Political Reform in China:

Elections, Public Goods and Income Distribution*

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Abstract

This study investigates the effects of introducing elections on public good expenditures, income distribution and land use in rural China. We collect a large and unique survey to document the history of political reforms and economic policies and exploit the staggered timing of the introduction of elections for causal identification. We find that elections significantly increase public goods expenditure funded by villagers, reduce the income of the richest households in each village and reduce the amount of village land that is leased away from household farming.

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] 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]

China is a prominent example. The agency problem between the state and local officials at the village level can take several forms. For example, the village official is responsible for raising funds from villagers in order to provide local public goods such as schooling. Difficulties in bureaucratic monitoring allow him to shirk from the substantial effort required by this activity. Lack of effective oversight also allows the local official to use his control over collectively owned means of production, such as land or village enterprises, to favor himself and his cronies. During the 1980s and 1990s, village-level elections were introduced to mitigate these problems. Policymakers intended elections to resolve agency problems by giving the local official incentives to implement policies that appeal to a majority of the constituency in order to obtain re-election.

The goal of this paper is to provide rigorous empirical analysis of the effects of the introduction of local elections on local public goods, land use and income, the results of which can help shed light on the effectiveness of elections in changing incentives for local officials.[5] We motivate these outcomes below.

Our study faces two notable difficulties. The first is the lack of detailed data on political change and economic policies in rural China. To address this we construct the *Village Democracy*

[1] Classics in this literature are **?**(1989), **?**(1999), **?**(2000).

[2] For recent overviews of this literature see **?**(2012) and **?**(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, **?** (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 **?** (2009).

[5] The theoretical basis for these claims comes from the rich literature on political accountability. In broad terms, the political economy literature tends to focus either on common interest goods – public goods, less rent-seeking– in models of political agency or on redistributive issues in models of electoral competition. These are two complementary approaches to model majoritarian incentives, which are sometimes combined (an early example is **?**, 1986). See the discussion in **?**, (2000, chapter 1). The recent theoretical literature on democratization is also based on these majoritarian incentives. These theories predict that democracies, relative to autocracies, provide more public goods (**?**, 2003; **?**, 2004 and **?**, 2008) and engage in more redistribution (**?**, 2000, 2001, 2006; **?**, 2003).

Survey (VDS), a panel of 217 representative villages from 29 provinces for the years 1982-2005. The survey documents the history of economic policies and political reforms during this period, and contain detailed economic data on public goods expenditure and financing. This is the longest and broadest panel ever constructed to describe Chinese villages and is the first data that systematically document the changes in the fiscal and political structure of village governments. In addition, we use supplementary economic data from the the *National Fixed-Point Survey* (NFS), which is collected from the same villages as the VDS by the Ministry of Agriculture.

The second difficulty is in establishing the causal effects of the introduction of elections, which were staggered in timing across villages. For example, do elections change economic outcomes or does economic change precipitate elections? Or, are the introduction of elections and economic change jointly determined by a third factor? To address these concerns, we take advantage of the facts that the timing of electoral reforms was largely quasi-random within provinces and that the electoral reforms were isolated to the village-level. We do not take these assumptions as given and use the rich data to provide a large body of quantitative evidence to support our claims.

The main empirical analysis proceeds in three steps. First, we document that the timing of elections across villages within provinces is mostly uncorrelated with a large number of observable characteristics at the village level. This is consistent with the anecdotal evidence that the timing of reforms was unrelated to 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 have already introduced elections and those that have not. The baseline specification includes village fixed effects to control for all time-invariant differences across villages, year fixed effects to 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. 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. Finally, we support our interpretation with a large number of robustness exercises. For example, we show that there is no evidence of pre-trends and that our estimates are robust to controlling for pre-election characteristics as well as the introduction of elections at the province level, which is the main source of endogeneity. Please see the section on Robustness.

The first main outcome of our analysis is village government expenditure on public goods. In rural China, local public goods are mostly financed by village contributions, which local officials need to exert substantial effort to collect. In the early post-reform (post 1978) era, it was widely believed that officials shirked in this task, leading to severe under-provision of local public goods.[6] Our results show that the introduction of elections led to a stark – approximately 27% – increase in total local government expenditure on public goods. This is driven exclusively by expenditures financed by villagers. In contrast, public goods financed by upper levels of government are unchanged by elections, which supports our interpretation that the results are not confounded by simultaneous changes at the upper levels of government. We also provide some evidence that the change in public goods expenditure corresponds to villagers' demand and was reflected in a change in provision.

The second outcome is land use. Village land is all collectively owned and most of it is allocated by the village leaders to households in long-term leases. The village leadership can retain some land under its direct control and lease it to village enterprises. The profits from this collectively held land are supposed to benefit all villagers equally, but the lack of transparency in managing land leases and enterprises provide local bureaucrats opportunities for rent seeking.[7] For this reason, villagers typically prefer land to be fully allocated to households. We find that amongst villages that ever leased any land to enterprises, the introduction of elections reduced the amount of land leased to enterprises by 44% and the probability that any land is leased to enterprises by 13% (31% of the sample mean).

Amongst the policies that village leaders are known to use to benefit elites, land use is the only one that we observe directly. However, if elections caused a systematic reduction in the pro-elite bias in policy, then elections should reduce the income of rich households relative to poorer ones.

Thus, the third outcome we examine is household income. We find that the introduction of elections has little effect on the income of the poorest half of households. However, the income of the 75th percentile household is reduced by 5.6% and the income of the 90th percentile household is reduced by 8.4%. Consequently, the ratio between the income of the median household and the 90th percentile household increases by 1.7 percentage points. These results are consistent with the existing elites' position deteriorating with the introduction of elections.[8]

[6] See **?**(2007) and **?**(2010).

[7] See Background section.

[8] Note that there are no positive effects for any income decile. It might well be that changes in land use and other

The main results are consistent with the hypothesis that the introduction of elections mitigates the agency problem at the village level: elected village governments provide more public goods, at least some of which correspond to villagers' demands. They also allocate land away from enterprises, which presumably favors a majority of the villagers. Consistent with the new policies being less favorable to the elite, elections reduce income for households at the top of the income distribution.

To shed light on the underlying mechanisms, we present several supplementary findings. First, we find that the effect of the first election is the same in villages that retain the incumbent leaders as in villages that experienced leader turnover. Since the only change incurred in villages that retain incumbent leaders is the introduction of re-election incentives, this provides evidence for the power of re-election incentives.[9]

Next, we examine the policies used to achieve the income changes that followed elections, which is especially interesting in the rural Chinese context since the village government does not have the legal authority to impose recurring taxes. We were able to obtain household-level panel data on tax and fee payments to the local government, household income by source and household land allocation for a small non-random subset of the villages in our main analysis. We find suggestive evidence that elections increase fees paid by households to the local government. This is consistent with the need for village governments to raise fees in order to finance public goods, and with households being willing to contribute more to a village government that is more accountable to villagers' preferences. We also find that the changes in total household income are paralleled by changes in household farmland allocation, as well as the income sources most easily affected by the village government through its control over village land and enterprises: agriculture, wages and enterprise dividends. The latter suggests that the village government partly addresses its inability to redistribute income through taxes by reallocating assets and employment. That we consistently find that elections affect a large number of outcomes under the direct control of the village government is also important because it supports our interpretation of the electoral reforms as changes at the village-level of government.

policies are inefficient, maybe because village and private enterprises were actually productive. Alternatively, it may be that the additional land that households receive increases their welfare in non-measurable ways, such as home production for own consumption, or that there are long-run gains that cannot be measured with our data.

[9]In principle, elections can also lead to policy changes caused by the ability of villagers to select better leaders. However, since turnover is not exogenously determined in our setting, we cannot determine the importance of this mechanism in driving our election results. See Section **??**.

This study adds directly to the growing body of evidence on the effects of elections on public goods and inequality in rural China.[10] Our study improves on existing studies by systematically documenting the history of electoral reforms and the political and economic structure of Chinese villages in detail for a long panel. The larger sample size and richness of our data allow us to be more empirically rigorous than past studies and examine a broader set of outcomes that can help shed light on the mechanisms underlying the effects of elections.[11] This study complements a companion paper, **?**, which provides a large body of evidence that the introduction of elections is successful in shifting the accountability of the village government towards villagers. We add to the nascent literature on governance in autocracies and, in particular, in China.[12]

This study is closely related to within-country studies that have focused on various aspects of elections in other countries.[13] Since elections are an essential element of democracy, these results can also speak to the broader literature on democracy and economic policy.[14] The existing empirical evidence relating democratic transition to public goods and redistribution, which mostly comes from cross-country studies, is inconclusive.[15] 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 results complement cross-country studies by showing that even a marginal move towards democratic elections in the highly restricted setting of an authoritarian central regime can substantially change village policy in public goods and asset

[10]Past studies have used either panel data of relatively few villages or a cross-section of many villages to provide important evidence on the effect of elections in rural China. For example, **?** examines the effect of elections on inequality and infer redistribution from changes in inequality. Several studies have related elections to public goods (e.g., **?**, 2004; **?**, 2007, 2010; **?**, 2011). Also, **?** examines the relationship between elections and villager health shocks.

[11]For example, earlier studies have not examined the sources of public goods financing, local taxes, land allocation, the evolution of household income or the contribution of re-election incentives.

[12]For example, studies such as **?**, **?**, and **?**.

[13]For example, **?** (1995), **?**, **?** (2008), **?** (2011), **?** (2011) and **?** (2012) provide evidence for the role of several election characteristics in the United States, Argentina, Brazil, the U.K. and Sweden, but they do not identify the effects of elections *per se*. **?** (2005) examines the effect of party competition and the introduction of rural elections on appropriate public good provision in India. Our results on public goods are consistent with theirs. However, the mechanisms underlying elections in the Chinese and India contexts are very different because party competition is unlikely to apply in China's one-party context. Our study also differs from theirs in examining a broader set of outcomes. There is also a related literature examining the differences between elected and appointed judges. For a recent example see **?**.

[14]For instance see **?**(2001, 2006), **?**(2003), **?**(2003), **?**(2004), **?**(2008) who all relate democratization or extensions of the franchise to either increased public goods provision or redistribution. **?**(1997) and **?**(2008) provide empirical evidence for increased welfare spending in the context of franchise extension in the US.

[15]In the cross-section, democracy has been found to be positively associated with government size (**?**, 2001), higher wages (**?**, 1999), lower inequality (**?**, 1998; **?**, 2003; **?**, 2001), higher human capital (**?**, 2001), and better health indicators (**?**, 2006; **?**, 2011). However, in a large study looking at several socioeconomic policy dimensions, **?** (2004) find that democracy is associated with no difference in the outcomes they examine.

allocation. Our focus on elections complements recent studies that emphasize the importance of constraints on the executive in determining economic outcomes (e.g. **?**, 2001; **?**, 2011).

This paper is organized as follows. Section **??** discusses the background. Section **??** presents the conceptual framework and empirical strategy. Section **??** briefly describes the data. Section **??** presents the main results, including the robustness checks. Section **??** presents evidence on the importance of re-election incentives and Section **??** presents some additional results on taxes, sources of income, and household income evolution. Section **??** offers a conclusion.

2 Background

2.1 Villages and Village Governance in Rural China

A majority of the rural population in China lives in villages, which comprise 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 a village is agriculture and households farm plots of land that are assigned to them in long-term leases. 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 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. There was no clear distribution of authority between the two bodies and anecdotal evidence suggests that the party branch had more power.[16]

The village government is responsible for two important village-level policies that directly affect the well-being of its citizens. First, village governments are responsible for the provision of local public goods such as irrigation and primary schools. They are supposed to decide which public

[16]In an earlier version of the paper, **?**, we explore the relative powers of the VC and PS by documenting the signatures of each leader on important village decisions. We document that there is a shift in power from the PS to the VS after elections are introduced such that the number of villages where most policies are decided unilaterally by the VC increases, and the number of villages where most policies are decided unilaterally by the PS decreases.

goods to provide and to raise funds from villagers to finance them.

Second, 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.[17] 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 the land retained by the village government was often leased to highly profitable village enterprises.[18]

Village governments do not have legal authority to impose regular or recurring taxes. Instead, to fund the activities of the village government, including local officials' salaries, they can use proceeds from collectively owned sources of income such as village enterprises or leasing land to enterprises (that can be from inside or outside the village), or raise revenues by imposing *ad hoc* fees and levies, which we will henceforth refer to as *local taxes* or *fees* for simplicity.

The obscurity of village enterprise and village government accounts typically means that upper-level bureaucrats encounter enormous difficulties monitoring the activities of local officials. As a consequence of this informational advantage, local officials who shirked in providing public goods or who engaged in rent-seeking were able to maintain their positions. There is an abundance of examples of corrupt village officials who neglected public good provision and systematically extracted personal rents from land and enterprises controlled by the village governments (e.g. **?**, 2007, **?**, 2004, **?**, 2000, **?**, 1994; and **?**, 1998). In response, villagers often resist paying local taxes and fees, which in turn, starves local governments of funds and limits its ability to provide public goods (e.g., **?**, 2000; **?**, 1994; **?**, 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 goods provision were an outcome of corruption, lack of effort by the local officials, the refusal of villagers of providing the necessary funds, or the lack of demand from villagers.

