

# The Rise and Fall of Local Elections in China\*

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## Abstract

We posit that autocrats introduce local elections when their bureaucratic capacity is low. Local elections exploit the citizens' informational advantage in keeping local officials accountable, but they also weaken vertical control. As bureaucratic capacity increases, the autocrat limits the role of elected bodies to regain vertical control. We argue that these insights can explain the introduction of village elections in rural China and the subsequent erosion of village autonomy years later. We construct a novel dataset to document political reforms, policy outcomes and *de facto* power for over three decades. We find that the introduction of elections improves popular policies and weakens unpopular ones. Increases in regional government resources lead to loss of village autonomy, but less so in remote villages. These patterns are consistent with an organizational view of local elections within autocracies.

**Keywords:** Political Economy, Economic Development, Institutions

**JEL Keywords:** O2, P3, P16

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# 1 Introduction

Many autocracies allow national elections. The main theories for this phenomenon rely on the intuition that elections cement the regime's grip on power by helping to distribute the spoils amongst the elite or to signal the mobilization capacity of the regime.<sup>1</sup> Less attention has been paid to the fact that several autocracies have introduced elections at the *local* level, such as Indonesia under Suharto (1968-1998), Pakistan under Zia (1977-88), China in the late 1980s and early 1990s, Saudi Arabia in 2005, Vietnam in 1998 and Yemen in 2001.

Our study addresses this gap in the literature and makes progress in understanding the role of local elections for autocracies. The functions of these locally elected bodies are typically managerial or administrative, with little political consequence. Therefore, the presence of local elections is not obviously explained by existing theories which focus on political interactions at the elite level.<sup>2</sup> In contrast to existing studies, our study will address this question by focusing on the “organizational”, rather than the political, benefits of local elections to the autocrat.

Local officials are tasked by the regime to implement local policies. However, implementation is costly to the official in terms of effort and forgone rents. Without access to precise local information, the autocrat cannot properly target incentives for misbehaving local officials. This principal-agent conflict curtails the ability of vertical control systems to address ineffective local governance.

There are two solutions for this institutional difficulty. On the one hand, the autocrat can strengthen bureaucratic capacity, investing in qualified personnel and improving information collection and processing systems to be able to intervene when local officials deviate. This requires time and money from the autocrat. On the other hand, the autocrat can implement local elections, which delegate the monitoring of local officials to those with local information.<sup>3</sup> Citizens are well-

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<sup>1</sup>See Geddes (2005) for a summary of the facts and a view of elections as deterring military and other insider rivals, an argument fully presented in Magaloni (2008). Also see Myerson (2008), Boix and Svolik (2013) and Bidner, Francois, and Trebbi (2015) for formal models on how electoral institutions allow efficient power-sharing among elites in weakly institutionalized countries. Gandhi and Przeworski (2006) propose that elections allow for the cooperation of outsider elites. Miller (2015) argues that, through elections, citizens can express dissatisfaction which allows the regime the information necessary to react in time. Gandhi and Lust-Okar (2009) offers a literature review on elections in autocratic regimes.

<sup>2</sup>For a general overview of local elections across countries, see United Cities and Local Governments (2007).

<sup>3</sup>A few studies in political science suggest this idea. See Geddes (2005), Manion (2006) and Birney (2007).

positioned to keep the official accountable due to their ability to monitor local officials and their knowledge of the needs of the locality. This solution requires little money or time, but carries an important cost for the autocrat: facing elections, local officials are not incentivized to implement unpopular centrally mandated directives. The choice of whether to allow local elections will therefore crucially depend on the autocrat's ability to meet the cost of strengthening bureaucratic capacity.

China is an ideal context for studying this organizational view of local elections. It is a stable autocracy. The country is large and heterogeneous, and it is difficult for the central government to monitor the vast population. In the 1980s, approximately 700,000 village governments implemented both popular (e.g., public goods provision) and unpopular policies (e.g., One Child Policy) to around one billion rural residents.<sup>4</sup> Elections were introduced at the village level as a low-cost device to address monitoring difficulties and concerns of ineffective local governance. The well-documented discussion which preceded this reform articulated the tradeoff between the increased efficacy of village bureaucrats and a weakening of vertical control.<sup>5</sup> Interestingly, three decades of economic growth transformed the central government from one of the poorest to one of the richest in the world. As the government's budget increased, the regime has been observed to reduce the autonomy of elected local officials. In short, the Chinese context allows us to investigate whether the introduction of elections posed the hypothesized tradeoffs to the autocrat, as well as the co-evolution of the autocrat's resources and her preference for vertical supervision versus local autonomy.

The key challenge for our study is the limited data available to study this question in this, or any other, autocratic context. One of the main empirical contributions is therefore to collect a large and novel panel dataset to document the details of village elections and the village political economy over approximately thirty years for over 200 representative Chinese villages.<sup>6</sup> The *Village Democracy Survey* (VDS) records village administrative data and includes information about the timing of elections, elected leaders, policies and *de facto* power.<sup>7</sup> To the best of our knowledge, this is the most comprehensive and nationally representative panel data of the political economy of rural

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<sup>4</sup>For a summary of the responsibilities of this body see O'Brien (1994) as well as Section 2.

<sup>5</sup>For further discussion please see Section 2.

<sup>6</sup>The villages were chosen in the early 1980s. Thus, they are representative of China in the early 1980s, with the exception that Tibet is not in the sample due to the difficulty in collecting data from rural Tibet.

