The Impact of Limited Democracy in Rural China[*]

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2/08/2011 (First Version: 9/1/2007)

Abstract

We study the impact of the introduction of local elections in rural China (1980-2005). We exploit variation in the timing of the top-down introduction of elections and use a unique nationwide survey on the history of electoral reforms to show that elections significantly increased the *de facto* powers of newly elected leaders, decreased the enforcement of unpopular policies, increased household land allocation and the provision of appropriate public goods. Our results provide evidence that even imperfect elections can increase leader accountability and that marginal shifts towards democracy can significantly improve the well-being of constituents.

Keywords: Institutions, Elections, Leader Accountability, Local Democracy, Public Goods.

1 Introduction

In recent years, a large number of developing countries have experimented with local governance reforms intended to make local policy makers more accountable to their constituents. These reforms range from small changes such as modified auditing procedures or increased information provision to voters, to more fundamental interventions such as the introduction of local elections and the decentralization of responsibilities to local governments.[1] Studies have found that reforms in local governance have had significant effects in India (Bardhan and Mookherjee,2000; Chattopadhyay and Duflo, 2004; and Foster and Rosenzweig, 2004).[2] The effect of local electoral reforms in other contexts, especially those that have authoritarian regimes, is less well understood. This is an important gap in the literature since a growing number of authoritarian governments

[1]Bardhan and Mookerje (2006) provides a review of decentralization reforms in developing countries.

[2]Related to this, several studies find that information can play an important role in local governance. Banerjee and Pande (2009); Besley and Burgess (2002) find that better voter information can significantly affect election or economic outcomes. Similarly, Gustavo Bobonis and Schwabe (2010); Ferraz and Finan (2008, 2011); Olken (2007) find that the structure of audits affects local electoral governance.

have begun to experiment with local democracy.[3] The theoretical predictions for such reforms are ambiguous. On the one hand, providing citizens with better information and with the capacity to hold local leaders accountable should improve leader performance.[4] On the other hand, there are many reasons to be skeptical. Autocratic regimes might not have the incentives to implement proper electoral procedures. Moreover, even if elections result in the leaders that citizens prefer, they might not have the *de facto* power to carry out the will of their constituents. Indeed, Acemoglu and Robinson (2008) argue that reforms can fail to result in significant policy change because they are easily circumvented by existing elites. This is a particularly pressing concern when reforms are marginal and only affect the lowest level of an otherwise autocratic structure.

The goal of our study is to examine whether a marginal reform towards a *limited* democracy at the local level can affect economic policies and outcomes. To investigate this question, we examine the effects of a unique policy experiment: the introduction of village level electoral reforms during 1980-2005 in rural China. China provides a uniquely advantageous context for studying the effect of limited democracy because the elections were introduced in isolation from any other local governance reforms or political disturbances. This allows us to identify the effects of a particular channel: an increase in accountability of village officials to the villagers. It is worth noting that Chinese elections, which have by now been implemented in all rural areas, have affected the lives of over one billion individuals. In terms of the number of people, this is the largest electoral reform ever undertaken. Understanding its impact is therefore a first order question.

The set of electoral reforms commonly called the *village democracy* reforms began in the early 1980s in rural China. The first reform changed the selection of village chiefs (henceforth VCs) from appointment by the regional and village Chinese Communist Party (CCP) branch to election by villagers. The initial reform mandated that villagers must be offered at least two candidates per position and are allowed to abstain from voting or vote negatively for any candidate. Further reforms opened candidate nominations to all villagers. Case studies and anecdotal provide mixed evidence regarding how these reforms affected the VC's accountability. Several reports document that elections often were poorly implemented and subject to the manipulation of the incumbent VCs and CCP officials (Oi and Rozelle, 2000). However, there are also numerous recorded instances in which villagers exercised their power to vote negatively resulting in the recall of unsatisfactory VCs (O'Brien and Li, 2006). This suggests that elections, despite being imperfect, might have provided villagers an effective channel to voice discontent and hold (some) VCs accountable. The objective of this paper is to provide systematic evidence on the impact of elections on VCs accountability and on policy outcomes.

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[3]See Gandhi and Lust-Okar (2009) for a recent literature review on elections in autocratic regimes. There are many examples of autocracies that have held regular elections at a sub-national level. For example, Indonesia under Suharto (1968 - 1998), Brazil during the military dictatorship (1964 - 1985) and Mexico under PRI (1929 - 2000). More recently, local elections have been introduced in Vietnam in 1998, Saudi Arabia in 2005 and Yemen in 2001.

[4]This is what the political accountability literature would predict. See Besley (2006) for a review of the theory. Several recent studies also show that citizens are effective monitors for the provision of public goods. For example, Björkman and Svensson (2010, 2009) find that citizen monitoring can greatly improve the provision of public goods.

Our study faces several challenges. First, existing documentation is very limited. Second, the effect of a shift in local-government accountability is not conceptually obvious in a context where the upper levels of government remain authoritarian. Finally, there is the empirical difficulty of establishing causality in estimating the effects of the electoral reforms.

The principal contribution of this paper is to address these difficulties and examine the causal effect of local elections on local leader accountability and policy outcomes in rural China. To address the first challenge, we document the history of political reforms, policies, public good investments, and the *de facto* powers of village leaders by conducting a large retrospective survey for over two hundred villages that covers the years 1980-2005. To the best of our knowledge, these data are the most comprehensive survey of the political reform history of Chinese villages in terms of geographic and temporal scope. We match this survey to annual economic data collected by the Ministry of Agriculture during 1987-2005, forming a village-level panel of political, economic and social variables that we use for our empirical analysis.

In order to address the second challenge, we develop a simple model of political accountability. Our model is adapted to the context of rural democracy because it allows the situation of the VC to change from being solely accountable to the upper-level government (e.g., CCP) to being accountable to both the upper-level government and the villagers. The model distinguishes the effects of elections on three types of policies: 1) policies that the village government has discretion over, but over which the upper levels of government and villagers disagree (e.g., upper-government expropriation of village land and the One Child Policy); 2) policies that the village government has discretion over, but over which the upper levels of government and villagers agree (e.g., village provision of public goods and household land allocation); and 3) policies that the village government has little discretion over (e.g., upper-government special aid for households below national poverty line and the provision of highschool education). The model predicts that if elections shift the accountability of village governments towards constituents, then elections will: unambiguously shift policies of the first type towards the villagers' preferences, shift policies of the second type towards villagers' preferences if villagers have an advantage in monitoring village officials, and have no effect on policies of the third type.

To estimate the causal effect of the introduction of elections on the outcomes described above, we exploit the fact that elections were implemented in a top-down fashion and use a fixed effects strategy. Consistent with top-down implementation, our data show that villages on average introduced elections very soon after higher levels of government adopted village elections. Therefore, the timing of the introduction of elections within the larger administrative units can be considered to be quasi-random. Our main empirical 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 in the same province (and year) which have not yet introduced elections: village fixed effects control for all time-invariant differences between villages, such as distance to a city; year

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fixed effects control for all changes over time that affect regions similarly, such as macro economic growth or nationwide policy changes; and province-year trends allow us to control for all differential changes across provinces which are broadly linear. They are important given the divergence in economic growth across China during this period.[5] Interpreting our baseline estimates as causal requires the assumption that within a province (and year), the introduction of elections did not coincide spuriously with other time-varying factors that could drive the outcomes we observe. We do not take this assumption as given and conduct a large number of robustness tests and counterfactual exercises. They are discussed in detail later in the paper.

The main results of the paper are the following. First, we establish that newly elected leaders increased their *de facto* power relative to non-elected village CCP leaders. For instance, the introduction of elections substantially increases the power of the VC to unilaterally reallocate land and to conduct large public investments. Second, we use data on policy outcomes to proxy for the three corresponding policy types from our model. We find that elections reduce the incidence of upper-government expropriation of village land and the enforcement of the One Child Policy; increase *appropriate* investment in village public goods and household land allocation; and have no effect on upper-government special aid or the distance to highschools. These results are consistent with the predictions of our simple model when elections succeed in making leaders more accountable to villagers. We also find that elections increase household land allocation for a fraction of villagers and reduce the amount of land controlled directly by the VC. Moreover, we find that the increase in public goods investment is entirely funded by villagers and that elections have no effect on upper-government special aid. This is important as it is inconsistent with the alternative explanation that our results partly reflect systematic upper-government favoritism rather than a shift in village-government accountability. The results are robust to a large range of controls such as election methods, the economic, demographic and geographic characteristics of villages, differential trends and the introduction of other policies.

The main concern over the causal interpretation of the empirical results arises from the possibility that the timing of the introduction of elections was endogenous to certain village characteristics. This would bias our estimates if elections have heterogenous treatment effects – i.e., our estimates will overstate the average effect of elections if the county governments delayed introducing elections to villages that benefit less from them. We address this potential problem with several strategies. The most important is to instrument for the introduction of elections in a village with the first introduction of elections in the same county. The 2SLS estimates are similar to our baseline estimates. Therefore, we conclude that our main estimates are unlikely to overstate the average effect of elections due to endogeneity concerns. See section 4 for more discussion.

The results of this study are important for policy makers and researchers in development eco-

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[5]Controlling for province-specific time trends is similar to province-year fixed effects, except that it imposes more structure on the province-specific changes over time. We do not have enough variation within province-year cells to estimate a more flexible regression where we control for province-year fixed effects. See section 4 for more discussion.

nomics and political economy. They show that a marginal movement towards democracy can generate significant improvements for the population, even in an authoritarian regime.

There are also several insights in addition to the main result that elections can improve outcomes for constituents. The finding that elections affect policy outcomes even for policies that the upper government and villagers are likely to share similar preferences over suggests that villagers are better at monitoring and restraining their leaders than the upper government. Furthermore, our finding that increasing the number of candidates and opening nominations to villagers has no additional discernible effect suggests that the main mechanism behind the increase in accountability is an increase in leader *incentives* rather than the improved *selection* of leaders. While the interpretation of our results depend on specific assumptions and should therefore be interpreted cautiously, these insights nevertheless make significant progress in our understanding of the mechanisms underlying the effect of elections in an authoritarian context. See section 3 for a more detailed discussion.

This paper contributes to a number of different literatures. First, we add to the growing body of work on the role of local governance in developing countries, which are mostly set within the larger context of democracies.[6] Among these, our study is most closely related to Björkman and Svensson (2009) which study the effect of citizen monitoring on local-government public good provision, and Foster and Rosenzweig (2004) which finds that the introduction of the *panchayat* system in India improved the provision of public goods. Our finding that elections increase appropriate public goods provision in China is consistent with the results of these previous studies. The implication that villagers are better than upper levels of government at monitoring village leaders are particularly complementary to the findings of Björkman and Svensson (2009). However, the larger context of our study and the mechanisms driving our results are very different from these earlier works.[7] The only empirical studies of democratic local governance within authoritarian regimes are the few about China's village elections (e.g., Rozelle and Boisvert, 1994, 1995; Oi and Rozelle, 2000; Kennedy, Rozelle, and Shi, 2004; Brandt and Turner, 2007).[8] These studies find that elections reduce inequality and increase the overall level of public goods provision (e.g., Gan, Xu, and Yao, 2007; Luo, Zhang, Huang, and Rozelle, 2007, 2010; Shen and Yao, 2008; Zhang, Fan, Zhang, and Huang, 2004).[9] We add to these studies by significantly broadening the range of

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[6]For example, see the papers cited at the beginning of this introduction. More generally, our work adds to the literature on the relationship between political institutions and economic outcomes, which mostly focuses on the comparison across countries and different clusters of institutions (e.g Mauro, 1995; Hall and Jones, 1999; Acemoglu, Johnson, and Robinson, 2001; Persson and Tabellini, 2004).

[7]Relative to China, Uganda has a much weaker state (e.g., bureaucratic capacity) and we are examining the effect of formal institutions (e.g., elections), of which citizen monitoring is only one of the many mechanisms. India is a democracy overall while China is not. In terms of mechanisms, Foster and Rosenzweig (2004), 2004 argue that their results are driven by party competition. In our context, there is only one party, so party competition cannot play a role. This institutional fact together with our findings that all of the effect of elections come from the introduction of the first election and not from the competitiveness of elections suggests that in China, elections matter mainly because they allow villagers to express discontent.

[8]In a related context Martinez-Bravo (2010) studies the implications of different local governance structures in Indonesia under Suharto in the persistence of patronage networks and voter manipulation in the first democratic election.

[9]Zhang, Fan, Zhang, and Huang (2004) uses a panel of 60 villages from two provinces and finds that elections have little effects on village government revenues but shift the distribution of taxation from individuals to enterprises; and that elections and power sharing improve the allocation of public expenditures. In a different sample of 48 villages, Wang and Yang (2010) finds that

outcomes, and the temporal and geographic scope examined. Moreover, we provide evidence for a mechanism that drives the election effects. Amongst studies of rural China, our examination of land expropriation and allocation is also closely related to two recent studies on land tenure in rural China (Rozelle and Li, 1998; Jacoby, Li, and Rozelle, 2001).

Our findings also provide valuable empirical evidence for understanding the differences in incentives and performance between appointed and elected officials.[10] Finally, the evidence that elections matter because of improved incentives is closely related to empirical studies on re-election incentives (e.g., Besley and Case, 1995; Bó and Rossi, 2008; Gustavo Bobonis and Schwabe, 2010; Alain de Janvry and Sadoulet, 2010).[11]

This paper is organized as follows. Section 2 describes the background of Chinese villages, and the motivation behind the implementation of electoral reforms. Section 3 provides a conceptual framework for local elections in an authoritarian regime. Section 4 presents the empirical strategy. Section 5 describes the data. Section 6 presents our empirical findings. Section 7 offers concluding remarks. This paper is supplemented by an unpublished Appendix which contains the formal model that underlies our conceptual framework, a data appendix, and a number of empirical results that are not presented in the paper due to space constraints.

2 Background

Villages are the lowest level of administration in rural China. The Chinese government, led by the Chinese Communist Party (CCP), is broadly ordered in a vertical hierarchy, from the central government in Beijing down to the rural levels comprised of 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. Today, there are 27 provinces (excluding city-level municipalities) governing 2,872 counties, which contain 14,677 townships and 623,669 rural villages (as defined by the number of village governments, *cunming weiyuanhui*). This means that the average county government supervises approximately 5 townships and 217 villages. On average, there are approximately 400 households in each village. Villages are not an official administrative level of government. Nevertheless, the village level is extremely important for the well-being of its citizens because it implements central-government policies within the village and

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elections increase the share of public expenditures in the village budget, but reduce the shares of administrative costs and income handed to the township government. Shen and Yao (2008) finds that elections reduce the Gini coefficient by 0.04, or 14.3% of the sample average. Gan, Xu, and Yao (2007) finds that the introduction of elections improve village healthcare provision. Luo, Zhang, Huang, and Rozelle (2007) uses a large cross section of village to find that elections are correlated with higher public goods provision. Brandt and Turner (2007), in a sample of 60 democratized villages, find that elections with better democratic quality are associated with better economic outcomes, and that even poorly conducted elections can provide leaders with strong incentives. Our results are consistent with the latter finding as we show that incentives provided under poor elections are better than under appointment, but we find no significant effects of increasing election quality.

[10]This literature has mostly been theoretical. For example, see studies such as Besley and Coate (2003), Maskin and Tirole (2004), Alesina and Tabellini (2007, 2008) and Martinez-Bravo (2010).

[11]For instance, Besley and Case (1995) and Daniel and JR. (1997) compare elected officials who face term limits with those who do not, Bó and Rossi (2008) study the effect on effort of a reform in Argentina that reduced term lengths, Gustavo Bobonis and Schwabe (2010) find that leaders reduce corruption in anticipation of audits before re-election, and Alain de Janvry and Sadoulet (2009) find that majors who face re-election implement cash transfer programs in a more transparent and efficient way.

determines many important village level policies such as public goods provision and land allocation (Oi and Rozelle, 2000; Rozelle and Boisvert, 1994, Brandt and Turner, 2007; Whiting, 1996).