[17] 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 **?** for a related study about tenure security in rural China.

[18] Additional responsibilities are the maintenance of law and order, the collection of grain taxes on behalf of the central government and the implementation of centrally mandated policies.

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 for the introduction of elections, as shown by this quote from the official debate.

> "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 (**?**, 1999).

Generally, 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.[19]

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.[20] 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.[21] Anecdotal evidence suggests that the power arrangements between these two bodies were very heterogeneous across villages. Indeed, in many areas the Party

[19] See **?** and **?** (1994, 1999) for descriptions of the policy debates that led to the official introduction of local elections.

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

[21] As **?** discusses, according to the law, the village committee operates under the leadership (*lingdao*) of 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 thus better understood as a marginal change that increased accountability of the local government towards 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.[22]

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 (**?**, 1999). Anecdotal evidence from interviews that the authors conducted with county- and province-level officials suggests that the pattern of the roll-out was 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.[23]

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

[23]In his detailed study of elections, Sinologist **?** (2002, p. 222) writes that "These [elections] should not be interpreted as bottom-up initiatives by the villagers themselves; they are not in a position to play any precedent-setting part in the initiation of new electoral reforms. There is a mistaken belief among some people outside China regarding 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". **?** 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.

Based on interview that the authors conducted with village-, county- and province-level officials as well as evidence from qualitative studies, there are only two exceptions to the quasi-random timing of the reforms emerge. First, the model villages that piloted the reform obviously received 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 (e.g., **?**, 2009; **?**, 2000). 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. We also check that our estimates are not driven by pilot or straggler villages more generally.

3 Conceptual Framework

3.1 Accountability

The anecdotal evidence described above suggests that prior to the introduction of elections, local officials could benefit from the agency problem in two distinct ways. First, they could shirk in their efforts to provide and maintain public goods. Second, they could engage in rent seeking and pursue policies that benefitted them and other village elites. Elections were introduced in order to mitigate these concerns. Hence, if the reforms were effective in making local officials accountable to villagers to some extent, we would expect the introduction of elections to affect several outcomes. Public goods provision should increase since villagers should find it easier to extract effort from local officials through the promise of re-election. Due to this increase in accountability villagers should also be willing to contribute more funds to the government, contributing further to the provision of public goods. Land use and other policies should change in favor of the majority preferences in the village, which would result in changes in income distribution.[24] The purpose of the rest of this paper is to document the effect of the introduction of elections on these outcomes.

[24]The effects of an increase in leader accountability on village policies are formalized in a companion paper, **?**.

3.2 Empirical Strategy

Our empirical analysis proceeds in several steps. In this section, we present the strategy used for our main analysis under the assumption that the introduction of elections was quasi-random within provinces. We use a *differences-in-differences* (DD) strategy, where we compare the evolution of outcomes of 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 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 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

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

[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 available upon request. Note that we do not control for other procedural differences in elections because they are much more likely to be endogenous.

bias in the clustered standard errors by also presenting p-values derived from wild bootstraps as recommended by **?**.[27] The main coefficient of interest is β. It will be statistically different from zero, if elections affect a particular policy outcome.

Interpreting β as the causal effect of introducing elections does not require us to assume that 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 for it later in the paper. In particular, 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. Then, after we present the main results, we show that there is no evidence of pre-trends and conduct a large number of additional robustness and sensitivity checks. In particular, we show that our estimates are similar when we control for the introduction of elections at the province level, which is the main source of 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 *The Village Democracy Survey* (VDS), a unique retrospective survey conducted by the authors of this paper. The first wave, conducted in 2006, records the history of electoral reforms, *de facto* leader power, public goods expenditures, and the enforcement of central government policies. The second wave, conducted in 2011, records the 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 relied on the recall of survey respondents, which include all current and former living village leaders and elders (e.g., teachers) in each village. This applies to very few of our variables and we will note them when they are relevant. The VDS forms a balanced panel of 217 villages for the years 1982-2005. The villages we survey are the same villages surveyed by the *National Fixed-Point Survey* (NFS), which we discuss next.

Our measures of income and land are reported by the NFS, a detailed village- and household-

[27] The bootstraps are estimated using 500-999 repetitions.

level economic survey collected and maintained by a research center of the Ministry of Agriculture of China. It is collected each year beginning in 1986, with the exception of 1992 and 1994 due to administrative issues. The NFS villages were chosen in 1986 to be nationally representative for rural China. 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.[28] From the NFS, we were able to obtain village-level data for variables such as the amount of village land that is dedicated to farming and the amount of village land that is leased out to enterprises. These variables will be used in several supplementary exercises examining public goods. We also obtained the total income of households at the 10th, 25th, 50th, 75th and 90th percentiles of the within-village-year total income distribution.[29] For a subset of villages in ten provinces, we were also able to obtain more detailed household-level data. Because this subsample is small and non-representative, we only use it to supplement the main analysis. They are discussed further in Section **??**.

The main analysis uses the VDS panel data with the addition of the variables for land and income percentiles discussed above from the NFS. The main sample comprises a balanced panel of 217 villages from 29 provinces.[30]

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 of time starting in the early 1980s. 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 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 policies and to assess the mechanisms driving the reduced-form effects. Second, the NFS economic data and the village administrative records that we surveyed in the VDS were collected

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

[29] We are grateful to Wu Zhigang for computing these statistics for us for each village-year.

[30] 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.

contemporaneously. Hence, we avoid recall bias. Third, the panel structure of the survey allows us to control for village fixed effects and province-year trends. Finally, the fact that the NFS samples a large number of households in each village means that we are able to examine the effects of elections on income distribution within villages.

The main drawback is that the variables included in the NFS change over time to meet the needs of the Ministry of Agriculture. Thus, we have a small number of observations for many interesting variables (e.g., school enrollment) and cannot examine them in the regression analysis.[31] All observations in the empirical analysis are at the village-year level. We describe the variables as they become relevant.

4.2 Descriptive Evidence

In this section, we focus on facts that are important for understanding the variation that underlies our empirical strategy and we show that our data are consistent with the anecdotal evidence from other sources discussed in Section **??**. First, the data show that there is substantial variation in the timing of the first election within provinces. When we regress the year of the first election on only province fixed effects in a cross-sectional regression, we find that the R-squared is 0.33. Thus, approximately 67% of the variation in the timing of elections is within provinces. This is important for our strategy, which largely relies on variation within provinces.

Second, the timing of the rollout is consistent with rapid top-down implementation within provinces and counties. Our data indicate that 60% of villages within a province introduce elections within three years of the first election in that province. In addition, 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.[32] Table **??** shows that the average village implemented its first election within the same year as the official introduction 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

[31] There are many other interesting variables that are inconsistently collected and therefore not used in our analysis (e.g., obligated working days, roads). Another drawback of the NFS is that it did not collect detailed demographic data.

[32] 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.

623,669 rural villages (as defined by the number of village governments, *cunming weiyuanhui*), these statistics imply that the average province was able to introduce reforms in 13,859 villages within three years and the average county was able to introduce elections in 143 villages within one year. Such rapid rollout is conducive to quasi-random timing.

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 it officially introduced it and also delayed elections in a few villages. Hence, it will be important for us to check that our baseline estimates are not driven by the early movers or the stragglers.

Finally, we provide direct evidence that the timing of the first election is uncorrelated to most pre-reform village characteristics conditional on the baseline controls. To condition the variables on the baseline controls, we compute the residuals of the year of the first election and potential correlates of election timing such as village income, population, income inequality, public goods expenditure, and as we discussed earlier, the incidences of exemptions of the One Child Policy and the permanent expropriation of land away from the village by the upper levels of government.[33] We then calculate the average of the residuals over time for each village and estimate the bivariate correlations between the residualized timing of the first election and the residuals of the potential drivers of introducing elections across villages. Table **??** presents the coefficients and standard errors for each bivariate correlation. The number of observations vary depending on data availability. All of the correlations except the one for election timing and the pre-reform incidence of upper-government land expropriations are statistically insignificant. The positive coefficient for this variable shows that villages that experienced more upper-government land expropriation prior to the reform introduced elections later. This is consistent with anecdotal evidence that elections were sometimes delayed for villages that had a difficult relationship with the upper government. Thus, in the section on robustness, we will be careful to control for this variable as well as to show that our results are

[33]To compute the residuals, we demean each variable by regressing it against all of the baseline controls except village fixed effects, i.e., year fixed effects, province fixed effects and province-year trends. Note that the baseline estimate in equation (**??**) controls for village fixed effects, which subsumes province fixed effects. Since, we are interested in the correlates of village characteristics with the timing of the first election within province in this exercise, we do not control for village fixed effects, but instead control for province fixed effects.

Online Appendix Table **??** shows that all the villages in our sample had introduced elections by the end of our study period. See the Online Data Appendix for a description of the variables for upper-government land expropriation and One Child Policy exemptions, which we collect in the VDS.

generally robust to the exclusion of villages that were the first or last to implement elections.

As it is difficult to compare magnitudes across different regressors, we also present standardized coefficients that measure the effect of a one standard deviation change of the explanatory variable on the dependent variable, also measured in terms of standard deviations. These show that none of the variables, including the one that has significant correlation, have much explanatory power for the timing of the first election. For example, the largest standardized effect effect is for upper-government land expropriation. But it is only 0.15, which implies that a one standard deviation change in upper-government land expropriation is only associated with a 0.15 standard deviation change in the timing of the first election across villages. Thus, even if the variables shown in Table **??** were statistically significantly correlated with the timing of the first election, we would still conclude that their explanatory power for the timing of elections was quantitatively limited.

The data also provide several pieces of descriptive evidence that 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 belief that opponents to the electoral reform were unable to fully derail the introduction of elections, and with qualitative accounts of dissatisfied villagers demanding and obtaining recalls (**?**, 2006). Table **??** shows that, as legally required, elections occur every three years on average.[34] Interestingly, 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%).

5 Main Results

5.1 Public Goods

We first examine public goods expenditure at the village level, which are recorded by villages as the sum of expenditures on 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".[35] These data are recorded in the VDS from village administrative records and are

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

[35]The villages in our sample began recording public goods expenditures in 1986 at the request of the Ministry of Agriculture. The accounting methods, the categories for public goods, and the sources of financing are all determined

available for all years and villages during 1986-2005. Table **??** presents the effect of elections on public expenditures from estimating equation (**??**) together with the sample means of these variables. Our data allow us to separately examine expenditures according to the source of the funds, which we categorize into funds from village and non-village sources.

Panel A of Table **??** shows the results for total public expenditures across all village public goods. Note that 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. Column (1) shows that elections increase total public good expenditures from all sources by approximately 27.2%. The estimate is significant at the 10% level. This increase in public goods provision is consistent with our hypothesis that elections changed the incentives of village leaders and led them to exert more effort. In particular, they seem to have become more responsive to villagers demands for more public goods (e.g., **?**, 2007; **?**, 2010).

To further support this interpretation, we analyze expenditure by source of funding in columns (2)-(3). The top row 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. The regression results show that the aggregate increase in public goods expenditure found in column (1) is entirely driven by an increase in funding from villagers. The estimate for village financing in column (2) is similar in magnitude to the estimate for total financing and statistically significant at the 5% level. In contrast, the estimate for non-village financing in column (3) is near zero in magnitude and statistically insignificant. This result 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.[36] 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.

by the ministry. 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.

[36]For example, see the classic work of **?** for a discussion of why democracy hinders the government's ability to raise taxes.

According to our interpretation of the effect of elections, the public goods funded by this increase in expenditures should correspond to the demands of a majority of villagers. To determine whether this is the case, we perform an additional exercise where we proxy for villager demand. Within the constraints of our data, we are able to examine irrigation expenditure, which is the single largest item of public goods provision comprising 24% of average expenditure. We proxy for the demand for irrigation expenditure with the log of total village land that is used for household farming with the logic that villagers that rely more on household farming have higher demand for irrigation. Following this logic, we repeat the baseline specification, equation (**??**), using the log of public expenditure on irrigation as the dependent variable and the interaction effect of the introduction of elections (and open nominations) and the average log amount of all village land that is used for household farming as explanatory variables (in addition to the explanatory variables in equation (**??**)). We use a time-invariant measure of land to address the possibility that time-varying measures may be outcomes of elections, and also to maximize the number of observations.[37]

Panel B in Table **??** shows the effect of elections on irrigation investment. The positive interaction effect between elections and average log village farmland shows that elections cause a larger increase in irrigation expenditure in villages with more farmland. The estimate is statistically significant at the 1% level. To assess the magnitude of the net effect implied by these estimates, we can conduct a back of the envelope calculation. The estimates in Panel B column (2) imply that a village would only experience a net reduction if it had less that 4.7 (0.193/0.147) log mu of farmland, which is only true for three villages in our sample. Hence, the results should be interpreted as elections increasing irrigation expenditure, but the increase being smaller in magnitude for villages with less farmland.