<sup>7</sup>The VDS supplements the administrative data with recall data. Since this study only uses administrative data in the analysis, we do not discuss the other data sources or collection methods.

China at the village-level. These are the only data that document the history of village elections, the characteristics of village government officials and their power for a large sample of villages.

Our analysis proceeds in several steps. First, we study the introduction of elections in the 1980s and 1990s, which changed the position of the Village Chairman (VC) from being appointed by the Communist Party to being elected by villagers. This reform was mandated by the central government and rolled out in a top-down manner. We document that the timing of the first election is uncorrelated with a large set of village observables. This is consistent with the conventional wisdom that rollout was fast across villages and suggests timing was quasi-random with respect to village characteristics. Thus, we exploit the staggered timing of the introduction of elections across villages to estimate a *difference-in-differences* effect of the introduction of elections. Our baseline estimate controls for village and year fixed effects, which account for time-invariant characteristics across villages and macro changes that affect all villages similarly, as well as province-year trends to account for the economic and political divergence across provinces over time.

One of the advantages of the rural Chinese context is that it is possible to identify uncontroversial examples of popular and unpopular policies *ex ante* – i.e., policies which require the VC to exert effort to implement, but which vary in popularity amongst villagers. For our study, we identify local government public goods expenditure and increased land availability to households as popular policies; and the One Child Policy and the permanent expropriation of village land as unpopular policies.<sup>8</sup> We find that the introduction of elections increased public goods provision and reduced the amount of village land being leased to enterprises (and away from households), while it increased the number of exemptions given out for the One Child Policy (i.e., reduced the enforcement of the One Child Policy) and reduced land being expropriated from the village. These findings are consistent with the organizational view of local elections. More specifically, they show that improving the performance of local officials comes at the cost of weakened vertical control, which is the trade-off that elections pose for the autocrat.

Three supplementary results support our interpretation. First, we find that the introduction of elections increased the share of policies approved by the VC relative to the village Party Secretary

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<sup>8</sup>See Section 1 for a more detailed discussion.

(PS), who was appointed by the Communist Party before and after the introduction of elections. This result is consistent with a shift in *de facto* power towards the newly elected VC, who would use this power to satisfy the demands of his constituents. Second, we document that the introduction of elections reduced the age of VCs, and had no effect on the age of PSs. This suggests that elections changed the set of people holding elected office and were not only for show. Finally, we document that re-election probabilities are positively correlated with the implementation of popular policies and negatively correlated with the implementation of unpopular ones. This is consistent with our notion of the popularity of the policies and the idea that the changes observed in policy outcomes are due to electoral accountability.

There are several caveats for our preferred interpretation. The first is that the results may be driven by omitted variables rather than the causal effect of the introduction of elections. The most obvious concern is that the findings may be driven by a change in upper government policy towards villages that have elections, rather than by an increase in electoral accountability to the voters. We address this by examining upper government transfers to villages as Placebo policies. These transfers are popular with voters and directly under the control of the upper government. We find that the introduction of elections has no effect on these policies. Another way to address omitted variables is to introduce additional controls. We show that our results are very robust to a large number of controls, including province-year fixed effects and the policies of interest measured at base year interacted with year fixed effects. The former controls for any province-specific changes that vary over time (e.g., province-specific political or economy shocks) and the latter addresses the concern that the results may be an artifact of different pre-conditions between villages which introduced elections earlier versus later. See Section 4.5 for a more detailed discussion.

The second part of the analysis examines the loss of autonomy of the elected local government, which began in the early 2000s. The central government did not make *de jure* changes to local elections, which were widely popular. Instead, it invested in direct supervision in a series of policies that undermined the autonomy of village governments. Unlike the introduction of elections, the implementation of these policies was mostly left to the county-level government, the level which

directly supervises villages for the Communist Party.<sup>9</sup> Viewed through the lens of our framework, these policies are consistent with the desire for the autocrat to recentralize power once her fiscal capacity increases and she can invest in strengthening vertical control. That recentralization was tailored to adapt to local heterogeneity is consistent with stronger central bureaucratic capacity during the latter phase.

The organizational view of local elections predicts specific patterns in the process of recentralization across villages. Counties that experience larger increases in revenue are able to develop better systems of information gathering and control. Within counties, the cost of these tasks is higher for villages that are further from the county seat. Hence, one expects greater recentralization in counties with higher revenues; and within these counties, less recentralization for more remote villages.

We examine the temporal and spatial patterns in the curtailing of VC autonomy measured as the *de facto* power of the VC and the policies which increased county-level oversight of village accounts and village cadres. We find that villages in counties with faster revenue growth experienced a reduction in autonomy, with the magnitude of the reduction declining in the distance between the village and the county seat. These results are consistent with the organizational view of local elections, and also consistent with the presumption that direct rule over villages is costly. They suggest that at the current level of resources, supervision frictions continue to render local autonomy worthwhile for distant villages.

This study thus sheds new light on the forces leading to decentralization and recentralization in autocracies. We accomplish this by developing a coherent conceptual framework and by collecting the largest and most comprehensive dataset on the introduction of local elections, the policies meant to curtail them, as well as a rich set of other political economic variables for rural China.

We build on several literatures. The fact that informational asymmetry introduces a basic tension between top-down authority and local autonomy has been recognized in many contexts.<sup>10</sup> We are the

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<sup>9</sup>Only villages have elections in China. All upper levels of government are controlled by the Communist Party.

