
Land was Expropriated by Upper Government	VDS	5,208	0.04	0.19	HH Farm Land 50th/90th	NFS	1264	0.57309	0.14242
					HH Farm Land 10th/50th	NFS	1235	0.35582	0.23797
					Median HH Fees & Levies to Village and County (100 RMB)	NFS	1,297	151.08	191.35
Village Expenditure in Public Goods (10,000 RMB)	VDS	4340	14.28	135.47					
By Source of Financing:					**C. The Village Government and Elections**				
Funded by Villagers	VDS	4340	9.77	119.29	The Number of Village Committee Members	NFS	2,287	4.36	2.36
Funded by Upper Governments	VDS	4340	4.42	64.21	The Number of Village Party Cadres	NFS	2,295	6.70	3.82
By Object of Investment:					Party Secretary Tenure	VDS	5,208	10.03	8.13
Schools	VDS	5,208	1.33	15.60	Village Chief: Tenure	VDS	5,208	6.69	6.24
Irrigation	VDS	5,208	1.71	36.54					
Enrollment Rate	NFS	2,682	96.42	9.27	Has Election	VDS	5208	0.72869	0.44468
					Has Open Nomination	VDS	5208	0.20161	0.40124
Total Village Land	NFS	3612	9244.825	14719.3	Years between 1st Election in Village and Province	VDS	217	5.01843	5.06865
Arable Land (Mu)	NFS	3612	2295.062	2328.88	Years between 1st Election in County and Province**	VDS	217	4.28111	4.66814
Share of Village Land that is Arable	NFS	3612	0.5097653	0.31619	Years between 1st Election in Village and County**	VDS	217	0.73733	2.28423
Used for HH Farming (Mu)	NFS	3612	2215.343	2311.73	Years since last election	VDS	1,084	3.16	1.02
Not Used for HH Farming (Mu)	NFS	3612	79.71938	367.259	VC different from previous term*	VDS	4731	0.17	0.38
Leased Out to Enterprises (Mu)	NFS	3612	60.45855	347.611	1st Election Changed VC*	VDS	182	0.38	0.49
Median HH Gross Income (RMB)	NFS	3,778	10,513	8,366	Anonymous Voting	VDS	1,281	0.85	0.36
Median HH Annual Gross Income Growth	NFS	3093	0.0687119	0.192	Proxy Voting	VDS	1,265	0.72	0.45
HH Income 50th/90th	NFS	3,778	0.53	0.12	Roving Ballot Box	VDS	1,251	0.65	0.48
HH Income 10th/90th	NFS	3,778	0.51	0.12	>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*, which surveyed village records over the years 1982-2005 in 2006 and 2011. NFS indicates that the variables is reported by the *National Fixed Point Survey*, which surveyed villages each year during 1986-1991, 1993, and 1995-2005. *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 Public Goods

	Dependent Variables					
	Ln Expenditure by Source			Public Goods Provision Measures		
	Total	Villagers	Non-Village	Ln Arable Land	Share of Arable Land	Enrollment Rate of Children Age 7-13
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Total Expenditure					
Dep. Var. Mean (not logged, 10,000 RMB)	*14.28*	9.77	4.42			
Post 1st Election	0.272	0.309	0.002			
	(0.116)	(0.105)	(0.075)			
Observations	4340	4340	4340			
R^2	0.191	0.171	0.188			
	B. Irrigation Expenditure					
Dep. Var. Mean (not logged, 10,000 RMB)	*3.43*	*2.02*	*1.41*	*2295.06*	*0.51*	
Post 1st Election	-5 -0.275	-0.193	-0.075	-3.297	-0.15025	
	(0.167)	(0.156)	(0.144)	(1.653)	(0.051)	
Post 1st Election x Avg Ln Village Farm Land	0.055	0.041	0.012	0.444	0.01834	
	(0.023)	(0.022)	(0.019)	(0.220)	(0.007)	
Observations	4340	4340	4340	3291	3277	
R^2	0.120	0.123	0.106	0.876	0.917	
	C. Primary Schools Expenditure**					
*Dep. Var. Mean (not logged, 10,000 RMB)***	*20.05*	*11.04*	*8.89*			*96.42*
Post 1st Election	-12.678	-3.133	-8.904			-1.340
	(16.846)	(15.346)	(8.987)			(1.837)
Post 1st Election x Avg. # Kids 7-13	0.013	0.014	-0.001			0.008
	(0.006)	(0.007)	(0.002)			(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 ln household farm land) with post first open nominations. **In Panel C columns (1) and (3), the dependent variables are multiplied by 1,000 for presentation purposes. Standard errors are clustered at the village level.

Table 3: The Effect of Elections on Ln Household Local Tax Payments

	Dependent Variable: Ln Household Local Taxes								
	Quantile of Within-village Tax Distribution								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep.Var. Mean (not logged, RMB)	*64.51*	*93.69*	*113.41*	*133.22*	*151.08*	*169.89*	*191.45*	*221.40*	*275.70*
Post 1st Election	0.992	0.644	0.716	0.884	0.894	0.912	0.917	0.701	0.283
	(0.419)	(0.430)	(0.427)	(0.439)	(0.472)	(0.472)	(0.486)	(0.438)	(0.357)
Observations	1,297	1,297	1,297	1,297	1,297	1,297	1,297	1,297	1,297
R^2	0.556	0.572	0.596	0.586	0.610	0.607	0.610	0.617	0.584