To the extent that our data allow, the results show that the increases in public expenditures correspond to the (admittedly crude) proxy for demand and are consistent with the notion that

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

The estimating equation can be written as

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

where X_{vp} is a measure of the average log amount of village land used for household farming. Since these variables are time-invariant, we do not control for their main effects, which are absorbed by village fixed effects. $\hat{\theta}$ is the effect of elections on villages where no land is used for household farming. $\hat{\beta} + \hat{\theta}x$ is the effect of elections for villages where the average log amount of land dedicated to household farming equals x.

elections induced village leaders to provide what most villagers wanted. Moreover, a comparison of the interaction effects in columns (2) and (3) shows that the increase in desired public goods investment is also driven by funding by villagers.

Finally, we investigate the effect on public goods provision. Since our outcome variable is obtained from village accounts, it is, in principle, possible that village leaders changed the numbers they recorded without actually raising funds or increasing provision. Alternatively, village leaders may have raised funds but stolen them and then fabricated the numbers for expenditure in the village accounts. The ideal way to check the relevance of these concerns for our context is to investigate whether elections actually increase the provision of public goods. We cannot do this due to data limitations. Instead, we are able to proxy for the provision of irrigation with the share of arable land in a village. This follows the premise that increased irrigation will likely increase the share of land that is arable. Panel B column (4) shows that the results for this outcome parallel that of spending, suggesting that our results on expenditure reflect changes in actual provision.

5.2 Land Use and Household Income

Recall from Section **??** that village leaders are responsible for the allocation of village land to households, and that some land is often leased out to village enterprises. Villagers typically dislike this practice, because they suspect it is a source of rents for the village leadership and its cronies, and they would rather have direct control over more land.[38]

Data for the use of village land is reported by the NFS for all villages for the years 1987-2005 (excluding 1992 and 1994). The villages in our sample dedicate approximately 96% of arable land (approximately 51% of total village land) to households for farming. Approximately 75% of the remaining arable land is leased out to "enterprises", a term which we use for firms run by collectives or villagers (see Table **??**). 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 first election. This reduces the sample to 108 villages from 28 provinces.[39]

The estimates are displayed in the first two columns of Table **??**, where we use the same specifi-

[38] Consistent with this view, in a cross-sectional study **?** find that redistributing collective land to villagers is positively correlated with re-election probabilities. We present evidence in Section **??** that income from enterprises is not shared equitably across households.

[39] Our results are similar if we alternatively restrict the sample to villages that ever leased any land to enterprises during any time in the sample.

cation as in equation (**??**). Column (1) shows that the introduction of elections reduced the amount of land that is leased out to enterprises by approximately 44% (0.57 log-points), and the probability that any land is leased out by thirteen percentage-points. Both are statistically significant at the 10% level. Hence both the intensive and the extensive margins of this practice are affected by the introduction of elections.

Since the village leadership and other village elites were likely capturing most of the rents from running village enterprises, reducing the amount of land allocated to enterprises and giving it back to village households is a change that reduces the pro-elite bias in village policies. Land allocation to enterprises is just a particular example of the many ways the village leadership can affect rent allocation across households.[40] If elections cause a reduction in the pro-elite bias in these policies, they should result in income losses for elite households. To examine this possibility, we look at the effect of elections on household income at several percentiles of the village distribution.

We have data on *total household income* for households on the 10th, 25th, 50th, 75th and 90th percentiles of the within village and year income distribution for the full sample of villages for 1986-2005 (excluding 1992 and 1994).[41] In the top row of Table **??**, we present descriptive statistics on the distribution of household income to illustrate the extent of income inequality in villages. In our sample, the richest households (ninetieth percentile) typically earn twice the income as the median household (fiftieth percentile), who again earns about twice as much as the tenth percentile. Hence, despite the fact that de-collectivization is recent, there is already 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 not mutually exclusive.

To examine the effect of the introduction of elections on income distribution, we estimate the baseline equation (**??**) with household income at each percentile as the dependent variable. Columns

[40] For instance, the leadership is responsible for choosing who obtains a salaried job in the village government or in village enterprises, and as we discussed earlier, it also decides household land allocation. In Section **??**, we examine other channels using a subsample of villages for which we have richer household data.

[41] For example, the income of the tenth percentile household refers to the income of the household that is on the tenth percentile of the income distribution within a village during a given year. These were computed by the Ministry of Agriculture from the NFS household level data.

(3)-(7) of Table **??** show that the estimated effects of elections are negative in sign, small in magnitude and statistically insignificant for poor households. In contrast, they are negative in sign, larger in magnitude and statistically significantly at the 5% level for the 75th and 90th percentile households, which lose approximately six and nine percent of income due to elections. In column (8), we examine the ratio of the median household income to the ninetieth percentile household. The estimates show that elections increased the income of the median household relative to the ninetieth 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 (see Panel A in Table **??**), this increase is moderate in magnitude.

These results suggest that upon the introduction of elections, the village leadership changed economic policies such as land distribution and the management of village enterprises in such a way that the former economic elites were less favored. The welfare implications of the reduction in inequality is unclear and partly depends on the origins of the pre-reform inequality. Note that we do not observe an increase in income at any level of the income distribution, so it might well be that these changes were inefficient. We speculate about this more in the conclusion. In any case, these 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.

5.3 Robustness

There are three main concerns for our empirical strategy. The first concern is that despite our controlling for province time trends, our baseline results are partly driven by cross-province variation in timing, which is not random. We address this concern by controlling for a dummy that indicates whether any village in a given province has introduced elections. The estimates are shown in Table **??** column (1). They are similar to the baseline estimates shown earlier. 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. An alternative way to check that our estimates are not confounded by province-level factors is to directly control for province-level variables such as per capita GDP, per capita agricultural GDP, and per capita government expenditure for public goods.[42] The estimates with these controls are shown in column (2).

The second concern is that our estimates could be driven by pilot or straggler villages in the

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

reform implementation (see Section **??**), which may have been chosen endogenously. In columns (3) and (4), we repeat our estimates on a restricted sample where we drop pilot and straggler villages.[43]

For brevity, we focus on select key outcomes. Panel A presents estimates for total public goods expenditure funded by villagers. Panel B presents the estimates for log income of households at the ninetieth percentiles of the village income distribution. Panel C presents estimates for the dummy variable indicating whether any arable land in the village is leased out to enterprises. The estimates in columns (1)-(4) show that all three outcomes are robust to the additional controls. The precision varies depending on the controls, but the coefficients are roughly similar in magnitude across specifications.

The third 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. In addition to the correlational evidence presented earlier in Table **??**, we provide several pieces of additional evidence against this possibility.

First, we provide evidence for the parallel trends assumption of the DD estimate that absent elections, the outcomes of villages that introduced elections earlier would have evolved along parallel trends with villages that introduced elections later. Since it is impossible to observe the counterfactual trend, we follow the literature in conducting a pre-trend analysis in the main outcomes of interest. A trend in the outcomes of interest in the years leading up to the introduction of elections would be a sign that they were evolving differently in villages that were about to implement elections. This would raise the suspicion that the timing of elections was endogenous and cast doubt over our interpretation of the results as the causal effect of elections.

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

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

where the outcome of interest is a function of a set of dummies indicating how many years since and from the introduction of elections, χ_{vpt}. The other explanatory variables are the same as the baseline specification, equation (**??**). Since elections begin early in our sample and many observations would

[43] A pilot village is defined as a village that implemented elections before all other villages in the same province. A straggler village is defined as one that implemented elections after all other 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.

be lost by estimating the effects of many lead years, the earliest lead we estimate is four years prior to the election. For this estimate, we group all observations that are four or more years prior to the first election together. These observations comprise the reference group. We also group all observations that are six or more years after the first election into one group. If there are no pre-trends, then the estimated dummies for the years prior to the first election should be similar, $\hat{\beta}_{\varsigma} \approx \hat{\beta}_{\varsigma-1}$ for $\varsigma < 0$. Moreover, if the main results reflect changes that begin when elections are introduced, one should find that the estimated dummies begin to differ from the previous level starting the first year of the election, $\hat{\beta}_{\varsigma} \neq 0$ for $\varsigma \geq 0$. This allows us to examine whether our main results reflect the introduction of elections or whether it captures spurious effects that occur after elections are introduced.[44]

The coefficients and the standard errors for the estimated effects of the number of years since the first election are presented in Online Appendix Table **??**. The coefficients from equation (**??**) are plotted in Figure **??**. There is no evidence of pre-trends for any of the three outcomes, which is consistent with the parallel trends assumption. Moreover, the trend break in the coefficients at year zero show that the effect of elections begin the year of the first election, which lends additional credibility to the interpretation that the DD estimates capture the influence of the electoral reforms rather than spurious trends.

Although there is no evidence of pre-trends, one may still be concerned that election timing is correlated with pre-conditions that affect our outcomes of interest through channels other than elections and that the limited number of villages do not allow us to precisely estimate the correlations between election timing and pre-election variables (e.g., Table **??**). We address this remaining concern with "brute force" and directly control for the interaction terms of the earliest available observation of each of the outcome variables of interest and the full vector of year fixed effects. The interaction with year fixed effects controls for the influence of pre-election public goods expenditure over time in a fully flexible manner, as well as the influences of all of its correlates over time.[45]

[44]Note that for this exercise, we exclude four outlier observations from the estimates for log public goods expenditure from villagers and log household income for the ninetieth percentile households (we do not exclude any observations for the village land leased out because the sample size for that analysis is already much smaller). We identify the outliers as the most influential observations according to estimates of DF betas. Then, we choose an arbitrarily small number of them (four) to drop (i.e., our estimates are similar if we omit three, five or six outlier observations). Relative to the full sample estimates, the ones that we present have less noisy estimates – i.e., we do not have many observations for many years before or after the first election, and they are therefore vulnerable to outliers. The estimates with the full sample are presented in Online Appendix Table **??**.

[45]For villages that introduced the first election very early, before we have outcome data, we use the sample average

For example, controlling for the interactions of early public goods expenditure controls for both the influence of the level of pre-existing public goods expenditure and the influence of pre-existing factors such as social capital, which may be correlated with pre-election public goods expenditure. In particular, we control for the pre-election averages of the probability that any household was permitted to have two or more children and the incidence of upper-government land expropriation, each interacted with the full set of year fixed effects. This is particularly important for the latter since the pre-reform incidence of upper-government land expropriation is the only variable that is statistically correlated to the timing of the first election (see Table **??**). Column (5) of Table **??** shows that the coefficients with this conservative set of controls are very similar to our main estimates, although some of them are less precisely estimated.[46]

An alternative set of factors that could potentially influence the effectiveness of elections include proximity to a city (interacted with year fixed effects), the size of the largest kinship clan (interacted with year fixed effects), proxies for social capital (interacted with year fixed effects), and a dummy for whether a village had ever experienced an administrative merger (interacted with year fixed effects). The size of the largest kinship clan is based on village rosters from 2011. **?** (2007) argues that strong informal institutions (e.g., social capital) are major determinants of policy outcomes, which could weaken the effect of elections. We follow her work in using the presence of a lineage group, which is measured as the presence of a household with a family tree, an ancestral temple, or the presence of a village temple to proxy for informal institutions. Since these variables are time-invariant, we control for their interactions with the full set of year fixed effects to allow their influence to vary fully flexibly over time. We also control for local fiscal reforms such as the *Tax and Fee Reform* which was introduced starting in the early to mid 2000s. Column (6) shows that our coefficients are similar to the baseline, but somewhat less precisely estimated.

In addition to the robustness tests presented in the paper, we conduct many others that are not presented here for brevity.[47] For example, we address the fact that several villages introduced elections prior to when our outcome data begin. To check that our estimates are not driven by

of the outcome variables in place of the pre-election average. Our results are very similar if we restrict the sample to villages for which we have pre-election data. These estimates are available upon request.

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

[47] They are available upon request.

villages for which there is no variation in elections, we re-estimate the effect of elections on each outcome where each regression is restricted to a sample of villages for which we observe public goods before and after the first election. Alternatively, we check that our estimates are not driven by especially motivated villages that implemented their first election before the national law by re-estimating our main equation using a sample restricted to villages that held their first elections after 1987. We also check that our estimates are not driven by selection within counties by instrumenting for the introduction of elections at the village level with the introduction at the county level or with the introduction at the province level.[48] All of the robustness exercises produce estimates that are very similar in magnitude to our baseline results. These results are available upon request.

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.