Village governments were first organized by the communist government during the early 1950s. Each village has two groups of leaders. The VC (e.g., village "chief", "head" or "chairman") leads a village committee that typically comprises three to five members. The second group of leaders are the CCP village branch. They are led by the village party secretary (henceforth PS). Before elections were introduced, all these positions were filled by appointment by the county government and village party branch.

Village government responsibilities are numerous. First, village governments implement some central-government policies and therefore have a degree of discretion over enforcement. Part of the discretion comes from the fact that many central policies are ambiguous. For example, village governments have significant discretion over family planning policies due to contradictory directives. On the one hand, to curb female infanticide, *State Council Document No. 7* of 1984 allows rural households to have a second child if the first child is a girl. On the other hand, this document does not explicitly states when the exemption is to be applied, and the central government continues to pressure regional leaders to minimize fertility. In practice, formal exemptions are granted at the county level, but *de facto* enforcement of the One Child Policy (e.g., persuading parents to have abortions of higher parity pregnancies, monitoring illegal births and administering fines) is carried out at the village level. Moreover, village leaders are the ones that lobby the county for formal exemptions. Therefore, village leaders can influence the extent to which the One Child Policy is enforced by choosing the amount of effort they exert in lobbying upper governments versus the amount of effort in monitoring and persuading village parents to have fewer children. This room for discretion of enforcement at the village level is consistent with the observation that there is much variation in the implementation of the One Child Policy (e.g., the conditions for which parents are allowed multiple children) across villages. This is true within provinces and even within counties.[12] Another important and unpopular policy that village governments can influence is the expropriation of village land by upper levels of government. This occurs rarely and is typically related to laws of eminent domain for the construction of infrastructure and the geographic expansion of cities and townships. However, they are always highly contentious as they result in the permanent loss of farmland to villagers. There are also accounts of how corrupt city and township officials use these legal means to obtain rents from the expropriated land. In both legal and corrupt cases of land expropriation, it is typically the county and township that benefit and the villagers that lose (Bernstein and Lu, 2003; Cai, 2003; Guo, 2001; O'Brien and Li, 1999).[13] As with the One Child Policy, the village government can attempt to prevent or delay expropriation by lobbying the

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[12]This variation is present both in our survey data and in the data on village level family planning policies reported by the *China Health and Nutritional Surveys* (1989, 1991, 1993, 1997, 2002).

[13]By law, villagers are financially compensated for the expropriated land. However, compensation is often inadequate in practice.

upper-level government. It can also organize villagers to peacefully resist.[14]

The second main duty of village governments is to allocate collectively owned village assets for production. The most important of these assets is land, which is all publicly owned in China. There are two types of land reallocations in rural China. The first type includes large scale reallocations which affect a significant proportion of households in the village. The second type includes smaller scale reallocations (e.g., marginal adjustments for a few households). Our study focuses on the latter because they occur with higher frequency and because village leaders have more discretion over them as they receive relatively little attention from upper levels of government. While most of the land is allocated in long term contracts to households for farming, a small fraction is retained under the direct control of the village government so that the village government could adjust land holdings for households that were growing in size without carrying out village-wide reallocations. Land that is directly controlled by the village government can be used to generate revenues for the village government. This is often done by leasing land to rich village farmers or village enterprises/factories.[15] Leasing out village land is widely believed to provide corrupt village leaders with rent-seeking opportunities (Brandt and Turner, 2007; Oi and Rozelle, 2000; Rozelle and Boisvert, 1994). Not surprisingly, land allocation is a sensitive and contentious issue in rural China, where the average household farms only approximately one mu (1/15th of a hectare). A small increase in land can therefore be extremely valuable by generating revenues with activities such as raising meat or poultry (Unger, 2002; p.145).

Third, village governments have an important role in the determination, provision, and financing of public goods within the villages, such as primary schools and irrigation projects. It is commonly believed that there is general under-provision of most public goods (Luo, Zhang, Huang, and Rozelle, 2007). Village governments share the responsibility for public goods with higher levels of government and some have argued that the pressure from the higher levels of government to conform with nationwide incentives can result in a choice of public investment that does not match the preferences of villagers (Oi, 1999).[16]

The village government also provides much of the financing for public goods, which are typically funded from two sources: profits generated by collectively owned property (e.g., village enterprises), and fees and case-by-case levies charged to households.[17] Ad hoc fees are sometimes added as surcharges to taxes that the village government collects on behalf of upper governments, and levies can be collected for specific purposes, such as to fund a public good. It is important to

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[14]O'Brien and Li, (2006: Ch3) provides many examples of how village governments coordinate "rightful" resistance in protest to land expropriation.

[15]There are many papers describing land use and contracts in rural China. For example, see Rozelle and Li (1998).

[16]For example, in cases of blanket central mandates to increase the number of primary schools or irrigation infrastructure, counties may simply force villages to construct a certain number of these objects without considering village-specific needs. Central directives in this case can mean explicit policies or simply a "wish" expressed by high-ranking government leaders. There are many examples of how an expressed wish of the central government is transformed into uniformly imposed policies by regional governments seeking political promotion (O'Brien and Li, 2006).

[17]In rare cases, village governments can generate revenues by selling or leasing land to outside entities. However, this is a very recent phenomenon.

note that the village government's ability to impose ad hoc fees changed towards the end of our study period when the *Tax and Fee Reform* (2002-3) banned the practice. We will control for this in our empirical analysis.

Electoral reforms   caused the positions of the village committee and the VC to be filled by election rather than by appointment. There was no change in the selection method of the members of the village party branch and PS positions.

Elections were first introduced in the early 1980s in some villages, often with the implicit encouragement and tolerance of provincial leaders. In 1987, village elections were formally codified by the central government in the *Organic Law on Village Committees* (henceforth OLVC). This law stipulated that the positions of VC and village committee had to be filled by elections in which at least two candidates would compete for each position. Henceforth, we refer to elections in which the number of candidates exceeds the number of positions as *competitive elections*. VCs were to be elected for three year terms with no stipulated term limits. The formal party structure was unchanged and the county level CCP was still entitled to select the candidates running in the new elections. Nevertheless, this was a significant change from the previous system where all positions were directly appointed by the party and villagers had no voice whatsoever. After the passage of the 1987 law, elections have been rolled out across rural villages, and have experienced varying degrees of interference by the village CCP party branch and county governments (Oi and Rozelle, 2000; Li, 2009). In 1998, the OLVC was revised. The revised law required all villages to implement elections, and explicitly laid out a set of electoral procedures. In particular, it made open nominations by villagers, commonly called *haixuan* (meaning an ocean of choices), mandatory.

Neither the original nor the revised OLVC clarified the relationship of authority between the VC and the PS. In fact, the latter stated that village decisions were to be taken by the VC and village committee under the "guidance"of the CCP Branch. This vagueness led to disputes and encroachment by the PS and county government on the independence of the VC (e.g., Oi and Rozelle, 2000; Guo and Bernstein, 2004). One of the goals of our survey is to document the *de facto* power of each leader and systematically examine the effect of elections on the relative power of the VC versus the PS.

Despite the revision of the OLVC, the quality of the electoral procedures continued to be uneven. In particular, there was substantial variation in the way elections were conducted in terms of nomination and balloting methods (Brandt, Rozelle, and Turner, 2004; Pang and Rozelle, 2006; Birney, 2007). Similarly, despite the mandate of holding competitive elections, villages did not always comply with this precept and some of the initial elections were uncontested. Our data documents these procedural differences so that we can control for them directly in the empirical analysis.

There were several additional facts about village elections to keep in mind. By law, there were no political parties and no slates of candidates with common platforms. Electoral campaigning was not allowed for the most part. At the same time, candidates were typically well-known by the

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villagers as they were from the same village and there has been very little labor mobility in rural China in the past few decades. As a consequence, candidates typically run on well-understood issues and were selected for qualities that are observable on a daily basis.[18]

The timing of elections is important for our empirical strategy. There are three key facts to note. First, the main force behind the introduction of elections was the province-level government which implemented the policy in a top-down fashion. Once the provincial government decided to introduce elections, almost all villages within that province followed within a few years (O'Brien and Li, 1999). Second, the only substantive resistance came from the county-level government. County leaders were concerned that implementing elections in "problematic" villages would make it even more difficult to enforce central directives. Such villages included those that were already resisting unpopular policies and those that were dominated by a large clan.[19] We control for these village characteristics in our empirical analysis.

Most importantly, by all accounts, villages had little discretion over the timing of introduction of elections, which is characteristic of reforms in rural China. This fact is articulated in the following quote:[20]

> 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).

Starting with the OLVC in 1987, pressure to adopt elections gradually increased on provinces that had not yet implemented them. This means that the motivation and the effect of elections could differ between provinces that introduced elections on their own initiative and those that were forced to do so by the OLVC. We will address this possibility of heterogenous treatment effects explicitly in the empirical analysis.

The motivation for electoral reforms arose from several sources. First, proponents wished to improve the enforcement of central policies by providing local leaders with a democratic mandate. They claimed that village elections could lead to better compliance with unpopular upper-government policies because elected leaders would have more legitimacy and would be more sen-

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[18]There very few accounts of electoral campaigning. In many cases elections were set up with only a few days' notice (Unger, 2002: p. 221).

[19]In the latter case, 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).

[20]The general passivity of village governments in implementing policy reforms is noted by Unger (2002, p. 96-98). He studies land reforms in twenty-six villages during the 1980s and find that they all wait for the county-township-level government to tell them when to move. He finds similar patterns in his study of the adoption of the *Household Responsibility Reform* during the mid 1980s in another set of villages.

sitive to villagers' demands, resulting in a more fair distribution of the burden of these policies. It was also hoped that local leaders with a democratically elected mandate could facilitate the co-ordination necessary for providing public goods and be more sensitive to the needs of villagers (O'Brien, 1994; Kelliher, 1997; O'Brien and Li, 1999).

Second, advocates of the reforms argued that having elected leaders would mitigate the difficulty of the central government in monitoring local leaders in a geographically vast and heterogeneous country. This problem had been endemic in the centrally planned regime since its conception in 1949. The increasing regional differences caused by post-Mao market reforms further increased the monitoring difficulty for the CCP. Morever, even if the central government could perfectly monitor local leaders' efforts, it still faced the difficulty of knowing the preferences of each locality and therefore would not know the appropriate targets for measuring performance.[21] The introduction of elections was seen as a potential solution as it shifted the responsibilities of leader monitoring onto villagers. In short, proponents argued that the shift in accountability to villagers would impose checks on cadres in the village and county-township governments, which was necessary when top-down supervision was insufficient.[22]

Together with improved monitoring, elections could also potentially improve the selection of village leaders. This is related to monitoring since villagers would vote for competent candidates and unseat corrupt incumbents (Kelliher, 1997). Villagers may also select better leaders if they have better information on the candidates, who almost invariably come from within villages.[23]

The promoters of local democracy faced resistance from skeptics, especially at the county level. They were concerned that without the power to appoint village leaders, the CCP would have little leverage to ensure the full implementation of centrally mandated policies (O'Brien and Li, 1999).

3   Conceptual Framework

Accountability and Policies.   The main conceptual question regarding local elections in China is whether they successfully increased the accountability of village leaders towards villagers. This is far from clear given their imperfect implementation and the continued supervisory role of the CCP. Moreover, the village government enacts a range of policies, and it is unclear ex-ante how an increase in the accountability of the village government towards the villagers affects each policy outcome. To shed light on this issue we develop a simple model of political accountability and illustrate the effects of an increase in accountability on different policies. This model is presented formally in the Appendix. In this section, we summarize the results.

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[21]See Meng, Qian, and Yared (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.

[22]Peng Zhen, vice-chairman of the NPC Standing Committee and a strong supporter of granting democratic rights to villagers, said "Who supervises rural cadres? Can we supervise them? No, not even if we had 48 hours a day...."(Peng Zhen's speech at the chairmanship meeting of the Standing Committee of the Sixth NPC, April 6, 1987. Cited in O'Brien and Li (1999)).

[23]Democratization was also seen by some as a policy that would boost the popularity of the central regimes. Kelliher (1997) argues that, as an afterthought, some of the national and provincial leaders advocated for the introduction of village elections because they made for "superb propaganda abroad". While no one has argued that elections were introduced to appease foreign pressure, it has been observed that The Ministry of Civil Affairs arranged several visits for foreigners to view the advances of self-government in rural areas after elections were introduced.

Our model distinguishes between three types of policies. The first type comprises policies for which villagers and upper governments disagree. Two examples are the enforcement of the One Child Policy, and upper government expropriation of village land. Both of these policies are hugely unpopular among villagers but promoted by the upper levels of government. The model predicts that a shift in accountability of village leaders from upper levels of government towards villagers leads to a shift in the policy outcomes of this type of policies in the same direction.

For the second type of policies, there is no conflict between citizens and upper government officials, but better implementation entails a reduction of the utility of the village leader. The provision of appropriate public goods is an example of this type of policies. The upper government has no stake in the particular composition of village public goods but wants villagers to be content. Therefore, it arguably agrees with the villagers. In contrast, providing the optimal level of public goods requires effort from the village leader as he needs to raise a significant amount of the funds and spend time in determining the correct object for investment. Another example is the amount of village government land leased out to enterprises. The central government might not have strong views on the within village land distribution, while villagers have a preference for minimizing the land not employed in household production. In contrast, village leaders benefit from leasing land out to enterprises since this raises village government revenues and provides rent-seeking opportunities.[24]

The effect of a shift in leader accountability on the second type of policies is more nuanced than the effect on the first type because both the upper government and villagers have similar preferences to begin with. Our model predicts that an increase in accountability will only shift policy outcomes towards the preferences of the villagers if villagers are better than the county government at monitoring and restraining village officials.

Finally, the third type of policies includes those that are beyond the control of the village government. Examples include special transfers from upper governments to combat poverty or the provision of high school education. Both policies are decided outside of the village. Special aid is a transfer from higher levels of government to villages depending on the number of households below the national poverty line. The provision of secondary education is decided by regional ministries of education. In fact, high schools in China are always in townships and cities and never located in villages. Since these policies are beyond the control of the village government, a shift in accountability should not affect them. These policies can therefore serve as *placebos* in the empirical analysis.

In the empirical section, we test for the effects of elections on these three sets of policies to determine whether elections effectively made village officials accountable to villagers. Our model also makes the interesting auxiliary prediction that an increase in accountability will increase the provision of excludable goods (that villagers demand) only for as many households as the VC re-

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[24]See section 2 for references.

quires in terms of votes for remaining in office.[25] We will investigate this additional hypothesis by examining the effect of elections on household land allocation, which is an excludable good.

Mechanisms   While the objective of this study is to estimate the reduced form effect of the introduction of elections on specific policies in rural China, it is important to keep in mind the different theoretical channels through which electoral accountability might influence outcomes. Here, we briefly discuss how these mechanisms would apply to our context.

It is important to note that the incentive effect of elections in rural China might be at play even if elections are not *competitive* (e.g., if the CCP nominates only one candidate per position). There are several reasons for this. First, elections create a formal and direct mechanism for villagers to express disapproval of their leaders by allowing villagers to vote negatively or abstain. Therefore, even in cases where county governments choose the candidates, they must internalize the villagers' preferences to some extent. In his book, O'Brien and Li (2006) documents numerous cases in which villagers, dissatisfied with the nominated candidates, appealed to province and central government officials, who investigated these concerns, recalled incumbent VCs, and called for new elections.[27]

Second, both villagers and county governments could interpret the introduction of village elections as a strong signal that the central government is concerned about the preferences of villagers. This empowers villagers and can contribute to a favorable change in policies even if the electoral process is imperfect.[28] Therefore, even non-competitive, non-open nomination elections can have important disciplining effects: the VC would try to accommodate villager preferences to avoid

The political agency literature proposes two main mechanisms through which voters use elections to hold politicians accountable.[26] First, elections can help voters to 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. Interestingly, as detailed above, an important element of the debate within the Chinese central government over whether or not to introduce elections was about the capacity of elections to restrain and control local officials, which is the central concern of the political agency literature.