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

Table 4: The Effect of Elections on Ln Median Household Farmland

	Dependent Variables												
	Quantile of Within-village Household Land Distribution									Village Arable Land Not Used by HH Farming			
										Pre-Election Arable Non-HH Land>0		Pre-Election Land Leased to Enterprises >0	
	10th	20th	30th	40th	50th	60th	70th	80th	90th	Ln Arable Non-HH Land	Arable Non-HH Land Dummy	Ln Land Leased Out to Enterprises	Ln Land Leased Out to Enterprises Dummy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Dep. Var. Mean (not logged, mu)	*2.14*	*3.18*	*3.95*	*4.63*	*5.22*	*5.88*	*6.61*	*7.54*	*9.00*	1.26	0.30	1.11	0.27
Post 1st Election	0.037	0.099	0.199	0.253	0.243	0.208	0.175	0.141	0.131	-0.705	-0.141	-0.639	-0.147
	(0.205)	(0.176)	(0.163)	(0.139)	(0.116)	(0.117)	(0.116)	(0.113)	(0.115)	(0.286)	(0.057)	(0.323)	(0.069)
Observations	1,297	1,297	1,297	1,297	1,297	1,297	1,297	1,297	1,297	3438	3438	2617	2617
R^2	0.735	0.790	0.808	0.857	0.884	0.890	0.888	0.882	0.881	0.529	0.529	0.507	0.514

Notes: All regressions control for the introduction of open nominations, province-time trends, village and year fixed effects. Standard errors are clustered at the village level. Columns (10)-(11) include villages that had some arable land not dedicated to household farming prior to the first election. Columns (12)-(13) includes villages that lease some arable land to enterprises prior to elections.

Table 5: The Effect of Elections on Ln Household Income

	Dependent Variables										
	Quantiles of the Within-Village Distribution of Total Household Income										
	10	20	30	40	50	60	70	80	90		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	A. Ln Income from Enterprises Dividends										
Dep. Var. Mean (not logged, RMB)				*24.09*	*35.00*	*52.00*	*89.51*	*181.87*	*663.35*		
Post Election				-0.023	-0.020	-0.008	-0.086	-0.093	-0.568		
				(0.027)	(0.032)	(0.042)	(0.076)	(0.127)	(0.277)		
Obs				1297	1297	1297	1297	1297	1297		
R^2				0.244	0.215	0.213	0.247	0.293	0.338		
	B. Ln Income from Wages										
Dep. Var. Mean (not logged, RMB)	*9.13*	*65.40*	*144.11*	*266.83*	*409.82*	*854.47*	*1452.32*	*2176.07*	*3128.85*		
Post Election	0.200	0.479	0.910	0.320	-0.163	-0.588	-1.084	-1.269	-0.715		
	(0.128)	(0.483)	(0.593)	(0.676)	(0.526)	(0.631)	(0.549)	(0.440)	(0.284)		
Obs	1297	1297	1297	1297	1297	1297	1297	1297	1297		
R^2	0.185	0.447	0.472	0.522	0.551	0.558	0.550	0.563	0.602		
	C. Ln Income from Agriculture										
Dep. Var. Mean (not logged, RMB)	*2578.77*	*4265.97*	*5853.31*	*7509.38*	*9489.56*	*12078.09*	*15739.96*	*21802.92*	*36451.07*		
Post Election	0.594	0.519	0.302	0.112	0.125	-0.052	-0.020	-0.035	0.069		
	(0.627)	(0.582)	(0.599)	(0.667)	(0.542)	(0.464)	(0.331)	(0.248)	(0.136)		
Obs	1297	1297	1297	1297	1297	1297	1297	1297	1297		
R^2	0.509	0.601	0.693	0.761	0.798	0.840	0.896	0.938	0.973		
	D. Ln Total Household Gross Income										
	Quantiles of the Within-Village Distribution of Total Household Income									Income Inequality	
	10		25		50		75		90	50/90	10/50
Dep. Var. Mean (not logged, RMB)	*5080.40*		*7369.40*		*10512.98*		*15314.59*		*24427.83*	*0.53*	*0.51*
Post Election	-0.012		-0.026		-0.039		-0.059		-0.087	0.017	0.006
	(0.054)		(0.024)		(0.023)		(0.025)		(0.034)	(0.009)	(0.011)
Obs	3778		3778		3778		3778		3778	3778	3778
R^2	0.611		0.921		0.932		0.928		0.909	0.594	0.540

Notes: All regressions control for the introduction of open nominations, province-time trends, village and year fixed effects. Standard errors are clustered at the village level. Regressions in panels A-C use an unbalanced panel of 73 villages for the years 1987-1991, 1993, 1995-2005. Regressions in panel D uses villages from the full 217 NFS village sample for the same years.

Table 6: The Effect of Elections on Leader Powers and Characteristics

	Dependent Variables				
	Appt. Enterprise Manager	Employ Enterprise Workers	Reimburse Public Expenditures	Land Reallocation	Public Goods Expenditure
	Panel A. VC has Unilateral Power				
Dep. Var Mean	*0.32*	*0.27*	*0.56*	*0.33*	*0.18*
Post Election	0.048	0.034	0.036	0.048	0.042
	(0.033)	(0.028)	(0.034)	(0.031)	(0.024)
Obs	3336	4103	4910	3936	4457
R^2	0.801	0.785	0.771	0.800	0.779
	Panel B. VC and PS Share Power				
Dep. Var Mean	*0.37*	*0.44*	*0.19*	*0.53*	*0.67*
Post Election	0.001	-0.005	0.014	-0.008	-0.026
	(0.029)	(0.027)	(0.025)	(0.031)	(0.026)
Obs	3336	4103	4910	3936	4457
R-Square	0.797	0.769	0.724	0.804	0.808
	Panel C. PS has Unilateral Power				
Dep. Var Mean	*0.29*	*0.25*	*0.13*	*0.15*	*0.31*
Post Election	-0.051	-0.031	-0.051	-0.042	-0.017
	(0.029)	(0.024)	(0.027)	(0.028)	(0.026)
Obs	3336	4103	4910	3936	4457
R^2	0.832	0.819	0.810	0.798	0.774
	Panel D. VC Characteristics				
	VC Turnover	Age	Male	Years of Edu	Party Member
Dep. Var Mean	*0.17*	*42.83*	*0.99*	*8.03*	*5853.00*
Post Election	0.045	-2.442	-0.012	0.791	-0.034
	(0.022)	(0.817)	(0.009)	(0.257)	(0.043)
Post Open Nomination	-0.052	0.762	-0.006	0.085	-0.087
	(0.022)	(0.818)	(0.009)	(0.240)	(0.042)
Obs	4312	4188	4312	4194	4274
R^2	0.065	0.430	0.276	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. In all panels, the observations vary across columns because an observation is missing if the specified power is not relevant for that village-year. Standard errors are clustered at the village level.

