Elections in China*

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Abstract

We examine the effects of introducing village elections on public goods expenditures, income distribution and land use in rural China. We construct a large panel data set of village administrative records to document the history of political reforms and economic policies for over two hundred villages. We exploit the staggered timing of the introduction of village elections to find that elections significantly increased public goods expenditure financed by villagers. In addition, we find that the introduction of elections caused a moderate decline in income inequality and likely reduced corruption. The results suggest that local officials are better controlled by local elections rather than by centrally managed bureaucratic monitoring.

Keywords: Institutions, Local Governance, Elections, Democracy; **JEL:** H4, H7, O1, P16

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1 Introduction

The control of large bureaucracies, as the extensive literature on bureaucratic corruption shows, is a difficult task.[1] The lack of information and appropriate oversight often results in the misbehavior of local officials.[2] In autocratic countries, the control of local officials is further complicated by the weakness of established channels to receive feedback from citizens.[3] To address this agency problem, several autocratic governments have introduced local elections in recent years.[4] To date, there is little systematic evidence on whether these reforms succeed in making local officials fulfill their duties.

China is the largest autocracy to try this institutional innovation. During the 1980s and 1990s village-level elections were introduced to a rural population that numbers almost one billion, which had never had any experience with elections before. In order to keep political control, the institutional changes implemented were limited. Historically, the village government was comprised of two bodies that were appointed by the Communist Party: the Communist Party Branch and the Village Committee. The reform put the Village Committee up for election and left the Party Branch unchanged. The goal of this paper is to provide rigorous empirical analysis of the policy consequences of this reform and, in the process, shed light on the effectiveness of elections in changing incentives for local officials in an otherwise autocratic context.

One of the most important responsibilities of village officials in rural China is the provision of local public goods such as schooling, irrigation or village roads. The limitations of bureaucratic monitoring meant that, before elections, there was widespread shirking among local officials. In principle, elections can resolve this agency problem by giving local officials incentives to implement

[1] A classic example in this literature is Wilson (1989).

[2] For recent overviews of this literature, see Banerjee et al. (2012) and Olken and Pande (2012).

[3] Autocracies typically limit the rights to associate, freedom of expression and freedom of the press, which in democratic countries are important for the transmission of information on local scandals and demands. For instance, Besley and Burgess (2002) show that a free press is important for government responsiveness.

[4] For example, local elections have occurred in Indonesia under Suharto (1968-1998), Brazil during the military dictatorship (1964-1985), and Mexico under the PRI (1929-2000). Recently, local elections were also introduced in Vietnam in 1998, in Yemen in 2001, and in Saudi Arabia in 2005. For a literature review of the nascent political science research on elections in dictatorships see Gandhi and Lust-Okar (2009) and Malesky and Schuler (2013) for an examination of the Vietnam case.

policies that appeal to a majority in the constituency in order to obtain re-election.[5] Village-level expenditures on public goods are therefore our main focus in evaluating the effectiveness of the introduction of elections.

Our study faces two notable difficulties. The first is the lack of detailed data on political and economic policies in rural China. For our main analysis, we construct the *Village Democracy Survey* (VDS), a panel of over two hundred nearly representative villages from 29 provinces for the years 1982-2005. The survey documents the history of economic and governance policies, and contains detailed economic data on public goods expenditure and the sources of funds. This is the longest and broadest panel ever constructed to describe Chinese villages and is the first data to systematically document the changes in the fiscal and political structure of village governments. In additional exercises, we supplement the VDS with economic data from the the *National Fixed-Point Survey* (NFS), which is collected yearly from the same villages as the VDS by the Ministry of Agriculture.

The second difficulty lies in establishing the causal effect of the introduction of elections, which were staggered in timing across villages. One concern is reverse causality as economic conditions might affect the demand for elections. Another concern is joint determination, since both elections and economic change could be the consequences of broader reforms of rural policies. To address these concerns we take advantage of two features of the Chinese context. First, according to the descriptive literature, the timing of the introduction of elections was mostly unrelated to village characteristics. Second, electoral reforms were isolated to the village-level and were not accompanied by changes in institutions or policies for upper-levels of government; nor did the reforms affect the *de jure* powers of the village leaders. In the paper, we provide a large body of anecdotal and quantitative evidence to support these two points.

The main empirical analysis proceeds in three steps. First, we document that the timing of elections across villages within provinces is uncorrelated with a large number of observable characteristics at the village level, such as baseline public goods expenditure. This is consistent with the anecdotal evidence which suggests that the timing of reforms was imposed top-down with little

[5]The theoretical basis for this claim comes from the rich literature on political accountability. In broad terms, this literature examines how elections induce politicians to provide more common interest policies such as more public goods. See the discussion in Persson and Tabellini, (2000, chapter 1).

regard for village-specific characteristics. Second, we implement a *difference-in-differences* (DD) strategy to estimate the causal effects of the introduction of elections: we compare outcomes before and after the first election in each village, between villages that had already introduced elections and those that have not. The baseline specification includes village fixed effects that control for all time-invariant differences across villages, year fixed effects that control for all changes over time that are similar across villages, as well as province-specific time trends to control for the economic and cultural divergence across China during our period of study. These trends improve the precision of our estimates, but do not affect the coefficients.

As with any DD strategy, causal interpretation relies on the assumption that in the absence of electoral reforms, the evolution of outcomes would be "parallel" across villages regardless of when they implemented the first election. We support our assumption by documenting average public goods expenditure before and after the first election and showing that there is very little expenditure in any village prior to the introduction of elections, and therefore no pre-trend. At the same time, the introduction of elections is accompanied by a dramatic rise in the level and frequency of expenditures. We also show that our estimates are robust to controlling for pre-election characteristics, the province-level decision to introduce village elections, which is the main source of endogeneity, and a large number of other variables.

Our results show that the introduction of elections increased total local government expenditure on public goods by approximately 50%. The large percentage increase is consistent with the fact that prior to the reforms, local public goods provision and expenditures were extremely low.[6] The per household increase in expenditure was 1.8% of the median household income. Because of the infrequent and lumpy nature of public goods expenditure, it is also interesting to examine the frequency of public goods investment. We find that elections increase the frequency of positive expenditures by six percentage-points, which is over one-third of the sample mean. This suggests that the newly introduced electoral accountability pushed officials to exert effort in providing public goods, which had been neglected under the appointment regime.

To better understand how the increase in public goods expenditures was reached, we investigate

[6]This is noted in previous studies (Luo et al., 2007, 2010) and can be observed in our data. See Section 4.2.

the source of funds used to pay for village public goods and the amount of fees paid by households to the local government. We find that the increase in public goods expenditure is entirely financed by villagers, and that the introduction of elections increased the amount of local fees paid by all households as a percentage of income by 0.3 to 0.5 percentage-points (the sample mean is approximately two percentage-points). These results again suggest that elections made local government more accountable to villagers, which increased the willingness of citizens to supply it with funds (e.g., Fujiwara, 2011).[7] Importantly, they contradict the traditional notion that elected governments are less able to provide public goods because of short-term consumption demands of citizens.[8]

The natural interpretation of our results is that elections made local governments more accountable towards villagers. The key concern with this interpretation is that there might have been other changes happening at the same time as the introduction of elections that would increase public goods expenditures. We address this concern in several ways. First, we use the VDS to document that elected village leaders had the power to make public goods investments and that this power was not undermined when elections were introduced. Second, we show that the introduction of elections had no effect on public goods expenditures financed by upper levels of government. Since transfers are the most direct method for the upper government to increase local public goods, this result strongly suggests that the introduction of elections was not confounded with other policies that changed the priorities of upper-level government. Third, we examine the effect of the reform on the characteristics of the newly elected officials versus those in positions that continue to be appointed. We find that the introduction of elections caused newly elected leaders to be younger and more educated, but had no effect on the Communist Party Branch Party Secretary, who continued to be appointed. All this supports the descriptive literature which documents that the introduction of elections was not accompanied by any other changes in the Communist Party or rural policies. Finally, as we discussed earlier, we subject these results to a large number of robustness checks.

According to the literature on political accountability, elections generate outcomes aligned with electorate preferences through two mechanisms: the presence of re-election incentives which lead

[7]For evidence that expanded democracy can increase public goods in the Brazilian context, see Fujiwara (2011), which finds that an expansion of the enfranchisement increases public goods provision.

[8]For example, see the classic work of Huntington (1968) for a discussion of why democracy hinders the government's ability to raise taxes.

politicians to exert more effort, and the selection of better politicians (e.g., Besley and Case, 1995; Dal-Bó and Rossi, 2011; Ferraz and Finan, 2011).[9] We find evidence to suggest that both mechanisms contributed to the effects of the introduction of rural elections in China.

In addition to our main results on public goods, we also examine land use and household income distribution. We find that the introduction of elections reduces the amount of land leased to enterprises and redistributes it back to households. Since the practice of leasing land to enterprises has been linked to rent-seeking by local officials, this finding suggests that elections also helped curb corruption. We also find that elections caused a moderate reduction in village income inequality by reducing the incomes of the richest households, which is consistent with a systematic reduction in pro-elite policies. Our data suggest that this change was achieved through a redistribution of productive assets such as land and employment at village enterprises, which is probably a consequence of the fact that village governments do not have the power to impose recurrent taxes and transfers.

In sum, our results suggest that local elections in rural China have helped align village-level policies with the interests of the majority by making local officials partially accountable to villagers. This delegation of monitoring from centralized bureaucratic structures to citizens seems to constrain local officials, even in a high state capacity autocratic context.

This study makes several contributions. First, it is closely related to the growing number of recent within-country studies that have focused on changes in various aspects of elections in poor or middle income economies such as Argentina, Brazil and India (Beaman et al., 2009; Dal-Bó and Rossi, 2011; Ferraz and Finan, 2011; Fujiwara, 2011).[10] We differ from these studies in examining a much starker institutional change: from no elections to elections. In addition, we do this in the context of an autocracy, where the credibility of the reforms is necessarily limited by the need of the regime to keep control.

Second, since elections are an essential element of democracy, our results also speak to the

[9] For example, Besley and Case (1995), Dal-Bó and Rossi (2011) and Ferraz and Finan (2011) provide evidence for the role of re-election incentives in the United States, Argentina and Brazil.

[10] Also, see Tyrefors and Pettersson-Lidbom (2014) for historical evidence from Sweden. These studies do not identify the effects of introducing elections *per se*. An older working paper by Foster and Rosenzweig (2005) examines the effect of the introduction of rural elections on public goods provision in India, but focuses on party competition mechanisms. There is also a related literature examining the differences between elected and appointed officials (e.g., Besley and Coate, 2003; Lim, 2013; Martinez-Bravo, 2014).

broader literature on democracy and economic policy.[11] The existing empirical evidence relating democratic transition to public goods and redistribution, which mostly comes from cross-country studies, is inconclusive (e.g., Acemoglu et al., 2013; Besley and Kudamatsu, 2006; Kudamatsu, 2012; Tavares and Wacziarg, 2001).[12] Relative to cross-country comparisons, Chinese villages are much more comparable with each other and the introduction of elections was not the result of social turmoil and other confounding factors. Our focus on elections complements recent studies that emphasize the importance of constraints on the executive in determining the effect of democracy on economic outcomes (e.g. Besley and Persson, 2011).[13]

Finally, our study adds to a small number of studies on the effects of village-level elections in rural China (e.g., Luo et al., 2010; Shen and Yao, 2008; Zhang et al., 2004). These earlier works inspired our study as they link village elections with changes in economic outcomes. However, the fact that they only had data for a few non-representative provinces meant that they could not estimate the average effect for China or adequately control for omitted variables.[14] Our expanded data significantly improves the rigor of the evidence by covering a nearly representative sample of villages for a long time horizon. In addition, we examine a much larger set of new outcomes (e.g., land allocation, local fees, household income sources and leader characteristics), which enables us to shed light on the mechanisms underlying the effect of elections and consider the generalizable insights from the Chinese experience.

[11] For instance see Acemoglu and Robinson (2001, 2006), Boix (2003), Bueno de Mesquita et al. (2003), Lizzeri and Persico (2004), Besley and Kudamatsu (2008) who all relate democratization or extensions of the franchise to either increased public goods provision or redistribution. Husted and Kenny (1997) and Miller (2008) provide empirical evidence for increased welfare spending in the context of franchise extension in the United States.