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[25]Details on the derivation of this auxiliary result are omitted for the sake of brevity. They are available upon request.

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

[27]For example, O'Brien documents a case in which angry crowds of villagers wanted to dismiss their village chief and protested in the township. In the end, they successfully obtained a recall, new elections were held and new VC obtained the position. In yet another example, villagers complained that their elections had been manipulated with fraudulent votes and argued that the nominated candidates were corrupt. They requested the right to nominate their own candidates (this was many years before open nominations was officially introduced in 1998) but were ignored by their county government. Then, they appealed to Beijing, where the Ministry of Civil Affairs investigated, forced a recall and a new election where the villagers nominated their own candidates (O'Brien and Li, 2006: Ch. 3).

[28]Villagers often challenge local policies based on the spirit of central party policies that have not been formalized into legislation. In the period of our study, commonly quoted phrases from such policies are "to clean up the government" (*lianzheng*), "to struggle against corruption" (*fan fubai douzheng*), "to build socialist democracy" (*jianshe shehuizhuyi*), or "to build a country ruled by law" (*jianshe fazhi guojia*) (O'Brien and Li, 2006: p. 218).

requests for a recall and complaints to upper-level governments.

The most straightforward way to examine the role of the *selection* mechanism is to compare the effect of elections for villages where the introduction of elections changed the leadership to villages that retained their previously appointed leader. Since the incentive effect exists in all elections, the difference in the effect of elections between these two types of villages can be interpreted as the selection effect. Unfortunately, we lack information on the VC prior to the introduction of elections.[29] However, we can use the introduction of open nominations, which occurred after the introduction of elections for most villages, to shed some light on this question. The selection mechanism should be particularly important for elections with open nominations, because they maximize the choice set of voters. Therefore we can assess the importance of the selection mechanism by examining whether opening nominations enhances the effect of elections. Note that the interpretation of the effect of open nominations depends on how the CCP selects candidates. Finding that open nominations enhanced the effect of elections would suggest that the selection mechanism contributes to the impact of elections. However, finding that open nominations has no additional effect can only rule out the presence of the selection mechanism if one knows for certain that the CCP and villagers would select different candidates.[30] While we believe that this is the most likely scenario, we have little concrete evidence or insight into the selection criteria of the CCP. Therefore, the results for the effect of introducing open nominations should be interpreted with this explicit caveat.[31]

4 Empirical Strategy

In this section, we present the main empirical strategy under the assumption that elections were implemented in a top-down fashion, where the provincial-level government initiated the reform and villages had no discretion over when to implement their first election. . Then, we discuss how departures from this assumption would affect our estimates and our strategies for addressing the potential issues which arise.

To establish the causal effect of elections, we employ a fixed effects strategy where we 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) at a finer level than province fixed effects. Year fixed effects control for changes over time that affect all villages similarly (e.g., national policy changes). Province-specific time trends control for the fact that provincial governments' decisions to introduce elections may

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[29]More specifically, we only have data on VCs characteristics prior to the first election for fourteen villages, which makes an econometric analysis non feasible.

[30]In other words, it is possible that the introduction of elections (with CCP nomination) led the CCP to *select* the candidates that the villagers prefer, and this would be a reason why no additional effect of open nominations can be found.

[31]In principle, another mechanism that can cause elections to increase accountability is candidate competition. The median voter literature suggests that candidate competition should favor candidates that run on platforms that are preferred by a majority of voters. See Black (1948) and Downs (1957) for the classical exposition of this theory, and Persson and Tabellini (2000) for a modern textbook treatment. This is not very relevant to our context since median voter models require candidates to be able to campaign on their platforms and to commit to the promises made during the campaign, while there is no campaigning our context. In fact, campaigning was not generally allowed in rural China for the period of our study. In many cases elections were set up with only a few days' notice (Unger, 2002: p. 221).

have been driven by time-varying characteristics of the province. These time trends also allow us to control for the widening differences across regions brought about by unequal economic growth during the long time horizon of our study. Our strategy is similar in spirit to a within-province-year *differences-in-differences* strategy where we compare the outcomes of villages that have had their first election to villages in the same province which have not yet implemented their first election. However, we control for province-time trends instead of the more flexible *province × year* fixed effects because elections within provinces are typically introduced within few years of each other and there are many province-year cells within which there is no variation in election.

In addition to estimating the effect of introducing elections, our baseline estimation also controls for whether an election is *competitive* and whether *open nominations* were used to select the candidates. Since our data show that none of these policies were ever reversed after their introduction, we control for them with dummy variables indicating "post" the year of introduction. The baseline estimation can be characterized as the following equation.

$$Y_{vpt} = \beta Election_{vpt} + \theta Competitive_{vpt} + \lambda OpenNom_{vpt} + \gamma_{pt} + \delta_v + \rho_t + \varepsilon_{vpt} \qquad (1)$$

Y_{vpt} is the outcome of village v in province p during calendar year t. It is a function of: whether the first election, first competitive election, and the first open nomination has taken place; province-year trends, γ_{pt}; 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 if elections had an effect on a particular policy outcome.

Interpreting β as the causal effect of elections requires the assumption that within a province (and year), the introduction of elections is uncorrelated with other factors that could affect the outcomes of interest, and that no other event occurred at the same time as the introduction of elections.

There are several potential threats to our identification strategy. The first concern arises from the possibility that the timing of the introduction of elections was endogenous to certain village characteristics. This will only bias our estimates if elections have heterogenous treatment effects. For example, if villages that will experience larger effects from elections introduce elections earlier, then our estimate will overstate the magnitude of the *average* effect of elections. In the next section, we show that this is very unlikely as most villages hold their first elections within few years of the first election in the same province and almost immediately after the first election in the same county.

Nevertheless, to be cautious, we address the potential endogeneity of the introduction of village elections to village characteristics by instrumenting for the timing of the first election in each village with the timing of the first election in the same county. This addresses the possibility that the OLS estimates are endogenous to village characteristics by only using variation in timing at the county level. Interpreting the 2SLS estimates as causal requires the assumption that the introduction of elections at the county level is not correlated with village level factors that affect the outcomes of interest. This seems highly unlikely since our sample of villages are chosen randomly

from each county, which typically contain over 200 villages. In other words, even if counties decided to introduce elections based on unobserved characteristics of certain villages, it is highly unlikely that they systematically made this decision based on the villages in our sample, which are randomly chosen from all of the villages in each county. In any case, in the next section we investigate this claim with the data.

We also address the concerns of endogeneity and heterogeneous treatment effects using several additional strategies. See the section on robustness for a more detailed discussion.

The second concern is that our main results might be driven by spurious correlations. Our study covers 25 years in a country that experienced vast changes that differed regionally. If any of these changes are correlated with the timing of the introduction of village elections, then our estimates could simply reflect a spurious correlation. This seems unlikely given our controls for province-time trends. Also, note that the introduction of elections was staggered in timing and eventually affected all villages in China. For our estimates to be driven by spurious factors, such factors would also need to cover all of China and have similar timing as the elections in each locality. We conducted an exhaustive review of national policies during the period of our study and find none with the same temporal and spatial pattern as the introduction of elections.[32] Therefore, for our results to be driven by spurious relationships, elections would have to coincidentally occur at the same time as a combination of unobserved factors across villages that produce exactly the same effects as those expected from a change in leader accountability. This seems implausible.

A more relevant and related concern is the possibility that there were changes in county policies that happened at a similar time as the introduction of elections. This would be the case, for instance, if favorable policies were used to induce villages to introduce elections, or if county governments used both favorable policies and elections to deal with particularly dissatisfied villages. For example, if the county government relaxed the One Child Policy or rescinded land expropriation requests at the same time that it induced villages to introduce elections, then our results could reflect upper-government favoritism rather than a shift in leader accountability at the village level.

This is only a concern for our estimates of the effect of elections on policies that the upper government has control over (e.g., upper-government land expropriation, implementation of the One Child Policy, and the part of public goods funded by the upper government). To address this, we examine the effect of elections on policies that are under the *sole* discretion of the upper-level government. If the upper government systematically favored certain villages with advantageous policies after the introduction of elections, then one would expect this favoritism to also appear in such policies, which are arguably easier for the upper government to change as they can do so unilaterally. In addition, for public goods provision, our data will allow us to separately examine the funding for public goods from villagers versus upper levels of government. Therefore, we can directly observe whether elections increase funding of public goods from upper levels of government.

Finally, we also check that our estimates are robust to a large number of potentially relevant

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[32]This list is not presented in the paper for the sake of brevity. It is available upon request.

factors such as differences in election procedures or the introduction of the *Tax and Fee Reform*, among others. We discuss these in the section on robustness.

5 Data

Our study uses village level data from a panel of 217 villages for the years 1980-2005. These data are the result of merging two data sets. The first is a unique retrospective survey on the history of electoral reforms and policies that cover the years 1980-2005. The second is the *National Fixed-Point Survey* (NFS), an economic survey collected and maintained by the Ministry of Agriculture of China. It is a longitudinal village level and household level survey that began in the mid-1980s. The villages were chosen to be nationally representative at the time the survey began. The Data Appendix provides further details on both data sources.

For this study, we obtain the NFS village-level data for all 217 villages of our survey and household level data for 72 of the 217 villages.[33] This results in our having many fewer observations when we examine household outcomes.[34] The NFS data was collected annually in the period 1987-2005, with the exception of 1992 and 1994 when it was not collected for administrative issues.

Our data have several key advantages. First, they 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.[35] 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. Second, the NFS economic data is collected contemporaneously. This avoids measurement error that would arise from using retrospectively recalled data on non-momentous details from the past. Third, the panel structure of the survey allows us to control for village fixed effects and province-year trends. Finally, the richness of the data allows us to provide a detailed analysis of the effect of elections and assess the mechanisms driving the reduced form effects.

The data also have several drawbacks. The main one 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. As a consequence, some interesting variables that are only in the survey for very few years (e.g., obli-

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[33]The RCRE limited our access to disaggregated household level data to one third of the villages in the NFS. In order to maximize the sample size, we chose those villages with the largest number of households.

[34]The one exception to the latter is household income, which the Ministry of Agriculture agreed to tabulate for us for each decile of the within village household income distribution each year, for all villages and all years.

[35]For example, our survey records the year that villages implemented the *Household Responsibility reforms* (mid-1980s), which decollectivized agricultural production, and the *Tax and Fee Reforms* (early 2000s), which reduced village leaders' ability to tax villagers.

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gated working days, roads) are not examined.[36], [37] The second drawback is that the NFS, which is mainly an agricultural labor and production survey, did not collect detailed demographic data. Therefore, we cannot examine outcomes such as fertility, or explore the interaction of the effects of elections and changes in the village demographic structure. Finally, because we have only household level data for 72 villages, the results from using the household dataset cannot be interpreted as representative for all non-autonomous regions in China.

5.1 Descriptive Statistics and Preliminary Results

Village Demographics Table 1 contains the descriptive statistics for the villages in our dataset. All observations are at the village-year level. Panel A shows the means for the full sample. On average, villages have approximately 420 households. According to recent work by Tsai (2000), social networks such as clans and organizations affect the provision of public goods in rural China. Since they can also affect the conduct and quality of elections, we collected information on the clan structure of villages. The largest clan makes up half of the village population on average.[38] In addition to clan size, we are also interested in the wealth and historical prominence of the largest clans within the village. One way to measure this is to observe whether any clan in the village has a written family tree or an ancestral temple for traditional ancestral worship. These typically belong to the largest clan, and clans with ancestral temples are typically a strict subset of clans with written family trees.[39] In our sample, approximately 10% of villages contain families with written family trees and 3% contain families with ancestral temples.

Approximately 9% of our sample are in suburbs of cities. 4% experience land expropriations by upper levels of government. 50% allow parents to have a second child when the first is a girl. The average standard deviation of this variable within a province and year is 0.37, showing that there is substantial regional and temporal variation in implementation. This supports our claim that higher levels of government allow local governments to have discretion over the implementation of family planning policies.

The average investment in village public goods is 91,795 RMB per year. Of this, approximately 62,700 RMB is funded by villagers and 30,290 RMB is funded by upper levels of government. This shows that raising village funding for public goods is an important task of the village government.

A village has 9,130 mu of land on average.[40] We will use the native unit of measurement for convenience because Chinese households typically have very little land per household. Approximately a fourth of village land is arable, 2,274 mu. Most of the arable land, 96%, is allocated to household

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[36]Even for variables that are collected consistently for most years, we find that the number of observations vary across variables. In particular, there are variables that are missing for some years. However, this should not be a problem for our analysis because we find no evidence of selection bias across villages – if a variable is collected in a given year, it is collected for all villages and households.

[37]There are a few variables that are reported as missing for only a couple of villages in a year. As far as we know, this is due to random errors that occur during the survey or data entry.

[38]The number of observations is smaller because villages with no dominant clan are recorded with missing values.

[39]Tsai (2000) also uses the presence of these objects as a proxy for the presence of informal institutions (i.e., they are assumed to be positively correlated with social capital).

[40]1mu is equivalent to 1/15 hectares.

farming. The remaining land is, on average, equally divided between collective management and leased out to village enterprises. In our analysis, we only refer to arable land.

Next, we examine the descriptive statistics for income. For the full sample of 217 villages, the median household earns approximately 10,512 RMB per year of gross income.[41] The median household income of each village grows at 9.2% per annum on average. This is similar to the national average household income growth rate for China during this period. Note that the large standard deviation reflects the widening inequality across regions. The ratio of the 10th percentile household to the median household income and the median household to the 90th percentile household income is approximately 0.5 in each village. Income inequality within village is therefore small and roughly symmetric above and below the median.

Panel B shows the descriptive statistics for select variables from the subsample of villages for which we have household level data. These villages are slightly larger in terms of the number of households and have similar median income and income growth as the full sample of villages. One of the facts that emerges from the income data is the importance of agriculture and land, which makes up almost two-thirds of gross household income on average.[42]

One of the most important variables from this subsample is the amount of land allocated to each household. The data highlight how little farmland rural Chinese households have. The median household in a village has approximately 1.26 mu on average.[43] There is inequality in land holding across households within a village, which may partly be driven by differences in household size. Since the NFS does not report household size for many years, we do not adjust for it in our regression analysis. The household subsample also allows us to examine the amount of fees and levies paid by households to the village and county governments, which amounts to 320 RMB per year on average. Unfortunately, the NFS does not separately report fees paid to the village government from fees to the county government. Appendix Table A1 panel A shows a more detailed account of household income and expenditure. Aside from production costs and food and housing consumption, the main expenses are the payment of fees and school tuition. This is consistent with past studies that argue that household fees are a contentious issue within the village (e.g., Luo, Zhang, Huang, and Rozelle, 2007; Zhang, Fan, Zhang, and Huang, 2004).

Village Government Structure Panel C shows the descriptive statistics for the village government. On average, there are four village committee members (including the VC) and seven party committee members in the village party branch (including the PS). The two leaders have comparable educational attainment. We only have data for VCs that were in office before the first election

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[41]This is not adjusted for inflation. In the regression analysis, price changes are largely absorbed in the year fixed effects and province-time trends.

[42]We report the decomposition of household incomes and expenditures in a web appendix.