Table 7: The Effect of Elections on Upper-Government Policies

	Dependent Variables					
	A. Villagers and Party "Disagree", Require VC Cooperation				B. Villagers and Party "Disagree", Do Not Require VC Cooperation	
	Dummy for Whether Any Household had 2+ Child	# Kids 0-6 per Household	Dummy for Upper Government Expropriation of Village Land	Ln Total Village Land (mu)	Ln Distance to Nearest Highschool (km)	Ln Upper-Government Special Aid (10,000 RMB)
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var. Means (not logged)	*0.50*	*0.58*	*0.04*	*9118.21*	*11.67*	*1.29*
Post 1st Election	0.086	0.025	-0.011	0.103	-0.035	0.006
	(0.033)	(0.027)	(0.008)	(0.054)	(0.033)	(0.017)
Observations	5,208	1127	5,208	3,296	4,692	5,208
R^2	0.755	0.873	0.080	0.909	0.958	0.079

Notes: All regressions control for post first open nomination, province-time trends, village and year fixed effects. Standard errors are clustered at the village level. 1 mu =1/15 hectare.

Table 8: The Effect of Elections and Leader Turnover

	Dependent Variables								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ln Public Investment	Ln Public Investment Financed by Villagers (x 1,000) **	Ln Median HH Land	Ln Median HH Tax	Dummy for Whether Any Household had 2+ Child	# Kids 0-6 per Household in year t+3	# Kids 0-6 per Household in year t+5	Dummy for Upper Government Expropriation of Village Land	Ln Total Village Land
	Panel A. The Effect of Open Nominations								
Post 1st Election	0.272	0.309	0.894	0.243	0.086	0.083	0.121	-0.011	0.103
	(0.116)	(0.105)	(0.472)	(0.116)	(0.033)	(0.056)	(0.054)	(0.008)	(0.054)
Post 2st Open Nominations	-0.110	-0.059	0.214	-0.110	-0.052	-0.048	0.006	-0.005	0.006
	(0.127)	(0.114)	(0.365)	(0.106)	(0.035)	(0.076)	(0.045)	(0.011)	(0.035)
Observations	4340	4340	1297	1297	5208	1585	1165	5208	3296
R-squared	0.191	0.171	0.610	0.884	0.755	0.542	0.674	0.080	0.909
	Panel B. The Differential Effect of Elections								
Post Election	0.301	0.349	0.998	0.228	0.100	0.090	0.128	-0.009	0.048
	(0.144)	(0.125)	(0.550)	(0.133)	(0.041)	(0.076)	(0.073)	(0.010)	(0.042)
Post Election x 1st Election VC Change	-0.133	-0.118	-0.211	0.061	-0.031	0.054	0.040	0.011	0.056
	(0.248)	(0.225)	(0.708)	(0.189)	(0.063)	(0.089)	(0.096)	(0.014)	(0.066)
Observations	3920	3920	1261	1261	4704	1435	1055	4704	2983
R^2	0.196	0.177	0.614	0.884	0.748	0.540	0.675	0.081	0.936

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 village level. **The dependent variable in column (2) is multiplied by 1,000 for presentation purposes. The number of observations vary across columns due to data availability. See discussion in text.

Table 9: The Effect of Elections – Robustness to the Exclusion of Villages that Introduced Elections Before 1987

	Dependent Variables									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Ln Total Public Investment		Ln Public Investment in Irrigation from Villagers		Ln Public Investment in Schooling from Villagers (x 1,000)**		Upper Government Land Expropriation		Dummy for Whether Any Household had 2+ Child	
	Baseline Sample	Omit if 1st Election <1987	Baseline Sample	Omit if 1st Election <1987	Baseline Sample	Omit if 1st Election <1987	Baseline Sample	Omit if 1st Election <1987	Baseline Sample	Omit if 1st Election <1987
Post 1st Election	0.272	0.273	-0.193	-0.322	-3.133	0.127	-0.011	-0.011	0.086	0.084
	(0.116)	(0.141)	(0.156)	(0.180)	(15.346)	(17.665)	(0.008)	(0.013)	(0.033)	(0.042)
Post 1st Election x Avg Ln HH Farmland			0.041	0.059						
			(0.022)	(0.026)						
Post 1st Election x # of Kids 7-13					0.014	0.014				
					(0.007)	(0.009)				
Observations	4340	3906	4340	3906	4220	3798	5,208	3,906	5,208	3,906
R^2	0.191	0.194	0.123	0.129	0.072	0.079	0.080	0.089	0.755	0.820

Notes: All regressions control for post first open nomination, province-time trends, village and year fixed effects, The regressions in columns (3)-(6) also control for the interaction of the relevant village characteristic (e.g., avg. number of kids age 7-13, average ln household farm land) with post first open nominations. **The dependent variables in columns (4)-(6) are multiplied by 1,000 for presentation purposes. Standard errors are clustered at the village level.