[12] In the cross-section, democracy has been found to be positively associated with government size (Tavares and Wacziarg, 2001), higher wages (Rodrik, 1999), lower inequality and higher human capital (e.g, Tavares and Wacziarg, 2001), and better health indicators (Besley and Kudamatsu, 2006; Kudamatsu, 2012). However, in a large study looking at several socioeconomic policy dimensions, Gil et al. (2004) finds that democracy is associated with no difference in the outcomes they examine. Using dynamic methods, Acemoglu et al. (2014) finds effects of democracy on subsequent growth, but the effect on inequality is more nuanced in Acemoglu et al. (2013).

[13] Democracy is typically viewed as being comprised of two elements: the presence of elections to determine who will be the executive authority and the presence of institutionalized constraints, such as an independent judiciary or media, on what the executive authority can do. In the Chinese context, such constraints are absent both before and after the introduction of electoral reforms. Therefore our paper isolates the effect of elections.

[14] For example, Shen and Yao (2008) examines the effect of elections on inequality using a panel of 48 villages in eight provinces. Several studies have related elections to public goods. For example, Zhang et al. (2004) examines a panel of sixty villages in one province; and Luo et al. (2010) examine over 2,000 villages in six provinces. Also, Gan et al. (2014) uses the same sample as Shen and Yao (2008) to examine the relationship between elections and villager health shocks.

Our finding that village leaders appointed by the Communist Party are less effective in providing public goods than those elected by villagers is consistent with Jia (2014), which finds that connectedness between province-level and central government politicians (i.e., within the Party) is associated with less effective pollution reduction. The cumulative evidence from studies of local elections complement recent works by Jia and Nie (2013) and Lorentzen (2013) in understanding why local outcomes typical to democracies are often tolerated or encouraged by the autocratic Chinese government.[15]

The extensive data collected in the VDS and the NFS variables used in our study, which will be made public, will be helpful for future research on the political economy and development of China, as the ICRISAT and REDS data have facilitated research of the Indian economy, and the *Penn World Tables* have facilitated cross-country studies.

This paper is organized as follows. Section 2 discusses the background. Section 3 presents the conceptual framework and empirical strategy. Section 4 briefly describes the data. Section 5 presents the main results on public goods. Section 6 presents additional results on land use and income. Section 7 concludes.

2 Background

2.1 Villages and Village Governance in Rural China

A majority of the rural population in China lives in villages, which are the lowest level of government administration. Above the village government, there are the semi-equivalent levels of county and township governments, the prefecture governments, the province governments, and ultimately, the central government in Beijing. The main economic activity in villages is agriculture. The average village comprises approximately 400 households.

Village governments were first organized by the communist government during the early 1950s, with two groups of leaders in each village. First, there is the village committee. It typically consists of three to five members and is led by the village chairman, henceforth VC. Second, there is the

[15] Jia and Nie (2013) studies the effect of political decentralization on coal mining accidents, and Lorentzen (2013) studies the role of local protests.

Chinese Communist Party branch in the village. It is similar in size to the village committee and is led by the village Party Secretary, henceforth PS. Before elections were introduced, all of these positions were filled by appointment by the county government with input from the village Party branch.

One of the most important policies under the discretion of the village government is the provision of local public goods, such as irrigation, local roads and primary schools. Village leaders are supposed to decide which public goods to provide and to raise funds from villagers to finance them. Village governments do not have legal authority to impose any regular or recurring taxes. Instead, to fund the activities of the village government, including public goods, they can raise revenues by imposing *ad hoc* fees and levies, which we will henceforth refer to as *local fees* or *local taxes* for simplicity.[16]

The geographic size and social and economic diversity of China means that upper-level bureaucrats encounter enormous difficulties monitoring the activities of local officials. As a consequence of this informational asymmetry, local officials who shirked in providing public goods were typically able to maintain their positions.[17] In response, villagers often resisted paying local fees, which in turn starved local governments of funds and limited their ability to provide public goods (e.g., Oi and Rozelle, 2000; Rozelle, 1994; Whiting, 1996). This negative feedback loop further complicated the monitoring problem of the upper-level bureaucrats since they could not distinguish whether low levels of public good provision were an outcome of corruption, lack of effort by the local officials, the refusal of villagers to provide the necessary funds, or lack of demand from villagers.

2.2 Electoral Reforms

Motivation The first local elections were introduced in the early 1980s as collectives were being dismantled. The difficulties in controlling local officials were paramount in the discussions leading to the introduction of elections, as shown by this quote from the official debate.

[16] Additional responsibilities of the village government are land allocation, the management of village enterprises, the maintenance of law and order, the collection of grain taxes on behalf of the central government and the implementation of centrally mandated policies. For more discussion see Section 6.

[17] There is an abundance of examples of corrupt village officials who neglected public good provision (e.g. Brandt and Turner, 2007, Kennedy et al., 2004, Oi and Rozelle, 2000, Rozelle, 1994; and Rozelle and Li, 1998).

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

Election proponents argued that village elections could fix the agency problems that were plaguing local administration and generating discontent towards the regime at large. More specifically, elections were expected to reduce the need for the central government to monitor local officials by shifting monitoring responsibilities onto villagers. The idea was that making local officials accountable to villagers would impose checks on the VC's behavior and would also allow villagers to select the most competent candidates.[18]

The Reform The initial introduction of elections changed the positions of all village committee members from being appointed by the party-led county-level government to being elected by villagers. The main legal requirements were that: *i*) the number of candidates must exceed the number of positions; *ii*) term lengths were to be three years; and *iii*) the winner must obtain 50% of votes in the last round of voting.[19] The village committee member who obtained the highest number of votes in the last round automatically became the VC. All adult villagers had the right to vote and could abstain from voting. The village Party Branch was unaffected by the reforms and remained appointed by the upper-levels of government. There was no change either to the size of the village committee or party branch (e.g., the number of positions).

The law did not clarify the power relationship between the village committee and the Party Branch, which remained ambiguous.[20] Anecdotal evidence suggests that the power arrangements between these two bodies were very heterogeneous across villages. Indeed, in many areas the Party maintained control over villages by allowing the local Party branch to nominate the candidates. For this reason, we refer to *village leaders,* which comprise both bodies, as the subject of village decision-making. Rather than wholesale democratization, this reform is better understood as a

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

[19] Elections with multiple candidates could thus undergo many rounds of voting.

[20] As Kelliher (1997) discusses, according to the law, the village committee operates under the leadership (*lingdao*) of the Party.

marginal change intended to make the local government more accountable to villagers. Ultimately, the main change of the reform was to give villagers the power to vote unsatisfactory VCs out of office.

In these elections, there are no political parties and no slates of candidates with common platforms. Candidates are drawn from the village and are thus typically well-known by the villagers. As a consequence, candidates typically run on well-understood issues and are probably selected for qualities that have been long observed by their fellow villagers.[21]

Elections typically occurred during the lunar Spring Festival, which usually takes place between mid-January and mid-February each year.

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

The decision to introduce elections at the province-level was the result of political pressure and bargaining between the central government and the provincial leaders. However, implementation within provinces was mainly imposed top-down by bureaucratic fiat. Each level of government would pilot the reform in a few select villages, and the reform would be widely implemented once the procedures and logistics were tested (O'Brien and Li, 1999).

To understand the process and details of the reform, we conducted a large number of interviews with county- and province-level officials and conducted focus groups with village officials and prominent citizens in over a dozen villages in four provinces during the summers of 2006 and 2007. All evidence points to the roll-out as having been mostly orthogonal to village characteristics. This is consistent with the speed of roll-out within provinces. By all accounts, villages had no discretion over the timing of introduction of elections, which is characteristic of reforms in rural China.[22]

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

[22] In his detailed study of elections, Unger (2002, p. 222) writes that "These [elections] should not be interpreted

The anecdotal evidence collected by us as well as that from qualitative studies only point to two exceptions to the "quasi-random" timing of the within-province introduction of village elections. First, the pilot villages used to test electoral procedures were obviously selected to introduce elections earlier. Second, there are a few accounts of elections being delayed in "problematic" villages that had a history of non-compliance with unpopular central government policies (e.g., One Child Policy or the permanent expropriation of village land by the upper-levels of government) or had a large kinship clan that could dominate other villagers in a majoritarian regime.[23] To examine the quantitative importance of these factors for determining the timing of elections, we collected data on the allowance of One Child Policy exemptions and the incidence of upper-government land expropriations in the VDS. Later, we will examine the correlation between these variables and the introduction of elections. Afterwards, in the robustness section, we control for them explicitly to check that they do not confound our main results. Finally, we also check that our estimates are not driven by pilot or straggler villages in each province.

3 Conceptual Framework

3.1 Accountability

The anecdotal evidence described above suggests that prior to the introduction of elections, local officials could benefit from asymmetric information by shirking in their efforts to provide and maintain public goods. Elections were introduced in order to mitigate these agency frictions. Hence, if the reforms were effective in making local officials at least partly accountable to villagers, we would expect the introduction of elections to increase public goods expenditure by the village government.[24] Due to this increase in accountability, villagers should also be willing to contribute more funds to

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

[23]The role of kinship groups in elections has also been discussed by Han and O'Brien (2009) and Oi and Rozelle (2000).

[24]The theoretical basis for this claim is the political agency literature in which voters express dissatisfaction with observed outcomes by ousting incumbents in elections. Please see a brief discussion in Section 5.6.4.

the government, further improving the provision of public goods. The purpose of the rest of this paper is to investigate the effect of the introduction of elections on public goods expenditure and the mechanisms driving this effect.

3.2 Empirical Strategy

The empirical strategy used in our main analysis assumes that the introduction of elections was quasi-random once province-specific time trends and village and year fixed effects are included. Specifically, we use a *differences-in-differences* (DD) strategy, where we compare the evolution of outcomes in villages that have had their first election to villages that have not yet implemented their first election. Our baseline estimates control for village and year fixed effects. Village fixed effects control for all time-invariant or slow-moving differences between villages, such as geographic characteristics (e.g., hilliness or distance from a city) or culture. Year fixed effects control for changes over time that affect all villages similarly (e.g., national policy changes, macroeconomic growth). In addition, we add province-time trends, which control for the widening differences across regions brought about by unequal economic growth during the long time horizon of our study. Since we believe that the timing of elections is endogenously determined at the province level, but quasi-random within provinces, these trends have the additional advantage of capturing a significant amount of the cross-province variation.[25] The baseline specification also controls for the second wave of reforms that opened the nomination of candidates to villagers. This allows us to control for potential heterogeneity in the effect of elections.[26] The baseline equation that characterizes the effect of elections is

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

where the policy outcome of village v in province p during calendar year t, Y_{vpt}, is a function of: a dummy variable, E_{vpt}, that takes the value of one after the first election in village v has taken

[25] In the robustness section, we show that the magnitude of our estimates is similar, though less precisely estimated, when we control for province-year fixed effects. We also provide several other checks to make sure that province-level decisions do not drive our estimates.

[26] This improves the precision of our estimates, but does not affect the magnitude of estimated effects of the introduction of elections. For brevity, we only report results where we control for the introduction of open nominations. Results without these controls are very similar and are available upon request. Note that we do not control for other procedural differences in elections because they are more likely to be endogenous.

place; a dummy variable, O_{vpt}, that takes the value of one after the first open nomination in village v has taken place; province-year trends, $\gamma_p t$; village fixed effects, δ_v; and calendar-year fixed effects, ρ_t. Since the timing of elections was largely decided at the province level, we cluster the standard errors at the province-level. As we only have 29 provinces, we address the possibility of small sample bias in the clustered standard errors by also presenting p-values derived from wild bootstraps as recommended by Cameron et al. (2008).[27] The main coefficient of interest is β. Following the discussion in Section 3.1, we expect β to be positive if elections increased public goods expenditure.

Interpreting β as the causal effect of introducing elections does not require us to assume that election timing within provinces was random. Instead, it requires the weaker assumption that conditional on the baseline controls, the introduction of elections is not correlated with time-varying village characteristics that affect the outcomes of interest through channels other than elections. We do not take this identification assumption as given and provide a large body of evidence supporting its validity later in the paper. Before we present the main results, we present evidence that the timing of elections within provinces was uncorrelated with a large number of village-level characteristics. We will also use the data on public goods expenditure to document that there are no pre-trends leading up to the first election, and that the rise in expenditure accompanies the introduction of elections. After we present the main results, we conduct a large number of additional robustness and sensitivity checks. In particular, we show that our estimates are similar when we control for the timing of the first introduction of village elections in each province, which is the main source of potential endogeneity.