[43]Note that the average household land allocation in the 72 village subsample is less than for the full 217 village sample because our subsample contains the villages with the largest number of households, which are also likely to be the most densely populated.

for fourteen villages.[44] Within this small subsample, we observe that 60% of the first elections resulted in a change in the VC, showing that the initial elections resulted in substantial turnover. If we examine all elections (and not just the first), we find that 46% result in a change of VC, which shows that elections continued to generate leader turnover.[45]

In Table 2, we document the power structure of the government by asking whether the VC or the PS had the unilateral power to make several important decisions, or if both consent was necessary to reach a decision. Our survey question is phrased to reflect the *de facto* rules rather than the *de jure* rules.[46]

Table 2 shows that there is substantial variation in the allocation of power and that VCs have *de facto* power. VCs have unilateral powers to appoint managers of village enterprises in 32% of the sample, to employ village-government employees in 27% of the sample, to reimburse expenses in 56% of the sample, to reallocate land in 33% of the sample and to make large public investments in 18% of the sample. Adding up the cases in which VCs have unilateral and joint power shows that their consent is needed for important decisions in about 70-90% of the sample.[47]

The Timing of the First Elections   Panel D of Table 1 presents important statistics on the timing of elections. It shows that the average village had held its first election by 1988, its first competitive election by 1991, and its first election with open nominations by 1997. Note that for the latter two variables, there are fewer observations. This reflects the fact that not all villages had introduced these policies by the end of our study period.

To investigate whether the data supports the belief that villages had little discretion in the timing of elections and mostly followed upper government directives, we examine the timing of the introduction of elections in each village relative to the introduction in the same county and same province. For county-level introductions, we can rely on our survey, which asked each village to recall when the first elections were held in the same county (excluding the respondent village).[48] As provinces are large and many villages will not know the first election in its province, we did not include such a question on the survey. Instead, we use the year of the first election in a village from our sample for each province as the first year that a province introduced the election. The following statistics should be interpreted with these particulars in mind.

We find that villages on average held their first elections within five years of the first election

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[44]The surveyors collected data on the VCs and PSs for all villages and years after the first election was introduced. For sixteen villages, the surveyors also collected these data for all the years before the first election.

[45]Excluding the first election, we have information about 1,043 electoral contests. Among those 477 lead to a change of VC. This contrasts to 229 changes in the PS over the same period.

[46]For each year, respondents check boxes for whether the signature of the VC, PS or both are needed to implement each policy. Not all policies are relevant to each village and time period. In case of an irrelevant policy, the variable is recorded as missing. Therefore, the sample size varies across outcomes. Note that the land allocation question specifically refers to land that is held collectively by the village and allocated at the sole discretion of the village government (e.g., *jidongdi*). This is important because our empirical analysis of the effect of elections on village land allocation only makes sense if the VC has power over land allocation.

[47]We report the accounting balance sheet for the village government in the web appendix.

[48]13 villages were unable to recall the year of the first election in their county. Therefore, our sample size for this variables is slightly reduced.

in the same province. In fact, over 60% of villages within a province introduced elections within three years of the first election in that province (not shown in the table). The timing coincides even more between the first election in a village and the first election within the same county. There is almost no discrepancy in timing on average. The negative sign of the mean difference between village and county timing is caused by the fact that 15% of the villages in our sample were the first to hold elections in their county. Similarly, approximately 16% of villages held their first election after the county.

Two very important facts emerge from these statistics. First, the closeness in timing between the first election in a village and the first in the same county supports the belief that the county government was the level that administered the reform on the villages, and that the villages had very little discretion regarding the timing of adoption. This is important for our main empirical strategy which assumes that the introduction of elections is not determined endogenously by villages. Second, the fact that our sample contains villages that held their first elections before, at the same time as, and after the first election held in the same county shows that our villages are not systematically the first in their counties to introduce elections. This is important evidence for our claim that county governments did not systematically introduce elections based on the characteristics of the villages in our sample. Recall from the previous section that this claim needs to be true for the robustness check where we instrument for the introduction of elections in a village with the introduction of the first election in the same county.

For the empirical analysis, it is important to note that by the end of our sample, all villages had introduced elections and that this introduction was irreversible (the same was true for the introduction of open nominations).[49] Therefore, taken literally, our empirical results reveal the effect of introducing elections earlier rather than later.

Electoral Procedures Panel D of Table 1 also shows that there is substantial heterogeneity in electoral procedures. Approximately 84% of elections have anonymous voting, 72% allow voting by proxy, and 65% use a roving ballot box. Only 79% of elections are competitive in that they have more than one candidate per position. These facts are consistent with anecdotal observations that many elections are procedurally unsound.

In addition to the statistics shown in the table, we also investigate whether the beliefs that elections provide villagers with a formal mechanism to voice discontent and recall unsatisfactory VCs are supported by the data. These are important for understanding how non-competitive elections can affect outcomes (recall the discussion from section 3). Although 80% of elections occurred exactly three years after the previous one (as stipulated by law), over 10% of elections follow the previous one within one or two years. Interestingly, all of these high-frequency elections occurred immediately after the initial introduction of elections. Moreover, they typically occurred after elections that were not competitive. The fact that non-competitive elections were systematically

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[49]A detailed timeline of the electoral reforms is reported on the web appendix.

followed by additional elections before the term's end is consistent with the qualitative accounts of dissatisfied villagers demanding and obtaining recalls provided by O'Brien and Li (2006) and the belief that the introduction of elections shifted accountability of village government towards villagers even when the elections were very imperfect.

6 Results

6.1 Main Results

Timing   Before we present the main results from the baseline specification, we first estimate the effect of the introduction of elections for each year before and after the first election. Our baseline pre-election versus post-election comparison captures all differences between these two periods, regardless of when exactly the difference arises. A yearly estimate enables us to verify that the effects of elections captured by the baseline estimates, actually occur at the time of the introduction of the first election, or closely after. The equation is similar to the baseline except that we introduce dummy variables for the number of years since the first election.[50] For this estimation, the outcomes we examine are limited to those that have sufficient variation within each year.[51] We only extend the pre-election analysis to four years prior to the first election to avoid losing too many villages that introduced elections early. Similarly, we examine up to six years after the first election to avoid losing villages that introduced elections towards the end of our sample.[52]

Figure 1 plots the estimated coefficients for the correlation between the dummy variable for each year before and after the first election with the power of the VC and PS to unilaterally appoint village enterprise managers. The plotted coefficients show that there is little change in the three years prior to the first election. However, elections immediately increased the likelihood that VCs would have unilateral power instead of the party secretary. The same pattern exists for all other of the decision power measures.[53] This evidence is consistent with elections increasing the power of VCs and that our baseline estimation does not capture spurious trends.

Figure 2 shows the estimated effects for whether One Child Policy exemptions were granted in a village. As with village leader powers, we see no effect in the years leading up to the introduction of the first election but a positive correlation in the years afterwards. We repeat this exercise for land holdings of the median household. Because this variable is only reported from 1987 onwards, we can only extend the analysis to two years prior to the first election without losing too many

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[50]$Y_{vpt} = \sum_{\tau=-3}^{6} \beta_\tau \bullet 1(Year_t = \tau_{vp}) + \theta CompElection_{vpt} + \lambda OpenNom_{vpt} + \gamma_{pt} + \delta_v + \rho_t + \varepsilon_{vpt}$.

τ denotes the number of years since the first election is introduced in village v of province p. Therefore, the outcome of village v in province p during calendar year t is a function of: the number of years since the first election is introduced, whether the first competitive election and the first open nomination have been introduced, province year trends, village fixed effects, and calendar year fixed effects. The dummy for four years before the first election, $\tau = -4$, is the reference group. All standard errors are clustered at the village level. If elections had an impact, then $\hat{\beta}_\tau \approx 0$ for $\tau \leq 0$ and $\hat{\beta}_\tau \neq 0$ for $\tau > 0$.

[51]Therefore, we cannot examine upper-government land expropriation and public goods investments, which occur rarely for each village (e.g., only 3.6% and 9% of our sample experience upper-government land expropriation and make public investments, respectively).

[52]We code all the years that are four or more years before the first election as four years before the election, and all the years that are six years or more after the first election as six years after the election.

[53]The estimated coefficients for all leader powers are shown in Appendix Table A1. We only plot one of the outcomes due to space constraints.

villages from the estimating sample. The estimated coefficients are plotted in Figure 3. It shows no change between one and two years before the introduction of elections. After the introduction, villages gradually allocate more land to the median household. The estimated coefficients and standard errors for the two policy outcomes are shown in Appendix Table A1.

These results illustrate that the effects of elections occur at the time of the first election or shortly after. This partly alleviates the concern that our results are driven by spurious changes taking place before or after the introduction of elections. These estimates also address the concern that local governments anticipated the introduction of elections and behaved differently immediately before the first election such that our main estimates will partly reflect the anticipation effect. We find no evidence of changes in the correlations in the years leading up to the first election. That the last appointed VC did not react to the upcoming elections is consistent with the fact that elections were implemented in a top-down fashion and, in many cases, were only announced a few weeks in advance (Unger, 2002: p. 221).

The Effect on Leader Powers   Next, we examine the effects of elections on *de facto* leader powers. The estimates for the baseline equation (1) are shown in Table 3. Recall that since the introductions of elections, competitive elections, and open nominations are never reversed, our coefficients capture the effect for all of the years after the initial introduction. The estimates in rows (1)-(5) show that elections increase the unilateral power of the VC across all of the surveyed village powers. With the exception of the power to employ village staff, they are significant at the 10%, 5% and 1% levels. This is paralleled by a decrease in the unilateral powers of the PS shown in rows (11)-(15), which are also statistically significant at the 5% and 1% levels with the exception of the power to employ staff. The estimates in this table also show that elections have no effect on the incidence of the PSs and VCs holding powers jointly. Therefore, these results show that elections increase the *de facto* power of the elected official.

Interestingly, the subsequent introductions of competitive elections and open nominations do not have additional effects. The estimates are mostly small in magnitude and statistically insignificant. Note that the estimated magnitude for the introduction of the first election is similar if we do not control for the subsequent introductions of competitive elections and open nominations. However, not controlling for this additional heterogeneity causes the election estimates to be less precise. Therefore, we include these controls in all of our specifications.[54]

Effect on Upper Government Policy   Recall from our earlier discussion that an increase in leader accountability towards villagers (due to the introduction of elections) has different effects across policy types. Specifically, it depends on whether the village government has any discretion over the policy and the congruence between the preferences of the upper government and villagers. Our simple model predicts that if elections increase accountability towards the villagers,

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[54]The estimates without controlling for competitive elections and open nominations are not reported in the paper for the sake of brevity. They are available upon request.

then upper-government policies that are unpopular with villagers will shift towards the preferences of villagers. It also predicts that elections should have no effect on policies over which the village government has no discretion. We examine these predictions in Table 4.

The two unpopular polices that we examine are the One Child Policy and upper-government land expropriation (recall the discussion from section 2). Column (1) shows the estimates of the effect of elections on the implementation of One Child Policy exemptions. Elections increase the incidence of relaxations in the One Child Policy by 8.2 percentage-points. Since approximately 50% of our observations allow the exemption, this represents a 16% increase in the likelihood of exemptions. The estimate is statistically significant at the 1% level. Column (2) examines the effects of elections on the probability of village land being expropriated by upper-level governments. As we can see, this probability is reduced by 1.7 percentage-points once elections are introduced. However, the point estimate is only statistically significant at the 15% level. The lack of precision most likely reflects the fact that there is very little variation in this measure because land expropriation rarely occurs; only 3.6% of our sample experiences land expropriation by the upper government. This also means that the estimated magnitude implies a large effect and that elections approximately reduce the average incidence of land expropriation by 49%.

In contrast, we find that elections have no effect on policies that the village government has little discretion over (e.g., the placebo outcomes). Columns (3) and (4) show that elections have no effect on the amount of public investment from special aid funds or distance to the nearest highschool.

These results are consistent with the predictions of our model when elections increase leader accountability. Note that to interpret these results, it is not necessary to assume that village leaders have much discretion over the unpopular policies or that they have absolutely no discretion over special aid or distance to the nearest highschool (e.g., in principle, village leaders could lobby the upper government). Rather, we only need to assume that village governments have *less* discretion over the latter two outcomes *relative* to the unpopular policies for these results to be consistent with elections increasing accountability.

As with the results on leader powers, we find that the introduction of open nominations and competitive nominations have little additional effect.

The finding that elections have no effect on upper-government policies that the upper government has the most discretion over is also important for another reason. It goes against an alternative explanation for our results which hypothesizes that the effect of elections on unpopular upper-government policies is driven by the upper government systematically favoring villages that introduce elections rather than by a shift in village leader accountability caused by the elections themselves.[55] The results below provide further evidence against this alternative explanation.

The Effect on Public Goods Investment and Village Land Allocation   Next, we analyze the effect of elections on public goods provision and within village land allocation. These polices cor-

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[55]We also examine other outcomes that are very difficult for the village leader to affect, and find that elections have no effect on these outcomes. They are not reported in the paper for brevity. They are available upon request.

respond to the type of policies in our model for which villagers and the upper government share similar preferences, but that imply a reduction of the utility of the village leader – either because they require some active effort from village leaders, or because they reduce rent-seeking opportunities. Our model predicts that an increase in accountability of the village government shifts these policies towards the preferences of the villagers only if villagers are better than the upper government at monitoring and restraining village officials.

Table 5 shows the results on the effect of elections on public investment according to the sources of funding. For the sake of brevity, we do not report the estimates for post-first competitive election and post-first open nomination.[56] Panel A shows the results for total public investment for all public goods. Column (1) shows that elections increase total public investment from all sources by approximately 17.5%. However, it is only significant at the 15% level. A comparison of the magnitude of the coefficients in column (1) and those in columns (2)-(7) suggest that the aggregate increase is driven by an increase in funding from villagers. However, these estimates are not statistically significant. The imprecision is very likely due to heterogeneity in the demand for public goods.

To address this issue, we predict the demand for public goods with village characteristics. Our data allows us to do this for two public goods: irrigation and schooling. Presumably, villagers living in villages where there is more household farming will demand more irrigation and those who live in villages with more children will have higher demand for schools. In order to examine the effect of elections on irrigation investment, we estimate an equation similar to the baseline equation, except that we add the interaction terms of the introduction of elections, competitive elections, and open nominations with the average log amount of village land used for household farming. Similarly, to examine the effect on schooling investment, we add interactions the three policy variables with the average number of children of ages 7-13 in a village.[57]

Panel B in Table 5 shows the effects on irrigation investment. The estimate for the main effect of post first election in column (1) shows that elections reduce public investment in irrigation for villages with no household farmland. However, the interaction effect between elections and average log household farmland is positive. Both the main and interaction effects are statistically significant at the 1% level. Taken literally, the estimates show that elections increase public investment in irrigation for villages that have 6.5 or more log mu of land (e.g., the sample mean is 7.23 log mu). The elasticity between household farm land and the effect of elections on public investment in irrigation is 0.37.

In Panel C, we examine the effect on public investment in schooling. Column (1) shows that elections have no effect on public investment in schooling for villages with no children. In fact, the sign of the estimate is negative. However, the introduction of elections increases public investment

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[56]They are available upon request.

[57]Recall that for the full sample of villages, we only have land data at the village level. Therefore, for each village, we calculate the average (over time) of the total amount of village land allocated towards household farming.

for villages with more children. The interaction term is statistically significant at the 1% level. Note that all of the coefficients for schooling are scaled by 1,000 for ease of presentation. Therefore, the results show that elections increase public investment from villagers in schooling for the average village, which has approximately 234 school age children, by 0.4% ($234 \times 0.0154/1000$) more than a village with no children. This is a small effect. However, the result is consistent with the observation that elections increase the provision in public goods for which there is demand. As with the investment on irrigation, we find that all of the effect of elections is driven by funding from villagers. The estimates for the effect of elections and the interaction effect of elections and the number of children on funding from other sources are much smaller in magnitude and statistically insignificant.