Table 10: The Effect of Elections – 2SLS Estimates

	Dependent Variables				
	(1)	(2)	(3)	(4)	(5)
	Ln Village Financing of Irrigation	Ln Village Financing of Schools (x 1,000) **	Dummy for Whether Any Household had 2+ Child	Upper Government Land Expropriation	Ln Total Village Land
	A. OLS Estimates				
Post 1st Election	-0.275	-12.678	0.086	-0.011	0.103
	(0.167)	(16.846)	(0.033)	(0.008)	(0.054)
Post 1st Election x Avg Ln Village Farm Land	0.055				
	(0.023)				
Post 1st Election x # Kids 7-13		0.013			
		(0.006)			
Observations	4340	4220	5,208	5,208	3,296
R^2	0.120	0.082	0.755	0.080	0.909
	B. 2SLS Estimates -- Instrument is post 1st election in the same county				
Post 1st Election	-0.376	-8.911	0.222	-0.033	0.237
	(0.298)	(33.594)	(0.075)	(0.020)	(0.131)
Post 1st Election x Avg Ln Village Farm Land	0.071				
	(0.041)				
Post 1st Election x # Kids 7-13		0.016			
		(0.005)			
Observations	4340	4220	5208	5208	3296
Cragg-Donald F-Statistic for the First Stage	*442.3*	*411.2*	*1047*	*1047*	*628.1*
	C. 2SLS Estimates -- Instrument is post 1st election in the same province				
Post 1st Election	-0.639	64.910	0.082	-0.124	0.754
	(1.020)	(224.597)	(0.078)	(0.157)	(0.762)
Post 1st Election x Avg Ln Village Farm Land	0.097				
	(0.177)				
Post 1st Election x # Kids 7-13		0.010			
		(0.013)			
Observations	4340	4220	5208	5208	3296
Cragg-Donald F-Statistic for the First Stage	*21.02*	*18.07*	*55.33*	*55.33*	*60.04*

Notes: All regressions control for post first open nomination, province-time trends, village and year fixed effects. The estimates in columns (1)-(2) also control for the interactions of post open nominations with the relevant village characteristics (e.g., avg. # of kids 7-13, average ln village household farm land). In panel B, post election in each village is instrumented by post first election in the same county, the interaction of post election x avg ln village farmland is instrumented by the interaction of post election in the same county x avg ln village farmland, and the interaction of post election x # kids 7-13 is instrumented by post election in the same county x # kids 7-13. The F-statistics from the first stage are presented at the bottom of panel B. The Stock-Yogo critical value for 5% maximal bias of the IV is 7.03. In panel C, post election in each village is instrumented by post first election in the same province, the interaction of post election x avg ln village farmland is instrumented by the interaction of post election in the same province x avg ln village farmland, and the interaction of post election x # kids 7-13 is instrumented by post election in the same province x # kids 7-13. The F-statistics from the first stage are presented at the bottom of panel C. The Stock-Yogo critical value for 5% maximal bias of the IV is 7.03. Standard errors are clustered at the village level. ** In column (2), the dependent variable is multiplied by 1,000 for presentation purposes.

Table 11: The Effect of Elections – Robustness to Controlling for Pre-Election Conditions

	Dependent Variables									
	Baseline	Pre Avg Share of Years where HH had 2+ kids x Year FE	Pre Avg Total Village Land x Year FE	Pre Avg Ln Total Investment x Year FE	Pre Avg Ln Investment in Irrigation x Year FE	Pre Avg Ln Investment in Schooling x Year FE	All Full Sample Controls	Pre Avg Ln Median HH Land x Year FE***	Pre Avg Ln Median HH Taxes x Year FE***	All Controls***
	(1)	(2)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	A. Ln Total Public Investment									
Post 1st Election	0.272	0.255	0.281	0.231	0.294	0.295	0.275	0.338	0.302	0.333
	(0.116)	(0.119)	(0.118)	(0.113)	(0.115)	(0.117)	(0.121)	(0.239)	(0.223)	(0.236)
Observations	4340	4340	4320	4340	4340	4340	4320	1460	1460	1460
R^2	0.191	0.195	0.198	0.203	0.199	0.197	0.223	0.187	0.189	0.272
	B. Ln Village Public Investment in Irrigation									
Post 1st Election x Avg Ln Village Farm Land	0.041	0.042	0.044	0.041	0.031	0.037	0.032	0.081	0.033	-0.057
	(0.023)	(0.023)	(0.024)	(0.022)	(0.025)	(0.022)	(0.02)	(0.045)	(0.029)	(0.060)
Observations	4340	4340	4320	4340	4340	4340	4320	1460	1460	1460
R^2	0.119	0.125	0.130	0.134	0.181	0.127	0.199	0.149	0.141	0.428
	C. Ln Village Public Investment in Schools (x 1,000)**									
Post 1st Election x # Kids 7-13	0.014	0.015	0.013	0.014	0.014	0.014	0.014	-0.120	-0.086	-0.095
	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.188)	(0.161)	(0.194)
Observations	4220	4220	4200	4220	4220	4220	4200	1420	1420	1420
R^2	0.072	0.080	0.076	0.086	0.077	0.086	0.109	0.087	0.082	0.190
	D. Ln Median HH Taxes									
Post 1st Election	0.894	0.954	0.913	0.863	0.890	0.964	0.993	0.600	1.181	1.066
	(0.472)	(0.480)	(0.469)	(0.457)	(0.468)	(0.468)	(0.444)	(0.375)	(0.473)	(0.401)
Observations	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297
R^2	0.610	0.614	0.617	0.615	0.616	0.618	0.642	0.621	0.625	0.667
	E. Ln Median HH Land									
Post 1st Election	0.243	0.214	0.239	0.219	0.246	0.234	0.199	0.230	0.267	0.218
	(0.116)	(0.107)	(0.113)	(0.123)	(0.115)	(0.111)	(0.096)	(0.116)	(0.128)	(0.104)
Observations	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297
R^2	0.884	0.886	0.892	0.887	0.885	0.885	0.897	0.885	0.886	0.900
	F. Upper Government Land Expropriation									
Post 1st Election	-0.011	-0.012	-0.012	-0.010	-0.009	-0.011	-0.009	-0.013	-0.019	-0.014
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.015)	(0.015)	(0.016)
Observations	5208	5208	5184	5208	5208	5208	5184	1752	1752	1752
R^2	0.080	0.083	0.082	0.086	0.092	0.085	0.106	0.084	0.096	0.183
	G. Dummy for Whether Any Household had 2+ Child									
Post 1st Election	0.086	0.111	0.086	0.086	0.083	0.083	0.108	0.191	0.184	0.213
	(0.033)	(0.033)	(0.034)	(0.033)	(0.033)	(0.034)	(0.034)	(0.070)	(0.072)	(0.071)
Observations	5208	5208	5184	5208	5208	5208	5184	1752	1752	1752
R^2	0.755	0.763	0.755	0.756	0.756	0.756	0.765	0.739	0.738	0.768