4 Data

4.1 The VDS and NFS Surveys

The primary data used in this paper for elections and public goods expenditure are from the *Village Democracy Survey* (VDS), a village-level survey conducted by the authors of this paper, where we record the administrative data kept by village governments. The first wave, conducted in 2006, records the history of electoral reforms, *de facto* leader power, public goods expenditures, the

[27] The bootstraps are estimated using 999 repetitions.

sources of funds for public goods expenditures, and the enforcement of central government policies. For public goods, the accounting methods, the categories for expenditure, and the sources of financing are all determined by the Ministry of Agriculture. The second wave, conducted in 2011, records the names and characteristics of all village leaders since 1982. To ensure accuracy of the historical data, the retrospective VDS relies on administrative records for each village when possible. When village records are not available, we rely on the recall of survey respondents, which include all current and former living village leaders and elders (e.g., teachers) in each village. This applies to very few of our variables and we will note them when relevant. The VDS forms a balanced panel of 217 villages for the years 1982-2005. However, villages only begin to record public goods data in 1986. Hence, our panel effectively covers the period 1986-2005. The main sample used in our analysis comprises a balanced panel of 217 villages from 29 provinces.[28]

The villages we survey are the same villages surveyed by the *National Fixed-Point Survey* (NFS), a detailed village- and household-level economic survey collected and maintained by a research center of the Ministry of Agriculture of China. It is collected each year beginning in 1986, with the exception of 1992 and 1994 due to administrative issues.[29] In the examination of mechanisms, robustness checks and the additional exercises in Section 6, we also use data from the NFS. We will describe these data as they become relevant.

Our data have several advantages. First, to the best of our knowledge, the VDS data are the most comprehensive data on village-level reforms ever constructed. They cover a period starting in 1982, when modern villages were defined after the Household Responsibility Reforms. In addition to recording the history of electoral reforms, we also record the timing of other major rural reforms, the occurrence of village mergers, and numerous other village-level characteristics. This allows us to control for heterogeneity across villages more comprehensively than past studies, which is

[28]There are 31 provinces in China at the end of our sample period. The two excluded provinces are Tibet and Chongqing. Tibet is excluded because it is subject to different political and economic policies. Chongqing is a city-municipality that is excluded because it did not achieve provincial status until 1997. The three other city-municipalities with provincial status (Beijing, Shanghai and Tianjin) are included in our data. Each contain a substantial rural population (30% or higher). We will control for whether a village is a suburb of a city later in the section on robustness and show that our results are not influenced by their inclusion.

[29]The NFS villages were chosen in 1986 to be nationally representative for rural China. Within each village, approximately 25% of households were randomly selected in 1986 and followed over time; new households were introduced over time to maintain representativeness. According to the Ministry of Agriculture, there is very little attrition and households and villages are mainly added to adjust for gradual demographic changes.

particularly important given the natural diversity across China. The richness of the data also allows us to provide a detailed analysis of the effect of elections on a range of outcomes and to assess the mechanisms driving the main results.

Second, the village administrative records that we surveyed in the VDS were collected contemporaneously. Hence, we avoid recall bias. Third, since the format of village records were mandated by the Ministry of Agriculture, the data are easily comparable across villages. Finally, the panel structure of the survey allows us to control for village fixed effects and province-year trends.

The main drawback of our data is that relative to the period through which elections were rolled out, the panel is short. This limits our ability to observe the long run effects of elections.

All observations in the empirical analysis are at the village-year level. We will describe the variables as they become relevant.

4.2 Descriptive Statistics

4.2.1 The Timing of Elections

Several pieces of descriptive evidence are consistent with the anecdotal evidence on the timing of electoral reforms discussed in Section 2. First, the data show that there is substantial within-province variation in the timing of the first election in each village. In a village-level cross-sectional regression, when we regress the year of the first election on province fixed effects we find that the R-squared is 0.33. Thus, approximately 67% of the variation in the timing of elections is within province. This is important for our empirical strategy, which largely relies on this variation.

Second, the timing of the rollout is consistent with rapid top-down implementation within provinces and counties. Our data indicate that 16% of villages held their first elections prior to the official introduction of elections by the county government, 66% held their first elections the year that the county introduced elections, and 18% held their first election afterwards.[30] Table 1 shows that the average village implemented its first election within the same year as the official in-

[30]Note that the timing of the official introduction of elections in each county is based on respondent recall. To maximize accuracy, our surveyors only record a date if all respondents surveyed in a given village agree. If there is no consensus, this variable is recorded as missing. Since provinces are large and respondents could not confidently recall the year of the first election within a province, the date of province-level introduction is inferred as the year of the first election in each province according to our survey.

troduction of elections in its county and five years after the first election in the same province. Since the 29 provinces of our sample include approximately 2,885 counties and 623,669 rural villages (as defined by the number of village governments, *cunming weiyuanhui*), these statistics imply that the average county was able to introduce elections in 143 villages within one year.

Third, the fact that a small number of villages implemented elections before and after the official introduction in each county is consistent with the anecdotal evidence that each administrative division typically piloted the reform before officially introducing it and also delayed elections in a few villages. Hence, given our identification assumption, it is important to check that our baseline estimates are not driven by the early movers or the stragglers, which we do in our robustness exercises.

In addition, we can provide direct evidence that the timing of the first election is uncorrelated to most pre-reform village characteristics. Since we are interested in within-province variation, we demean the year of the first election and village characteristics by province fixed effects. We then estimate bivariate regressions of the residualized election timing on each of a large number of residualized village characteristics such as village size, proximity to an urban area, proxies for social and economic structure, measures of the pre-existing level of public goods provision and other outcomes of interest. We measure all these village characteristics in the first year that data are available.[31] The sample for this estimation is therefore a cross section of villages. For brevity, we present the results for village demographic and physical characteristics, the main outcome variables of our regression analysis, and the unpopular upper-government policies that we discussed in Section 2.[32] Since it is difficult to compare magnitudes across different regressors, Table 2 presents the standardized coefficients for each regression. Only one of the correlates is statistically significant. Given the large number of correlates that we examine (we examine over eighty additional insignificant socioeconomic correlates that are not presented in the paper for brevity), the significance of one correlate

[31]Most variables reported by the NFS are available starting in 1986. Land variables are available starting in 1987. Measures of the One Child Policy and upper-government land expropriation from the VDS are available starting in 1982. The results are similar if we measure the latter two variables in 1986. The results are also similar if we measure all variables as the average of the first two years for which they are available. These alternative results are available upon request.

[32]See the Online Data Appendix for a description of the variables: *upper-government land expropriation* and *One Child Policy exemptions*, which we collect in the VDS.

is not necessarily meaningful. Nevertheless, in the robustness exercises later in the paper, we will control for this and other potentially confounding variables.

The data also provide several pieces of descriptive evidence to suggest that elections were effectively implemented. We find that 79% of elections had more candidates than positions, as the law required. Most of the elections with too few candidates were the first elections in their villages, and were all immediately followed by fresh elections in the subsequent year. This is consistent with the view that opponents to the electoral reform were unable to fully derail the introduction of elections, and with qualitative accounts of dissatisfied villagers demanding and obtaining recalls (O'Brien and Li, 2006). Table 1 shows that, as legally required, elections occur every three years on average.[33] Finally, and not reported in the table, we find that there was a 38% VC turnover rate for the first election, which is more than twice as high as the average turnover rate in the sample (17%).

4.2.2 Trends in Public Goods Expenditures

Before presenting the regression results, we examine the raw data on public goods expenditures and provide evidence for the parallel trends assumption. Villages record public goods expenditures as the sum of expenditures on seven categories that are defined by the Ministry of Agriculture: irrigation, primary schools, sanitation, within-village roads, electricity, the environment (e.g., planting trees), and "other".[34]

Our DD strategy assumes that in the absence of elections, the outcomes of villages that introduced elections earlier would have evolved along parallel trends with the outcomes of villages that introduced elections later. Since it is impossible to observe the counterfactual trend, we follow the literature in conducting a pre-trend analysis. The presence of a trend in the years leading up to the introduction of elections would suggest that late reformers evolved on different trends from early reformers and cast doubt on our identification strategy.

Our data allow us to separately examine expenditures according to the source of funds, which we categorize into funds from village sources and from outside the village (mostly upper government

[33] Note that there is variation in this variable (the standard deviation is approximately one year), which mitigates the concern that village records report elections as they are supposed to occur rather than when they actually occur.

[34] In addition to public goods expenditures, village government expenditures also cover other items such as salaries of local cadres and expenditure on festivals and celebrations. In our data, public goods expenditures account for approximately 27% of total village government expenditures.

transfers). Since according to our discussion in Section 3.1, an increase in accountability should only affect the amount of expenditures financed by villagers, we focus on this outcome first.

To investigate the presence of pre-trends we would like to plot average expenditure in public goods for each year before and after the first election. However, if we use the full sample of villages, the patterns observed could suffer from a potential confounding factor: each year average comprises a different set of villages. This is because elections were staggered in timing across villages. While this is an advantage for our identification strategy in that it allows us to control for calendar time effects, it also means that when we reorganize the data according to the number of years since the first election, the interpretation of the figure may be confounded with composition effects.

To address this, we restrict the sample to keep a constant set of villages. We do this for several different windows since the villages in the sample change as we adjust the window. For brevity, we present the figures for two windows for which we can observe a symmetric number of years before and after the first election. Figure 1a plots average expenditures from two years before the first election until two years after the first election (since elections have three year terms, this is the end of the first term after the introduction of elections). Year 0 on the x-axis indicates the year of the first election.[35] Figure 1a shows that on average, villages make large investments one year after the reform. Figure 1b extends the window to begin nine years prior to the first election until nine years after the first election. This reduces the number of villages in the sample. However, the pattern is similar to the pattern from the full sample. There is very little activity prior to the introduction of elections and public goods expenditures increase beginning one year after the first election. Expenditures are not made every year, and in between large expenditures, there are smaller expenditures. The latter is consistent with the fact that several of the public goods in our data will require large fixed costs to construct (e.g., irrigation, roads), and smaller expenditures in between for maintenance.

Three facts emerge clearly from these figures. First, there was very little activity in public goods expenditures prior to the introduction of elections and the pre-reform level is close to zero. Second, the level of expenditure and the amount of activity increased with the introduction of elections.

[35]Throughout the paper, we report real values that are deflated using the official province-specific rural CPI. We use the conversion rate of 7 RMB = 1 USD, which was the historical average for the period we study.

Finally, the lumpy nature of public goods expenditures translates into the noise observed after the first election.

These figures also illustrate the variation driving our empirical estimates. The main post-reform estimate from equation (1) is essentially the difference in the average expenditures in the pre-reform and post-reform periods shown in the figure (after controlling for village and year fixed effects, and province trends).

Figures 2a and 2b display average public good expenditure financed by the upper government for the same restricted samples as Figures 1a and 1b. We see no increase after the introduction of elections. This shows that only expenditures financed by villagers change as a result of elections and supports the claim that the introduction of elections was not accompanied by other changes in government policy.

5 Public Goods

5.1 Main Results

Table 3 presents the effect of elections on public expenditures from estimating equation (1). For all estimates we report standard errors that are clustered at the province level in parentheses and wild bootstrapped p-values in square brackets immediately below.

Panel A presents the regression estimates where the dependent variable is total public goods expenditure of the village. Before presenting the baseline equation, we first present estimates where we control for only village and year fixed effects. Column (1) shows that the coefficient for the post-first election dummy variable is 18.4. In column (2), we show that the effect on public goods financed by villagers is similar in magnitude, 16.4. Both estimates are statistically significant at the 10% level. The bootstrapped p-values indicate slightly less precisely estimated. In column (3), we find that there is no effect on public goods financed by other sources. The coefficient is small and statistically insignificant. The top row, which reports the means, shows that, consistent with the anecdotal literature, village leaders are responsible for raising most of the funds required for village public goods – approximately 70% of total funding for village public goods comes from village sources.

In Columns (4)-(6), we add province-year trends. The coefficients are similar, but the estimates for all public goods and public goods financed by villagers are now significant at the 5% level. The bootstrapped p-values are similarly precise. The increase in precision is likely to be due to the fact that the province trends control for the significant social-economic divergence across regions during the period of our study. This is our baseline specification. The estimate for public goods financed by other sources (column 6) continues to be statistically indistinguishable from zero.

For brevity, and to be conservative, we will refer to the bootstrapped p-values when discussing statistical significance in the remainder of the paper.