<sup>10</sup>The notion that private information and preference divergence are crucial to delegation decisions in organizations stems from the seminal contributions in Aghion and Tirole (1997). Region-specific information and divergent incentives also feature prominently in second generation theories of federalism, as surveyed in Oates (2005). In the clientelism literature, local knowledge is a good reason to devolve power to the villages, but decentralized resources can be captured

first to provide evidence that this organizational tension can explain ostensibly surprising autocratic institutions, such as local elections.<sup>11</sup> More generally, the fundamental point that autocrats face informational frictions has been explored in studies such as Egorov, Guriev, and Sonin (2009) and Lorentzen (2013).<sup>12</sup>

A growing number of empirical studies provide important evidence on local governance in developing countries.<sup>13</sup> For local elections in China, there is a large body of in-depth studies in political science and a smaller number of studies in economics. We are the first to encapsulate this collective knowledge and highlight the importance of organizational frictions in this context, and to propose a framework for understanding the motivations of the central government. The data that we collect are also the largest and most comprehensive in terms of the richness of the variables, the length and breadth of the panel structure, and its attempt to measure the introduction and undermining of local elections. See Section 3 for a more detailed discussion.<sup>14</sup> Recent empirical studies have also examined informational frictions for the Chinese autocrat in the context of the Great Famine (Meng, Qian, and Yared, 2015) and the media (Stromberg, Qin, and Wu, 2017; Chen and Yang, 2019), and governance strategies in the context of school curricula (Cantoni, Chen, Yang, Yuchtman, and Zhang, 2017).

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by local power-brokers, as in Bardhan and Mookherjee (2006).

<sup>11</sup>At the theoretical level, (e.g., Myerson, 2015) makes a related point, except in his work the self-interest of the autocrat is also a crucial friction.

<sup>12</sup>These studies typically focus on regime survival and rent extraction. Gehlbach, Sonin, and Svolik (2016) provides a review of the recent literature.

<sup>13</sup>In particular, we build on the increasing number of empirical studies on re-election incentives (e.g., Dal-Bó and Rossi, 2011; de Janvry, Finan, and Sadoulet, 2010; Ferraz and Finan, 2011) and on the effect of information on electoral accountability (e.g., Besley and Burgess, 2002; Ferraz and Finan, 2008; Bobonis, Fuentes, and Schwabe, 2010). There is also a smaller literature on the effects of electoral rules (e.g. Beath, Christia, Egorov, and Enikolopov, 2016). The implication that villagers are better than upper levels of government at monitoring village leaders is similar to the findings of Björkman and Svensson (2009). Khan, Khwaja, and Olken (2016), Callen, Gulzar, Hasanain, Khan, and Rezaee (forthcoming), Dal-Bó, Finan, Li, and Schechter (forthcoming) study the effects of improvements in information collection on vertical control in public bureaucracies.

<sup>14</sup>For other examples of economics studies of Chinese local elections, see (Zhang, Fan, Zhang, and Huang, 2004), Gan, Xu, and Yao (2007), Luo, Zhang, Huang, and Rozelle (2010), Shen and Yao (2008). These studies focus on total public goods provision and inequality. They do not distinguish public goods according to the source of financing, or examine the other policies we study. This study complements two companion papers that use the VDS data and attempt to understand the extent to which social capital and heterogeneity influence electoral accountability (Martinez-Bravo, Padró i Miquel, Qian, Xu, and Yao, 2017; Martinez-Bravo, Padró i Miquel, Qian, and Yao, 2012). Suarez Serrato, Wang, and Zhang (2017) studies the limits of vertical bureaucratic control in China outside of the local election context. For examples of studies in political science, see the Background Section. None of these studies provide evidence on the question of why elections were introduced or curtailed.

This paper is organized as follows. Section 2 summarizes the political discourse and implementation of local elections in China. Section 3 describes the data. Section 4 examines the effects of introducing elections. Section 5 examines the undermining of elected village governments. Section 6 concludes.

## 2 Background

### 2.1 The Introduction of Local Elections

Policy implementation has been a perennial challenge for the central government of China.<sup>15</sup> The fundamental difficulty is the size and heterogeneity of its geography and population. The Chinese Communist Party governs an autocratic state organized in several layers. Elections have only ever existed at the village level – the lowest level of administration.<sup>16</sup> In this study, we focus on the tension between the villages and the levels of government above them, which we interchangeably refer to as the central or upper government. During the early reform era (1978-), governance problems were particularly acute at the approximately 700,000 villages which governed the day-to-day lives of nearly one billion people. Villages had two governing bodies: Village committees, led by the Village Chairman (henceforth VC) and the village branch of the Chinese Communist Party, led by the village Party Secretary (henceforth PS). The central government lacked the bureaucratic capacity to hold village officials accountable, which led to ineffective governance.

Two issues, in particular, were a common source of discontent amongst villagers. First, the level of local public good provision was extremely low in rural areas. Village governments were responsible for public investments such as ditches for irrigation, school buildings or local roads. Local officials had to determine village investment needs and raise funds from villagers, since village governments received few transfers from the central government. Since the village government did not have the authority to raise recurrent taxes, each investment was funded on an *ad hoc* basis. Significant effort was required on the part of the village government to coordinate and cajole the

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<sup>15</sup>Meng, Qian, and Yared (2015) document that the founders of modern China were well aware of these difficulties from as early as the 1940s and show that the inability of the autocrat to address these difficulties contributed to tens of millions of deaths during the Great Famine during the period 1959-61.