Notes: All regressions control for province-time trends, village, and year fixed effects. Each column incorporates the pre-election average of the corresponding variable listed in the column heading interacted with the full set of year dummies. For villages that do not have information of the corresponding variable prior to the adoption of elections we interact the first ocurrence of that variable for that village with the year dummies. The regressions in Panels F and G also control for the interaction of the relevant village characteristic (e.g., number of kids age 7-13, ln total household farm land) with post first open nominations. Standard errors are clustered at the village level. **In Panel C, the dependent variable is multiplied by 1,000 for presentation purposes. ***The controls for household taxes and land are only available for a subsample of villages with household level data.

Table 12: The Effect of Elections –Robustness to Controlling for Factors that Can Influence the Effectiveness of Elections

	Dependent Variables						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	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	All controls
	A. Ln Total Public Investment						
Post 1st Election	0.165	0.189	0.166	0.169	0.165	0.158	0.198
	(0.084)	(0.127)	(0.085)	(0.084)	(0.084)	(0.086)	(0.131)
Observations	5,208	5,208	5,208	5,208	5,208	5,208	5,208
R^2	0.199	0.199	0.202	0.208	0.199	0.202	0.214
	B. Ln Village Public Investment in Irrigation						
Post 1st Election x Avg Ln HH Farmland	0.041	0.040	0.042	0.040	0.042	0.040	0.038
	(0.022)	(0.023)	(0.023)	(0.022)	(0.022)	(0.023)	(0.023)
Observations	4340	4340	4340	4340	4340	4340	4340
R^2	0.123	0.123	0.125	0.130	0.123	0.125	0.136
	C. Ln Village Public Investment in Schooling (x 1,000)**						
Post 1st Election x # of Kids 7-13	0.014	0.012	0.013	0.012	0.014	0.012	0.009
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	4220	4220	4220	4220	4220	4220	4220
R^2	0.072	0.074	0.078	0.087	0.072	0.077	0.097
	D. Ln Median HH Taxes						
Post 1st Election	0.894	0.875	0.898	0.997	0.896	0.824	0.991
	(0.472)	(0.529)	(0.476)	(0.490)	(0.476)	(0.486)	(0.575)
Observations	1,297	1,297	1,297	1,297	1,297	1,297	1,297
R^2	0.610	0.614	0.614	0.622	0.612	0.615	0.638
	E. Ln Median HH Land						
Post 1st Election	0.243	0.318	0.248	0.231	0.242	0.259	0.304
	(0.116)	(0.150)	(0.117)	(0.110)	(0.114)	(0.119)	(0.141)
Observations	1297	1297	1297	1297	1297	1297	1297
R^2	0.884	0.889	0.885	0.889	0.887	0.886	0.897
	F. Upper Government Land Expropriation						
Post 1st Election	-0.011	-0.019	-0.011	-0.010	-0.011	-0.010	-0.016
	(0.008)	(0.013)	(0.008)	(0.008)	(0.008)	(0.008)	(0.013)
Observations	5,208	5,208	5,208	5,208	5,208	5,208	5,208
R^2	0.080	0.081	0.084	0.090	0.080	0.084	0.099
	G. Dummy for Whether Any Household had 2+ Child						
Post 1st Election	0.086	0.077	0.089	0.088	0.086	0.095	0.086
	(0.033)	(0.042)	(0.034)	(0.033)	(0.033)	(0.034)	(0.042)
Observations	5,208	5,208	5,208	5,208	5,208	5,208	5,208
R^2	0.755	0.755	0.756	0.756	0.755	0.758	0.761

Notes: All regressions control for province-time trends, village, and year fixed effects. Each column incorporates the pre-election average of the corresponding variable listed in the column heading interacted with the full set of year dummies. For villages that do not have information of the corresponding variable prior to the adoption of elections we interact the first ocurrence of that variable for that village with the year dummies. The regressions in Panels F and G also control for the interaction of the relevant village characteristic (e.g., number of kdis age 7-13, ln total household farm land) with post first open nominations. Standard errors are clustered at the village level. **The dependent variables in panel C are multiplied by 1,000 for presentation purposes.