6 Re-Election Incentives

The literature in political agency proposes two mechanisms for elections to be tools for the citizens to control politicians.[49] First, elections can help voters address moral hazard problems by rewarding good performance with re-election – i.e, elections serve as means to provide the correct *incentives* to office holders. Second, voters can use elections to *select* the politicians that are more competent or whose preferences are better aligned with citizens' preferences. In this section, we investigate the relevance of each force in our context. We provide two pieces of evidence to suggest that the change in leader incentives is a powerful force in rural China. In our setting, we are unfortunately unable to speak to the role of selection effects.

The VDS records the names of VCs both before and after the implementation of elections. This allows us to see whether the average effects that we have identified in the previous sections vary as a function of whether the incumbent village chairman kept his position or was replaced. If the effects of elections are present in villages where the first election does not generate chairman turnover, it follows that these chairmen are acting differently under elections than they were under appointment. Moreover, since there is no change in leadership, selection cannot possibly drive the

[48] Instrumenting with province (county) level reform timing addresses potential selection at the county (village) level. These strategies do not address selection at the province level.

[49] This literature is large, starting with the seminal contribution of **?**. For textbook treatments, see **?**, and **?** .

effect of elections in these villages. Hence, if this is true, it follows that re-election incentives must play an important part in the effects of the introduction of elections.

We present the results of our examination in Table **??**. Panel A shows the average effect of the introduction of elections on our main outcomes for reference: public goods expenditure funded by villagers, the incomes of the ninetieth percentile and land leased out to enterprises. In Panel B, we introduce the interaction of the introduction of elections with a dummy for VC turnover in the first election.[50] In this specification, the effect of elections on villages that experienced no turnover (64% of the villages in our sample) is captured by the main effect. For all the outcomes examined, the main effect is almost identical to the average effect displayed in Panel A. All estimates are statistically significant. Introducing elections had strong effects in villages where it did not trigger turnover. Since selection effects cannot operate in these villages, re-election incentives must play an important part in explaining our results.

The effect of elections on villages that experience turnover is the sum of the main election coefficient and the interaction coefficient. It reflects the combination of re-election incentives and the ability to select a better chairman. The interaction term is the differential effect of elections for villages that experienced turnover. It is small in magnitude, sometimes negative in sign and statistically insignificant. This shows that elections had similar effects in villages that experienced turnover and villages that experienced no turnover. However, this does not mean that selection effects are not an important driver of the main results because turnover is endogenous. For example, villages that experienced turnover after the first election can differ from villages that did not in many ways, and these differences could induce a smaller incentive effect which coupled with a sizable selection effect adds up to a similar total effect of introducing elections.[51] Therefore, the results in this section show that re-election incentives are an important force in rural elections, but are silent on the contribution of selection effects.

7 Mechanisms

To further explore the policy mechanisms driving our main results, we examine the rich household-level data on the sources of income, local tax and fee payments and household farmland. This is

[50] This equation is similar to equation (**??**), with the addition of the interaction term.

[51] We thank an anonymous referee drawing our attention to this possibility.

interesting for several reasons. First, the household data provides a breakdown of the sources of household income as well as some information on expenditures. This allows us to examine the importance of the sources of income that are controlled by the village government relative to total household income. Second, it allows us to provide additional evidence for our interpretation of the main results by checking that the election-induced changes at the village level correspond to relevant changes at the household level. Third, the household data allow us to trace households over time and to precisely estimate the effect of the introduction of elections on the income trajectory of households for different parts of the pre-election village income distribution. This is interesting since the inability to impose recurrent taxes on income may cause the village government to redistribute assets for production (e.g., land) or employment, which can cause redistribution to not be rank-preserving. Such "lumpy" redistribution would cause the main analysis on the time-varying village income distribution to understate the amount of redistribution that occurred due to elections.

We were able to obtain household-level data from the NFS for seventy-three of the 217 villages in the full sample for the years 1986-2005 (excluding 1992 and 1994).[52] To maximize sample size for the following exercises, we interpolate the data for 1992 and 1994 as the average of the preceding and following years.[53]

7.1 Sources of Household Income

Table **??** presents household total income and a breakdown by source. It shows that for the average household, income from agriculture comprises almost 70% of total income. Wage income, which is mostly earned from village enterprises, accounts for about 17% of total income, and dividends and other enterprise income comprise less that 5%.[54] The allocation of land, the main determinant of agricultural income, as well as the management of village enterprises are under the control of the village government. Hence, well over 90% of average household income comes from sources that can be influenced by the village government.

[52] We chose the most populous villages for this subsample so that we can examine the within-village distributional outcomes. The dataset covers 25% of households in each village. The descriptive statistics for the income variables for the full sample and subsample of villages are shown in Online Appendix Table **??**. The subsample turns out to be very similar to the full sample.

[53] Our results are not sensitive to the interpolation. Alternative results are available upon request.

[54] 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.

Next, we investigate how rents from village enterprises are distributed across households, to substantiate anecdotal evidence that claims highly unequal distribution prior to the introduction of elections. Table **??** panel A shows the average pre-election total income and the breakdown by source according to a household's position on the pre-election village distribution for total household income. The means show that prior to the introduction of elections, agricultural income, land and enterprise income all increase with total household income. For enterprise income in particular, the median and bottom quintiles were only getting 47% and 26% of the enterprise income of the richest households, so these rents benefit households that were already richer due to agricultural income. However, this is not the case for wage income, which is almost evenly distributed among the top three quintiles. Two observations follow from these statistics. First, since enterprise income is unevenly distributed, the reduction in land leased to enterprises can contribute to the reduction of total household income for rich households that we found in the full sample. Second, salaried jobs were allocated at least in part for reasons different from allocation of land, which could be a sign of more meritocracy in job allocation.

In Panel B, we present similar descriptive statistics according to households' positions on the pre-election distribution of each type of income. This highlights that the unequal distribution of each type of income does not perfectly align with inequality in total household income. For instance, compared to the analogous statistics shown in panel A, we find that income from village enterprises and wages are even more unevenly distributed. Columns (6) and (7) show that the enterprise incomes from median and lowest enterprise income groups are only 16% and 3% of that from the highest enterprise income group. Similarly, wage incomes from the median and lowest wage income groups are only 25% and 2% of that of the highest wage income group. Since the benefits from enterprise activity –income and wages– are so unevenly distributed, it is credible that the reduction in land leased out to enterprises was likely to have been supported by a large constituency, as we hypothesize in the previous sections.

Given that the categorization used in Panel B exhibits more inequality across households, we will continue to divide households according to their positions on the pre-election village distribution of each type of income when we later examine the effect of the introduction of elections in each type of income.

7.2 Local Taxes and Fees

Since the increase in public goods expenditures due to elections is driven by funds from villagers, we check that it was paralleled by a corresponding increase in the amount of local fee payments by households. Unfortunately, the NFS only reports the sum of taxes collected on behalf of upper levels of government and local fees; it does not distinguish payments to the village government from payments to other levels of government (e.g., county, township). Therefore, interpreting the following results requires the assumption that elections did not change the taxes paid to upper levels. To the best of our knowledge, this is true.[55]

Table **??** shows that households pay 159 RMB per year in local taxes and fees on average, which is approximately 1% of total household income. According to our data, this amounts to 64% of the taxes that households pay to the central government (excluding the value of grain taxes).[56] To examine the distribution of taxes across village households, we examine household payment of local taxes and fees according to households' positions in the within-village distribution of taxes paid for each year. In Table **??**, we present the mean level of local taxes and fees paid by households and the estimated effects of the introduction of elections on taxes and fees for each decile. The estimates show that elections caused a large increase in the amount of local taxes and fees paid by households. The magnitude of the increase (approximately 150-290%) does not systematically vary across deciles, which is consistent with the belief that the newly elected village government did not use taxes to redistribute. The estimates are statistically significant for households at the 10th and 30th-70th percentiles. To assess the plausibility of the magnitude of the effect, it is important to keep in mind that average local taxes and fees are extremely low. Thus, the large percentage increase implied by our estimates does not result in implausibly high payments by households.

The results on local taxes are consistent with the increase in public goods expenditure financed by villagers. A plausible explanation is that elected officials can raise funds because voters trust that elected officials are more likely to allocate public funds according to the preferences of most villagers.

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

[56]The largest proportion of taxes that agricultural households pay to the central government is in the form of grain, which the government buys at a below-market price it sets. The value of this tax payment is not included in the NFS category of taxes to the central government.

7.3 Tracing Households over Time

The final exercise tracks households over time to examine the effect of elections for households according to their positions on the pre-election village distribution. We are able to do this since the NFS comprise a panel of households and attrition rates are low. We divide households into quintiles for each village according to the average of their pre-election outcomes. The need to examine pre-election outcomes restricts our sample to the villages that have income and land data prior to the introduction of elections (i.e., villages that introduced elections after 1986). Thus, our sample is restricted to only 34 villages within ten provinces. We examine total income, wage income, enterprise income, agricultural income and household farmland (i.e., arable land).

The coefficients in Table **??** illustrate a clear pattern. Elections reduced income of richer households and increased the incomes of poorer households. The poorer the household, the bigger the gains. The richer the households, the larger the losses. The estimates are most precisely estimated for the richest and poorest households.[57]

Column (6) of each panel shows the effect of the election on the ratio of income of the middle quintile to the income of the top quintile. The estimates show that elections increased the ratios of total income, agricultural income, village enterprise income and wage income by 12.3, 16.4, 37.3 and 53.4 percentage-points respectively. All of the estimates are statistically significant at the 1% level.

Panel E shows that households with little arable land prior to the election gained a large amount of land (approximately 30%), while households with a large amount of land lost some land. The estimates are statistically significant at the 1% level for the bottom and middle quintiles. Elections increased the ratio of land for the middle quintile to the top quintile by approximately thirteen percentage-points and is statistically significant at the 1% level. The distributional pattern of household farmland corresponds to the distributional patterns of household income, and in particular agricultural income.[58]

[57] They are statistically significant at the 10% or higher level for the bottom and top quintiles for total income in panel A; the bottom two and top quintiles for agricultural income in panel B; and all quintiles for wage income in panel C

[58] Note that increases in farmland for pre-election land poor households come from three sources: *i*) land that is redistributed from pre-election land rich households, *ii*) land that was previously leased to enterprises, and *iii*) the increase in arable land from irrigation. A back-of-the envelop calculation shows that all three changes need to be taken into account to explain the increase in household farmland for poor households.

These results are consistent with our interpretation that the loss in total income by rich households shown in the main results is caused by a change in village government policies that takes place when elections are introduced. It is interesting to note that the implied redistribution is much larger in magnitude when we examine the distribution according to households' positions on the pre-election income distribution (in this section) than when we examine the changes in the village income distribution (main results in section **??**).[59]

To clearly illustrate the evolution of income and assets after the introduction of elections, we estimate the year-by-year effects since the introduction of elections on each outcome (see equation (**??**)). The reference group is comprised of observations that are four or more years prior to the first election. The coefficients and standard errors are presented in Online Appendix Table **??**.[60] To visualize the effect of elections on the income distribution, Figure **??** plots the fitted values over time. Each sub-figure plots focuses on one outcome variable. The figures show that previously (land) rich households experience losses, and previously (land) poor households experience gains. Middle households experience little change. Although the convergence between the pre-election rich and poor continues gradually over time, we do not observe large changes in income ranks (e.g., the fitted values rarely cross). This may reflect the fact that the ability to redistribute is limited, or the fact that our data does not allow us to examine a longer time horizon after the introduction of elections.[61]

Note that our DD strategy implies that the estimates are highly unlikely to be driven by mechanical mean reversion due to idiosyncratic shocks. Such a stochastic process would make rich

[59] We also estimate the effect of elections on the time-varying income distribution for the villages for which we have household level data. The coefficients have a similar pattern as with the full sample and exhibit less re-distribution than the time-invariant analysis in this section. They show that rich households lose in terms of wage income, agricultural income and land, the dimensions controlled by the village government. We report these results in Online Appendix Table **??**.

[60] The point estimates are plotted in Online Appendix Figure **??**.

[61] The interpretation for these redistribution results is not obvious. The pre-election elites lost and the pre-election poor gained, while the pre-election median quintile experienced little improvement. These results do not fit a median voter model (e.g. **?**, 1981). Instead, the pattern of re-allocation across households could reflect two different and non-mutually exclusive forces. First, it could be a consequence of the coarse redistributive tools of the village leadership. For example, if the village leadership can only reassign lumpy goods across households, such as good plots of land, then a plausible result of its desire to redistribute in order to gain votes is to reallocate some of these plots from rich households to poor households, thereby creating the pattern observed in the data. Second, it could be that the relevant social connections necessary to obtain favors changed with the introduction of elections. For example, consider the possibility that a connection to the PS was valuable in the pre-election regime, whereas a connection to the VC became valuable after elections are introduced. If having a connection with the VC is only a weak predictor of having a connection with the PS, then elections would generate the pattern revealed by the data. Unfortunately, distinguishing between these two explanations is beyond the scope of our data.

households more likely to suffer relative income losses and poor households more likely to experience relative income gains on a year-to-year basis. However, as long as this underlying process does not systematically change when elections are introduced, it is not driving our results.