The estimates in Panels B and C provide strong evidence that elections increase appropriate provision of public goods. Furthermore, a comparison of columns (2) to columns (3)-(7) shows that the effects of elections on public investment are entirely driven by increases in funding from villagers. The estimated main effects and interaction effects for investment funded by upper-government special aid, other upper-government sources, other sources, the sum of the first two sources, and the sum of all three are statistically insignificant and much smaller in magnitude relative to investment funded by villagers. These results are important because they are at odds with the alternative explanation that our results are driven by the upper government favoring villages that introduce elections rather than by a shift in village leader accountability caused by the elections.[58] To fund more public investment, villagers need to pay higher fees. Using the subsample with household data, we indeed find that elections increase the total amount of fees paid to the village and county for all households across the income distribution. These results are shown in Appendix Table A2.[59]

Table 6 shows the estimates for the effects of elections on within-village land allocation, which is one of the most important and contentious policies in the village. It is also almost exclusively under the discretion of the village government. Columns (1) to (9) show the estimated effects of elections on household land allocation for each decile of the within-village household land distribution. The estimated coefficients are positive for the entire distribution. However, they are larger in magnitude and more precisely estimated for households near the median. Columns (4)-(6) show that the introduction of elections increase household farm land by approximately 20-28% for the 40th, 50th and 60th percentile households. They are statistically significant at the 10%, 1% and 10% levels.[60]

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[58]An increase in accountability for village leaders can result in an increase in the village leader's lobbying effort to obtain upper-government resources. Hence, finding an effect on funds from the upper-level government will leave open the possibility that part of our estimated effect is driven by upper-government policy changes but will not rule out the possibility that there is an effect on village leader accountability. However, finding that elections increase funding for public goods from villagers but not the upper government allows us to conclude that our baseline estimates for public goods are not driven by upper level policy changes.

[59]Recall that the fees data reflects fees paid to village and county governments. Therefore, these results must be interpreted cautiously as only suggestive evidence.

[60]For land allocation and for all outcomes that we examine by quantile, we estimate the distribution for each year. Therefore, a given household which is at the 10th percentile in year X, might not be on the 10th percentile in year Y. For robustness, we check that our results are similar if we use a time-invariant definition. As the results are very similar (because there is little change in

The finding that land allocation does not decrease for any households suggests that the increase in land is made possible by a reduction in land collectively held and managed by the village government. Recall from the section on Background that this land is often leased to enterprises and is a source of rents for corrupt village officials. Column (10) shows that the increase of household land due to elections is paralleled by a reduction in the amount of land that is leased out to firms. Elections reduce land leased to firms by approximately 68%. The estimate is statistically significant at the 10% level.[61]

Note that the results on village land allocation are consistent with our finding that elections increase the unilateral powers of the VC to reallocate collectively held land. Furthermore, consistent with this land reallocation, we find suggestive evidence that elections reduce household income from village enterprises (e.g., dividends) and income from wages, which are often earned from village enterprises. See Appendix Table A3.[62]

As with all of the earlier results, we find that the introduction of open nominations and competitive elections have no additional effects. The estimated magnitudes in Table 6 are small in magnitude and statistically insignificant.

Under the assumption that the upper government shares the preferences of the villagers for village public goods investment and household land allocation, and that implementing these policies reduce the utilities of village leaders, our findings are consistent with elections making the village government more accountable to villagers. In the context of our conceptual framework, these results imply that villagers are better than upper levels of government at monitoring and restraining village leaders. Moreover, the finding that elections only increase land allocation for some households within the village is consistent with an auxiliary prediction from our model: an increase in accountability only increases the provision of excludable goods for as many households as the VC needs in terms of votes for remaining in office.[63]

6.2 Robustness

Endogeneity of Election Timing The main concern for our empirical strategy emerges from the possibility that the timing of the introduction of elections is endogenous to village characteristics. To check that our main results are robust to this concern, we instrument for the first election in a village with the year of the first election in the same county. This strategy addresses the potential endogeneity of the timing of village elections to both observable and unobservable characteristics. It requires that the county government's decision to introduce elections does not systematically de-

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the relative positions of households on the village distribution of land or income), we only report the time-varying definition in the paper. The other results are available upon request.

[61]Note that the sample size is smaller for this estimate because we restrict the sample to the villages that have ever leased land to factories in our sample period, and should therefore not be interpreted as representative for our entire sample.

[62]Appendix Table A3 shows that elections reduced the incomes of the richest households relative to the poorest. Moreover, this reduction is driven by a reduction in income from village enterprises and income from wages (earned in village enterprises). These findings are inconclusive because they are imprecise. However, as suggestive evidence, they are consistent with the closing or downsizing of factories that would result from the reallocation of land from these entities to village household farming.

[63]The derivation of this auxiliary result is not shown in the Appendix for the sake of brevity. It is available upon request.

pend on characteristics of the villages in our sample. Recall that our villages are randomly sampled from all the villages in a county and that the descriptive statistics show that 30% of the villages in our sample held their first elections before or after the first election in the same county. Therefore, even if county governments decide their first election based on the characteristics of some villages, it is unlikely that this decision is systematically based on the villages in our sample. Note that we only have enough variation for this estimate with our full sample variables.

The first stage and 2SLS estimates are shown in Table 7. Column (1) shows the first stage estimate when we instrument for post-first village election with post-first election in the same county. Columns (2) and (3) show the first stage for when we instrument for post-first village election and post-first village open nomination with post-first election and post-first open nomination in the same county.[64] The estimates show that village implementation is strongly positively correlated to county implementation. The estimated first stage effects for both elections and open nominations are positive and statistically significant at the 1% level. In columns (4) and (5), we estimate the 2SLS effect on upper-government land expropriation. We use two alternative specifications, one where we only instrument for the first election and a second where we instrument for both the first election and the first open nominations. The estimates are very similar between the two specifications. They show that elections reduce the incidence of land expropriation by five percentage-points and these results are statistically significant at the 10% level. The estimates are larger in magnitude than the baseline fixed effects estimate presented in Table 4. Columns (6) and (7) show that the same is true for the effect of elections on One Child Policy exemptions. The 2SLS estimates are larger in magnitude and statistically significant at the 1% level.

Columns (8)-(11) show analogous estimates for public investments. In these estimates, the interaction terms for post-first-election and post-first-open-nomination are instrumented with the interaction terms of the relevant village characteristics and the dummy variables for post-first-election in the same county and post-first-open-nomination in the same county.[65] The estimates for the interaction term of post-first-election and the number of school age children on public investment in schooling are similar in magnitude as our baseline fixed-effects estimates and statistically significant at the 1% level. The estimates for the interaction term of post-first-election and average log household farm land on public investment in irrigation are similar in magnitude as our baseline estimate but not statistically significant.

These estimates provide very strong evidence that our main results do not overstate the true effect of elections due to unobservable village characteristics. Moreover, they are consistent with the baseline estimates in showing that the introduction of open nominations and competitive elections

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[64]Our survey also asked each village to recall the first haixuan by any village in the same county. Note that we cannot instrument for the first competitive election. This is because official policy required all elections to be competitive from the beginning. Therefore, a survey question that asked villagers when the first competitive election was held in the same county would elicit an imprecise response. Similarly, we cannot infer the first competitive election within the same county from our survey since we often observe only one village per county.

[65]For the sake of brevity, we do not report the first stage regressions of the interaction effects. They are available upon request

had little additional effect over the introduction of elections. The estimates for these coefficients are much smaller in magnitude than the estimates for the introduction of elections and statistically insignificant.

In addition to the results above, we also directly examine the correlates of election timing in the data and add all statistically significant or near significant correlates as controls to the baseline specification. We find that our baseline results are extremely robust to these controls, and to a large set of other demographic and economic variables that may affect our estimates. See the Appendix for a detailed discussion and Appendix Table A4 for the results. Another concern related to heterogeneity is the possibility that villages that introduce elections before the OLVC in 1987 were the villages that benefited the most from elections. In that case, our estimates will overstate the true average effect of elections. To address this, we re-estimate our baseline regression on a sample restricted to villages that held their first election after 1987. The results are very similar to the baseline estimates (see Appendix Table A5).

Overall, the findings presented in this section provide strong evidence that our main results are not biased by endogenous timing of introduction of elections.

Additional Factors   In Table 8, we examine other factors which may influence the adoption or effectiveness of elections. We present the baseline estimates for comparison in column (1). First, we examine the influence of election procedures discussed in section 2: the presence of a roving ballot box, anonymous ballots and proxy ballots, which could all corrupt the election process. For example, a roving ballot can decrease the ability of citizens to monitor the ballot box. 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. Proxy voting enables a better representation of their preferences. We control for these electoral procedures in column (2). The estimates are very similar to the baseline for all outcomes.

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 their 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. As we can see in the table, our estimates are robust to this control.[66]

Next, we consider Tsai's (2007) argument that strong informal institutions 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

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[66]We also used as controls 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. The estimates are very similar to the baseline. They are not reported for the sake of brevity and are available upon request.

tree or an ancestral temple, to proxy for informal institutions (e.g., social capital). 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. Therefore, we conclude that our results show that formal elections do have an effect, even when we control for the presence of informal institutions.

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 would greatly curb the village government's ability to provide public goods. We investigate whether our baseline results, particularly those on public goods investment, are robust to controlling for the introduction of this reform. The estimates in column (5) show that our estimates are very similar to the baseline. Hence, our findings seem to be robust to controlling for this reform.[67]

In column (6), we control for whether a village had experienced a merger with another village. This is important for two reasons. First, it could be problematic for our data because our retrospective survey in 2006 allowed 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. A second 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. To address this, we asked villages in our survey to report the years they experienced mergers. Approximately 19% of our sample ever experienced a merger. We create a dummy variable indicating whether a village ever experienced a merger (since 1952), and control for the interaction of this dummy variable with year fixed effects. As we can see from column (6), the estimated effect of elections is very similar in magnitude to the baseline.[68]

Finally, in column (7), we control for all of the factors described above simultaneously. The estimates from this extremely conservative estimation are very similar in magnitude to the baseline.

In addition to the exercises above, we also test that our results are not 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.

In light of the results in this section, we conclude that our estimates are robust to controlling for several additional factors.

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[67] Our survey asked each village to report the introduction of the *Tax and Fee Reform* (and the *Household Responsbility System*). The data show that the timing of the introductions of these policies is uncorrelated with the timing of the introduction of elections. Hence, it is quite unlikely that our estimates are confounded by these other policies.

[68] 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 the sake of brevity. They are available upon request.

7 Conclusion

This study explores the effect of introducing local democratic reforms within the context of a regime that is widely recognized as authoritarian. We collect a large unique dataset on the introduction of village elections and the political economy of villages in rural China that spans 25 years to empirically assess the impact of such reforms. We find that the introduction of village elections shifts policies towards the preferences of villagers for those policy outcomes in which village leaders have discretion. These results provide very strong evidence that local elections in China shifted accountability of village leaders towards their constituents. We also find that elections have no effect on upper-government policies over which the village government has little or no discretion. This rules out the main competing hypothesis that the changes in policy outcomes are driven by systematic upper-government favoritism rather than by a shift in village-government accountability caused by elections.

Our results support theories which predict that leader accountability can significantly affect policy outcomes. In particular, they provide evidence of such predictions holding even when the larger regime is authoritarian and when the elections are imperfectly implemented. The fact that we find that the introduction of open nominations and competitive elections have no incremental effect relative to the introduction of elections is very interesting as it suggests that the increase in accountability is driven by stronger incentives rather than by better selection of leaders. In addition to the main results, the finding that the introduction of elections shifts policies in favor of villagers even for policies that villagers and the upper government are likely to have similar preferences supports the belief that constituents are better than the upper government at monitoring and restraining local leaders.

The findings of this study have several implications. For policymakers, our results indicate that making local officials more accountable to the population at large might have noticeable effects even if such changes are marginal, imperfectly implemented, and occur in a broader context of authoritarianism. This can be an important factor in the current debate on political institutional change because it suggests that wholesale reforms at all levels of a regime might not be necessary to obtain sizable gains for the population. It is unknown, of course, how much of this conclusion depends on the bureaucratic strength of the Chinese state and the high degree of social capital amongst the population, which may not be present in other cases. Understanding the preconditions necessary for the beneficial effects of incremental institutional change and the interaction between potentially relevant factors such as an effective bureaucracy, social capital, and political reform are important avenues for future research.

Our results confirm the belief that local democracy pose important trade-offs for the authoritarian central government. On the one hand, the enforcement of certain unpopular central policies is relaxed when appointed leaders are replaced by elected ones. On the other hand, elected village leaders seem to be able to better provide for the needs of villagers on average. This could be ben-

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eficial for the central government in the long run as it could decrease the probability of political unrest in the rural areas. Improved investment in productive factors such as irrigation and schooling can also increase income in the long run. Further research is needed to determine these longer term effects of electoral reforms.

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Appendix

Model

Our framework applies the insights from the political agency literature to the context of rural China. The introduction of elections shifts the situation of village leaders from being exclusively accountable to upper levels of government, to being accountable to both villagers and upper levels of government. We therefore need a framework that can capture three levels of players – the village government, the upper levels of government and villagers.[69]

Consider a model with a continuum of size 1 of villagers (V), one village leader (VC) and an upper-government superior (S). The VC decides on two types of policies. The first type contains policies that V and S disagree over. We call these policies Misaligned (*M*) policies and denote the VC's decisions on this type of policies by $m \in [\underline{m}, \overline{m}]$. The second type of policies include all policy dimensions that V and S agree on, but that imply a reduction of the utility of the VC to implement.[70]

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[69]As the broader theoretical literature on electoral accountability shows, elections are conducive to better policies for the citizens due to at least two channels. The first channel is the provision of incentives for the politician seeking re-election. The second channel is the selection of more competent or more congruent politicians. Our model focuses on the first channel because it is the most relevant for our context and for simplicity. Adding selection does not change the main results. For the sake of brevity, this extension is not shown in the paper. It is available upon request.

[70]For example, any policy that constrains the ability of village leaders to skim funds for themselves would belong to this type.

We call these policies Aligned (A) policies and we denote the VC's decisions by $a \in [\underline{a}, \bar{a}]$. [71]

We assume that the preferences of villager i are additively separable in these two sets of policies and given by $U_i^V = m + a + \varepsilon$, where ε captures other elements that affect V's utility. We assume that $\varepsilon \sim N(0, \sigma_\varepsilon^2)$. Aggregate utility in the village is therefore given by

$$U^V = \int U_i^V \, di = m + a + \varepsilon.$$

The S has a dual mandate. It is responsible for the welfare in the villages under its control and at the same time it is requested to make them comply with central government policies. We therefore have

$$U^T = U^V - c(m)$$

where $c(\cdot)$ is an increasing, convex and twice differentiable function. $c(m)$ captures the cost that S bears if there is a lax implementation of central government policies. This cost includes the reprimands from superiors and the negative career consequences on bureaucrats that are seen as failing to implement central directives. Because each township government is responsible for several dozen villages, where each has its own particular preferences and experiences some unique shocks, we assume that the township government cannot perfectly observe V's utility and instead only observes an informative signal. Denote this signal by $s = U^V + \theta$, where $\theta \sim N(0, \sigma_\theta^2)$ and is independently distributed from ε. When S takes the decision to retain or replace the village leader it can only use this noisy measure

$$\tilde{U}^T = s - c(m).$$

We assume that the VC values being in office and suffers a loss $l(a)$ associated with the implementation A policies, where $l(\cdot)$ is an increasing, convex and twice differentiable function. This loss captures both economic or social rents lost, or effort exerted in such policy implementation. We assume that the value of retaining the village leadership position is 1.[72]

The decision to keep or replace the VC as a function of the observed outcomes is made by S before elections are introduced, and by V and S after elections. We denote by μ the weight that V's opinion receives in this decision. Before elections are introduced, $\mu = 0$. As discussed in section 2, village elections were imperfectly implemented and the village CCP branch were not directly affected by the electoral reforms. To capture this reality, we assume that after elections are introduced, the township keeps deciding on the retention of village leaders with exogenous probability $1 - \mu$. In contrast, with probability μ, the village leader is re-elected according to villagers' votes. This uncertainty from the point of view of the village leader also captures the mutable nature of

In addition, this reduction in utility might also be due to the need for the village leader to exert effort which is costly. For example, villagers may want village leaders to exert more effort towards the provision of appropriate public goods.