Figure 1: The Effect of Elections on Ln Total Public Goods Investment

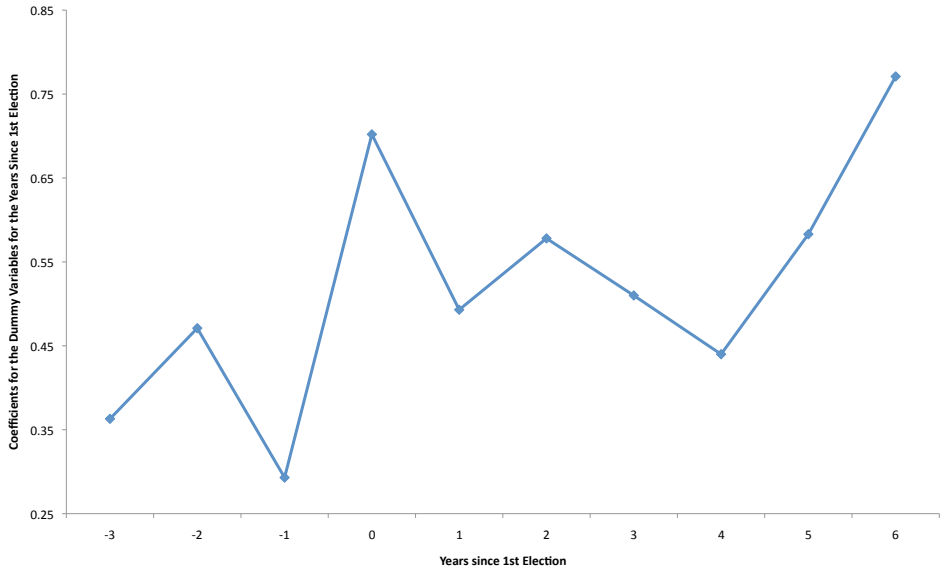

Figure 2: The Effect of Elections on Ln Median Household Farm Land

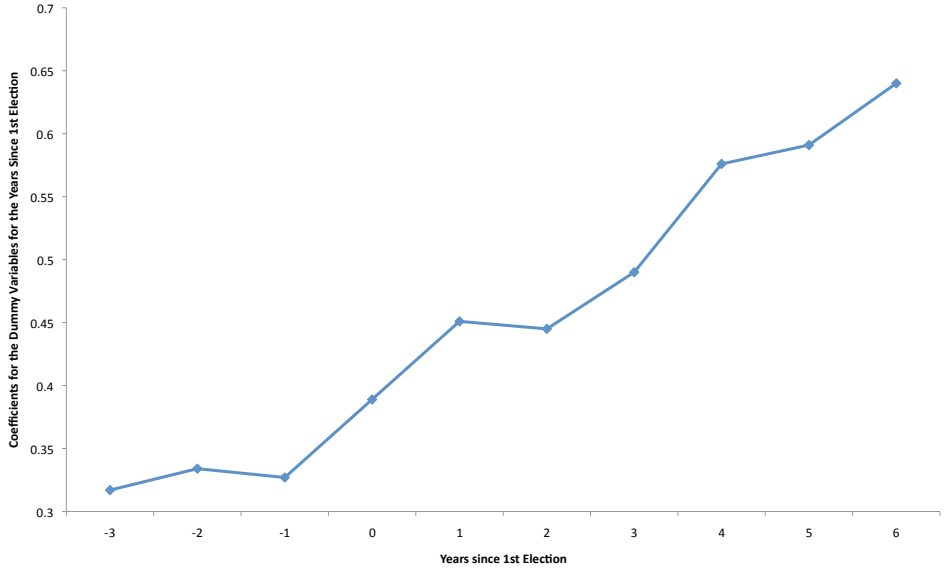

Figure 3: The Effect of Elections on Ln Household Local Tax Payments

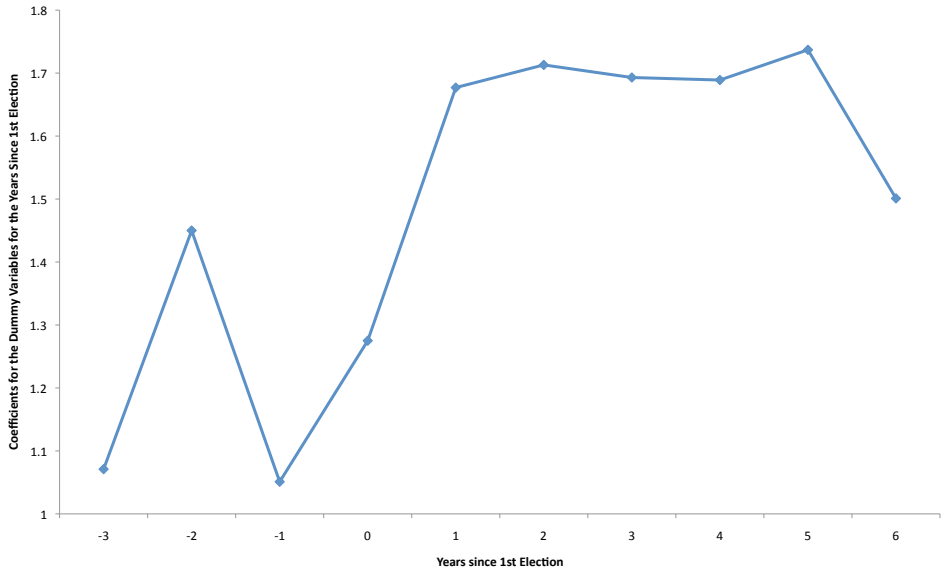

Figure 4: The Effect of Elections on If Any Households had 2+ Children

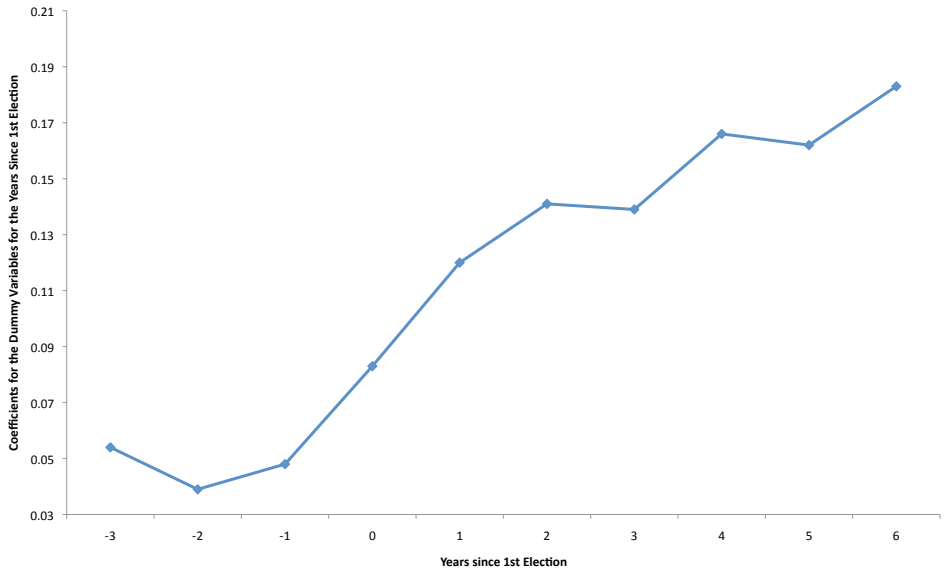

Figure 5: The Effect of Elections on Ln Total Village Land

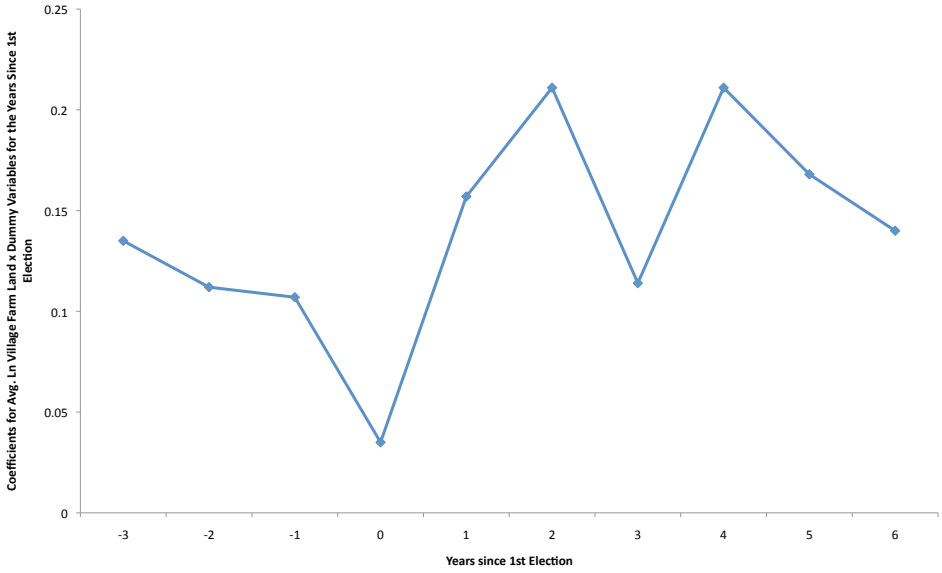

Figure 6: The Effect of Elections on Ln Total Village Arable Land

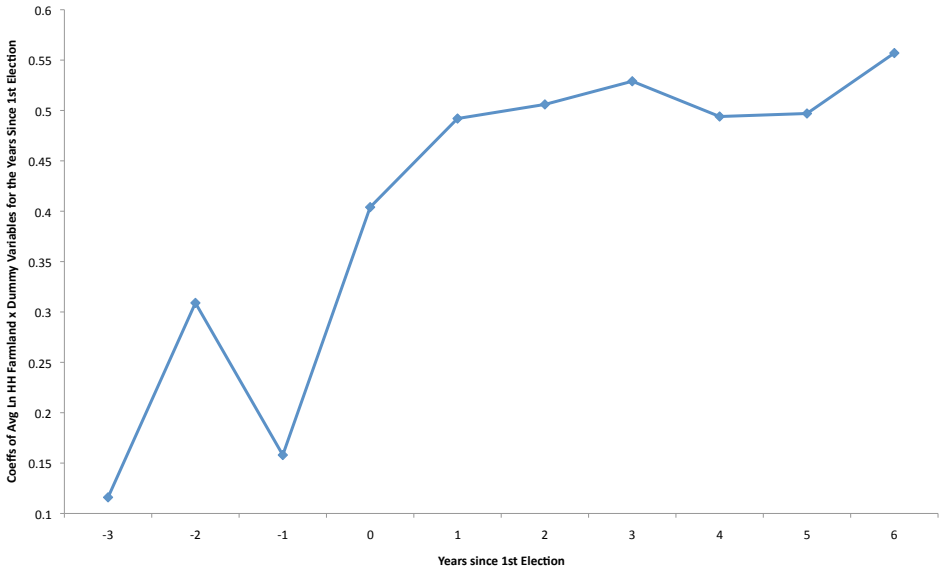

Figure 7: The Effect of Elections on School Enrollment Rates

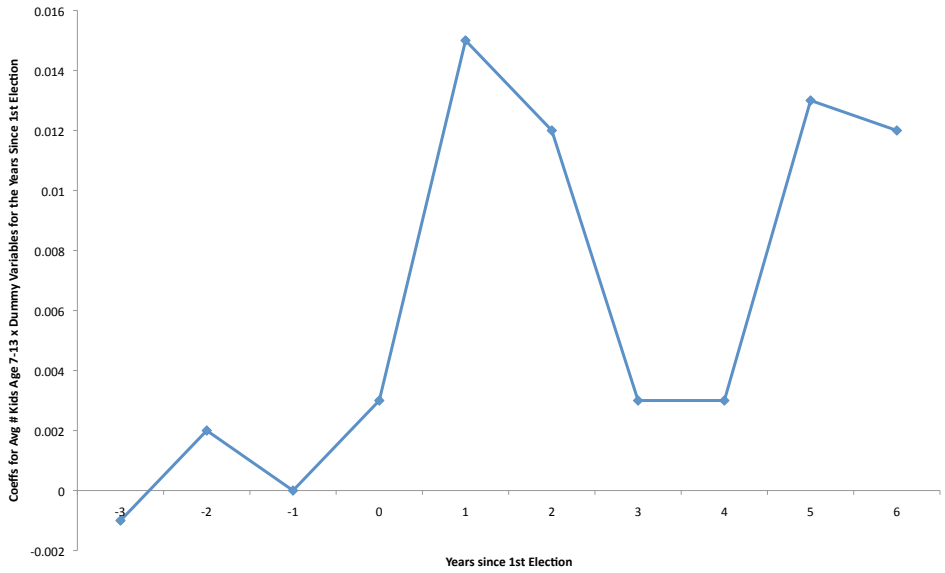

Table A.1: The Timing of Electoral Reforms

	First Election		First Open Nominations (Haixuan)	
Year	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: Balance Sheet for Village and Household Revenue and Expenditures

	Obs	Mean	Std. Dev.
A. Village Governments			
Total Revenues (100 RMB)	3,278	5,199	32,250
Total Expenditures (100 RMB)	3,278	4,934	34,304
B. Households			
Total Income	1,297	15,658	19,746
from collectives	1,296	375	814
from agriculture and home production	1,297	10,433	13,176
from wages	1,297	2,546	3,720
other	1,297	1,344	3,240
Total Expenditures	1,297	13,324	17,480
Household management expenditures	1,297	4,777	10,151