To check the robustness of our estimates, we conduct the same set of sensitivity checks as for the full sample estimates. They are shown in Table **??**. For brevity, we focus on the income and land ratios for households that were in the middle of the village distribution to households that were on the top of the village distribution. Our estimates are very robust.

In summary, the household-level analysis provides several insights that support our interpretation of the main results. First, the result that elections increased household local tax payments is consistent with the finding that the increase in public goods expenditures after elections were introduced was financed by villagers. Second, the results show that the village government responded to the introduction of elections by redistributing income with the policy tools available to them – i.e., farmland, revenues from village enterprises, and village enterprise employment– which reduced village income inequality.

8 Conclusion

In order to better control local officials and prevent shirking and rent-seeking, the Chinese government introduced village-level elections starting in the 1980s. Our study investigates the effects of this introduction. The results suggest that reforms were successful in changing the incentives of local officials to favor the interests of the village majority. In particular, we find that public goods expenditure made by the village government increases, and that this increase is financed by funds collected from villagers. With a smaller sample of household-level data, we find consistent results that suggest that the introduction of elections increased the amount of local fees and taxes paid by villagers. Moreover, to the extent that the data allow, we find that the mix of public goods provided is consistent with villager demand. Together, these results are consistent with elections causing villagers to be more willing to give funds to the local government because the latter is more likely to provide the public goods desired by villagers.

We also find that elections induce village governments to rent less land to village enterprises, which we show benefit only a portion of villagers. Consistent with the view that the rents created by these enterprises were being captured by the village elite, we find income reductions among the

richest households that lead to a moderate reduction in inequality. When we track households over time, we see that the pre-election rich experience a decline in income, while the pre-election poor experience an increase in income. Moreover, these changes in income are driven by the types of income that the village leaders can most easily influence through their control over land allocation and village enterprises. For instance, the pre-election land-rich experience a reduction in household farmland, while the pre-election land-poor experience a gain in household farmland. These results suggest that the introduction of elections induced policy changes that were not favorable to the elites, but were demanded by a majority of villagers.

Interestingly, we observe that elections have the same effects in the set of villages where the appointed incumbent village chairman remains in office when elections are introduced. This provides evidence that re-election incentives are an important mechanism behind the policy changes induced by the introduction of elections in rural China. Unfortunately, for the reasons that are discussed in the paper, our setting does not allow an assessment of the relative importance of the capacity of villagers to select better leaders.

It is beyond the scope of our study to provide a full welfare assessment of the policy changes induced by elections. On the one hand, given the extremely low prevailing levels of public good provision, the increase in public good expenditure is probably welfare enhancing. On the other hand, village enterprises were productive and jobs might have been allocated efficiently in the pre-election regime. If this was the case, the scaling down of enterprises and the change in the allocation of salaried jobs might have reduced aggregate welfare and therefore simply constitute an inefficient way of engaging in redistribution in order to court the votes of the majority. However, it is also possible that the households benefitting from enterprise activity and salaried jobs were not the entrepreneurial and successful ones but the cronies of the village government.[62] In this case, the reductions in income among the richest households would be consistent with a reduction in rent-seeking. The truth probably lies somewhere between these two extremes and the causes for there to be inequality prior to the introduction of elections are likely to vary across villages.

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 theories that characterize democracies as regimes that are likely to engage in redistribution (e.g. **?**, 2001, 2006; **?**, 2003) and to provide more

[62]This is certainly the interpretation of village government conduct in **?**, **?**, **?**, **?** and **?**

public goods (e.g., **?**, 2008; **?**, 2003).

The results from this study open several interesting questions for future research. One important question regards the heterogeneous effects of elections, which can potentially provide insights into the pre-conditions of successful elections. For example, in subsequent research we investigate the roles of social fragmentation and social capital in determining the effects of elections on public goods. Also, the motivation for autocratic regimes to introduce local democracy is an important subject for future research. In particular, 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.[63] We hope that the detailed institutional and economic data we have constructed will help future researchers in addressing these and other important questions.

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

Table 1: Descriptive Statistics

Variable	Source	Obs	Mean	Std. Dev.
# of HH in Village	NFS	3,641	421.81	280.81
Near City	NFS	5,208	0.30	0.46
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
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	5208	0.73	0.44
Has Open Nomination	VDS	5208	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 Election Timing

Dependent Variable: Year of 1st Election Residuals				
	Coefficient	Standard Error	Standardized Effect	Obs.
Village Size (Tot # of HH)	0.001	(0.001)	0.043	217
At least one clan has an ancestral temple *(ci tang)*	-0.280	(0.684)	-0.028	217
Village has a family tree (*jia pu*)	0.363	(1.047)	0.024	217
Village has a large clan	0.600	(1.347)	0.030	217
Near City Dummy	-0.038	(0.709)	-0.004	217
Ln Total Income	0.809	(0.662)	0.083	217
Ln Income of 10th Percentile	1.353	(0.821)	0.112	217
Ln Income of 50th Percentile	1.297	(0.932)	0.095	217
Ln Income of 90th Percentile	0.676	(0.741)	0.062	217
Income Growth 10th Percentile	1.151	(2.059)	0.038	217
Income Growth of 50th Percentile	2.312	(3.239)	0.049	217
Income Growth of 90th Percentile	0.434	(1.573)	0.019	217
Income Ratio 0-20/80-100	-0.165	(5.769)	-0.005	34
Agricultural Income Ratio 0-20/80-100	-1.312	(4.586)	-0.051	34
Wage Income Ratio 0-20/80-100	-1.627	(1.784)	-0.159	34
Land Ratio 0-20/80-100	-2.073	(4.415)	-0.083	34
HH Arable Land 10th Percentile	-1.152	(0.851)	-0.158	74
HH Arable Land 50th Percentile	0.454	(0.850)	0.063	74
HH Arable Land 90th Percentile	0.307	(0.866)	0.042	74
One Child Policy Exemption Dummy	-0.139	(0.781)	-0.012	217
Upper Government Land Expropriation Dummy	26.553	(12.299)	0.146	217
Ln Spending in Special Aid	1.967	(2.117)	0.063	217
Distance to High School	-0.235	(0.300)	-0.055	203
Ln Total Investment in Public Goods	0.057	(0.291)	0.013	217
Ln Total Spending in Irrigation	0.735	(1.069)	0.047	217
Ln Total Spending in Education	-2.956	(3.396)	-0.059	217
Ln Total Arable Land	0.016	(0.011)	0.092	216
School Enrollment Rate	0.039	(0.068)	0.040	211

Notes: Each row corresponds to one cross-sectional regression in which the dependent variable is the year in which the village introduced elections. The explanatory variable of interest corresponds to the pre-election average of that characteristic after it has been partialled out against year fixed effects, province fixed effects, and province-specific time trends. For villages that introduced elections before the explanatory variable started being reported, the pre-election average is substituted by the average over the entire sample period. Then, the year of introduction of elections is regressed against the corresponding explanatory variable and a constant term.

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

	Dependent Variable			
	Ln Expenditure by Source			Provision
	Total	Villagers	Non-Village	Share of Arable Land
	(1)	(2)	(3)	(4)
	A. Total Expenditure			
Dep. Var. Mean (not logged, 10,000 RMB)	*14.28*	*9.77*	*4.42*	
Post 1st Election	0.272	0.309	0.002	
	(0.150)	(0.119)	(0.079)	
Wild Bootstrap p-value	[0.066]	[0.024]	[0.972]	
Observations	4,340	4,340	4,340	
R^2	0.191	0.171	0.188	
Years	1986-2005	1986-2005	1986-2005	
# of villages, # provinces	217, 29	217, 29	217, 29	
	B. Irrigation			
Dep. Var. Mean (not logged, 10,000 RMB)	*3.43*	*2.02*	*1.41*	*0.51*
Post 1st Election	-0.275	-0.193	-0.075	-0.150
	(0.103)	(0.147)	(0.091)	(0.056)
Wild Bootstrap p-value	[0.012]	[0.076]	[0.570]	[0.096]
Post 1st Election x Avg Ln Village Farm Land	0.055	0.041	0.012	0.018
	(0.013)	(0.019)	(0.013)	(0.007)
Wild Bootstrap p-value	[0.002]	[0.012]	[0.474]	[0.104]
Observations	4,340	4,340	4,340	3,277
R^2	0.120	0.123	0.106	0.917
Years	1986-2005	1986-2005	1986-2005	1986-2005§
# of villages, # provinces	217, 29	217, 29	217, 29	217, 29

Notes: All regressions control for post first open nomination, province trends, village and year fixed effects. The regressions in Panel B also control for the interaction of average ln household farm land with post first open nominations. Standard errors, clustered at the province level are presented in parentheses. Wild bootstrap p-values, clustered at the province level are presented in square brackets. Arable land is measured with one year lead to capture that investments in irrigation in time t will have an effect on the amount of arable land in t+1. The years and villages of data coverage are shown at the bottom of each panel. The sample comprises a balanced panel of 217 villages. § Except years 1992 and 1994.

Table 4: The Effect of the Introduction of Elections on Land Leased to Enterprises and the Village Income Distribution

	Dependent Variable							
			Ln Percentiles of the Within-Village Distribution of Total Household Income (in RMB)					
	Ln Village Land Leased Out to Enterprises	Village Land Leased Out to Enterprises Dummy	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}	Income Ratio $50^{th}/90^{th}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. Mean (not logged)	*114*	*0.41*	*5,083*	*7,378*	*10,527*	*15,337*	*24,476*	*0.529*
Post 1st Election	-0.571	-0.127	-0.013	-0.026	-0.037	-0.056	-0.084	0.017
	(0.265)	(0.069)	(0.054)	(0.023)	(0.023)	(0.025)	(0.029)	(0.007)
Wild Bootstrap p-value	[0.064]	[0.096]	[0.728]	[0.274]	[0.114]	[0.034]	[0.012]	[0.024]
Observations	1,796	1,796	3,763	3,763	3,763	3,763	3,763	3,763
R^2	0.442	0.423	0.611	0.923	0.933	0.928	0.909	0.597
Years	1987-2005§	1987-2005§	1986-2005§	1986-2005§	1986-2005§	1986-2005§	1986-2005§	1986-2005§
# of villages, # provinces	108, 28	108, 28	217, 29	217, 29	217, 29	217, 29	217, 29	217, 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, clustered at the province level are presented in square brackets. The years and villages of data coverage are shown at the bottom of each panel. The sample comprises a balanced panel of villages. § Except years 1992 and 1994.

Table 5: The Effect of the Introduction of Elections on Public Goods, Land and Income – Robustness to Controls

	Additional Controls & Robustness					
	Control for Province Introduction	Other Province - Level Controls	Omit pilot villages in each province	Omit straggler villages in each province	Control for 1st year conditions	Additional Controls
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Dependent Variable: Ln Total Public Goods Expenditure from Villagers					
Post 1st Election	0.325	0.333	0.280	0.303	0.267	0.313
	(0.123)	(0.120)	(0.126)	(0.123)	(0.129)	(0.115)
Wild Bootstrap p-value	[0.014]	[0.016]	[0.036]	[0.028]	[0.044]	[0.012]
Observations	4,340	4,018	4,020	3,880	4,320	4,340
R^2	0.172	0.178	0.175	0.170	0.214	0.194
Years	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005
# of villages, # provinces	217, 29	217, 29	201, 27	194, 27	216, 29	217, 29
	B. Dependent Variable: Ln Total Household Income (90th percentile)					
Post 1st Election	-0.082	-0.069	-0.096	-0.055	-0.083	-0.083
	(0.029)	(0.029)	(0.031)	(0.030)	(0.027)	(0.031)
Wild Bootstrap p-value	[0.012]	[0.022]	[0.004]	[0.106]	[0.008]	[0.012]
Observations	3,763	3,477	3,487	3,363	3,750	3,763
R^2	0.909	0.909	0.915	0.911	0.912	0.913
Years	1986-2005	1986-2005§	1986-2005§	1986-2005§	1986-2005§	1986-2005
# of villages, # provinces	217, 29	217, 29	201, 27	194, 27	216, 29	217, 29
	C. Dependent Variable: Village Land Leased Out to Enterprises Dummy					
Post 1st Election	-0.117	-0.142	-0.153	-0.119	-0.118	-0.145
	(0.069)	(0.078)	(0.054)	(0.087)	(0.075)	(0.069)
Wild Bootstrap p-value	[0.140]	[0.122]	[0.02]	[0.228]	[0.142]	[0.078]
Observations	1,796	1,687	1,626	1,621	1,796	1,796
R^2	0.424	0.458	0.410	0.405	0.432	0.451
Years	1987-2005§	1987-2005§	1987-2005§	1987-2005§	1987-2005§	1987-2005§
# of villages, # provinces	108, 28	108, 29	98, 26	97, 27	108, 28	108, 28

Notes: All regressions control for post first open nomination, province-time trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values, clustered at the province level, are presented in square brackets. In column (2), provincial controls include GDP per capita, agricultural GDP per capita, GDP per capita growth rate, percentage of population working in agriculture, provincial expenditure per capita, expenditure in education and health per capita, administrative expenditures per capita, enrollment in primary, secondary, and higher education. In column (3), we omit pilot villages from the sample. A pilot village is defined as those that are the first to introduce elections in their province and that introduce elections alone (i.e., there is no other village in the same province that introduces elections in the same year). In column (4), we omit straggler villages from the sample. A straggler village is defined as those that are the last to introduce elections in a province and that introduce elections alone (i.e., there is no other village in the same province that introduces elections in the same year). In column (5), first year controls include the interaction of year fixed effects with the value of the following variables measured in the first year of sample period. The variables are: ln public goods expenditure, the probability that any households was allowed to have two or more children and the incidence of upper government land expropriation. In column (6), the controls include a dummy variable for whether a village is near city interacted with year fixed effects, the population share of the largest kinship group interacted with year fixed effects, social capital proxies (three dummy variables indicating the presence of a family with a family tree, an ancestral temple, and the presence of a village temple) interacted with year fixed effects, and the introduction of the Tax and Fee Reform. The years and villages of data coverage are shown at the bottom of each panel. Unless otherwise stated, the sample comprises a balanced panel of 217 villages. § Except years 1992 and 1994.