<sup>16</sup>There have been showcase elections at other levels. We do not discuss them for brevity.



villagers to make contributions. National policymakers and villagers widely complained that village governments shirked from this effort.<sup>17</sup>

Second, there was widespread discontent about corruption, and specifically, about the misallocation of village land. Village cadres were widely suspected of enriching themselves using village collective property, the most important of which was land. A common corrupt practice involved leasing village land to enterprises. While this was ostensibly legal, rents were easily captured by the village leadership and its cronies because of accounting opacity. These suspicions made renting land to enterprises extremely unpopular.<sup>18</sup> Without precise information, it was hard for the upper government to assess which rental contracts and rates were legitimate and which were not.

In addition, village governments were the ultimate enforcers of vertically mandated policies such as the One Child Policy. Therefore, lethargic local governments could seriously limit the effectiveness of important national-level policy initiatives.

The central government was acutely aware of the shortcomings of local governance.<sup>19</sup> Proponents of the introduction of local elections, such as Peng Zhen, vice-chairman of the National People's Congress Standing Committee, acknowledged in 1987 that there was poor control of local officials, saying "Who supervises rural cadres [local officials]? Can we supervise them? No, not even if we had 48 hours a day..." (cited in O'Brien and Li;1999). At the same time, the central government could not afford expanding bureaucratic capacity to better supervise local officials.<sup>20</sup>

Local elections were proposed as a reform that could improve governance by involving the villagers in the running of local affairs, while at the same time saving on costs.<sup>21</sup> Innovative regional governments began experimenting with elections in the early 1980s and elections were formally

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<sup>17</sup>Several studies document the paltry provision of public goods and widespread corruption. See, for example, Luo, Zhang, Huang, and Rozelle, 2010. Shen and Yao (2008).

<sup>18</sup>Consistent with this view, Brandt and Turner (2007) find in a cross-sectional study that reducing land rented out to enterprises is positively correlated with re-election probabilities.

<sup>19</sup>The comparative literature has studied the debate surrounding the introduction of village elections. White (1992), Kelliher (1997) and O'Brien and Li (2000) summarizes the main issues in the policy debate as well as descriptions of the implementation process of elections.

<sup>20</sup>See O'Brien (1994) for a discussion of the tight budgetary constraints under which this discussion was taking place.

<sup>21</sup>O'Brien and Li (2000) writes that "As the chief justification for self-government, supporters of the Law argued that passing the bill would help curb arbitrary and predatory behavior by rural cadres," and White (1992) contends that costs were another incentive behind the introduction of elections: "Economic pressure to streamline administration, reduce personnel, and increase cadre accountability led to political reforms".

codified by the central government in the *Organizational Law on Village Committees* (OLVC) in 1987. From this point onwards, provinces were pushed by the central government to introduce elections in all villages. Implementation, managed by the Ministry of Civil Affairs, was imposed top-down on all villages by bureaucratic fiat (O'Brien and Li, 1999). By the late 1990s, local elections had been implemented in the vast majority of villages.

The electoral reform introduced direct elections for the village committee. The village committee member who received the highest number of votes would become the VC. The reform left the village Party branch, the responsibilities of the village government, and the extant fiscal arrangements as before. It did not clarify the power relationship between the village committee and the Party branch, which remained ambiguous and heterogeneous across villages. Party branches could potentially influence the candidate slate, but they had to allow more candidates than positions in the ballot.<sup>22</sup> Thus, the main effect of the reform was to make local governments marginally more accountable to villagers by giving the latter the power to vote unsatisfactory VCs out of office.

It is important to note that in these elections, there are no political parties and no slates of candidates with common platforms. Candidates step forward from within the village and are thus typically well-known by the villagers. As a consequence, candidates typically run on very local and well-understood issues and are probably selected for qualities that have been long observed by their fellow villagers.<sup>23</sup> It is also very important to keep in mind that these are not official positions in the state bureaucracy, and thus not a part of the bureaucratic ladder which begins above the village level and culminates in Beijing. In other words, positions in the village government are not stepping stones for higher positions in the state administration.

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<sup>22</sup>For more information on the heterogeneous relationship between village committees and Party branches see the discussion in Kelliher (1997). Candidate selection methods varied across villages. In most villages, candidates self-nominated. In other villages, the Party Branch appointed the candidates. A second OLVC reform in 1998 mandated that all villages have open nominations. Controlling for the introduction of open nominations does not affect the results. Thus, we do not discuss them any further in this paper. A more detailed discussion and empirical analysis of open nominations are available in earlier versions of this paper.

<sup>23</sup>There are very few accounts of actual electoral campaigning. In many cases, elections were set up with only a few days' notice (Unger, 2002: p. 221).

## 2.2 Undermining Village Committees

Starting in the mid 2000s a series of reforms emanating from the central government have reduced the authority, autonomy and scope of village governments. The village governments lost their fiscal autonomy with the national 2003 *Tax and Fee Reform*, which made it illegal to use *ad hoc* fees to fund public investment. The central government made up the shortfall by increasing central transfers for local public goods. The amount of central expenditure for rural schools increased by 685% in the decade to 2011, from 14.38 billion dollars to 110.16 billion dollars. For transportation infrastructure, it increased by 32%, from 6.51 billion dollars to 8.58 billion dollars, and for agricultural infrastructure, it increased by 166%, from 12.39 billion dollars to 31.63 billion dollars (all measured in constant 2015 USD).<sup>24</sup> While the stated purpose of these reforms was to reduce the tax burden on rural households and increase redistribution towards rural areas, vertical command and control has substituted local choice in the quantity and the targets of public goods investment.