[71]The results of the model do not change if the implementation of M policies also carry a cost component for the village leader, as would be the case when the VC lobbies S for the relaxation of an unpopular upper-government policy. To keep the conceptual distinction clean between the two locus of conflict, in what follows we ignore the leader's costs in M policies.

[72]Note that this is more than a normalization. The value of holding office might be different across regimes. However, the sign of this difference is far from obvious. If democracy is better at reining in corruption, the value of office will be lower. At the same time, however, the legitimacy that being democratically elected confers might increase the ego-rent component of the value of holding office.

elections in the first few years of implementation, which are the focus of our analysis. A village with higher μ is thus one in which the will of the villagers has stronger influence on re-election decisions.

In keeping with the theoretical literature, we consider threshold strategies k' and k'' for villagers and township respectively such that at the time of deciding, they retain the village leader if their satisfaction is above the threshold.[73] Facing these strategies, the VC maximizes

$$\max_{a,m} \mu \Pr(U_i^V \geq k') + (1-\mu)\Pr(U^T \geq k'') - l(a). \tag{2}$$

The following proposition describes how policies change as a result in a shift in accountability.

Proposition 1 *As μ increases, implemented policies react as follows:*

1. m^* unambiguously increases

2. a^* increases if and only if $\sigma_\theta^2 > 0$. [74]

The intuition for these comparative statics is straightforward. As accountability shifts from the S to the V, the preferences of the latter gain more weight in the re-election decision. It is therefore natural that the VC moves M policies in favor of V. The effect on A policies is more nuanced because these are policies for which the S and the V agree. It is therefore not obvious that shifting accountability should affect policy decisions. In the context of the model, such an effect is only present if V are better than S at monitoring the VC. In particular, if S observes the utility of the V perfectly, $\sigma_\theta^2 = 0$, changing the direction of accountability does not make a difference. Needless to say, policies that the village leader has no control over should not change as a result of elections.[75]

Data Appendix

Our first source of data is a unique survey collected by the authors. We implemented a retrospective survey of the political reform histories of 217 villages from 1980-2005. Our surveyors gathered all current and former village leaders and respected persons (e.g., teachers) together and

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[73]See Alesina and Tabellini (2007) for a recent example and Persson and Tabellini (2000) for a textbook treatment of this type of models.

[74]**Proof.** [Proof of Proposition 1] The first order conditions of the program are

$$\mu f\left(k'-a-m;\sigma_\varepsilon^2\right) + (1-\mu)\left(1-c'(m)\right)f\left(k''-a-m+c(m);\sigma_\varepsilon^2+\sigma_\theta^2\right) = 0$$

$$\mu f\left(k'-a-m;\sigma_\varepsilon^2\right) + (1-\mu)\frac{\partial u}{\partial b}f\left(k''-a-m+c(m);\sigma_\varepsilon^2+\sigma_\theta^2\right) = l'(a).$$

V and S want to maximize their effective weight in the decision taken by the VC. In the face of these reaction functions, the optimal cutoffs for villagers and township are $k' = a^* + m^*$ and $k'' = a^* + m^* - c(m^*)$, where a^* and m^* are the equilibrium policies. Hence, the first order conditions can be rewritten as

$$\mu\frac{1}{\sqrt{2\pi}}\frac{1}{\sigma_\varepsilon} + (1-\mu)\left(1-c'(m^*)\right)\frac{1}{\sqrt{2\pi}}\frac{1}{\sqrt{\sigma_\varepsilon^2+\sigma_\theta^2}} = 0 \tag{3}$$

$$\mu\frac{1}{\sqrt{2\pi}}\frac{1}{\sigma_\varepsilon} + (1-\mu)\frac{1}{\sqrt{2\pi}}\frac{1}{\sqrt{\sigma_\varepsilon^2+\sigma_\theta^2}} = l'(a^*). \tag{4}$$

An examination of these conditions establishes the comparative statics stated in the proposition. ∎

[75]While the model can accommodate several alternative results, it is falsifiable. In particular, the model is not consistent with finding an effect of elections on policies of type A and no effect on policies of type M.

asked them to collectively recall the histories of reforms.[76] In general, there was little problem in recalling and forming a consensus on the dates in which elections and other reforms were introduced since these were very important events in the lives of villagers. Moreover, in most villages, surveyors could access administrative records which corroborated villagers recalled dates. Similarly, history of public investments, land expropriation, and exemptions from the One Child Policy were easy to remember for villagers since these were very contentious issues for them.[77]

The second source of data is the NFS, which has been maintained as a panel of villages and households since 1987. The survey used a stratified sampling approach. For each province, it first randomly selects a number of counties, and then randomly selects a number of villages within each county. Households are then randomly selected from each village. For the household surveys, approximately 100 households are sampled to be representative at the village level. Overtime, villages and households were added to the sample so that the NFS remained representative. There is little attrition according to the RCRE. Villages are surveyed every quarter and households are surveyed monthly (and sometimes with higher frequency). Only the annual aggregate data are kept and made available to researchers.[78]

Income and Consumption Results

In our context, the effect of elections on income is ambiguous. On the one hand, the increase in household land allocation and better provision of public goods can translate into higher income. On the other hand, the reduction in village government controlled land which is often leased to factories may decrease villagers' incomes from enterprises. This is typically earned as wages (for villagers who work for the enterprises) or as dividends (since, in principle, villagers own equal shares of all village assets, including enterprises) (Che and Qian). Since one may suspect that villagers benefit differentially from village enterprises based on their income and education level – e.g., richer households benefit more either because they are able to contribute more in the production of the enterprises or because they are better able to capture the profits, we examine the effect of elections on income for the entire within-village income distribution.

Appendix Table A3 rows (1)-(6) show the estimates from our baseline equation for the effect of elections on log total household gross income by income quantile. We find no effect on household

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[76]For personal characteristics of the village chief and the village party secretary, we asked for age, sex, level of education, whether he/she belonged to a family that owned land before the communist land reforms in the early 1950s, whether that individual was persecuted during the Cultural Revolution, *pidou*. For village leader power, we asked them to check a box indicating if the village chief, secretary or accountant's signature was necessary for employing village personnel, or spending money from village funds. We also ask the villagers to recall the method of the election (e.g., anonymous ballot). Documentation for these data is available on the websites of the authors of this paper http://www.econ.yale.edu/~nq3/NANCYS_Yale_Website/Surveys.html.

[77]Our information regarding the implementation of the One Child Policy was collected with special care. Since not enforcing an upper-government policy is illegal, we carefully worded our question such that the respondents would not feel wary and answer truthfully. We asked whether all parents in a village in a given year were allowed to have a second child if the first was a girl. There was little difficulty for the villagers to recall the policy for each year as it is one of the most contentious policies in villages and villagers can recall with accuracy the policy around the years when they were planning to have children or grandchildren. In this sense, it was a great advantage for our survey to have a large group of villagers collectively recall histories. However, the fact that our survey question on the One Child Policy does not explicitly reveal whether the policy in practice was decided by village governments should be kept in mind as a caveat when interpreting the results on this outcome.

[78]For a detailed description of the NFS, please see http://www.econ.yale.edu/~nq3/NANCYS_Yale_Website/Research_files/Description%20of%20th

total income. The estimates are small in magnitude and statistically insignificant. The estimates in rows (7)-(12) suggest that elections reduce income from enterprises from the richest households. But these estimates are not statistically significant. Rows (13)-(18) show that elections reduce wage income for richer households. However, they are also statistically insignificant.[79] The results presented here are not conclusive due to the lack of precision. But the patterns of the coefficients are consistent with our finding that elections shift land from being leased to factories to being farmed by households.

Additional Robustness Controls

In section 6, we showed that our main results are not biased upwards (in magnitude) by the endogeneity of the introduction of election to village characteristics by instrumenting for the first election in a village with the first election in the same county. This addresses endogeneity to both unobservable and observable factors at the village level. For the latter, we can also use the data to reveal the correlates of village election timing and control for them directly in the regression.

We examine the correlates of first election timing by collapsing our data to a village level cross-section and regressing the year of the first election in a given year on the years that the province and county governments officially introduce elections, among other factors. We find very few village variables that are significantly correlated with the timing of elections, which is consistent with the decision residing at the upper levels of government and having little to do on average with the characteristics of our villages. We find that conditional on the timing of the first elections in the same province and county, the timing of the first village election is positively correlated to median household land holding, the presence of lineage groups (e.g., ancestral temples), and negatively correlated to the proximity to cities. Conditional on timing at the province and county level, village election timing is notably not correlated to median household income, household income growth (for any part of the income distribution), income inequality and the amount of fees households pay on average. However, one may wish to check that our main estimates are robust to controlling for these factors as they may affect the effect of elections.[80]

In Appendix Table A4, we present the baseline estimates for each of our outcomes and then add controls for all of the factors mentioned above. In addition, we also check that our estimates are robust to the average tendency of upper-government land expropriation, One Child Policy exemptions, fertility rate, and average labor ratio. Since these village averages are time-invariant, we control for their interactions with the full set of time dummies. Similarly, to address the concern that the time varying variables may be outcomes of elections, we calculate the average for the

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[79]Note that we only examine enterprise and wage incomes for households in the upper income percentiles because poor households in our sample do not earn income from theses sources. To address the possibility that elections cause rich households to fear expropriation and thereby to under-report their incomes, we repeat the estimation with total household consumption as the dependent variable. Rows (19-23) show that we find a similar pattern for consumption across income quantiles. The magnitude of the estimates are very similar to those for the reduction in total household income. However, the estimates are not statistically significant. We interpret these results as suggestive evidence that our income data does not suffer from systematic under-reporting by the rich after elections were introduced.

[80]Because of space constraints, we do not present these correlates in a table. They are available upon request.

village and interact it with the full set of year dummies.[81] Note that some of this additional controls are only available for the 72 village sample, for which we have household level data. Hence, we will not be able to conduct those robustness checks for irrigation and schooling public investments, since they occur relatively infrequently, and consequently there is not enough variation for estimation. We find that our estimates are extremely robust to any of these additional controls. The estimates only change in magnitude when we move from using the full sample to the restricted 72 village sample.[82] In column (15), we control for all of the factors mentioned above in one regression. For ease of comparison with the baseline, we only control for variables that are available for all villages for the outcomes in panels C-G (e.g., government land expropriation, One Child Policy Exemption, and total public investment, public investment in irrigation and schooling). For similar reasons, we do not include controls for average fertility for any of the estimations in column (15).[83] The estimated magnitudes in this rigorous specification are very similar to the baseline.

These results show that our main results are extremely robust to a large set of controls. Consistent with the 2SLS estimates we present in the main text, these results show that our baseline estimates are very unlikely to be confounded by observable factors that are correlated both with election timing and with the outcomes we examine.[84]

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[81]The robustness controls include average median household land allocation x year FE, average village household land allocation inequality x year FE, average village total land for household farming x year FE, average village land leased to firms x year FE, average incidence of upper-government land expropriation x year FE, dummy for city suburb x year FE, average household fees paid x year FE, average fertility rate of village x year FE, average percentage of laborers x year FE, average incidence of One Child Policy exemptions x year FE, average median household income x year FE, average household income inequality x year FE, and average household income growth x year FE.

[82]We find a similar change in magnitude when we estimate the baseline equation using the restricted sample without the additional robustness controls.

[83]The omission of the control for average fertility interacted with year fixed effects does not change the estimates. For government expropriation of land and One Child Policy exemptions, including the land and average fee controls reduce the sample size significantly, causes the magnitude to be larger, and does not affect the precision of the estimate. When we include these controls and reduce the sample size for estimates on specific public goods investment, the estimate for irrigation is similar in magnitude but no longer statistically significant and the estimate for schooling investment is larger in magnitude but statistically insignificant. These results are available upon request.

[84]Note that we also checked that our results (in particular, the estimate for public investment in schooling) are robust to the inclusion of the average number of children interacted with year fixed effects. These regressions are not included for the sake of brevity. They are available upon request.

Table 1: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Variable	Obs	Mean	Std. Dev.
A. Full Sample of Village				**B. Subsample of Villages with Household Data**			
# of HH in Village	3291	419.769	279.765	# of HH in Village	1121	449.771	279.265
Fraction of Largest Clan	4992	0.523	0.238	Median HH Gross Income (RMB)	1297	11391.210	10519.790
Jiapu (Family Tree)	5208	0.097	0.296	Median HH Gross Income Growth	1133	0.095	0.226
Citang (Ancestral Hall)	5208	0.034	0.181	Median HH Income/90th Percentile HH Income	1297	0.512	0.130
Near City	5208	0.088	0.284	10th Percentile HH Income/Median HH Income	1297	0.503	0.125
Land was Expropriated by Upper Government	5208	0.036	0.187	Median HH Arable Farm Land (Mu)	1297	1.261	1.048
Allow a 2nd Child if the 1st is a Girl	5208	0.501	0.500	Median HH Land/90th Percentile Land (Mu)	1264	0.573	0.142
Village Investment into Public Goods (10,000 RMB)	5208	9.180	89.776	10th Percentile HH Land/50th Percentile HH Land (Mu)	1235	0.356	0.238
				Median HH Payment of Village and County Fees and Levies (100			
for Schools	5208	1.330	15.596	RMB)	1297	3.197	3.081
for Roads and Sanitation	5208	2.645	58.234	**C. Government**			
for Electricity	5208	0.615	7.174	The Number of Village Committee Members	2287	4.365	2.360
for Irrigation	5208	1.706	36.543	The Number of Village Party Cadres	2295	6.698	3.815
for Planting Trees	5208	0.189	6.050	Village Chief Tenure	5208	6.688	6.238
for Other	5208	1.791	44.092	Party Secretary Tenure	5208	10.032	8.126
Public Investment funded by Villagers	5208	6.270	92.484	**D. Elections**			
Public Investment funded by Upper Levels of Government	5208	3.029	55.973	1st Election in Village	5208	1988.512	5.240
Total Village Land (Mu)	3291	9,130.517	14,831.330	1st Competitive Election (e.g., +1 candidate per position)	5112	1991.873	6.426
Total Village Arable Land (Mu)	3291	2,273.782	2,309.547	1st Election with Open Nomination of Candidates	3336	1997.446	5.256
				Years between 1st Election in Village and Province Implementation	5208	4.963	5.062
Arable HH Farm Land (Mu)	3291	2,196.106	2,291.943	Years between 1st Election in County and Province Implementation	4416	5.299	5.010
As a fraction of total arable land	3291	0.959	0.152	Years between 1st Election in Village and County Implementation	4416	-0.141	3.962
Arable Collective Land (Mu)	3291	33.799	233.408	Anonymous Voting	1281	0.848	0.359
As a fraction of total arable land	3291	0.016	0.095	Proxy Voting	1265	0.721	0.449
Arable Land Leased to Village Enterprises (Mu)	3291	25.453	251.757	Roving Ballot Box	1251	0.652	0.476
As a fraction of total arable land	3291	0.010	0.074	There were more than 2 Candidates per Position	1293	0.791	0.407
Median HH Gross Income (RMB)	3778	10512.980	8365.713	Years between Elections	1084	3.162	1.024
Median HH Annual Gross Income Growth	3165	0.092	0.218				
Median HH Income/90th Percentile HH Income	3778	0.529	0.123				
10th Percentile HH Income/Median HH Income	3778	0.509	0.124				

Notes: Observations are at the village-year level.