Levies and fees to local governments	1,297	454	530
Total Consumption	1,297	7,385	8,126
food	1,297	3,169	2,344
grain food	1,297	1,020	479
non-grain food	1,297	1,702	1,655
clothes	1,297	432	356
house	1,297	1,308	2,383
fuel	1,297	238	159
living services	1,297	487	622
tuition	1,297	655	691
other	1,297	742	1,139

Notes: Panel A includes an unbalanced panel of 217 villages. Panel B includes an unbalanced panel of a subsample of the villages in panel A.

Table A.3: The Effect of Elections for Each Year Since the First Election

	Dependent Variables													
	Ln Total Public Investment		Ln Median HH Farmland		Ln Median HH Taxes		Dummy for Whether Any Household had 2+ Child		Ln Total Village Land		Ln Village Arable Land**		Ln School Enrollment Rate***	
	(1) coef	(2) se	(3) coef	(4) se	(5) coef	(6) se	(7) coef	(8) se	(9) coef	(10) se	(11) coef	(12) se	(13) coef	(14) se
Dummy for Years Since 1st Election														
-3	0.363	(0.225)	0.317	(0.131)	1.071	(0.434)	0.054	(0.030)	0.135	(0.062)	0.116	(0.056)	-0.001	(0.005)
-2	0.471	(0.248)	0.334	(0.144)	1.45	(0.461)	0.039	(0.034)	0.112	(0.074)	0.309	(0.171)	0.002	(0.006)
-1	0.293	(0.232)	0.327	(0.155)	1.051	(0.524)	0.048	(0.038)	0.107	(0.077)	0.158	(0.080)	0	(0.008)
0	0.702	(0.215)	0.389	(0.171)	1.275	(0.571)	0.083	(0.045)	0.035	(0.135)	0.404	(0.201)	0.003	(0.005)