In a detailed review of recent fiscal policies and data on transfers across the different levels of government, Oi, Babiarz, Zhang, Luo, and Rozelle (2011) argues that “the increasing reach of the central state is the direct result of explicit state policies that have taken power over economic resources that were once under the control of village and township cadres”. The authors document that many village officials have lost control over village budgets, and some counties have implemented a policy of account oversight (*shuang daiguan*) where villages need explicit permission from county authorities to access village funds.

County governments have also resorted to other practices of direct supervision. A recent study by He and Wang (Forthcoming) examines a new policy of introducing graduates from urban universities to villages for three-year terms in exchange for Party membership and/or a position in the state bureaucracy afterwards. Another prominent example is the cadre-in-residence program in which county officials spend one to two days a week in a village. While the explicit aim of both programs is to give technocratic support to village cadres, the students-in-residence and cadres-in-residence are directly accountable to the county and thus serve as mechanisms to limit the autonomy

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<sup>24</sup>See the National Bureau of Statistics (NBS) Yearbooks, the Bureau of Labor Statistics (BLS), the *China Educational Finance Statistical Yearbook* (2000) and the *China Rural Statistical Yearbook* (2000).

of elected village bodies.

While the introduction of village elections was a centrally mandated policy to be equally implemented everywhere, recentralization is the result of a sequence of policies among which county-level governments have discretion. There has not been an official abrogation of village elections, but these recent reforms have severely eroded the authority and autonomy of village officials.

### **2.3 Conceptual Framework**

We propose an “organizational” explanation for the introduction of local elections as well as the subsequent reforms that limit their relevance. Village officials are tasked by the central government to implement numerous policies. This requires effort, so in the absence of well-targeted incentives, local officials shirk from their tasks and instead pursue other objectives such as personal rent-seeking. How well the vertical system of control can deal with this moral hazard depends crucially on the quality of information that reaches the upper government.<sup>25</sup>

There are two potential solutions to this problem. The first one is to invest in the vertical system of bureaucratic supervision. This improves the collection and processing of local information and, therefore, allows for correctly targeted incentives to ensure that local officials perform. The problem with this solution is that it is costly in money and time.<sup>26</sup> An alternative solution is to introduce local elections to delegate the provision of incentives to citizens, who have much better information about the needs of the village and the activities of local officials. This solution harnesses existing information and therefore addresses the moral hazard problem of local officials without incurring high costs. However, since the objectives of the citizens are not fully aligned with those of the autocratic government, this solution imposes a price on the latter: local officials facing elections have little incentive to implement unpopular policies mandated by the upper government. Thus, delegation of authority creates a tension between the better use of local information versus the loss of vertical control that has long been identified in the organizational economics literature.<sup>27</sup>

In the early 1980s, the local governance problem in rural China was addressed with local elec-

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<sup>25</sup>This is a classic moral hazard issue, as in Holmstrom (1979).

<sup>26</sup>Current improvements in information technology have likely reduced these costs. See Dal-Bó, Finan, Li, and Schechter (forthcoming) for an empirical examination of such an effort in rural Paraguay.

<sup>27</sup>See for instance, Aghion and Tirole (1997), Dessein (2002) and Mookherjee (2006).

tions, in part because there were no available funds to invest in the rural bureaucracy. However, the tension we highlight between the delegation of authority to villagers and the loss of vertical control was salient to policymakers at the time. White (1992) notes that “Advocates argued that the best way to stabilize the situation at the grass roots was to create institutions that would hold cadres directly accountable to the peasantry for their behavior in office [...] opponents saw the proposed bodies as threats to the leading role of the Party, and feared that cadres held accountable to fellow villagers would be loathe to carry out unpopular directives”. This tension was particularly problematic in rural China because local officials must carry out unpopular policies such as the One Child Policy.<sup>28</sup>

The organizational framework suggests that as the quality of the vertical control bureaucracy improves, the benefits of local autonomy diminish. Better bureaucratic capacity allows upper governments to detect and intervene when elected officials are deviating from the upper government’s preferences over village affairs.<sup>29</sup> Hence, increases in bureaucratic capacity lead to reductions in the autonomy of elected village bodies. A direct implication of this logic is that loss of autonomy should go hand in hand with improvements in the bureaucracy. Aggregate data are consistent with this pattern.

The central government dramatically increased investment in the bureaucracy as the economy grew.<sup>30</sup> From 1980 to 2015, expenditure on the bureaucracy increased from 1.46% to 2.73% of GDP – and central government personnel (civil servants) increased from approximately one million in the mid-1980s to 7,167,000 in 2015, growing much faster than the population during this period.<sup>31</sup> The

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<sup>28</sup>O’Brien and Li (2000) directly points to this potential loss of control by the upper government: “[reform opponents] believed that without tight control over [local officials] and an ability to issue direct commands, village cadres would be tempted to ignore state interests and disregard township instructions. Elected [local officials] might, in a word, be inclined to take their cues from below rather than above. This could interfere with tax collection, grain procurement and enforcing the birth control policy, and might ultimately cripple township authority”.