Table 6: The Effect of the Introduction of Elections for Villages where the Pre-Election Incumbent Remained in Office

	(1) Ln Public Goods Expenditure from Villagers	(2) Ln Income 90th percentile	(3) Ln Village Land Leased Out to Enterprises
	A. Baseline		
Post 1st Election	0.309	-0.084	-0.127
	(0.119)	(0.029)	(0.069)
Wild Bootstrap p-value	[0.024]	[0.012]	[0.096]
Observations	4,340	3,763	1,796
R-squared	0.171	0.909	0.442
Years	1986-2005	1986-2005§	1987-2005§
# of villages, # provinces	217, 29	217, 29	108, 28
	B. Interaction Effects		
Post 1st Election	0.349	-0.076	-0.122
	(0.151)	(0.028)	(0.057)
Wild Bootstrap p-value	[0.038]	[0.024]	[0.076]
Post 1st Election x 1st Election VC Change	-0.118	0.006	0.015
	(0.279)	(0.053)	(0.177)
Wild Bootstrap p-value	[0.664]	[0.884]	[0.930]
Observations	3,920	3,407	1,696
R^2	0.177	0.910	0.418
Years	1986-2005	1986-2005§	1987-2005§
# of villages, # provinces	196, 28	196, 28	102, 27

Notes: All regressions control for post first open nomination, province-time trends, village and year fixed effects. The regressions in Panel B also control for the interaction term of post first open nomination and whether the first election caused VC turnover. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values, clustered at the province level are presented in square brackets. The years and villages of data coverage are shown at the bottom of each panel. Unless otherwise stated, the sample comprises a balanced panel of 217 villages. § Except years 1992 and 1994.

Table 7: The Sources of Household Income and Expenditure

	Obs	Mean	Std. Dev.
Total Income	1,443	15,194	18,964
from agriculture (and home production)	1,443	10,455	13,793
from wages	1,443	2,630	3,907
village enterprise income	1,443	718	1,963
other	1,443	1,391	3,685
Total Expenditures	1,443	11,976	17,052
Household management expenditures	1,443	3,806	8,858
Taxes to central government	1,443	249	509
Levies and fees to local governments	1,443	159	196
Total Consumption	1,443	6,638	8,019

Notes: Village enterprise income is the sum of income from collectives, dividends and parternship/ cooperatives. The sample comprises of an unbalanced panel of 73 villages in 10 provinces for the years for the years 1986-2005. Data for 1992 and 1994 are interpolated as the average of data from one year prior and one year afterwards.

Table 8: Pre-Election Income Distribution and Sources

	Pre-Election Household Income by Source						
	0-20th (1)	20th-40th (2)	40th-60th (3)	60th-80th (4)	80th-100th (5)	40-60/80-100 (6)	20-40/80-100 (7)
	A. Quintiles According to Pre-Election Distribution of Total Household Income						
Total Income	4,107	6,166	7,752	10,283	23,407	0.33	0.18
Agriculture and Home Production	2,573	3,961	5,123	7,484	19,372	0.26	0.13
Enterprise Income	201	287	368	504	776	0.47	0.26
Wages	756	1,148	1,365	1,228	1,324	1.03	0.57
Farmland	5	6	7	7	8	0.90	0.61
	B. Quintiles According to Pre-Election Distribution of the Corresponding Type of Income						
Total Income	4,107	6,166	7,752	10,283	23,407	0.33	0.18
Agriculture and Home Production	2,182	3,582	5,169	7,487	19,652	0.26	0.18
Enterprise Income	38	106	213	414	1,336	0.16	0.03
Wages	67	365	780	1,460	3,135	0.25	0.02
Farmland	3	5	6	7	10	0.58	0.32

Notes: The sample comprises of an unbalanced panel of 73 villages in 10 provinces for the years for the years 1986-2005. Data for 1992 and 1994 are interpolated as the average of data from one year prior and one year afterwards.

Table 9: The Effect of the Introduction of Elections on Household Payments of Local Taxes

Dependent Variable: Ln Household Local Taxes at each Percentile of the Village Distribution of Local Tax Payments									
	10th	20th	30th	40th	50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep.Var. Mean (not logged, RMB)	*67.11*	*97.53*	*119.1*	*139.5*	*158.4*	*178.5*	*200.8*	*232.3*	*291.1*
Post 1st Election	1.571	0.821	0.891	1.156	1.099	1.206	1.197	0.817	0.260
	(0.567)	(0.644)	(0.696)	(0.697)	(0.729)	(0.742)	(0.684)	(0.690)	(0.488)
Wild Bootstrap p-value	[0.009]	[0.222]	[0.186]	[0.082]	[0.138]	[0.100]	[0.068]	[0.224]	[0.628]
Observations	1,443	1,443	1,443	1,443	1,443	1,443	1,443	1,443	1,443
R^2	0.501	0.499	0.506	0.488	0.516	0.512	0.509	0.493	0.433

Notes: All regressions control for post open nomination, province trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values, clustered at the province level are presented in square brackets. The sample comprises of an unbalanced panel of 73 villages in 10 provinces for the years for the years 1986-2005. Data for 1992 and 1994 are interpolated as the average of data from one year prior and one year afterwards.

Table 10: The Effect of the Introduction of Elections on Household Income and Farmland According to Pre-election Positions on the Village Distribution

	Pre-Election Income Distribution (within villages)					Income Ratio
	0-20	20 - 40	40 -60	60 -80	80 -100	40-60/80-100
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Dependent Variable: Ln Total Household Income					
Dep. Var. Mean (not logged)	*9,650*	*11,031*	*12,299*	*14,324*	*21,823*	*0.56*
Post 1st Election	0.170	0.022	0.026	-0.081	-0.219	0.123
	(0.064)	(0.055)	(0.054)	(0.065)	(0.060)	(0.029)
Wild Bootstrap p-value	[0.096]	[0.680]	[0.624]	[0.266]	[0.014]	[0.010]
Obs	673	673	673	673	673	673
R^2	0.872	0.879	0.881	0.871	0.831	0.554
	B. Dependent Variable: Ln Agricultural Income					
Dep. Var. Mean (not logged)	*6,145*	*6,706*	*7,350*	*9,592*	*17,172*	*0.43*
Post 1st Election	0.459	0.284	0.069	-0.011	-0.309	0.164
	(0.101)	(0.047)	(0.079)	(0.075)	(0.062)	(0.045)
Wild Bootstrap p-value	[0.000]	[0.004]	[0.408]	[0.936]	[0.008]	[0.010]
Obs	673	673	673	673	673	673
R^2	0.833	0.781	0.805	0.801	0.798	0.555
	C. Dependent Variable: Ln Wage Income					
Dep. Var. Mean (not logged)	*2,122*	*2,379*	*2,427*	*2,775*	*3,727*	*0.65*
Post 1st Election	7.265	3.428	1.322	-0.209	-0.736	0.534
	(0.720)	(0.707)	(0.255)	(0.305)	(0.177)	(0.210)
Wild Bootstrap p-value	[0.000]	[0.001]	[0.000]	[0.764]	[0.008]	[0.000]
Obs	672	672	672	672	672	672
R^2	0.769	0.669	0.648	0.617	0.600	0.163
	D. Ln Village Enterprise Income					
Dep. Var. Mean (not logged)	*317.9*	*344.7*	*416.8*	*490.0*	*1366*	*0.564*
Post 1st Election	5.652	4.654	2.965	0.841	-0.076	0.373
	(0.708)	(0.690)	(0.578)	(0.329)	(0.290)	(0.145)
Wild Bootstrap p-value	[0.000]	[0.000]	[0.000]	[0.010]	[0.812]	[0.108]
Obs	673	673	673	673	673	649
R^2	0.681	0.654	0.632	0.550	0.466	0.253
	E. Ln Household Farmland					
Dep. Var. Mean (not logged)	*5.051*	*6.125*	*6.792*	*7.505*	*9.266*	*0.73*
Post 1st Election	0.302	0.035	0.057	-0.072	-0.12	0.129
	(0.101)	(0.047)	(0.015)	(0.029)	(0.049)	(0.037)
Wild Bootstrap p-value	[0.006]	[0.467]	[0.012]	[0.044]	[0.030]	[0.010]
Obs	662	662	662	662	662	662
R^2	0.929	0.935	0.929	0.945	0.921	0.407

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, clustered at the province level are presented in square brackets. The sample comprises a balanced panel of 34 villages in 10 provinces for the years 1986-2005. The number of observations vary due to missing values in the dependent variables. In particular, for some villages and years no household reports having received wage income, generating some missing values in the wage ratio. Data for 1992 and 1994 are interpolated when possible.

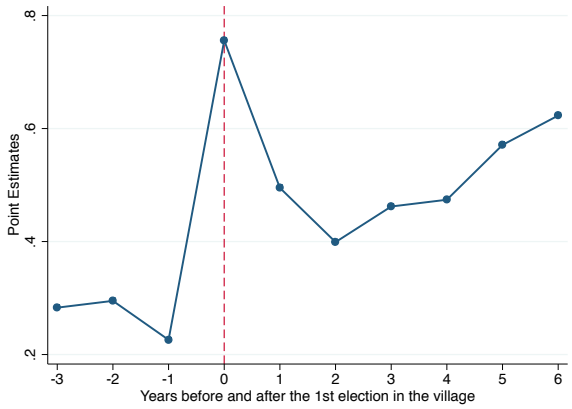
Table 11: The Effect of the Introduction of Elections on Household Income and Farmland Ratios according to Pre-election Positions on the Village Distribution – Robustness to Controls

Additional Controls & Robustness	Control for Province Introduction	Other Province - Level Controls	Omit pilot villages in each province	Omit straggler villages in each province	Control for 1st year conditions	Additional Controls
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Dependent Variable: Ratio of Total Household Income 40-60/80-100					
Post 1st Election	0.123	0.103	0.123	0.131	0.151	0.147
	(0.029)	(0.033)	(0.029)	(0.043)	(0.047)	(0.036)
Wild Bootstrap p-value	[0.016]	[0.018]	[0.019]	[0.010]	[0.032]	-
Observations	673	655	673	575	673	673
R^2	0.554	0.573	0.554	0.552	0.588	0.619
Years	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005
# of villages, # provinces	34, 10	34, 10	34, 10	29, 9	34, 10	34, 10
	B. Dependent Variable: Ratio of Agricultural Income 40-60/80-100					
Post 1st Election	0.164	0.159	0.164	0.146	0.172	0.171
	(0.045)	(0.055)	(0.045)	(0.048)	(0.065)	(0.038)
Wild Bootstrap p-value	[0.014]	[0.036]	[0.012]	[0.054]	[0.038]	-
Observations	673	655	673	575	673	673
R^2	0.555	0.570	0.555	0.551	0.584	0.647
Years	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005
# of villages, # provinces	34, 10	34, 10	34, 10	29, 9	34, 10	34, 10
	C. Dependent Variable: Ratio of Wage Income 40-60/80-100					
Post 1st Election	0.534	0.495	0.534	0.520	0.550	0.314
	(0.210)	(0.171)	(0.210)	(0.256)	(0.207)	(0.182)
Wild Bootstrap p-value	[0.004]	[0.000]	[0.000]	[0.006]	[0.000]	-
Observations	663	645	663	565	663	663
R^2	0.163	0.179	0.163	0.167	0.190	0.400
Years	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005
# of villages, # provinces	34, 10	34, 10	34, 10	29, 9	34, 10	34, 10
	D. Dependent Variable: Ratio of Enterprise Income 40-60/80-100					
Post 1st Election	0.373	0.249	0.373	0.328	0.368	0.448
	(0.145)	(0.170)	(0.145)	(0.114)	(0.157)	(0.164)
Wild Bootstrap p-value	[0.076]	[0.226]	[0.088]	[0.022]	[0.064]	-
Observations	649	631	649	551	649	649
R^2	0.253	0.267	0.253	0.277	0.288	0.324
Years	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005
# of villages, # provinces	34, 10	34, 10	34, 10	29, 9	34, 10	34, 10
	E. Dependent Variable: Ratio of Household Farmland 40-60/80-100					
Post 1st Election	0.126	0.107	0.127	0.149	0.151	0.108
	(0.036)	(0.031)	(0.036)	(0.048)	(0.033)	(0.036)
Wild Bootstrap p-value	[0.024]	[0.008]	[0.024]	[0.058]	[0.018]	-
Observations	662	644	662	575	662	662
R^2	0.407	0.435	0.407	0.374	0.451	0.539
Years	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005	1986-2005
# of villages, # provinces	34, 10	34, 10	34, 10	29, 9	34, 10	34, 10