Column (4) in Table 3 shows that the introduction of elections increased total public goods expenditures from all sources by approximately 153,110 RMB (22,044 USD). This is a large increase relative to the sample mean of 138,100 RMB (21,873 USD), which is rather low due to the negligible expenditures in the early years of the sample. To assess the magnitude another way, we can compare it to the average increase in total public goods expenditure between the first and last years of our sample (1986 and 2005). This was 483,499 RMB (69,071 USD). Thus, the introduction of elections explains 32% of the total increase observed in the data. The result is consistent with our hypothesis that elections changed the incentives of village leaders and led them to exert more effort and be more responsive to villagers' demands for more public goods.

In Panel B, we examine an alternative dependent variable: a dummy variable which equals one if any public goods expenditure is made in a year. This examination is motivated by Figure 1, which suggests that the introduction of elections not only increased the level of expenditure, but also the frequency of expenditure. Public goods expenditure data turns out to be very lumpy. Villages do not make any public goods investments in most years. The means at the top of Panel B show that only in about 22% of the village-year observations in our sample public investments are positive. Consistent with the fact that most investments are funded by villagers, we see that village-financed investments are made in seventeen percent of the sample, while upper government funded investments are made in only seven percent of the sample.

We focus our discussion of the results on frequency on the baseline specifications in columns (4)-(6). Column (4) shows that the introduction of elections increased the probability that any

expenditure is made during a year by 5.7 percentage points. The estimate is statistically significant at the 10% level. Column (5) shows that for expenditures financed by villagers, the introduction of elections increased the probability by 6.3 percentage points. The estimate is statistically significant at the 5% level. Column (6) shows that there is no effect on the frequency of expenditures financed by other sources.

These results show that elections increased both the level and frequency of public goods investments, and that the increase is driven by villager financing. The latter is important for three reasons. First, it demonstrates that elections affect policy at the village level, since the responsive funds are under the control of village leaders. Second, it contradicts the notion that "democratically" elected leaders cannot raise revenues for public goods in our context. Third, it contradicts the alternative interpretation that the estimated effects of the introduction of elections are driven by an increased willingness of the upper-government to fund local public goods, since direct transfers are the most immediate policy tool that upper levels of governments would use to affect local public good provision.

5.2 Alternative Measures

One may wonder whether the increase in total public goods expenditure (funded by villagers) is driven by an increase in village population, or whether the large magnitude of the increase in total expenditures is plausible. We investigate these two questions by examining expenditure per household and per worker as dependent variables. The NFS data do not record population. Instead, they record the number of households and workers. Columns (2) and (3) of Table 4 show that the introduction of elections increased expenditures by 180 RMB (25.7 USD) per household and 110 RMB (15.7 USD) per worker. Both estimates are statistically significant at the 5% level.

These results show that the increase in total expenditures due to elections was not entirely driven by population increases. They also show that the magnitude of the increase is very plausible. Median household income in our sample is 10,126 RMB. The estimate in column (2) thus implies that the increase in public goods expenditures is approximately 1.8% of household median income ($180/10126 = 0.018$).

Next, we examine the logs of expenditures and per capita (per household and per worker) expenditures. The estimates are positive and statistically significant at the 5% level in columns (4)-(6). Thus, our main result that the introduction of elections increase public goods expenditures is not sensitive to the functional form.[36]

5.3 Interpreting the Magnitude

Note that the estimated levels in column (1) of Table 4 yields a coefficient that is about 170% of the sample mean, while in column (4), we obtain an effect of 44 log-points, which is equal to approximately 55%. This is consistent with the size of investments varying widely across villages (for the 20% of observations that experience positive expenditures, the mean expenditure financed by villagers is 570,776 RMB and the standard deviation is 2,846,690 RMB). Thus, one may be concerned that the estimated average effect of elections on the level of investment might be driven by outlier observations that make particularly large investments.

To investigate this possibility, we omit observations where expenditures are within the top one percent of the sample. Table 5 shows that this lowers mean expenditure from 94,560 RMB for the full sample in column (1) to 23,860 RMB for the restricted sample in column (2). Not surprisingly, the coefficients show that the extent to which elections increases the level of expenditure is also lower in the restricted sample. The coefficient declines from 16.08 for the full sample to 1.121 for the restricted sample. Both are statistically significant at the 5% level.

One explanation for the change in the estimates is that the effect of elections is proportional. To investigate this possibility, we examine log expenditure as the dependent variable. The estimates in columns (3)-(4) for the full sample and restricted sample are 44 and 40 log-points respectively (which correspond to an increase of 55% and 49%, respectively).[37] These coefficients are very similar in magnitude and both are statistically significant at the 5% level. The stability of these

[36] We compute the log of total public goods as the log of total public goods + 0.1 in order to retain observations with zero public good investments. Note that the increase in log total public goods expenditure in column (4) is much larger in magnitude than the increase in log per household or log per worker expenditures in columns (5) and (6). This is most likely due to the aging of the baby boom cohort (rural fertility during the 1950s-1970s was very high). Over the period of our study, this will increase the number of working age adults as well as the number of households in each village (as children grow up, marry, and move out of their parents' homes). Note that because of policy restrictions, there is very little permanent rural migration in rural China.

[37] Column (3) in Table 5 replicates Column (4) in Table 4, for ease of comparison.

coefficients suggests that the average effect of elections is best captured in proportional terms – i.e., elections increase public goods expenditures financed by villagers by approximately 50%.

Another way to normalize the effect of elections is to report standardized coefficients. Columns (1)-(2) show that the standardized coefficients are also stable across the different samples. They show that a one standard deviation change in the timing of the first election causes approximately a 0.04 to 0.05 standard deviation change in public goods expenditures.

5.4 Dynamic Effect

In Table 6, we investigate whether the effect of elections persisted after their initial introduction by estimating the level of public goods expenditures after the first term. Column (1) re-states the full sample estimates for comparison purposes. Column (2) omits the year of the first election. Column (3) additionally omits the year afterwards. Column (4) additionally omits two years after the first election. Since elections terms are three years, column (4) effectively omits the entire first term after the reform. The precision of the estimates declines as we reduce the sample size. However, the estimate in column (4) is very similar to the full sample estimate in column (1) and we conclude that the effects of the reform are not exclusive to the first election term.

In columns (5)-(8), we repeat this exercise for the frequency of investments. As with the estimates on levels, these coefficients are similar across sample restrictions. They are always significant at the 10% level or higher.

5.5 Robustness

There are three main concerns for our empirical strategy. The first concern is that despite controlling for province time trends, our baseline results are partly driven by cross-province variation in timing, which is determined by provincial leaders for potentially endogenous reasons. We address this issue in three ways. First, to control for the province-level timing of the decision to introduce village elections, we add a dummy variable that indicates whether any village in a given province has introduced elections (Table 7 column (2)). The results are similar to the baseline estimates, which are displayed in column (1) for ease of comparison. Hence, our results are not an outcome

of province-level variation in the timing of the introduction of the reform, which is the main source of endogeneity concerns. A second way of accounting for province-level factors is to introduce province-year fixed effects instead of province-year trends. This allows the influence of province characteristics to vary flexibly over time. Column (3) shows that this stringent set of controls reduces the precision of our estimates. However, what is important is the fact that the coefficient is nearly identical in magnitude as the baseline specification. Finally, we control for province-level variables such as per capita GDP, per capita agricultural GDP, and per capita government expenditure in public goods.[38] In column (4), the coefficients with these controls are statistically similar to our baseline.

The second main concern is that our estimate could be driven by pilot or straggler villages in the reform implementation (see Section 2), which may have been chosen endogenously. In column (5), we repeat our estimate on a restricted sample where we drop pilot and straggler villages.[39] The estimate is similar to the baseline.

The final concern is that there may be village-specific and time-varying determinants of the introduction of elections that are not controlled for by the baseline controls and that affect the outcomes of interest through channels other than elections. The strongest evidence against this concern was shown earlier in Section 4.2. Recall that Table 2 showed that the timing of the introduction of elections is uncorrelated with most observables features of the village. Furthermore, Figure 1 documented that there was no pre-trend in public goods expenditures and that the rise in public goods expenditures occurs with the introduction of elections.

Nevertheless, one may still be concerned that the timing of the introduction of elections is correlated with pre-conditions that affect our outcomes of interest through channels other than elections. We address this concern by directly controlling for the interaction term of the village characteristic that was significantly correlated with the timing of the introduction of elections – the occurrence of upper government land expropriation (see Table 2) – and the full vector of year fixed effects. Given

[38] These data are reported by *China Statistical Yearbooks.*

[39] A pilot village is defined as a village that implemented elections more than two standard deviations before the average timing of villages in the same province. A straggler village is defined as one that implemented elections more than two standard deviations after the average timing of villages in the same province. If all of the villages in a province implement elections in the same year, then that province will have no pilot or straggler villages.

the anecdotal evidence on the delay of elections for villages with a history of non-compliance to unpopular central government policies, we also control for the interaction of whether there were exemptions to the One Child Policy and year fixed effects. These characteristics are measured in the first year that data are available (1982).[40] The interaction with year fixed effects controls for the influence of these variables over time in a fully flexible manner, and to some extent, it also controls for the influences of all of its correlates over time. Column (6) of Table 7 shows that the coefficient with these additional controls is very similar to our main estimates.

We also consider the possibility that several other village-level factors could potentially confound the effect of elections on our outcomes of interest. These factors include whether a village is a suburb of a city (a dummy variable for being in a suburb interacted with year fixed effects), whether the *Tax and Fee Reform* had been implemented (a dummy variable which takes the value of one if the reform has been introduced), and the level of village social capital (a proxy for social capital interacted with year fixed effects). To proxy for the latter we follow Tsai's (2007) work in using the presence of a lineage group (i.e., an ancestral hall, family tree), village temple, or a large kinship group to proxy for social capital.[41] To maximize the statistical precision of our estimates, we use the principal component of these three measures as our social capital proxy. Column (7) shows that the resulting coefficient is similar to the baseline.

In column (8), we re-estimate the baseline on a sample restricted to villages that never experienced an administrative merger with another village since 1982. This addresses the possibility that our main results are somehow confounded because the probability of having experienced a merger is correlated with the timing of the electoral reform, and whether a village experiences a merger is correlated with some factor that can affect our outcomes of interest. The coefficient is also similar to the baseline.

Additional Sensitivity Checks We conduct many additional sensitivity checks that are not presented here for brevity.[42] For example, we check that our estimates are not driven by selection

[40]Note that if we measure these policies in 1986, we obtain similar results. These alternative estimates are available upon request.

[41]To measure the size of the kinship groups, the VDS recorded names from the village roster.

[42]They are available upon request.

within counties by instrumenting for the introduction of elections at the village level with the introduction at the county level or with the introduction at the province level. We also control for public goods expenditures in 1986 (the first year that data are available) interacted with year fixed effects to control for the fact that villages with different levels of public goods in the base year may evolve differently over time; or the average annual increase in public goods expenditures between 1986 and 1988 for each village interacted with year fixed effects to control for the fact that villages with different trends in public goods in the base years can evolve differently over time. All of these robustness exercises produce estimates that are similar in magnitude to our baseline results.

The results in this section show that the baseline results are very robust and unlikely to be confounded by other factors. They support the interpretation of the estimates as capturing the causal effects of the introduction of village elections.

5.6 Mechanisms

In this section, we provide evidence on the policy mechanisms and theoretical channels through which the introduction of elections increased expenditure on public goods.

5.6.1 Local Taxes

In addition to the village-level data used in the main analysis, we were able to obtain household level data for a subsample of villages of the NFS survey. This subsample comprises 160 villages in eighteen provinces. The observable characteristics of these villages do not differ significantly from the full sample.[43] The disaggregated expenditure data report the fees and levies paid by households. Thus, we can use these data to examine whether the increase in public goods expenditure from villagers was accompanied by an increase in the amount of fees and levies paid by households.[44]

On average, households in the subsample pay 198 RMB per year in local fees, which is approximately two percent of total household income.[45] At the top of Table 8, we present mean household

[43] See Online Appendix Table A.3 for a comparison of observables.

[44] Unfortunately, the data only report the sum of local fees paid to the village and county governments and do not distinguish payments to the village government. Therefore, interpreting the following results requires the assumption that elections did not change the taxes paid to county governments. To the best of our knowledge, this was the case. The main reform affecting local fees was the *Tax and Fee Reform*. The VDS documents the introduction of this reform which occurred towards the very end of our sample. Controlling for the introduction of this reform has no effect on our results.