<sup>29</sup>The upper government can intervene in the village in two ways. On the one hand, it can give direct orders to the village government, likely using the village Party Secretary. On the other hand, it can take tasks away from the village government and bring them to the upper government. Both strategies require high levels of bureaucratic capacity, and both erode the autonomy of elected VCs.

<sup>30</sup>Government revenues in real terms increased by more than twenty times between 1980 and 2015. From 1980 to 2015, per capita GDP in China increased from 740.28 dollars to 8,068.04 dollars and tax revenues increased from 90.43 billion dollars in 1980 to 2005.69 billion dollars in 2015 (all measured in constant 2015 USD). See *Statistical Yearbooks*.

<sup>31</sup>Expenditure data is from NBS, BLS, *China Statistical Yearbook* 1981 (pp. 397), and the Ministry of Finance of the People’s Republic of China. Data for government personnel was released for the first time in 2015 by the Ministry of Human Resources and Social Security of the People’s Republic of China. The number of personnel for the mid 1980s is

first steps in the recentralization process began with the change in national leadership in 2002, and continued as investment on bureaucratic capacity accelerated.

Therefore, the logic of organizational delegation provides a parsimonious explanation for both the introduction of elections in the 1980s and the progressive upward transfer of authority after 2002.

Three predictions follow from our conceptualization of the introduction of local elections as delegating the supervision of local officials to villagers. First, elected VCs need to have a meaningful degree of real authority over village affairs if elections are to improve policy implementation outcomes. Delegation would be ineffective if *de facto* authority over village affairs is in the hands of the unelected PS.

Second, election-induced changes on policies should have opposite signs on popular policies and unpopular, vertically mandated policies. The framework predicts improvements in popular policies as shirking and rent-seeking by local officials is limited by re-election incentives, but it also predicts a worse implementation of unpopular policies as elected VCs have lower incentives to upset their constituents. This is the central trade-off that local elections pose for an autocrat.

Third, since elections must render local officials accountable to villagers, re-election patterns should reflect villager preferences. More specifically, the VCs that improve popular policies should be re-elected at a higher rate, while the opposite should happen to those who better implement vertically mandated, but unpopular policies.

Finally, the logic of the delegation problem suggests that upper governments that wish to curtail the autonomy of a village need to invest to meet the costs of collecting information and intervening in that village. This suggests that resources at the county level and village-specific costs of supervision should both influence the degree of recentralization. More specifically, those counties with better fiscal positions should recentralize more, but this effect should be weaker for far flung villages where direct supervision is more costly.

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an approximation provided by a central government official interviewed by the authors.

### 3 Data

Our study requires data about the history of village-level electoral reforms, the policies used to undermine the autonomy of elected leaders, as well as other political and economic outcomes. Given the size and heterogeneity of China, we aimed for as representative a sample as possible. Since the introduction of local elections dates back to the 1980s, rigorous empirical analysis requires a long panel of villages with detailed data of village policies and political-economic conditions. The data that we examine were not available at the time of our study and were difficult to collect. A survey of past outcomes faces recall bias and China is linguistically heterogeneous – the hundreds of spoken dialects in rural areas combined with low literacy rates make it difficult to administer a nationally representative survey.<sup>32</sup>

To address these challenges, we worked with the Ministry of Agriculture (MoA). Our survey, the *Village Democracy Survey* (VDS), uses the villages that participate in the MoA's *National Fixed Point Survey* (NFS), a detailed annual village- and household-level economic survey that focuses on agricultural production. The NFS sample was chosen in 1986 to be representative of rural China and stratified at the province level. NFS surveyors have visited these villages monthly since 1986. These villages maintain high quality administrative records, some of which are then aggregated and recorded in the NFS. The NFS was only used for internal research by the MoA, which allays concerns about systematic manipulation of the records maintained by the villages.

The VDS was administered by thousands of NFS personnel across China. This was an important advantage since these individuals had access to and the trust of local village officials, as well as knowledge in local customs and dialects. Similar to the NFS, the VDS mostly contains data from administrative village records. The analysis in this paper exclusively uses data from administrative records. Thus, recall bias does not play a role in the interpretation of our results.

The VDS expands on the NFS in two ways. First, it collects information on elections and non-agricultural aspects of village records which the NFS excludes. Second, it collects disaggregated

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<sup>32</sup>In principle, the national dialect, Mandarin, is used across the country and is the one written language. However, many rural residents are uncomfortable with Mandarin and are often semi-literate. This is particularly problematic for older cohorts who would have the best knowledge of the early reform era.

data in several instances where the NFS data only contains village-level aggregated variables.<sup>33</sup>

The key variables of interest for our study, such as the dates of each election, basic information on village officials and authorization documents of village-level policy decisions are kept by villages as a part of their routine book-keeping practices. Similarly, villages also record detailed information about public expenditures, including the amount spent, the object for spending and the source of the funds. The categorization and definition of the economic variables are determined by the MoA. The categorization and definition of the governance and important policy variables are determined by the Ministry of Civil Affairs. The data are thus comparable across villages. Importantly for our study, village records also include the implementation of policies mandated by the central government, such as the number of official exemptions to population control policies and land allocation decisions, as well as the policies used to limit village autonomy. We describe the variables in more detail as they become relevant for our analysis.