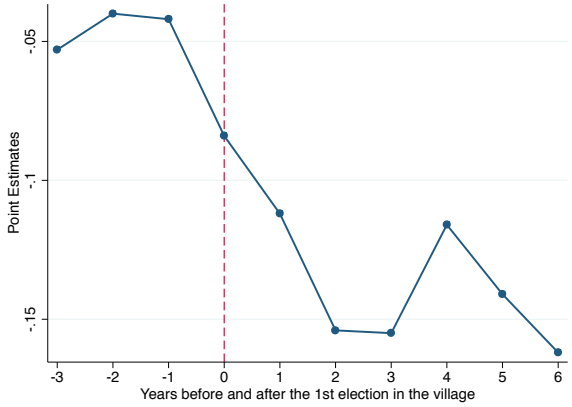
Notes: All regressions control for post first open nomination, province-time trends, village and year fixed effects. Standard errors, clustered at the province level, are presented in parentheses. Wild bootstrap p-values, clustered at the province level are presented in square brackets. In column (2), provincial controls include GDP per capita, agricultural GDP per capita, GDP per capita growth rate, percentage of population working in agriculture, provincial expenditure per capita, expenditure in education and health per capita, administrative expenditures per capita, enrollment in primary, secondary, and higher education. In column (3), we omit pilot villages from the sample. A pilot village is defined as those that are the first to introduce elections in their province and that introduce elections alone (i.e., there is no other village in the same province that introduces elections in the same year). In column (4), we omit srtaggler villages from the sample. A straggler village is defined as those that are the last to introduce elections in a province and that introduce elections alone (i.e., there is no other village in the same province that introduces elections in the same year). In column (5), first year controls include the interaction of year fixed effects with the value of the following variables measured in the first year of sample period. The variables are: public goods expenditure, the probability that any households was allowed to have two or more children and the incidence of upper government land expropriation. In column (6), the controls include a dummy variable for whether a village is near city interacted with year fixed effects, the population share of the largest kinship group interacted with year fixed effects, social capital proxies (three dummy variables indicating the presence of a family with a family tree, an ancestral temple, and the presence of a village temple) interacted with year fixed effects and the introduction of Tax and Fee Reforms. Bootstrap p-values could not be computed for column (6) because of insufficient data. The sample comprises a balanced panel of 34 villages in 10 provinces for the years 1986-2005. The number of observations vary due to missing values in the dependent variables. In particular, for some villages and years no household reports having received wage income, generating some missing values in the wage ratio. Data for 1992 and 1994 are interpolated when possible.

Figure 1: The Year-by-Year Effects of Elections on Public Goods, Household Income and Village Land Leased Out to Enterprises

(a) Ln Expenditure on Public Goods



(b) Ln ninetieth Percentile Household Income



(c) Ln Land Leased Out to Enterprises

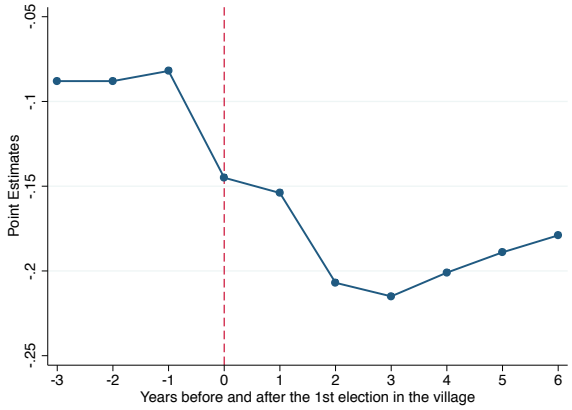
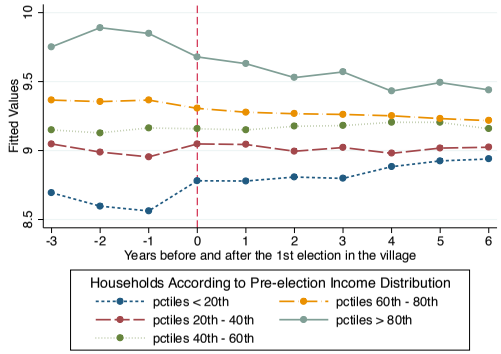
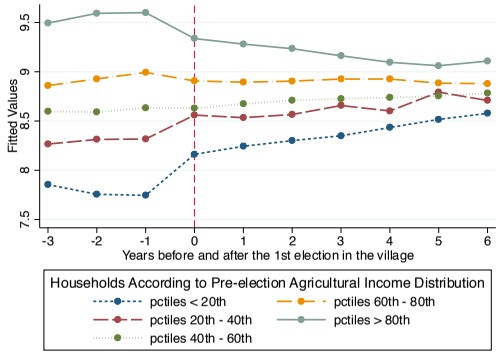


Figure 2: Predicted Evolution of Household Income and Farmland According to Households' Positions on the Pre-Election Village Distribution (Household Subsample)

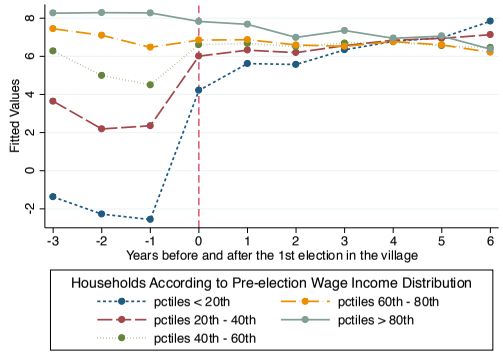
(a) Ln Total Household Income



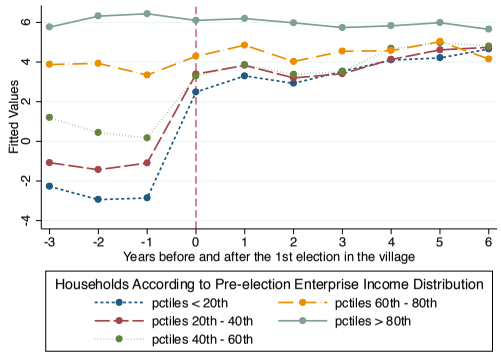
(b) Ln Agricultural Income



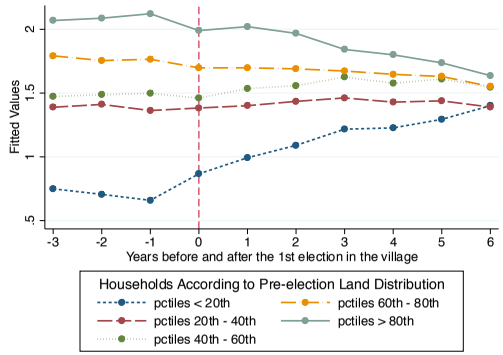
(c) Ln Wage Income



(d) Ln Village Enterprise Income



(e) Ln Household Farmland



ONLINE APPENDIX

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. The 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/styled-4/styled-5/index.html.

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: Descriptive Statistics for Household Incomes of the Full Sample and the Household Subsample of Villages

Variable	Source	Obs	Mean	Std. Dev.
A. Full Sample				
Median HH Gross Income (RMB)	NFS	3,763	10,527	8,363
Median HH Annual Gross Income Growth	NFS	3,084	0.069	0.189
HH Income 50th/90th	NFS	3,763	0.529	0.122
HH Income 10th/90th	NFS	3,763	0.275	0.104
B. Sub-sample with Household Variables				
Median HH Gross Income (RMB)	NFS	1,443	10,915	9,941
Median HH Gross Income Growth	NFS	1,353	0.10	0.32
HH Income 50th/90th	NFS	1,443	0.51	0.13
HH Income 10th/50th	NFS	1,443	0.26	0.11
Median HH Farm Land (Mu)	NFS	1,426	5.34	6.09
HH Farm Land 50th/90th	NFS	1,412	0.58	0.14
HH Farm Land 10th/50th	NFS	1,385	0.38	0.23
Median HH Fees & Levies to Village and County (100 RMB)	NFS	1,443	158.38	185.06

In Panel A, the sample comprises a balanced panel of 217 villages in 29 provinces for 1986 to 2005, excluding 1992 and 1994. In Panel B, the sample comprises of an unbalanced panel of 73 villages in 10 provinces for the years for the years 1986-2005. When possible, data for 1992 and 1994 are interpolated as the average of data from one year prior and one year afterwards. In both panels, the number of observations vary due to missing values.

Table A.3: Year-by-Year Estimates of the Effect of Elections on Public Goods, Household Income for the 75th and 90th Percentiles of Households and Land Leased out to Enterprises from the Full Sample Data

	Full Sample		Omit Outliers	
	(1)	(2)	(3)	(4)
The # of years since the first election:	coef	se	coef	se
A. Dependent Variables: Ln Total Public Investment from Villagers				
-3	0.288	(0.213)	0.283	(0.213)
-2	0.439	(0.222)	0.295	(0.206)
-1	0.232	(0.189)	0.226	(0.189)
0	0.762	(0.203)	0.756	(0.203)
1	0.500	(0.218)	0.495	(0.219)
2	0.401	(0.193)	0.399	(0.193)
3	0.466	(0.203)	0.462	(0.203)
4	0.479	(0.194)	0.474	(0.194)
5	0.57	(0.214)	0.571	(0.214)
6	0.621	(0.224)	0.623	(0.224)
Obs	4340		4,336	
R^2	0.173		0.172	
p-value F test, pre-election =0	0.272		0.497	
B. Dependent Variable: Ln Total Household Income (90th percentile)				
-3	-0.083	(0.044)	-0.053	(0.044)
-2	-0.041	(0.045)	-0.040	(0.045)
-1	-0.042	(0.050)	-0.042	(0.050)
0	-0.082	(0.051)	-0.084	(0.051)
1	-0.112	(0.055)	-0.112	(0.055)
2	-0.153	(0.057)	-0.154	(0.057)
3	-0.154	(0.061)	-0.155	(0.061)
4	-0.115	(0.067)	-0.116	(0.067)
5	-0.140	(0.071)	-0.141	(0.071)
6	-0.161	(0.074)	-0.162	(0.074)
Obs	3,763		3,759	
R^2	0.910		0.910	
p-value F test, pre-election =0	0.161		0.663	
C. Dependent Variable: Village Land Leased Out to Enterprises Dummy				
-3	-0.088	(0.115)		
-2	-0.088	(0.104)		
-1	-0.082	(0.109)		
0	-0.145	(0.113)		
1	-0.154	(0.117)		
2	-0.207	(0.125)		
3	-0.215	(0.133)		
4	-0.201	(0.145)		
5	-0.189	(0.149)		
6	-0.179	(0.159)		
Obs	1,796			
R^2	0.424			
p-value F test, pre-election =0	0.849			

Notes: All regressions control for post-open nomination, province-time trends, village, and year fixed effects. Standard errors are clustered at the province level. Columns (3) to (6) show the estimates when omitting outlier observations. Unless otherwise stated, the samples used are the following. In Panel A, the sample comprises a balanced panel of 217 villages in 29 provinces for 1986 to 2005, excluding 1992 and 1994. In Panel B, the sample comprises of a balanced panel of the same villages for the years for the same years. In Panel C, the sample comprises a balanced panel of 108 villages in 10 provinces for the same years.