[45] According to our data, this amounts to 64% of the fees that households pay to levels of government above the county (excluding the value of grain taxes). The largest proportion of taxes that agricultural households pay to the central

payments of local fees as a percentage of total household income – i.e., the fee rate – according to households' positions in the income distribution. Below that, we present the effect of the introduction of elections on the fee rate. The coefficients for post first election are all positive and statistically significant at the 10% level or higher. They are similar in magnitude for households in different quintiles of the village income distribution. The results indicate that the introduction of elections increased the amount of local fee rates by approximately 0.3 to 0.5 percentage points. This is almost one-fourth of the sample mean rate of two percentage points.

These results support the idea that local tax revenues increase when villagers feel the government is more accountable. At the same time there is no evidence of progressivity in local fees. Together with the low levels of collection, the lack of progressivity suggests that while local taxes can be used to fund local public goods, they are not an important tool for income redistribution.

5.6.2 Signature Rights

To support our interpretation that elected VCs increased public goods expenditures to appease voters, we document that VCs had power to make public goods investments and that this power was not undermined when elections were introduced. To do this, we document the "signature rights" of VC and PS on several relevant policies. Village policy decisions are recorded in writing, and each decision is approved with the signature of one or more officials. The VDS copied the village records to document whether public goods expenditures by the village government were approved by the VC, PS or both. The means in Table 9 show that 19% require only the VC's signature for making public goods expenditures, 66% require both the VC and PS, and 15% require only the PS's signature. When we estimate equation (1) with these variables as dependent variables, we find that the introduction of elections increased the VC's unilateral power. In column (1), we see that the probability that only the VC's signature was required increased by 4.2 percentage points. The coefficient is statistically significant at the 10% level. At the same time, columns (2)-(3) show that elections reduced the probability that both signatures were required, and also reduced the probability that only the PS's signature was required. However, these estimates are not significant at conventional lev-

government is in the form of grain, which the government buys at a below-market price set by it. The value of this tax payment is not included in the NFS category of taxes to the central government.

els. These results suggest that the local Communist Party branch did not circumvent the reform by taking power away from the VC. If anything, the introduction of elections seems to have increased the VC's capacity to decide on relevant policies such as public goods, which is consistent with the notion that elections gave VCs a democratic mandate.[46]

5.6.3 The effect of Elections on Leader Characteristics

Anecdotal and qualitative evidence suggest that the timing of electoral reforms did not coincide with changes in the village Communist Party branch. Such simultaneous change would confound our interpretation of the results as the consequence of increased accountability of the newly elected leader in the village. We can substantiate the anecdotal evidence by comparing the effect of the introduction of elections on the characteristics of the VCs, who were affected by the electoral reform, to those of the PS, who continued to be appointed by the Communist Party after elections were introduced. If there is no change in the Communist Party, then PS turnover and characteristics should not change with the introduction of elections. Table 10 Panel B shows the estimates from regressing PS turnover rate, age and years of educational attainment on the baseline explanatory variables. Elections have no effect on any of these outcomes. In contrast, Panel A shows that elections increased turnover rate, reduced age, and increased the years of educational attainment of the VC. The estimates are significant at the 5% level or higher.

The comparison of the results for VCs and PSs support the view that reforms were circumscribed to the VC and is consistent with the anecdotal literature which emphasizes that the introduction of village elections was not accompanied by other changes at higher levels of government.

5.6.4 Political Agency Channels

In the literature on political agency, citizens use elections to control politicians through two mechanisms.[47] First, elections can help voters address moral hazard problems by rewarding good performance with re-election – i.e., elections serve as means to provide the correct *incentives* to office

[46] We also document the signature powers over the management of village enterprises, the right to reimburse expenditures from public funds, and land reallocation. The data show that VCs had de facto power over village policies that are relevant to the outcomes we examine later in the paper (income, land, etc.).

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

holders. Second, voters can use elections to *select* the politicians that are more competent or whose preferences are better aligned with citizens' preferences. In this section, we explore whether these forces are relevant in our context.

We first recall the results discussed in the previous subsection (Table 10 Panel A). The finding that the introduction of elections changed the characteristics of the average VC supports the notion that villagers used elections to select a different type of leader from the type that was appointed prior to the introduction of elections. This is consistent with the presence of the selection mechanism that is highlighted in the theoretical literature.

To investigate the presence of an increase in incentives, we examine whether the introduction of elections had any effect in villages where there was no leader turnover in the first election – i.e., we estimate the interaction effect of the introduction of elections and whether there was turnover for the first election. The impact of elections in villages where there was no turnover isolates the effect of re-election incentives, since the presence of the same official before and after elections rules out the possibility that changes in policy are due to villagers having selected a better official.

Table 11 presents the results. We introduce the interaction of the introduction of elections with a dummy variable that equals one for villages where there is VC turnover in the first election. In this specification, the effect of elections on villages that experienced no turnover (64% of the villages in our sample) is captured by the uninteracted post-first-election coefficient. To address the fact that the weights across villages change depending on how we transform the dependent variable, we show the results for total public goods expenditures, per household public goods expenditures, and the logs of each of these measures.

For all of the outcomes examined, the sign of the uninteracted post-first-election dummy variable is positive, and is similar or larger in magnitude as the average effect shown in Table 4. This implies that the re-election incentives contributed to the average effect of introducing elections on raising public goods expenditure. The estimates are statistically significant at the 10% or higher level for total expenditures and expenditure per capita in columns (1) and (2). However, these results become insignificant when we estimate the standard errors using the wild-bootstrap method. We also note that the magnitude and precision are sensitive to functional form.

Together with our earlier finding that elections changed the characteristics of village leaders, the results are suggestive that both selection and incentive effects were present in our context. Note that we cannot use the interaction effects presented in Table 11 panel B to quantify the relative contribution of the incentive and selection mechanisms due to the fact that whether there was VC turnover in the first election is endogenous. For example, elections could cause leader turnover where villagers do not have particularly high demand for public goods.

6 Additional Results

In this section, we investigate whether the introduction of elections affected rent seeking and pro-elite bias in village policies, which should in principle decline if elections increased accountability. We note that the evidence for rent-seeking is suggestive since we do not directly measure corruption, and instead infer it from proxies; and that the results on income will use both the full sample of villages as well as a subsample for which we have household-level data. For these reasons, we interpret the results in this section as suggestive and supplementary to the main results for public goods. For brevity, we only present the baseline results in the paper. We subject the results in this section to a similar set of robustness checks as the public goods estimates. Please see Online Appendix Tables A.2 and A.6.

6.1 Land Use

In addition to public goods, local officials are also responsible for the use and allocation of collectively owned means of production. The most important asset is arable land, all of which is publicly owned in China. Most land is allocated to households for farming in long-term contracts.[48] A fraction of land, no more than fifteen percent according to national law, may be retained under the direct control of the village government so that it can make small adjustments to household allocations without implementing a large-scale reallocation for the entire village. During the 1980s and 1990s, land retained by the village government was often leased to highly profitable village enter-

[48] Rural households cannot sell their land rights in China and, during the period of our study, were also prohibited from renting out their land. In most cases rural households were also restricted from hiring laborers because households that did not farm their own land would lose land rights. See Jacoby et al. (2002) for a related study about tenure security in rural China.

prises. However, villagers typically dislike this practice, because they suspect it is a source of rents for the village leadership and its cronies.[49]

The village leadership and other village elites likely captured most of the rents from running village enterprises (only the top half of villagers shared any profits from enterprises). Therefore, the introduction of elections may be expected to reduce the amount of land leased to enterprises because voters wish to reduce pro-elite bias in village policies.

Data for the use of village land is reported by the NFS for all villages for the years 1987-2005 (excluding 1992 and 1994). The villages in our sample use approximately 96% of arable land (approximately 51% of total village land) for household farming. Approximately 75% of the remaining arable land is leased out to "enterprises", a term which we use for firms run by collectives or villagers. Since elections can only reduce the amount of land leased out to firms if such land existed prior to the first election, we restrict our analysis to villages that ever used any arable land for non-household farming prior to the introduction of the first election. This reduces the sample to 108 villages from 28 provinces.

The estimates are displayed in column (1) of Table 12, where we use the same specification as in equation (1). It shows that the introduction of elections reduced the amount of land that is leased out to enterprises by approximately 57.7 mu (3.85 hectares), where the sample average is approximately 108.5 mu (7.23 hectares). The estimate is statistically significant at the 10% level.

6.2 Income

If the introduction of elections caused a reduction in the pro-elite bias of policies under the discretion of the village leadership, then we should find that it also caused income losses for elite households. To examine this possibility, we look at the effect of elections on household income across the village income distribution.

We have data on *total household income* for households on the 10th, 25th, 50th, 75th and 90th percentiles of the within-village-year income distribution for the full sample of villages for 1986-2005 (excluding 1992 and 1994). All income measures are deflated using a province-specific rural

[49] Consistent with this view, in a cross-sectional study Brandt and Turner (2007) find that redistributing collective land to the direct control of villagers is positively correlated with re-election probabilities.

CPI. In the top row of Table 12, we present the mean household income for each group to illustrate the extent of income inequality in villages. In our sample, the richest households (90th percentile) typically earn twice as much as the median households (50th percentile), which earn about twice as much as the 10th percentile households. There is substantial inequality within villages. This can be the result of cronyism if the richest households are benefitting from their connections to the village leadership. At the same time, it could also be a consequence of some households being entrepreneurial and successful and thereby experiencing more rapid income growth during the early reform era than less productive households. These two causes of inequality are, of course, not mutually exclusive.

To examine the effect of the introduction of elections on income distribution, we estimate the baseline equation (1) with the household income of each percentile of the village income distribution as the dependent variable. The coefficients in columns (2)-(6) of Table 12 are progressively more negative and larger in magnitude for richer households, which is consistent with the introduction of elections reducing incomes by larger amounts for richer households.

In column (7), we examine the ratio of the median household income to the 90th percentile household. The estimates show that elections increased the income of the median household relative to the 90th percentile household by 1.7 percentage points. The estimate is statistically significant at the 5% level. Relative to the mean income ratio of 0.53, the increase is moderate in magnitude.

These results suggest that upon the introduction of elections, the village leadership changed economic policies in such a way that the former economic elites were less favored. The relative drops of income only affect the richest households, which is consistent with the interpretation that they are a result of policy changes designed to please the majority of villagers.

The welfare implications of this reduction in inequality are unclear and partly depend on the origins of pre-reform inequality. Moreover, since we do not observe a significant increase in income at any level of the income distribution, we cannot rule out the possibility that the changes due to the introduction of elections increased economic inefficiencies. For instance, it is very much possible that village enterprises were creating surplus, but the lack of benefits for the majority of villagers coupled with lack of commitment on how to share the surplus might have led villagers to prefer

direct access to land.

6.3 Policies for Redistribution

In this section, we provide additional evidence on the means through which village leaders reduced elites' incomes. For this analysis, recall that village governments cannot impose recurrent taxes and that the lack of progressivity on local fees suggests that taxes are not the main instrument for redistribution.

First we document that household income mostly comes from agriculture (67%); wages (15%), which are mostly earned from employment with village enterprises; and village enterprise profits (7%).[50] The village government controls household access to land, which is the main input for agriculture, and as we discussed earlier, also controls the management of village enterprises. Thus, the data show that about 90% of average household income can be influenced by the village government. If the VC desires to engage in limited redistribution, he can reallocate land, salaried jobs or dividend income across households.

In Table 13, we present the estimates for the effect of the introduction of elections on each source of income. For brevity, we focus on the median-to-top income ratios. Column (1) presents the result for total income, which is comparable with the result for the full sample in Table 12 and statistically significant at the 5% level. Columns (2)-(4) show that the introduction of elections had similarly sized effects on each source of income; the ratios for agricultural income, wage income and enterprise (profit sharing) income all increase by around two percentage points. The estimates for agricultural and enterprise income are significant at the 5% level.[51]

The results on land and income provide suggestive evidence that the introduction of elections curbed rent-seeking and reduced income inequality by reducing the income of the richest households of the villages. The change in income inequality is moderate in magnitude and achieved

[50] See Online Appendix Table A.4 for a balance sheet for average household income and expenditure. Village enterprises were officially called different names at different stages of the early reform era due to regulatory changes regarding the ownership of former collective assets. Hence, our category of income from village enterprises is the sum of all of these types of incomes reported in the NFS (e.g., income from collectives, partnerships or cooperatives, and enterprise dividends). Income from collectives and enterprise dividends make up over 97% of this category.