We collect three waves of the VDS. The first one, conducted in 2006, records the history of electoral reforms, public goods expenditures, the sources of funds for public goods expenditures, and the number of One Child Policy official exemptions granted by the village government. The second wave, conducted in 2011, records the names and characteristics of all village leaders from 1982 until 2005.

Using the first two waves, we construct a balanced panel of 217 villages for the years 1986-2005.

The third wave was collected in 2019 to document the loss of autonomy of elected village governments. Our survey form was limited to one physical sheet of paper. Thus, the survey narrowly focused on the timing and implementation of recentralization policies and on measures of *de facto* power of the VC, beginning in 2006. We discuss these data in more detail when we use them in the recentralization analysis in Section 5.

The sample of villages in the VDS is nearly nationally representative. It is not entirely representative for the entire period of the VDS because the sample was chosen in 1986, and because it excludes Tibet.<sup>34</sup> The geographic coverage of our data is mapped in Figure 1.

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<sup>33</sup>For example, the NFS only records an aggregate measure of total public goods expenditure even though villages record disaggregated information such as source of funding for each expense. The VDS records these more granular data, which are not in the NFS.

<sup>34</sup>The villages were chosen by random sampling stratified at the province level in 1986. There are 31 provinces in



To the best of our knowledge, the VDS provides the most comprehensive data on village-level reforms ever constructed. They include more variables, cover a longer time horizon and a more representative sample than other datasets.

In the analysis, we supplement the VDS data with variables from the NFS when necessary. Appendix Table A.1 summarizes the main variables and their sources. We will describe the variables as they become relevant.

## 4 The Introduction of Elections

### 4.1 Descriptive Statistics

#### 4.1.1 Reform 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. 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 (O'Brien and Li, 1999). Once a province began implementation, elections were rapidly rolled out throughout its territory. It is important to note that villages had no discretion over the timing of introduction of elections, which is characteristic of reforms in rural China.<sup>35</sup>

Our data are consistent with this narrative. The timing of the first election is uncorrelated to pre-reform village characteristics within provinces. We examine a large number of village characteristics which reflect the political and socio-economic conditions prior to the introduction of

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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 and thus was not a part of the 1986 NFS sample. The three other city-municipalities with provincial status (Beijing, Shanghai and Tianjin) are included in our data since each contained a substantial rural population (30% or higher).

<sup>35</sup>In his detailed study of elections, Unger (2002, p. 222) writes that “These [elections] should not be interpreted as bottom-up initiatives by the villagers themselves; they are not in a position to play any precedent-setting part in the initiation of new electoral reforms. There is a mistaken belief among some people outside China regarding this [...] elections are quietly being instituted at levels above the village, engineered first in selected districts at a distance from Beijing, through the connivance of the [central] Ministry of Civil Affairs and middle-ranking officials out in the regions”. Unger (2002) also notes the general passivity of villages in implementing rural reforms such as land reforms and the adoption of the *Household Responsibility Reform* earlier in the reform era.

elections. Importantly, we also examine the main policy outcomes measured prior to the introduction of elections. Specifically, the variables are: a dummy variable for whether a village is near a city, village size measured as the number of households, median household income and income growth, household income inequality measured as the ratio of the 50th percentile to the 90th percentile income and the growth of this inequality measure, the fractionalization of the village in terms of surnames (kinship groups are the most important dimension of social cleavage within villages), total arable land within the village, the total amount of land used for household farming, total public goods expenditure, the amount of public goods funded by villagers, the amount of public goods funded by upper-levels of government, the amount of land leased out to enterprises, the number of One Child Policy exemptions granted by the village government, the number of permanent expropriations of village land, and the amount of transfers from the upper government levels for special aid (e.g., elderly individuals below the poverty line with no living adult children).

We measure all village characteristics in the base year (i.e., the first year that data are available) and regress the year of the first election against each variable, while controlling for province fixed effects. The sample for these regressions is a cross-section of villages. Table 1 presents the results. None of the correlations are statistically significant. This means that within a province, the timing of the first election is not associated with any of these variables. This is an important validation of the notion that the rapid rollout within provinces caused the introduction of elections to be quasi-random and uncorrelated with omitted variables which would confound our interpretation. We will present additional evidence against omitted variables after we discuss the estimation and main results.

Note that the VDS also documents electoral procedures. Consistent with other contexts of newly introduced elections, the data indicate that procedures were imperfect. In our sample of elections, 60% had roving ballot boxes, 67% did not have anonymous ballots and 67% allowed voting by proxies. There was little change over time.

## 4.2 Estimation

We exploit the staggered introduction of elections and estimate a simple *difference-in-differences* specification, controlling for village and year fixed effects.

The baseline equation is the following

$$Y_{vpt} = \beta E_{vpt} + \gamma_{pt} + \delta_v + \rho_t + \varepsilon_{vpt}, \quad (1)$$

where the policy outcome of village  $v$  in province  $p$  during calendar year  $t$ ,  $Y_{vpt}$ , is a function of: a dummy variable,  $E_{vpt}$ , that takes the value of one after the first election in village  $v$  has taken place; province-year trends,  $\gamma_{pt}$ ; village fixed effects,  $\delta_v$ ; and calendar-year fixed effects,  $\rho_t$ . We cluster the standard errors at the village level, since that is the level of variation for the introduction of elections.<sup>36</sup>

Village fixed effects control for all time-invariant or slow-moving differences across villages, such as geographic characteristics (e.g., hilliness or distance from a city) or culture. Year fixed effects control for country-wide changes over time such as national policy changes and macroeconomic growth. In addition, we control for province-time trends, which account for economic and political divergence across regions. Controlling for province-year trends means that the regression mostly exploits within-province variation. Later, we will also control for province-year fixed effects, which are more stringent and force the regression to only exploit within-province variation.