Table 2: De Facto Power of Village Leaders

Signature Rights	Mean	Std. Dev.
Appoint managers of village enterprises:		
Village Chief	0.3225	0.4675
Party Secretary	0.3073	0.4614
Village Chief & Party Secretary	0.3684	0.4824
Employ village government public employees:		
Village Chief	0.2661	0.4420
Party Secretary	0.2944	0.4558
Village Chief & Party Secretary	0.4392	0.4963
Reimbursement:		
Village Chief	0.5582	0.4966
Party Secretary	0.2485	0.4322
Village Chief & Party Secretary	0.1931	0.3948
Reallocate Land:		
Village Chief	0.3285	0.4697
Party Secretary	0.1347	0.3414
Village Chief & Party Secretary	0.5305	0.4991
Large Public Investment:		
Village Chief	0.1770	0.3817
Party Secretary	0.1497	0.3568
Village Chief & Party Secretary	0.6731	0.4691

Observations are at the village-year level.

Table 3: The Effect of Elections on De Facto Powers of Village Leaders

		Dependent Variables: Powers of Village Leaders								
		Post 1st Election		Post 1st Competitive Election		Post 1st Haixuan			R-squared
		Coeff	Std Err	Coeff	Std Err	Coeff	Std Err	Obs	
Unilateral VC Power	(1)	Appoint TVE Manager	0.0647	(0.0389)	-0.0276	(0.0353)	-0.0334	(0.0423)	3336	0.801
	(2)	Employ Village Staff	0.0473	(0.0376)	-0.0208	(0.0370)	-0.0184	(0.0384)	4103	0.785
	(3)	Reimbursement	0.0510	(0.0384)	-0.0240	(0.0339)	-0.0529	(0.0397)	4910	0.771
	(4)	Land Reallocation	0.0763	(0.0348)	-0.0448	(0.0310)	-0.0073	(0.0391)	3936	0.800
	(5)	Public Investment	0.0726	(0.0271)	-0.0488	(0.0248)	0.0095	(0.0342)	4457	0.780
Joint VC PC Power	(6)	Appoint TVE Manager	0.0012	(0.0389)	-0.0002	(0.0389)	0.0228	(0.0444)	3336	0.797
	(7)	Employ Village Staff	-0.0253	(0.0411)	0.0336	(0.0420)	0.0479	(0.0437)	4103	0.769
	(8)	Reimbursement	0.0138	(0.0302)	0.0007	(0.0299)	0.0617	(0.0383)	4910	0.724
	(9)	Land Reallocation	-0.0019	(0.0389)	-0.0099	(0.0332)	0.0261	(0.0406)	3936	0.804
	(10)	Public Investment	-0.0236	(0.0305)	-0.0045	(0.0272)	0.0190	(0.0383)	4457	0.808
Unilateral PS Power	(11)	Appoint TVE Manager	-0.0666	(0.0352)	0.0273	(0.0310)	0.0216	(0.0351)	3336	0.832
	(12)	Employ Village Staff	-0.0227	(0.0305)	-0.0133	(0.0306)	-0.0299	(0.0337)	4103	0.819
	(13)	Reimbursement	-0.0655	(0.0274)	0.0229	(0.0227)	-0.0090	(0.0319)	4910	0.810
	(14)	Land Reallocation	-0.0752	(0.0310)	0.0541	(0.0213)	-0.0191	(0.0307)	3936	0.799
	(15)	Public Investment	-0.0496	(0.0278)	0.0529	(0.0210)	-0.0288	(0.0300)	4457	0.775

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 Upper Government Policies

	Dependent Variables			
	Unpopular Policies		Placebo Outcomes	
	Dummy for Allowing a 2nd Child if the 1st is a Girl	Dummy for Upper Government Expropriation of Village Land	Ln Public Investment from Special Aid	Ln Distance to Nearest High school
	(1)	(2)	(3)	(4)
Post 1st Election	0.0822	-0.0175	0.00450	-0.0117
	(0.0395)	(0.0107)	(0.0249)	(0.0275)
Post 1st Competitive Election	0.00640	0.0101	0.00233	-0.0278
	(0.0316)	(0.0109)	(0.0291)	(0.0172)
Post 1st Open Nomination	-0.0527	-0.00653	0.00771	-0.0288
	(0.0347)	(0.0110)	(0.0243)	(0.0247)
Observations	5208	5208	5208	4692
R-squared	0.755	0.080	0.079	0.955

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

Table 5: The Effect of Elections on Village Public Investment

	Dependent Variables						
	Total	Villagers	Upper Gov Special Aid	Upper Gov Other	Other	Upper Gov Sum	All Non Village
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Ln Total Public Investment						
Post 1st Election	0.175	0.0849	0.00450	0.0448	-0.0112	0.0464	0.0488
	(0.114)	(0.0962)	(0.0249)	(0.0470)	(0.0342)	(0.0539)	(0.0651)
Observations	5208	5208	5208	5208	5208	5208	5208
R-squared	0.199	0.176	0.079	0.161	0.120	0.157	0.173
	B. Irrigation						
Post 1st Election	-2.446	-1.599	-0.472	0.330	-0.0965	-0.142	-0.238
	(1.265)	(1.191)	(0.268)	(0.816)	(0.0958)	(0.876)	(0.868)
Post 1st Election x Avg Ln HH Farm Land	0.374	0.236	0.0666	-0.0289	0.0111	0.0377	0.0488
	(0.165)	(0.160)	(0.0370)	(0.113)	(0.00944)	(0.122)	(0.121)
Observations	5208	5208	5208	5208	5208	5208	5208
R-squared	0.106	0.106	0.058	0.109	0.065	0.106	0.106
	C. Schooling*						
Post 1st Election	-2.898	6.273	-0.509	-2.633	-0.963	-3.142	-4.105
	(15.55)	(13.47)	(3.660)	(5.109)	(3.257)	(5.528)	(6.021)
Post 1st Election x # Kids 7-13	0.0132	0.0154	-0.000195	-0.00132	0.000236	-0.00152	-0.00128
	(0.00488)	(0.00571)	(0.000773)	(0.00169)	(0.000611)	(0.00154)	(0.00152)
Observations	5064	5064	5064	5064	5064	5064	5064
R-squared	0.077	0.069	0.057	0.108	0.062	0.099	0.086

All regressions control for post first competitive election, 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., number of kids age 7-13, ln total household farm land) with post first competitive election and post first open nominations. Standard errors are clustered at the village level. Notes: *The coefficients for the estimates on public investment in schooling are scaled by 1,000 for ease of interpretation.

Table 6: The Effect of Elections on Household Farm Land Allocation

	Dependent Variables: Village Land Distribution									Ln Land Leased Out to Firms, Land Leased Out>0
	Within-village HH Land Distribution									
	10th	20th	30th	40th	50th	60th	70th	80th	90th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post 1st Election	0.00608	0.248	0.257	0.280	0.253	0.203	0.162	0.113	0.0969	-0.679
	(0.222)	(0.203)	(0.172)	(0.150)	(0.117)	(0.113)	(0.109)	(0.103)	(0.105)	(0.388)
Post 1st Competitive Election	0.0537	-0.262	-0.101	-0.0478	-0.0188	0.00816	0.0245	0.0503	0.0600	-0.103
	(0.195)	(0.183)	(0.138)	(0.128)	(0.0924)	(0.0843)	(0.0857)	(0.0923)	(0.0996)	(0.455)
Post 1st Open Nomination	-0.102	-0.144	-0.263	-0.128	-0.106	-0.0834	-0.0385	0.0318	0.101	0.387
	(0.182)	(0.147)	(0.153)	(0.122)	(0.103)	(0.106)	(0.0929)	(0.0812)	(0.0821)	(0.388)
Observations	1297	1297	1297	1297	1297	1297	1297	1297	1297	315
R-squared	0.735	0.792	0.808	0.857	0.884	0.890	0.888	0.882	0.881	0.927

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

Table 7: 2SLS Estimate of the Effect of Elections

	Dependent Variables										
	Post 1st Election	Post 1st Election	Post 1st Open Nomination	Land Expropriation Dummy		Dummy for Allowing a 2nd Child if the 1st is a Girl		Ln Public Investment in Irrigation from Villagers		Ln Public Investment in Schooling from Villagers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	1st Stage (1 Instrument)	1st Stage (2 Instruments)	1st Stage (2 Instruments)	IV for Post 1st Election	IV for Post 1st Election, Post 1st Open Nom.	IV for Post 1st Election	IV for Post 1st Election, Post 1st Open Nom.	IV for Post 1st Election & Interaction	IV for Post 1st Election, Post 1st Open Nom. & Interactions	IV for Post 1st Election & Interaction	IV for Post 1st Election, Post 1st Open Nom. & Interactions
Post 1st Election in County	0.288 (0.0307)	0.287 (0.0305)	0.00593 (0.0303)								
Post 1st Open Nomination in County		0.0164 (0.0247)	0.176 (0.0411)								
Post 1st Election				-0.0527 (0.0289)	-0.0569 (0.0309)	0.294 (0.111)	0.303 (0.119)	19.47 (40.18)	16.19 (38.66)	-1.753 (1.824)	-2.052 (2.206)
Post 1st Open Nomination	-0.0206 (0.0224)			-0.00673 (0.0110)	0.0187 (0.0630)	-0.0515 (0.0347)	-0.111 (0.184)	-10.74 (26.07)	15.00 (107.9)	-0.0389 (0.832)	2.304 (3.047)
Post 1st Competitive Election	0.476 (0.0372)	0.473 (0.0370)	0.129 (0.0289)	0.0293 (0.0178)	0.0280 (0.0185)	-0.109 (0.0629)	-0.105 (0.0628)	-14.43 (26.37)	-15.76 (25.89)	1.551 (1.928)	1.361 (1.943)
Post st Election x Avg # of Kids 7-13 in Village								0.0137 (0.00469)	0.0140 (0.00486)		
Post 1st Election x Avg Ln HH Arable Land										0.285 (0.240)	0.315 (0.276)
Observations	5208	5208	5208	5208	5208	5208	5208	5064	5064	5208	5208

All regressions control for province time trends, village and year fixed effects. Post 1st Election and Post 1st Haixuan are instrumented with post 1st election and post open nomination in the same county. The regressions in columns (8)-(11) 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 competitive election and post first open nominations. The interaction terms are instrumented by the interaction of the introduction of elections and haixuan in the county and the same relevant characteristics. Standard errors are clustered at the village level.

Table 8: The Effect of Elections – Robustness to Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			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
	Baseline	Election Procedures					
A. Ln Median HH Land							
Post 1st Election	0.253	0.318	0.257	0.252	0.249	0.271	0.304
	(0.117)	(0.150)	(0.118)	(0.116)	(0.115)	(0.119)	(0.141)
Observations	1297	1297	1297	1297	1297	1297	1297
R-squared	0.884	0.889	0.885	0.889	0.887	0.886	0.897
B. Ln Median HH Fees and Levies							
Post 1st Election	0.956	0.875	0.962	1.015	0.969	0.910	0.991
	(0.503)	(0.529)	(0.508)	(0.515)	(0.506)	(0.524)	(0.575)
Observations	1297	1297	1297	1297	1297	1297	1297
R-squared	0.610	0.614	0.614	0.622	0.613	0.615	0.638
C. Upper Government Land Expropriation							
Post 1st Election	-0.0175	-0.0187	-0.0167	-0.0156	-0.0177	-0.0170	-0.0162
	(0.0107)	(0.0125)	(0.0108)	(0.0110)	(0.0107)	(0.0107)	(0.0128)
Observations	5208	5208	5208	5208	5208	5208	5208
R-squared	0.080	0.081	0.084	0.090	0.080	0.084	0.099
D. Dummy for Allowing a 2nd Child if the 1st is a Girl							
Post 1st Election	0.0822	0.0765	0.0874	0.0830	0.0816	0.0899	0.0865
	(0.0395)	(0.0419)	(0.0392)	(0.0391)	(0.0395)	(0.0398)	(0.0417)
Observations	5208	5208	5208	5208	5208	5208	5208
R-squared	0.755	0.755	0.756	0.756	0.755	0.758	0.761
E. Ln Total Public Investment							
Post 1st Election	0.175	0.189	0.173	0.178	0.175	0.174	0.198
	(0.114)	(0.127)	(0.116)	(0.115)	(0.114)	(0.115)	(0.131)
Observations	5208	5208	5208	5208	5208	5208	5208
R-squared	0.199	0.199	0.202	0.208	0.199	0.202	0.214
F. Ln Public Investment into Irrigation (from Village Funds)							
Post 1st Election x Avg Ln HH Farmland	0.236	0.246	0.235	0.260	0.240	0.239	0.275
	(0.160)	(0.164)	(0.160)	(0.163)	(0.159)	(0.160)	(0.168)
Observations	5208	5208	5208	5208	5208	5208	5208
R-squared	0.106	0.106	0.109	0.113	0.106	0.109	0.120
G. Ln Public Investment into Schooling (from Village Funds)*							
Post 1st Election x # of Kids 7-13	0.0154	0.0151	0.0146	0.0148	0.0160	0.0149	0.0137
	(0.00571)	(0.00574)	(0.00561)	(0.00548)	(0.00576)	(0.00601)	(0.00576)
Observations	5064	5064	5064	5064	5064	5064	5064
R-squared	0.069	0.070	0.073	0.083	0.069	0.072	0.090

All regressions control for province time trends, village and year fixed effects, 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 competitive election and post first open nominations. Standard errors are clustered at the village level. Notes: *The coefficients for the estimates on public investment in schooling are scaled by 1,000 for ease of interpretation.