Table A.4: The Effect of the Introduction of Elections on Household Income and Farmland According to Households' Positions on the Village Distribution in year *t*

	Income Distribution (within villages)					Income Ratio
	10th	25th	50th	75th	90th	50th/90th
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Dependent Variable: Ln Total Household Income					
Dep. Var. Mean (not logged)	*5,003*	*7,290*	*10,497*	*15,554*	*25,314*	*0.504*
Post 1st Election	0.031	-0.044	-0.057	-0.056	-0.036	-0.015
	(0.168)	(0.058)	(0.056)	(0.053)	(0.064)	(0.012)
Wild Bootstrap p-value	[0.709]	[0.500]	[0.414]	[0.417]	[0.571]	[0.196]
Obs	673	673	673	673	673	673
R^2	0.465	0.857	0.893	0.893	0.877	0.636
	B. Dependent Variable: Ln Agricultural Income					
Dep. Var. Mean (not logged)	*2,218*	*3,730*	*6,081*	*10,394*	*19,303*	*0.449*
Post 1st Election	0.408	-0.270	-0.332	-0.036	-0.027	0.005
	(0.465)	(0.330)	(0.279)	(0.063)	(0.069)	(0.012)
Wild Bootstrap p-value	[0.449]	[0.606]	[0.106]	[0.656]	[0.702]	[0.656]
Obs	673	673	673	673	673	673
R^2	0.513	0.771	0.700	0.861	0.864	0.803
	C. Dependent Variable: Ln Wage Income					
Dep. Var. Mean (not logged)	*108*	*541*	*1,710*	*3,780*	*6,623*	*0.146*
Post 1st Election	0.031	0.326	-0.377	-1.626	-0.516	0.001
	(0.342)	(0.492)	(0.417)	(0.447)	(0.287)	(0.014)
Wild Bootstrap p-value	[0.868]	[0.536]	[0.370]	[0.017]	[0.020]	[0.984]
Obs	672	672	672	672	672	635
R^2	0.446	0.622	0.617	0.591	0.688	0.665
	D. Ln Village Enterprise Income					
Dep. Var. Mean (not logged)	*1.16*	*3.87*	*15.70*	*101.90*	*465.20*	*0.042*
Post 1st Election	0.096	-0.120	-0.624	-0.346	-0.524	-0.063
	(0.078)	(0.106)	(0.245)	(0.480)	(0.428)	(0.036)
Wild Bootstrap p-value	[0.472]	[0.350]	[0.102]	[0.518]	[0.279]	[0.096]
Obs	673	673	673	673	673	205
R^2	0.319	0.473	0.470	0.537	0.623	0.674
	E. Ln Household Farmland					
Dep. Var. Mean (not logged)	*2.78*	*4.60*	*6.56*	*8.83*	*11.26*	*0.583*
Post 1st Election	0.420	0.175	0.034	-0.013	-0.032	0.018
	(0.257)	(0.138)	(0.056)	(0.017)	(0.024)	(0.022)
Wild Bootstrap p-value	[0.160]	[0.207]	[0.736]	[0.432]	[0.166]	[0.420]
Obs	662	662	662	662	662	661
R^2	0.744	0.867	0.937	0.953	0.952	0.630

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, clustered at the province level are presented in square brackets. The sample comprises 34 villages in 10 provinces for the years 1986-2005. The data for 1992 and 1994 are interpolated. The number of observations vary due to missing values in the dependent variables.

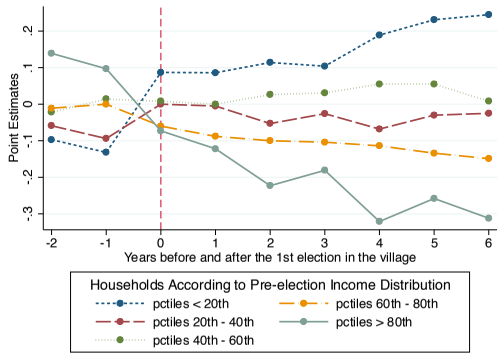
Table A.5: Year-by-Year Estimates of the Effect of Elections on Household Income by Source and Farmland According to Households' Pre-Election Positions on the Village Distribution

# of years since the first election:	Quantiles of Pre-Election Village Distribution of the Dependent Variable									
	0-20	20 - 40	40 -60	60 -80	80 -100	0-20	20 - 40	40 -60	60 -80	80 -100
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	A. Ln Total Income					B. Ln Agricultural Income				
-2	-0.097	-0.059	-0.022	-0.012	0.139	-0.098	0.047	-0.007	0.067	0.099
	(0.056)	(0.050)	(0.046)	(0.054)	(0.076)	(0.073)	(0.067)	(0.057)	(0.052)	(0.074)
-1	-0.132	-0.094	0.014	-0.000	0.097	-0.108	0.051	0.037	0.133	0.106
	(0.117)	(0.079)	(0.063)	(0.073)	(0.080)	(0.111)	(0.092)	(0.082)	(0.063)	(0.083)
0	0.087	-0.000	0.008	-0.060	-0.074	0.308	0.295	0.032	0.047	-0.157
	(0.093)	(0.074)	(0.079)	(0.091)	(0.140)	(0.102)	(0.096)	(0.091)	(0.116)	(0.156)
1	0.086	-0.005	-0.000	-0.088	-0.122	0.389	0.268	0.077	0.034	-0.212
	(0.110)	(0.089)	(0.070)	(0.086)	(0.086)	(0.163)	(0.117)	(0.096)	(0.091)	(0.106)
2	0.114	-0.054	0.026	-0.100	-0.223	0.447	0.300	0.113	0.045	-0.260
	(0.091)	(0.087)	(0.077)	(0.088)	(0.100)	(0.151)	(0.103)	(0.109)	(0.098)	(0.117)
3	0.104	-0.027	0.032	-0.103	-0.181	0.497	0.393	0.129	0.066	-0.332
	(0.106)	(0.102)	(0.088)	(0.091)	(0.099)	(0.160)	(0.133)	(0.117)	(0.115)	(0.121)
4	0.189	-0.068	0.055	-0.114	-0.322	0.581	0.337	0.142	0.065	-0.398
	(0.125)	(0.110)	(0.096)	(0.100)	(0.122)	(0.171)	(0.134)	(0.107)	(0.120)	(0.146)
5	0.231	-0.030	0.054	-0.134	-0.259	0.661	0.527	0.158	0.024	-0.433
	(0.126)	(0.102)	(0.110)	(0.105)	(0.120)	(0.187)	(0.155)	(0.110)	(0.143)	(0.153)
6	0.245	-0.025	0.008	-0.149	-0.312	0.724	0.443	0.186	0.018	-0.386
	(0.145)	(0.112)	(0.114)	(0.109)	(0.141)	(0.212)	(0.147)	(0.123)	(0.164)	(0.171)
Obs	673	673	673	673	673	673	673	673	673	673
R^2	0.875	0.880	0.881	0.871	0.835	0.841	0.785	0.807	0.802	0.802
	C. Ln Wage Income					D. Ln Household Land				
-2	-0.901	-1.449	-1.273	-0.339	0.011	-0.043	0.022	0.014	-0.037	0.018
	(0.451)	(0.577)	(0.432)	(0.194)	(0.130)	(0.070)	(0.035)	(0.049)	(0.055)	(0.041)
-1	-1.184	-1.284	-1.766	-0.970	-0.004	-0.091	-0.027	0.025	-0.027	0.053
	(0.493)	(0.567)	(0.587)	(0.424)	(0.176)	(0.073)	(0.053)	(0.044)	(0.041)	(0.042)
0	5.584	2.369	0.340	-0.592	-0.454	0.117	-0.007	-0.012	-0.094	-0.079
	(0.682)	(0.564)	(0.302)	(0.412)	(0.172)	(0.096)	(0.061)	(0.067)	(0.065)	(0.059)
1	6.994	2.684	0.408	-0.569	-0.632	0.244	0.011	0.061	-0.095	-0.048
	(0.842)	(0.670)	(0.348)	(0.313)	(0.259)	(0.116)	(0.068)	(0.046)	(0.049)	(0.050)
2	6.945	2.556	0.240	-0.841	-1.334	0.341	0.046	0.084	-0.102	-0.101
	(0.905)	(0.655)	(0.369)	(0.392)	(0.349)	(0.117)	(0.074)	(0.044)	(0.049)	(0.059)
3	7.714	2.921	0.426	-0.929	-0.964	0.469	0.071	0.164	-0.119	-0.227
	(0.885)	(0.693)	(0.303)	(0.487)	(0.228)	(0.117)	(0.090)	(0.083)	(0.057)	(0.097)
4	8.161	3.205	0.477	-0.669	-1.362	0.478	0.039	0.103	-0.145	-0.269
	(0.888)	(0.730)	(0.352)	(0.294)	(0.308)	(0.149)	(0.103)	(0.063)	(0.063)	(0.102)
5	8.350	3.307	0.291	-0.832	-1.254	0.544	0.052	0.137	-0.162	-0.332
	(0.991)	(0.885)	(0.449)	(0.374)	(0.292)	(0.158)	(0.106)	(0.066)	(0.073)	(0.107)
6	9.214	3.497	0.187	-1.226	-1.954	0.652	-0.000	0.073	-0.240	-0.432
	(1.169)	(0.994)	(0.562)	(0.541)	(0.441)	(0.173)	(0.130)	(0.091)	(0.088)	(0.129)
Obs	673	673	673	673	673	662	662	662	662	662
R^2	0.789	0.682	0.662	0.623	0.610	0.936	0.935	0.930	0.946	0.928
	E. Ln Enterprise Income									
-2	-0.667	-0.357	-0.760	0.062	0.554					
	(0.472)	(0.480)	(0.519)	(0.587)	(0.263)					
-1	-0.585	-0.016	-1.023	-0.531	0.676					
	(0.669)	(0.500)	(0.487)	(0.611)	(0.339)					
0	4.759	4.467	2.090	0.414	0.339					
	(1.015)	(0.888)	(0.927)	(0.680)	(0.502)					
1	5.573	4.909	2.675	0.977	0.434					
	(1.054)	(1.009)	(0.889)	(0.739)	(0.508)					
2	5.200	4.277	2.179	0.154	0.218					
	(1.071)	(0.908)	(0.827)	(0.818)	(0.476)					
3	5.802	4.483	2.312	0.669	-0.020					
	(1.114)	(1.105)	(0.848)	(0.731)	(0.544)					
4	6.370	5.203	3.491	0.700	0.067					
	(0.944)	(1.041)	(0.975)	(0.885)	(0.587)					
5	6.479	5.695	3.779	1.158	0.223					
	(1.269)	(1.168)	(1.017)	(0.862)	(0.676)					
6	6.930	5.817	3.608	0.285	-0.110					
	(1.211)	(1.116)	(0.998)	(1.156)	(0.788)					
Obs	673	673	673	673	673					
R^2	0.691	0.660	0.644	0.555	0.472					

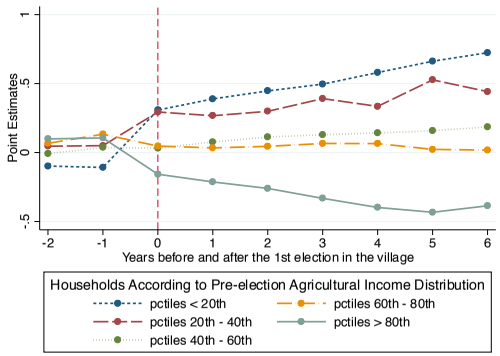
Notes: All regressions control for post-open nomination, province-time trends, village, and year fixed effects. Standard errors are clustered at the province level. The sample comprises a balanced panel of 34 villages in 10 provinces for the years 1986-2005. The number of observations vary due to missing values in the dependent variables. In particular, for some villages and years no household reports having received wage income, generating some missing values in the wage ratio. Data for 1992 and 1994 are interpolated when possible.

Figure A.1: The Year-by-Year Effects of the Introduction of Elections on Income and Farmland According to Households' Positions on the Pre-Election Village Distribution (Household Subsample)

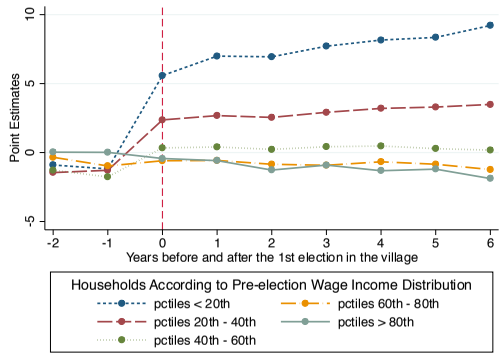
(a) Ln Total Household Income



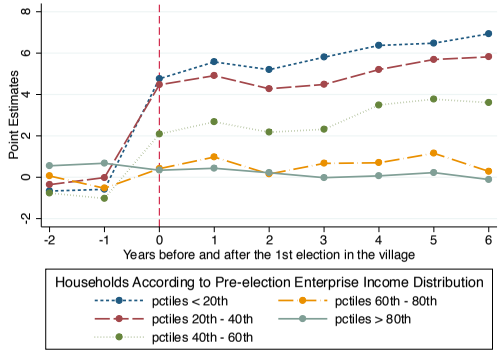
(b) Ln Agricultural Income



(c) Ln Wage Income



(d) Ln Household Farmland



(e) Ln Household Farmland