[51] Note that we also examine household farmland. The estimates for the median-to-top ratios of household farmland are positive but statistically insignificant. The lack of precision is likely to be partially due to our inability to measure land quality. For example, a household may prefer to have a small piece of fertile land over a large piece of poor quality land.

through the manipulation of income streams that the village government can influence through its control over land allocation and village enterprises. These results are consistent with the fact that the village government could not impose recurrent taxes, which means that elected leaders could not easily commit to ex-post income redistribution. Since productive assets might have originally been allocated to productive households, this mode of redistribution may be one of the reasons why we do not find that local elections in China increased income. We discuss other possible reasons in the conclusion.

7 Conclusion

In order to better control local officials and prevent shirking, the Chinese government introduced village-level elections starting in the 1980s. These reforms were partial as the local branch of the Communist Party was not affected. In this paper, we examine whether the introduction of elections in such an autocratic context can be successful in changing incentives of local officials. We find that elections prompted local officials to dramatically increase expenditures in public goods. This increase was financed almost entirely by funds raised directly from households. This suggests that elections were successful in making local officials more accountable to villagers and that the latter became more willing to fund the local government in return for more public goods provision. In addition, we find some evidence on land use and income distribution to suggest that elections might have also curbed rent-seeking and reduced the pro-elite bias in village policies.

To interpret our results, it is important to note that the quantitative magnitudes of the empirical estimates are specific to the context of our study. For example, the large positive effect of elections on public goods is likely to be partly due to the severe under-provision of public goods in rural areas during the early reform era. Similarly, the moderate size of the effect of the introduction of elections on income inequality is likely to be due to the fact that village governments cannot impose recurrent taxes. It is reasonable to speculate that if village leaders had access to the standard policies for redistributing income, we might find much larger reductions of inequality from elections.

At the same time, our qualitative results provide generalizable insights. The finding that local elections improve public goods is consistent with the idea that elections are powerful tools for con-

trolling local officials. Our finding that electoral control can dominate bureaucratic control even in China is striking given that this is a context long known for its efficient and entrenched bureaucracy. It is perhaps not surprising that several autocratic countries have introduced similar reforms in recent years.

To the extent that this institutional reform can be understood as a marginal increase in democracy in rural China, our results are consistent with recent democratization theories that characterize democracies as regimes that are likely to engage in redistribution (e.g. Acemoglu and Robinson, 2006) and to provide more public goods (e.g., Bueno de Mesquita et al., 2003).

This study and the data it provides open several interesting avenues for future research. First, we note that autocratic central governments are interested in maintaining political control. This suggests that a partial introduction of representative institutions such as village elections might be an unstable institutional equilibrium. While elections can improve citizen satisfaction with the regime by improving local governance, they can also help villagers coordinate to resist unpopular policies designed by upper-levels of government. We explore this in a companion paper, Martinez-Bravo et al. (2011). Another important question regards the heterogeneous effects of elections, which can potentially provide insights into the pre-conditions for successful representative institutions. For example, in subsequent research, Padró i Miquel et al. (2014), we investigate the roles of social fragmentation and social capital in determining the effects of elections on public goods. Finally, it is important to understand when local elections are a step towards wider regime change versus when they are used as an instrument of control by the central regime. [52]

[52] This is related to the recent work of Cantoni et al. (2014), which investigates whether a reform in Chinese high school political curricula led to long-run changes in political attitudes.

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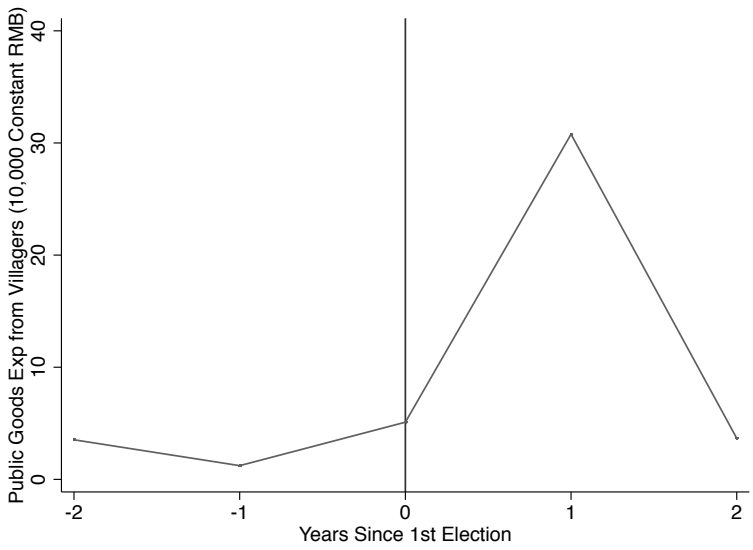
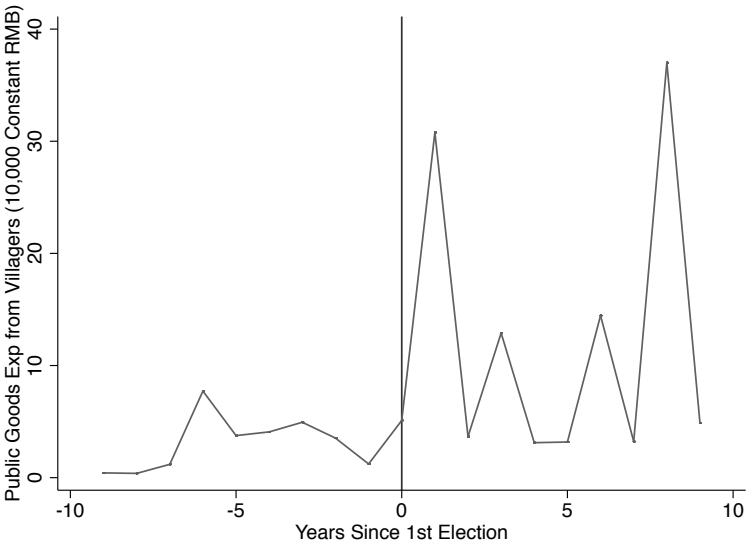
Figure 1: Public Goods Expenditure by Villagers vs. the Number of Years Since the 1st Election – Restricted Sample

(a) Two-Year Window



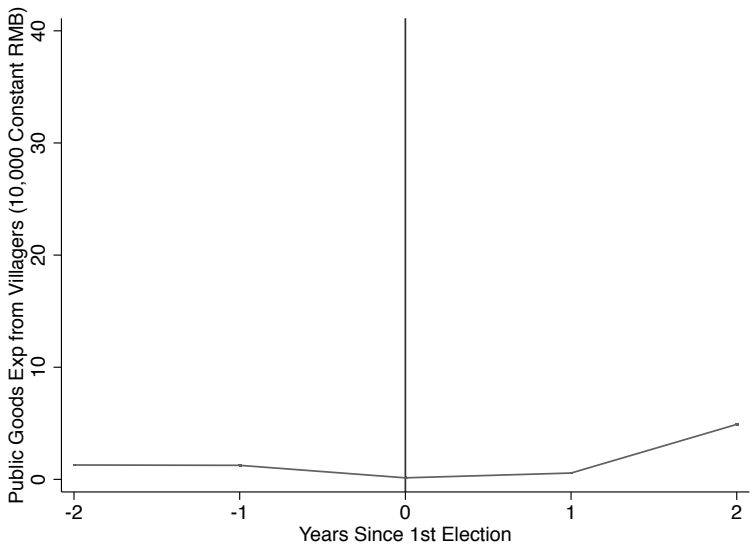
(b) Nine-Year Window



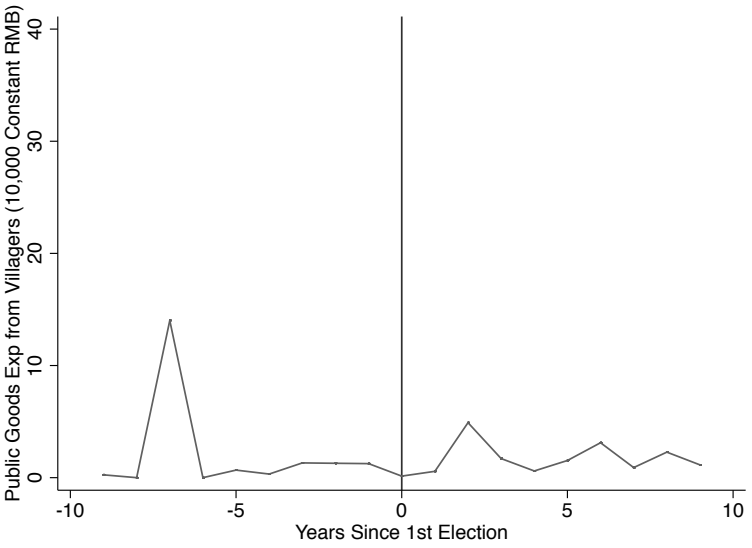
Notes: The y-axis is the average public goods expenditure financed by villagers. The x-axis is the number of years since the first election (year 0 is the year that elections are introduced). Public goods expenditures are deflated by a province-specific rural CPI. The sample is restricted such that the same villages are observed for each year since the first election.

Figure 2: Public Goods Expenditure by the Upper Government vs. the Number of Years Since the 1st Election – Restricted Sample

(a) Two-Year Window



(b) Nine-Year Window



Notes: The y-axis is the average public goods expenditure financed by the upper government. The x-axis is the number of years since the first election (year 0 is the year that elections are introduced). Public goods expenditures are deflated by a province-specific rural CPI. The sample is restricted such that the same villages are observed for each year since the first election.

Table 1: Descriptive Statistics

Variable	Source	Obs	Mean	Std. Dev.
# of HH in Village	NFS	4,340	416.15	276.17
Near City	NFS	5,208	0.30	0.46
Total Public Goods Expenditure (10,000 Constant RMB)	VDS	4,340	13.81	133.23
Irrigation	VDS	4,340	3.31	63.69
Schooling	VDS	4,340	0.02	0.34
Roads & Sanitation	VDS	4,340	4.98	88.25
Electricity	VDS	4,340	0.71	7.75
Environment	VDS	4,340	0.31	12.41
Other	VDS	4,340	2.58	48.04
Total Village Land	NFS	3,612	9,245	14,719
Arable Land (Mu)	NFS	3,612	2,295	2,329
Share of Village Land that is Arable	NFS	3,612	0.51	0.32
Used for HH Farming (Mu)	NFS	3,612	2,215	2,312
Not Used for HH Farming (Mu)	NFS	3,612	79.72	367.26
Leased Out to Enterprises (Mu)	NFS	3,612	60.46	347.61
Median HH Annual Gross Income Growth	NFS	3,084	0.08	0.19
The Number of Village Committee Members	NFS	2,287	4.36	2.36
The Number of Village Party Cadres	NFS	2,295	6.70	3.82
Party Secretary Tenure	VDS	5,208	10.03	8.13
Village Chief: Tenure	VDS	5,208	6.69	6.24
Has Election	VDS	5,208	0.73	0.44
Has Open Nomination	VDS	5,208	0.20	0.40
Years between Election Introductions in Village and Province	VDS	217	5.02	5.07
Years between Election Introductions in County and Province**	VDS	217	4.28	4.67
Years between Election Introductions in Village and County**	VDS	217	0.74	2.28
Years since last election	VDS	1,084	3.16	1.02
VC different from previous term*	VDS	4,312	0.16	0.36
1st Election Changed VC*	VDS	182	0.38	0.49

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

Table 2: Correlates of the Timing of the Introduction of Elections

Dependent Variable: Year of 1st Election	(1) Coef.	(2) Obs	(3) R-Square
Near City	-0.029	217	0.001
Presence of Family with a Family Tree (jiapu)	0.109	217	0.012
Presence of Family with Ancestral Temple (citang)	0.140	217	0.019
Population Share of largest Clan	0.198	217	0.039
Distance to High School	0.155	217	0.024
Total Number of Households	0.324	217	0.105
Total Public Goods Exp (10,000 RMB)	0.016	217	0.000
Income 90th Percentile (RMB)	0.131	217	0.017
Income 50th Percentile (RMB)	-0.081	217	0.007
Income 10th Percentile (RMB)	-0.160	217	0.026
Income Growth 90th Percentile	0.159	217	0.025
Income Growth 50th Percentile	-0.023	217	0.001
Income Growth 10th Percentile	-0.074	217	0.005
Land Leased Out to Enterprises (Mu=1/15 Hectare)	-0.145	217	0.021
Land used for Household Farming	0.136	217	0.018
Total Village Arable Land	0.156	217	0.024
One Child Policy Exemptions	-0.222	217	0.049
Upper Government Land Expropriation	-0.260**	217	0.068

Notes: Standardized coefficients are presented in column (1). Each row corresponds to a different bivariate regression estimated in a cross section of villages. The dependent variable is the year of the 1st election while the regressor of interest is defined by each row. The regressors of interest are measured in the base year (defined as the first year that data are available for each variable). All variables are demeaned by province fixed effects. *, **, and *** indicates statistical significance at the 10%, 5% and 1% levels, respectively.