1	0.493	(0.236)	0.451	(0.186)	1.677	(0.648)	0.12	(0.050)	0.157	(0.069)	0.492	(0.241)	0.015	(0.008)
2	0.578	(0.231)	0.445	(0.191)	1.713	(0.662)	0.141	(0.052)	0.211	(0.073)	0.506	(0.239)	0.012	(0.008)
3	0.51	(0.230)	0.49	(0.209)	1.693	(0.714)	0.139	(0.058)	0.114	(0.090)	0.529	(0.228)	0.003	(0.006)
4	0.44	(0.222)	0.576	(0.227)	1.689	(0.736)	0.166	(0.060)	0.211	(0.074)	0.494	(0.236)	0.003	(0.006)
5	0.583	(0.251)	0.591	(0.247)	1.737	(0.781)	0.162	(0.062)	0.168	(0.073)	0.497	(0.232)	0.013	(0.006)
6	0.771	(0.266)	0.64	(0.259)	1.501	(0.885)	0.183	(0.066)	0.14	(0.082)	0.557	(0.223)	0.012	(0.006)
Observations	4340		1297		1297		5208		3296		5208		5064	
R^2	0.193		0.888		0.615		0.756		0.909		0.107		0.071	

Notes: All regressions control for the introduction of open nominations, province-time trends, village and year fixed effects. Standard errors are clustered at the village level. **In columns (11)-(12), we report the interaction effects of the dummies for years since 1st election x ln avg village farm land. These estimates control for post 1st election and the interaction of post open nomination x ln avg village farm land in addition to those stated earlier. ***In columns (13)-(14), we report the interaction effects of the dummies for years since 1st election x # kids 7-13. These estimates control for post 1st election and the interaction of post open nomination x # kids 7-13 in addition to those stated earlier.