Figure 1: The Effect of Elections on Village Leader Power
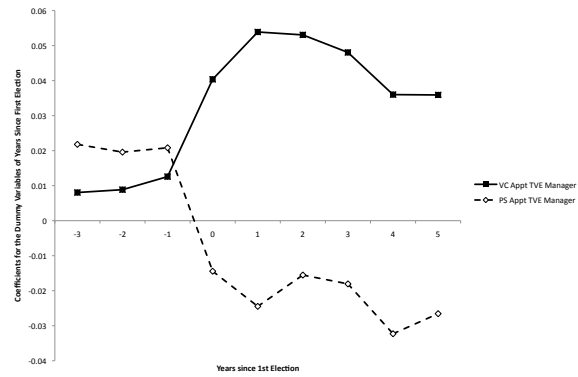by the Number of Years Since the 1st Election

Figure 2: The Effect of Elections on One Child Policy Exemptions
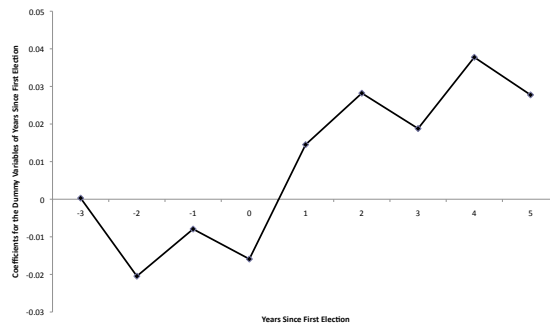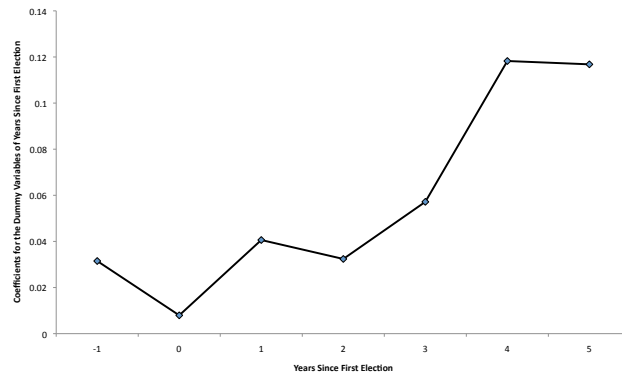by the Number of Years Since the 1st Election

Figure 3: The Effect of Elections on Median Household Land Allocation
by the Number of Years Since the 1st Election

APPENDIX Table A1: The Effect of Elections by Years Since the 1st Election

	Dependent Variables												
	Village Chief					Party Secretary							
Years since 1st Election Dummy Variables	(1)	(2)	(3)	(4)	(5)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	Appt TVE Manager	Employ Staff	Reimburse	Land Realloc.	Public Inv	Appt TVE Manager	Employ Staff	Reimburse	Land Realloc.	Public Inv	OCExempt	Ln Median HH Land
Dummy -3	0.00804	0.0266	0.0134	0.0183	-0.00378	0.0218	-0.00389	0.0120	0.0246	0.0156	0.000296	
	(0.0166)	(0.0192)	(0.0164)	(0.0181)	(0.0127)	(0.0189)	(0.0189)	(0.0153)	(0.0158)	(0.0160)	(0.0288)	
Dummy -2	0.00885	0.0309	0.0212	0.0320	0.00833	0.0196	-0.00875	0.00543	0.0189	0.0138	-0.0205	
	(0.0173)	(0.0183)	(0.0178)	(0.0169)	(0.0122)	(0.0225)	(0.0206)	(0.0159)	(0.0184)	(0.0176)	(0.0293)	
Dummy -1	0.0126	0.0366	0.0294	0.0432	0.0112	0.0208	0.000417	-0.00536	0.0211	0.0169	-0.00794	0.0315
	(0.0179)	(0.0186)	(0.0211)	(0.0179)	(0.0124)	(0.0226)	(0.0212)	(0.0184)	(0.0188)	(0.0179)	(0.0302)	(0.0652)
Dummy -0	0.0404	0.0322	0.0494	0.0504	0.0374	-0.0144	0.00194	-0.0381	-0.0313	-0.0355	-0.0159	0.00795
	(0.0236)	(0.0209)	(0.0205)	(0.0183)	(0.0143)	(0.0251)	(0.0219)	(0.0157)	(0.0179)	(0.0175)	(0.0310)	(0.0544)
Dummy -1	0.0539	0.0358	0.0509	0.0610	0.0409	-0.0245	0.00135	-0.0304	-0.0229	-0.0247	0.0145	0.0406
	(0.0242)	(0.0204)	(0.0207)	(0.0188)	(0.0159)	(0.0270)	(0.0230)	(0.0165)	(0.0176)	(0.0172)	(0.0306)	(0.0550)
Dummy -2	0.0530	0.0452	0.0490	0.0796	0.0504	-0.0155	-0.000403	-0.0254	-0.0206	-0.0205	0.0282	0.0324
	(0.0243)	(0.0214)	(0.0214)	(0.0208)	(0.0173)	(0.0271)	(0.0222)	(0.0165)	(0.0175)	(0.0169)	(0.0293)	(0.0540)
Dummy -3	0.0480	0.0374	0.0542	0.0576	0.0430	-0.0181	0.00286	-0.0275	-0.0177	-0.0113	0.0188	0.0572
	(0.0227)	(0.0204)	(0.0204)	(0.0192)	(0.0166)	(0.0237)	(0.0199)	(0.0159)	(0.0166)	(0.0162)	(0.0288)	(0.0564)
Dummy -4	0.0360	0.0288	0.0407	0.0522	0.0358	-0.0323	-0.00461	-0.0328	-0.0220	-0.0145	0.0378	0.118
	(0.0220)	(0.0204)	(0.0202)	(0.0183)	(0.0168)	(0.0216)	(0.0185)	(0.0161)	(0.0164)	(0.0162)	(0.0265)	(0.0520)
Dummy -5	0.0359	0.0295	0.0435	0.0376	0.0214	-0.0266	-0.00633	-0.0340	-0.0165	-0.00771	0.0278	0.117
	(0.0225)	(0.0203)	(0.0202)	(0.0178)	(0.0157)	(0.0201)	(0.0177)	(0.0147)	(0.0167)	(0.0165)	(0.0231)	(0.0622)
Observations	3336	4103	4910	3936	4457	3336	4103	4910	3936	4457	5208	1297
R-squared	0.802	0.785	0.772	0.801	0.780	0.832	0.819	0.809	0.798	0.775	0.754	0.883

All regressions control for post first competitive election, post first open nomination, province time trends, village and year fixed effects. Standard errors are clustered at the village level.

Table A2: The Effect of Elections on Household Fees

	Dependent Variables: Ln Levies and Fees paid to Village, County and Township by Percentile (on the within-village Distribution of Household Fees)								
	(1) 10th	(2) 20th	(3) 30th	(4) 40th	(5) 50th	(6) 60th	(7) 70th	(8) 80th	(9) 90th
Post 1st Election	1.027	0.797	0.765	0.930	0.956	0.971	1.069	0.922	0.539
	(0.450)	(0.468)	(0.449)	(0.473)	(0.503)	(0.506)	(0.522)	(0.480)	(0.401)
Post 1st Competitive Election	-0.0620	-0.269	-0.0853	-0.0803	-0.109	-0.102	-0.268	-0.390	-0.452
	(0.381)	(0.381)	(0.342)	(0.354)	(0.344)	(0.355)	(0.363)	(0.369)	(0.367)
Post 1st Open Nomination	0.103	-0.131	-0.0638	0.0862	0.234	0.140	0.109	0.0790	0.0108
	(0.389)	(0.384)	(0.416)	(0.373)	(0.362)	(0.376)	(0.381)	(0.397)	(0.400)
Observations	1297	1297	1297	1297	1297	1297	1297	1297	1297
R-squared	0.556	0.572	0.596	0.586	0.610	0.607	0.611	0.618	0.586

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

Table A3: The Effect of Elections on Household Income and Consumption

	Dependent Variables		Post 1st Election			
			Coeff	SE	Obs	R-Sq
(1)		p10	0.0281	(0.130)	1297	0.496
(2)	Ln Total HH Gross Income	p25	-0.0205	(0.0508)	1297	0.876
(3)		p50	-0.0549	(0.0433)	1297	0.903
(4)		p75	-0.0821	(0.0513)	1297	0.901
(5)		p90	-0.0961	(0.0684)	1297	0.884
(6)		Mean	-0.0769	(0.0599)	1297	0.893
(7)		p10				
(8)	Ln HH Income from Village Enterprises	p25				
(9)		p50	0.0147	(0.0241)	870	0.107
(10)		p75	-0.292	(0.341)	870	0.257
(11)		p90	-1.421	(0.966)	870	0.290
(12)		Mean	-1.208	(1.003)	868	0.460
(13)		p10	0.146	(0.134)	1297	0.185
(14)	Ln HH Income from Wages	p25	0.496	(0.562)	1297	0.473
(15)		p50	-0.920	(0.653)	1297	0.556
(16)		p75	-1.718	(0.622)	1297	0.549
(17)		p90	-0.547	(0.321)	1297	0.603
(18)		Mean	-0.377	(0.151)	1297	0.736
(19)		p10	0.0295	(0.126)	1297	0.527
(20)		p25	-0.0254	(0.0577)	1297	0.890
(21)	Ln Total HH Consumption	p50	-0.0497	(0.0536)	1297	0.904
(22)		p75	-0.0812	(0.0574)	1297	0.899
(23)		p90	-0.106	(0.0703)	1297	0.864
(24)		Mean	-0.0863	(0.0627)	1297	0.895

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

Table A4: The Effect of Elections – Robustness to the Correlates of Election Introduction and Additional Controls

	(1) Baseline	(2) Avg Med HH Land x YrFE*	(3) Avg Land50/90 x YrFE*	(4) Avg Ln Total HH Land x YrFE	(5) Avg Ln Land Lease x YrFE	(6) Avg Land Expropriation x YrFE	(7) Near City x YrFE	(8) Avg Ln Med HH Fee x YrFE*	(9) Avg Fertility x YrFE	(10) Avg Labor-Pop Ratio x YrFE	(11) Avg Incidence of OCP Exempt x YrFE	(12) Avg Ln Median HH Inc x YrFE	(13) Avg Inc50/90 x YrFE	(14) Avg Med Inc Growth x YrFE	(15) Controls from columns (2)-(8), (10)-(14)*
A. Ln Median HH Land															
Post 1st Election	0.253	0.207	0.240	0.253	0.260	0.224	0.253	0.253	0.286	0.276	0.257	0.304	0.252	0.254	0.220
	(0.117)	(0.129)	(0.114)	(0.117)	(0.116)	(0.105)	(0.117)	(0.117)	(0.112)	(0.120)	(0.115)	(0.102)	(0.114)	(0.119)	(0.105)
Observations	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297
R-squared	0.884	0.904	0.896	0.884	0.888	0.898	0.884	0.884	0.883	0.888	0.885	0.900	0.891	0.885	0.928
B. Ln Median HH Fees and Levies															
Post 1st Election	0.956	0.810	0.879	0.956	0.980	0.938	0.956	0.956	0.989	0.990	0.931	0.860	0.890	0.957	0.693
	(0.503)	(0.514)	(0.505)	(0.503)	(0.507)	(0.496)	(0.503)	(0.503)	(0.539)	(0.507)	(0.510)	(0.516)	(0.511)	(0.510)	(0.514)
Observations	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297	1297
R-squared	0.610	0.616	0.617	0.610	0.616	0.615	0.610	0.610	0.619	0.616	0.618	0.614	0.616	0.613	0.664
C. Upper Government Land Expropriation															
Post 1st Election	-0.0175	-0.0440	-0.0407	-0.0175	-0.0170	-0.0179	-0.0498	-0.0175	-0.0164	-0.0173	-0.0175	-0.0167	-0.0171	-0.0182	-0.0139
	(0.0107)	(0.0175)	(0.0166)	(0.0107)	(0.0108)	(0.0104)	(0.0220)	(0.0107)	(0.0111)	(0.0107)	(0.0108)	(0.0108)	(0.0107)	(0.0107)	(0.0103)
Observations	5208	1752	1752	5208	5208	5208	1297	5208	5064	5208	5208	5208	5208	5208	5208
R-squared	0.080	0.094	0.091	0.080	0.082	0.087	0.101	0.080	0.087	0.083	0.083	0.083	0.085	0.082	0.125
D. Dummy for Allowing a 2nd Child if the 1st is a Girl															
Post 1st Election	0.0822	0.174	0.196	0.0822	0.0864	0.0824	0.178	0.0822	0.0723	0.0843	0.0861	0.0805	0.0830	0.0825	0.0968
	(0.0395)	(0.0771)	(0.0779)	(0.0395)	(0.0401)	(0.0396)	(0.0985)	(0.0395)	(0.0402)	(0.0396)	(0.0390)	(0.0399)	(0.0394)	(0.0396)	(0.0402)
Observations	5208	1752	1752	5208	5208	5208	1297	5208	5064	5208	5208	5208	5208	5208	5208
R-squared	0.755	0.739	0.737	0.755	0.756	0.755	0.758	0.755	0.757	0.757	0.762	0.755	0.757	0.756	0.771
E. Ln Total Public Investment															
Post 1st Election	0.175	0.175	0.164	0.175	0.171	0.178	0.303	0.175	0.143	0.171	0.176	0.180	0.182	0.181	0.206
	(0.114)	(0.225)	(0.220)	(0.114)	(0.114)	(0.114)	(0.293)	(0.114)	(0.113)	(0.115)	(0.114)	(0.115)	(0.115)	(0.114)	(0.112)
Observations	5208	1752	1752	5208	5208	5208	1297	5208	5064	5208	5208	5208	5208	5208	5208
R-squared	0.199	0.202	0.202	0.199	0.201	0.205	0.197	0.199	0.204	0.201	0.206	0.204	0.202	0.201	0.231
F. Ln Public Investment in Irrigation from Village Funds															
Post 1st Election x Avg Ln HH Farmland	0.237			0.236	0.238	0.217	0.227		0.226	0.239	0.231	0.238	0.225	0.232	0.215
	(0.160)			(0.160)	(0.161)	(0.156)	(0.164)		(0.160)	(0.156)	(0.159)	(0.162)	(0.161)	(0.161)	(0.159)
Observations	5208			5208	5208	5208	5208		5064	5208	5208	5208	5208	5208	5208
R-squared	0.102			0.106	0.110	0.110	0.114		0.109	0.108	0.108	0.111	0.109	0.107	0.137
G. Ln Public Investment in Schools from Village Funds**															
Post 1st Election x # of Kids 7-13	0.0154			0.0154	0.0153	0.0136	0.0157		0.0146	0.0155	0.0151	0.0153	0.0153	0.0216	0.0134
	(0.00571)			(0.00571)	(0.00577)	(0.00550)	(0.00564)		(0.00568)	(0.00576)	(0.00568)	(0.00545)	(0.00560)	(0.00797)	(0.00543)
Observations	5064			5064	5064	5064	5064		5064	5064	5064	5064	5064	3001	5064
R-squared	0.069			0.069	0.072	0.076	0.072		0.074	0.071	0.072	0.077	0.073	0.107	0.098

All regressions control for province time trends, village and year fixed effects, 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 competitive election and post first open nominations. Standard errors are clustered at the village level. Notes: *For outcomes in Panels C-G, we do not control for the starred (*) land and household fee controls in column (15). ***The coefficients for the estimates on public investment in schooling are scaled by 1,000 for ease of interpretation.

Table A5: The Effect of Elections – Robustness to Excluding Villages that Introduced Elections before OLVC (1987)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Omit if 1st Election <1987		Omit if 1st Election <1987		Omit if 1st Election <1987		Omit if 1st Election <1987
	Baseline Sample		Baseline Sample		Baseline Sample		Baseline Sample	

Dependent Variables

Panel A.

	Ln Median HH Land		Ln Median HH Fees		Upper Government Land Expropriation		Dummy for Allowing a 2nd Child if the 1st is a Girl	
Post 1st Election	0.253	0.302	0.956	0.982	-0.0175	-0.0227	0.0822	0.0817
	(0.117)	(0.134)	(0.503)	(0.556)	(0.0107)	(0.0164)	(0.0395)	(0.0462)
Observations	1297	1152	1297	1152	5208	3906	5208	3906
R-squared	0.884	0.896	0.610	0.625	0.080	0.089	0.755	0.820

Panel B

	Ln Total Public Investment		Ln Public Investment in Schooling from Villagers		Ln Public Investment in Irrigation from Villagers	
	Baseline Sample	Omit if 1st Election <1987	Baseline Sample	Omit if 1st Election <1987	Baseline Sample	Omit if 1st Election <1987
Post 1st Election	0.175	0.340	6.273	11.16	-1.599	-1.788
	(0.114)	(0.172)	(13.47)	(18.64)	(1.191)	(1.855)
Post 1st Election x # of Kids 7-13			0.0154	0.0149		
			(0.00571)	(0.00971)		
Post 1st Election x Avg Ln HH Farmland					0.236	0.286
					(0.160)	(0.243)
Observations	5208	3906	5064	3798	5208	3906
R-squared	0.199	0.194	0.069	0.085	0.106	0.126

All regressions control for the introduction of the first competitive election and open nominations, province-year trends, village and year fixed effects. Those in Panel B also control for the interactions of these introductions with the relevant village characteristics (e.g., # of kids 7-13, average ln total household farm land). The standard errors are clustered at the village level.