Table 3: The Effect of the Introduction of Elections on Village Government Public Goods Expenditure

	Dependent Variable					
	From All Sources	From Villagers	From Other Sources	From All Sources	From Villagers	From Other Sources
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Public Goods Expenditures (Constant 10,000 RMB)					
Dependent Variable Mean	*13.81*	*9.46*	*4.28*	*13.81*	*9.46*	*4.28*
Post 1st Election	18.401	16.351	2.165	15.311	16.080	-0.672
	(9.247)	(9.433)	(2.029)	(7.190)	(7.717)	(1.525)
Wild Bootstrap p-value	[0.086]	[0.148]	[0.384]	[0.042]	[0.046]	[0.738]
Controls:						
Province - Trend	N	N	N	Y	Y	Y
Observations	4,340	4,340	4,340	4,340	4,340	4,340
R-squared	0.108	0.097	0.067	0.113	0.103	0.073
Clusters	29	29	29	29	29	29
	B. Any Public Goods Expenditures (Dummy Variable)					
Dependent Variable Mean	*0.22*	*0.17*	*0.07*	*0.22*	*0.17*	*0.07*
Post 1st Election	0.056	0.050	0.025	0.057	0.063	0.007
	(0.024)	(0.023)	(0.014)	(0.031)	(0.024)	(0.015)
Wild Bootstrap p-value	[0.032]	[0.048]	[0.118]	[0.078]	[0.012]	[0.594]
Controls:						
Province - Trend	N	N	N	Y	Y	Y
Observations	4,340	4,340	4,340	4,340	4,340	4,340
R-squared	0.176	0.162	0.183	0.187	0.170	0.198
Clusters	29	29	29	29	29	29

Notes: All regressions control for the introduction of open nominations, village and year fixed effects. Columns (4) to (6) also control for province trends. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. The number of clusters is stated at the bottom of the table. The sample is a balanced village-level panel of 217 villages for the years 1986-2005.

Table 4: The Effect of the Introduction of Elections on Village Government Public Goods Expenditure – Alternative Measures

	Dependent Variable: Public Goods Expenditures from Villagers (10,000 Constant RMB)					
	Village Total	Per Household	Per Worker	Log Village Total	Log Per Household	Log Per Worker
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable Mean	*9.46*	*0.02*	*0.01*	*-3.48*	*-4.38*	*-4.44*
Post 1st Election	16.080	0.018	0.011	0.446	0.111	0.081
	(7.717)	(0.008)	(0.005)	(0.173)	(0.044)	(0.034)
Wild Bootstrap p-value	[0.046]	[0.030]	[0.024]	[0.020]	[0.014]	[0.020]
Observations	4,340	4,340	4,335	4,340	4,340	4,335
R-squared	0.103	0.167	0.165	0.171	0.152	0.151
Clusters	29	29	29	29	29	29

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. The number of clusters is stated at the bottom of the table. The sample is a balanced village-level panel of 217 villages for the years 1986-2005. The number of observations vary because of missing values in the dependent variable.

Table 5: The Effect of the Introduction of Elections on Public Goods – Excluding High Expenditure Observations

	Dependent Variable: Public Goods Expenditures from Villagers			
	Constant 10,000 RMB		Ln Expenditure	
	(1)	(2)	(3)	(4)
	Full Sample	Omit Top 1%	Full Sample	Omit Top 1%
Dependent Variable Mean	*9.456*	*2.386*	*-3.479*	*-3.575*
Post 1st Election	16.08	1.121	0.446	0.405
	(7.717)	(0.381)	(0.173)	(0.136)
Wild Bootstrap p-value	[0.046]	[0.020]	[0.020]	[0.002]
Standardized Coef.	**0.0509**	**0.0418**	**0.0637**	**0.0619**
Observations	4340	4296	4340	4296
R-squared	0.103	0.133	0.171	0.164

Notes: All regressions control for the introduction of open nominations, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. Sample restrictions are stated in column headings. Columns (2) and (4) exclude observations if the amount of public expenditures is equal to or above the 99th percentile of the sample.

Table 6: The Effect of the Introduction of Elections on Village Government Public Goods Expenditure After the First Election Term

	Dependent Variable: Public Goods Expenditure from Villagers							
	Village Total (10,000 Constant RMB)				Any (Dummy Variable)			
	Full Sample	Omit Year of 1st Election	Omit Year of 1st Election and 1 Year Afterwards	Omit Year of 1st Election and 2 Years Afterwards	Full Sample	Omit Year of 1st Election	Omit Year of 1st Election and 1 Year Afterwards	Omit Year of 1st Election and 2 Years Afterwards
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable Mean	*9.46*	*9.62*	*8.83*	*9.10*	*0.17*	*0.16*	*0.17*	*0.17*
Post 1st Election	16.080	18.078	13.377	16.115	0.063	0.051	0.058	0.073
	(7.717)	(8.931)	(7.878)	(9.049)	(0.024)	(0.025)	(0.023)	(0.025)
Wild Bootstrap p-value	[0.046]	[0.042]	[0.126]	[0.128]	[0.012]	[0.064]	[0.044]	[0.010]
Observations	4,340	4,194	4,045	3,854	4,340	4,194	4,045	3,854
R-squared	0.103	0.107	0.123	0.125	0.170	0.172	0.171	0.181
Clusters	29	29	29	29	29	29	29	29

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. The number of clusters is stated at the bottom of the table. The sample of columns (1) and (5) is a balanced village-level panel of 217 villages for the years 1986-2005.

Table 7: The Effect of the Introduction of Elections on Village Government Public Goods Expenditure, Village Land and Household Income – Robustness to Controls

	Dependent Variable: Public Goods Expenditure from Villagers (10,000 Constant RMB)							
	Baseline	Control for Prov Introduction of 1st Election	Control for Prov-Year FE	Control for Prov Per Capita GDP and Growth, Per Capital Agric GDP	Omit Early and Late Introducers	Control for Year FE x Base Year Vars (One Child Policy and Upper Government Land Expropriation)	Control for Year FE x Near City, Social Capital, Post Tax & Fee	Omit if Ever Merged with Another Village
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post 1st Election	16.080	14.190	14.626	18.077	14.338	16.072	16.692	16.734
	(7.717)	(6.620)	(9.509)	(8.509)	(8.012)	(7.690)	(7.740)	(8.459)
Wild Bootstrap p-value	[0.046]	[0.036]	[0.192]	[0.048]	[0.040]	[0.050]	[0.016]	[0.076]
Observations	4,340	4,340	4,340	4,018	4,100	4,340	4,340	3,500
R-squared	0.103	0.105	0.221	0.119	0.119	0.203	0.114	0.105
Clusters	29	29	29	29	29	29	29	29

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects, and the additional controls stated in the column headings. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. The number of clusters is stated at the bottom of the table. The sample is a balanced village-level panel of 217 villages for the years 1986-2005. The number of observations vary due to missing values in the control variables.

Table 8: The Effect of the Introduction of Elections on Household Payment of Local Fees

	Dependent Variable: Household Annual Local Fee Payments (% of Household Annual Income)				
Total Household Income Quintiles:	0 - 20th	20th - 40th	40th - 60th	60th - 80th	80th - 100th
	(1)	(2)	(3)	(4)	(5)
Dep. Variable Mean	*3.141*	*2.746*	*2.476*	*2.173*	*1.749*
Post 1st Election	0.522	0.391	0.406	0.388	0.315
	(0.246)	(0.214)	(0.206)	(0.190)	(0.131)
Wild Bootstrap p-value	[0.072]	[0.094]	[0.088]	[0.076]	[0.036]
Observations	2,792	2,792	2,792	2,792	2,791
R^2	0.542	0.598	0.609	0.625	0.583
Clusters	18	18	18	18	18

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. The number of clusters are stated at the bottom of the table. The samples are village-level panels and include 160 village for the years 1986-2005. Data for 1992 and 1994 are interpolated. The panel is not fully balanced because of missing values in the dependent variables.

Table 9: The Effect of the Introduction of Elections on the De Facto Powers of the VC and PS

Dependent Variable: Signature Right for Public Goods Investment (Dummy Variable)			
	VC	VC + PS	PS
	(1)	(2)	(3)
Dependent Variable Mean	*0.19*	*0.66*	*0.15*
Post 1st Election	0.042	-0.026	-0.017
	(0.023)	(0.028)	(0.027)
Wild Bootstrap p-value	[0.098]	[0.376]	[0.554]
Observations	4,457	4,457	4,457
R-squared	0.779	0.808	0.774
Clusters	29	29	29

Notes: All regressions control for the introduction of open nominations, province trends, and village and year fixed effects. Standard errors, clustered at the province level, are reported in parentheses. Wild bootstrap p-values, clustered at the province level are presented in square brackets. The sample is a panel of 189 villages for the years 1982-2005. There are fewer villages than the full sample (217) because of missing data for village leaders.

Table 10: The Effect of the Introduction of Elections on the Characteristics of the VC and PS

	Dependent Variable: Characteristic of Village Leader			
	Turnover	Age	Years of Edu	Party Member
	(1)	(2)	(3)	(4)
	A. Village Chairman (VC)			
Dependent Variable Mean	*0.16*	*42.15*	*7.88*	*0.77*
Post 1st Election	0.045	-2.442	0.791	-0.034
	(0.020)	(1.037)	(0.224)	(0.043)
Wild Bootstrap p-value	[0.001]	[0.040]	[0.000]	[0.468]
Obs	4,312	4,188	4,194	4,274
R^2	0.065	0.430	0.611	0.484
	B. Party Secretary (PS)			
Dependent Variable Mean	*0.15*	*44.35*	*8.42*	
Post 1st Election	-0.006	0.312	-0.121	
	(0.018)	(0.632)	(0.139)	
Wild Bootstrap p-value	[0.199]	[0.607]	[0.478]	
Obs	4,365	3,546	4,356	
R^2	0.071	0.677	0.572	

Notes: All regressions control for the introduction of open nominations, province trends, and village and year fixed effects. Standard errors, clustered at the province level, are reported in parentheses. Wild bootstrap p-values, clustered at the province level are presented in square brackets. The sample is a panel of 189 villages for the years 1982-2005. There are fewer villages than the full sample (217) because of missing data for village leaders. The number of observations vary across columns due to missing values in the dependent variable.

Table 11: The Effect of the Introduction of Elections for Villages where the Pre-Election Incumbent Remained in Office

	Dependent Variable: Public Goods Expenditure from Villagers (10,000 RMB)			
	Total, In Levels (1)	Per Household, In Levels (2)	Total, In Logs (3)	Per Household, In Logs (4)
Post 1st Election	54.509	0.060	0.855	0.153
	(28.762)	(0.034)	(0.563)	(0.110)
Wild Bootstrap p-value	[0.432]	[0.530]	[0.388]	[0.280]
Post 1st Election x 1st Election VC Change	-40.826	-0.045	-0.435	-0.045
	(28.515)	(0.033)	(0.540)	(0.107)
Wild Bootstrap p-value	[0.548]	[0.606]	[0.538]	[0.714]
Observations	4,340	4,340	4,340	4,340
R^2	0.110	0.170	0.173	0.154
Clusters	29	29	29	29

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. The number of clusters is stated at the bottom of the table. The sample is a balanced village-level panel for the years 1986-2005.

Table 12: The Effect of the Introduction of Elections on Village Land and the Household Income

	Dependent Variable						
		Percentiles of the Within-Village Distribution of Total Household Income (Constant RMB)					
	Village Land Leased to Enterprises (Mu=1/15 Hectare)	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}	Income Ratio $50^{th}/90^{th}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable Mean	*108.5*	*4,878*	*7,090*	*10,126*	*14,767*	*23,615*	*0.529*
Post 1st Election	-57.73	282.0	179.7	-80.00	-901.3	-4,806	0.017
	(32.47)	(198.0)	(326.7)	(602.7)	(1,254)	(3,011)	(0.007)
Wild Bootstrap p-value	[0.070]	[0.172]	[0.590]	[0.912]	[0.50]	[0.104]	[0.046]
Observations	2,034	3,763	3,763	3,763	3,763	3,763	3,763
R^2	0.558	0.775	0.823	0.828	0.812	0.708	0.597
Clusters	28	29	29	29	29	29	29

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. The number of clusters are stated at the bottom of the table. The samples are village level panels. The sample in column (1) is restricted to villages that ever leased land to enterprises prior to the first election and includes the years 1986-2004. The dependent variable is the amount of land leased out in year t+1 (i.e., a lead variable). Columns (2)-(7) include all 217 villages and the years 1986-2005, except for 1992 and 1994. The panels are not fully balanced because of missing values in the dependent variables.

Table 13: The Effect of the Introduction of Elections on Household Income Sources

	Dependent Variable: 50th/90th Income Ratio			
	Total	Agriculture	Wage	Enterprises
	(1)	(2)	(3)	(4)
Dependent Variable Mean	*0.53*	*0.46*	*0.15*	*0.08*
Post 1st Election	0.016	0.021	0.019	0.026
	(0.008)	(0.007)	(0.017)	(0.013)
Wild Bootstrap p-value	[0.090]	[0.018]	[0.284]	[0.016]
Obs	2,792	2,792	2,564	2,257
R^2	0.568	0.723	0.651	0.658
Clusters	18	18	18	18

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. The number of clusters are stated at the bottom of the table. The samples are village-level panels and include 160 villages for the years 1986-2005. Data for 1992 and 1994 are interpolated. The panel is not fully balanced and the number of observations vary across columns because of missing values in the dependent variables.

ONLINE APPENDIX

A Data Appendix

The variables for upper-government land expropriation and One Child Policy exemptions are from the VDS. To ensure truthful reporting, we ask the respondents to check the years in which the village experienced any permanent loss of village land due to upper-government expropriation, and to check the years in which any household in the village had a second or higher parity birth. Respondents' answers are based on village records and rosters. In a companion paper, we check that changes in expropriation correspond to changes in total village land (reported in the NFS) and changes in One Child Policy exemptions correspond to the number of children age 7-13 seven years later (reported in the NFS). The questionnaire for the VDS is available online at
http://www.econ.yale.edu/~nq3/NANCYS_Yale_Website

Table A.1: The Timing of Electoral Reforms

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

Notes: Each observation is a village. The sample comprises a balanced panel of 217 villages for 1982 to 2005.

Table A.2: The Effect of the Introduction of Elections on Land Use and Income of the 90th Percentile

	Dependent Variable							
	Baseline	Control for Prov Introduction of 1st Election	Control for Prov-Year FE	Control for Prov Per Capita GDP and Growth, Per Capita Agric GDP	Omit Early and Late Introducers	Control for Year FE x Base Year Vars (One Child Policy and Upper Government Land Expropriation)	Control for Year FE x Near City, Social Capital, Post Tax & Fee	Omit if Ever Merged with Another Village
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Village Land Leased to Enterprises (Mu=1/15 Hectare)							
Post 1st Election	-57.727	-55.563	-49.482	-54.178	-57.211	-50.189	-58.631	-52.162
	(32.472)	(32.898)	(30.816)	(30.785)	(33.405)	(39.526)	(31.474)	(37.345)
Wild Bootstrap p-value	[0.070]	[0.076]	[0.082]	[0.088]	[0.058]	[0.140]	[0.084]	[0.190]
Observations	2,034	2,034	2,034	1,919	1,940	2,034	2,034	1,740
R-squared	0.558	0.559	0.744	0.591	0.559	0.564	0.578	0.614
Clusters	28	28	28	28	28	28	28	27
	B. Income Ratio 50th/90th (Constant RMB)							
Post 1st Election	0.017	0.016	0.020	0.018	0.012	0.018	0.017	0.014
	(0.007)	(0.007)	(0.010)	(0.008)	(0.006)	(0.007)	(0.007)	(0.007)
Wild Bootstrap p-value	[0.046]	[0.034]	[0.026]	[0.022]	[0.088]	[0.032]	[0.024]	[0.094]
Observations	3,763	3,763	3,763	3,477	3,553	3,763	3,763	3,035
R-squared	0.597	0.598	0.637	0.606	0.601	0.603	0.602	0.605
Clusters	29	29	29	29	29	29	29	29

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects, and the additional controls stated in the column headings. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. The number of clusters are stated at the bottom of the table. The samples are village-level panels. Panel A is restricted to villages that ever leased land to enterprises prior to the first election and includes the years 1986-2004. The dependent variable is the amount of land leased out in year t+1 (i.e., a lead variable). Panel B includes all villages and the years 1986-2005. Additional sample exclusions are stated in column headings. Data from 1992 and 1994 are interpolated. The panels are not fully balanced because of missing values in the dependent variabes.

Table A.3: Descriptive Statistics for Household Land and Incomes in the Full Sample and the Household Subsample of Villages

	Full Sample			Household Sample		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
# of HH in Village	4,340	416.15	276.17	2,798	418.31	285.91
Near City	5,208	0.30	0.46	2,798	0.27	0.44
Total Village Land	3,612	9,245	14,719	2,495	7,224	10,119
Arable Land (Mu)	3,612	2,295	2,329	2,495	1,782	1,188
Share of Village Land that is Arable	3,612	0.51	0.32	2,495	0.52	0.31
Used for HH Farming (Mu)	3,612	2,215	2,312	2,495	1,705	1,173
Not Used for HH Farming (Mu)	3,612	79.72	367.26	2,495	77.25	351.67
Leased Out to Enterprises (Mu)	3,612	60.46	347.61	2,655	61.85	327.37
Median HH Annual Gross Income Growth	3,084	0.08	0.19	2,145	0.080	0.186
The Number of Village Committee Members	2,287	4.36	2.36	1,583	4.22	2.08
The Number of Village Party Cadres	2,295	6.70	3.82	1,587	6.98	3.84
Party Secretary Tenure	5,208	10.03	8.13	2,798	10.71	7.82
Village Chief: Tenure	5,208	6.69	6.24	2,798	7.38	5.89
Has Election	5,208	0.73	0.44	2798	0.84	0.36
Has Open Nomination	5,208	0.20	0.40	2798	0.24	0.43
Years between Election Introductions in Village and Province	217	5.02	5.07	2798	5.10	4.98
Years between Election Introductions in County and Province**	217	4.28	4.67	2798	4.40	4.63
Years between Election Introductions in Village and County**	217	0.74	2.28	2798	0.70	2.25
Years since last election	1,084	3.16	1.02	717	3.19	1.07
VC different from previous term*	4,312	0.16	0.36	2,505	0.16	0.36
1st Election Changed VC*	182	0.38	0.49	2,505	0.16	0.36

Notes: Each observation is at the village-year level. *Not all villages retained records of VC's names from prior to the first election. **The year of the first election in a county is based on respondent recall.

Table A.4: Balance Sheet of Household Income and Expenditure

	Obs	Mean	Std. Dev.
Total Income	2,792	12,516	14,978
from agriculture (and home production)	2,792	8,391	10,834
from wages	2,792	2,022	3,201
village enterprise income	2,792	851	2,138
other	2,792	1,252	3,047
Total Expenditures	2,792	5,917	13,325
Household management expenditures	2,792	1,887	6,520
Taxes to central government	2,792	123	379
Levies and fees to local governments	2,792	78	157
Total Consumption	2,792	3,275	6,465

Notes: Village enterprise income is the sum of income from collectives, dividends and parternship/ cooperatives. The sample comprises of an unbalanced panel of 160 villages in 18 provinces for the years for the years 1986-2005. Data for 1992 and 1994 are interpolated.

Table A.5: The Effect of the Introduction of Elections on Household Income – Comparison of Full Sample and Village Subsample Estimates

	Dependent Variable: Household Income (RMB)				
Percentiles of the Village Income Distribution:	10th	25th	50th	75th	90th
	(1)	(2)	(3)	(4)	(5)
	A. Full Sample				
Dependent Variable Mean	*4,878*	*7,090*	*10,126*	*14,767*	*23,615*
Post 1st Election	282	180	-80	-901	-4,806
	(198)	(327)	(603)	(1254)	(3011)
Wild Bootstrap p-value	[0.172]	[0.590]	[0.912]	[0.50]	[0.104]
Observations	3,763	3,763	3,763	3,763	3,763
R-squared	0.775	0.823	0.828	0.812	0.708
Clusters (# Provinces)	29	29	29	29	29
# Villages	217	217	217	217	217
	B. Household-Level Subsample				
Dependent Variable Mean	*4,478*	*6,605*	*9,496*	*13,977*	*22,267*
Post 1st Election	166	-74	-564	-1,912	-5,239
	(193)	(360)	(682)	(1406)	(3275)
Wild Bootstrap p-value	[0.372]	[0.862]	[0.48]	[0.192]	[0.05]
Observations	2,792	2,792	2,792	2,792	2,792
R-squared	0.716	0.774	0.787	0.774	0.712
Clusters (# Provinces)	18	18	18	18	18
# Villages	160	160	160	160	160

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. The samples are village-level panels and include the years 1986-2005. The panels are not fully balanced because of missing values in the dependent variables. The number of villages and provinces are stated at the bottom of each panel.

Table A.6: The Effect of the Introduction of Elections on Household Income Ratios – Robustness to Controls

	Baseline	Control for Prov Introduction of 1st Election	Control for Prov Year FE	Control for Prov Per Capita GDP and Growth	Omit Early and Late Introducers	Control for Year FE x Base Year Vars (One Child Policy and Upper Government Land Expropriation)	Control for Year FE x Near City, Social Capital, Post Tax & Fee	Omit if Ever Merged with Another Village
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Dependent Variable: 50th/90th Total Income Ratio							
Post 1st Election	0.016	0.015	0.015	0.018	0.010	0.017	0.017	0.014
	(0.008)	(0.008)	(0.011)	(0.009)	(0.007)	(0.008)	(0.008)	(0.006)
Observations	2,792	2,792	2,792	2,582	2,616	2,792	2,792	2,215
R^2	0.569	0.569	0.613	0.583	0.576	0.577	0.579	0.567
Clusters	18	18	18	18	18	18	18	18
	B. Dependent Variable: 50th/90th Agricultural Income Ratio							
Post 1st Election	0.021	0.020	0.021	0.023	0.016	0.024	0.020	0.022
	(0.007)	(0.007)	(0.010)	(0.009)	(0.007)	(0.006)	(0.008)	(0.007)
Observations	2,792	2,792	2,792	2,582	2,616	2,792	2,792	2,215
R^2	0.724	0.724	0.746	0.730	0.720	0.731	0.728	0.741
Clusters	18	18	18	18	18	18	18	18
	C. Dependent Variable: 50th/90th Wage Income Ratio							
Post 1st Election	0.019	0.020	0.029	0.013	0.013	0.019	0.019	0.016
	(0.017)	(0.017)	(0.022)	(0.019)	(0.019)	(0.016)	(0.015)	(0.015)
Observations	2,564	2,564	2,564	2,367	2,394	2,564	2,564	2,065
R^2	0.651	0.651	0.686	0.679	0.648	0.657	0.656	0.657
Clusters	18	18	18	18	18	18	18	18
	D. Dependent Variable: 50th/90th Enterprise Income Ratio							
Post 1st Election	0.026	0.025	0.020	0.024	0.024	0.024	0.022	0.022
	(0.013)	(0.013)	(0.016)	(0.013)	(0.017)	(0.014)	(0.015)	(0.012)
Observations	2,257	2,257	2,257	2,065	2,089	2,257	2,257	1,811
R^2	0.658	0.660	0.699	0.675	0.644	0.664	0.668	0.610
Clusters	18	18	18	18	18	18	18	18

Notes: All regressions control for the introduction of open nominations, province trends, village and year fixed effects, and the additional controls stated in the column headings. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values are presented in square brackets. The number of clusters are stated at the bottom of the table. The samples are village-level panels and include 160 villages for the years 1986-2005. Data for 1992 and 1994 are interpolated. Additional sample exclusions are stated in column headings. The panels are not fully balanced because of missing values in the dependent variabes.