Interpreting  $\beta$  as the causal effect of the introduction of elections assumes parallel-trends: the outcomes of interest for villages which introduced elections earlier versus later would have evolved along parallel trends absent the difference in election timing. In other words, we assume that, conditional on the baseline controls, there is no other variable that is correlated with both the outcome of interest and the timing of the first election. This assumption is supported by the descriptive evidence that the timing of the elections within a province is uncorrelated with village-specific characteristics. We will provide additional evidence to support our interpretation when we discuss robustness.

### 4.3 The Effect on Leaders

In this section, we investigate whether the newly elected VCs have a meaningful degree of autonomy and power over village affairs, and whether the people in office change after the introduction of

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<sup>36</sup>We show that the results are similar if we estimate wild-bootstrapped standard errors clustered at the province level. See the Robustness Section.

elections. First, we examine *de facto* power. This is far from obvious due to the continued presence of the unelected PS in each village. To examine the relative *de facto* power of these two officials, we collect data on who authorizes the important financial decisions made by the village government. In our context, the most important decisions relate to: (i) reimbursements of personal expenses incurred on behalf of the village, (ii) allocation of village funds to public investments, (iii) land-reallocations across households, and (iv) appointments of managers of village-owned businesses. Each time one of these decisions are made by the village government, it is recorded and signed by the VC, the PS or both. The signatory bears institutional responsibility for the decision in the case of a complaint from the villager or an audit from the upper levels of government. Thus, we interpret who signs important policy documents as a credible proxy for real authority.

Table 2 presents the sample averages and standard deviations of different measures of policy signatories, as well as estimates of the changes in these measures due to the introduction of elections (equation (1)). Panel A row 1 examines the number of policies over which the VC is the unilateral signatory. Row 2 examines the number of policies which are signed by both the VC and the PS. Row 3 examines the number of policies over which the PS is the sole signatory. The values of the measures in rows 1-3 range from zero to four, since there are up to four policies. Row 4 examines the average of an index measure of VC's signature power. It is the average of the indices for each policy, which are shown in rows 8, 12, 16 and 20: each index equals 1 if the VC is the sole signatory, 0 if both the VC and the PS sign and -1 if the PS is the sole signatory.<sup>37</sup>

The sample means in Panel A indicate that the VC, on average, unilaterally signs 1.1 policies (row 1), while the PS signs 0.63 (row 3). 1.46 policies are signed by both the VC and the PS (row 2). The sample means are consistent with the idea that, on average, the VC had at least as much as, if not more, *de facto* power than the PS. The mean of the average index is 0.156 (row 4).

The column of the post-first-election coefficients shows that the introduction of elections increased the number of policies for which the VC is the sole signatory (row 1), had no effect on the number of policies signed by both the VC and the PS (row 2), reduced the number of policies over

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<sup>37</sup>Note that not all policies are relevant for all villages and time periods. The average index that we examine in row (4) corresponds to the average across the policies relevant for each village and year. Consequently, the number of observations of this average index is larger than each individual index used to compute the average. This is also why the number of observations vary across policies in Panels B-E.

which the PS is the sole signatory (row 3), and increased the average index of VC power. The estimates in rows 1, 3 and 4 are statistically significant at the 5% or higher levels and are economically meaningful. For example, the estimate in row 1 implies that the number of policies signed unilaterally by the VC increase by 22% of the sample mean. These results suggest that the introduction of elections led to a shift in power from the PS to the VC.

In Panels B to E, we examine each of the four policies separately. While statistical precision varies, the means and coefficients exhibit patterns that are consistent with our interpretation.

These results are consistent with the idea that VC's have real, but incomplete, authority over village affairs, and that the introduction of elections increased this *de facto* power across the range of village policies for which we have signatory data. The autocrat needs the elected body to have some real authority for delegation through elections to be effective. Both the level of VC signature frequency and the change induced by elections are consistent with this idea.<sup>38</sup>

Next, we examine the characteristics of leaders to investigate whether the introduction of elections changed the people in office. Table 3 Panel A examines the characteristics of the VC. Column (1) examines the years of educational attainment, a common indicator for leader quality, of the office holder as the dependent variable. Column (2) examines age. Column (1) shows that the introduction of elections increases the educational attainment of VCs by 0.5 years. Note that the sample mean is eight years. Thus, this is a sizable effect. The coefficient is significant at the 10% level. Column (2) shows that the introduction of elections reduced the average age of VCs by 2.6 years, where the sample mean is 42 years. The coefficient is significant at the 5% level. Column (3) examines Party membership as the outcome of interest. It shows that the introduction of elections has a negative effect on the probability that the VC is a Party member. But the estimate is only statistically significant at the 15% level.

Panel B examines the years of education and age for PSs. All PSs are Party members. The estimates are statistically zero. At the bottom of the table, we present the p-values for the statistical difference between the estimates for the VC and PS. They show that the effects of the introduction

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<sup>38</sup>Note that there could be several reasons for the shift in *de facto* power to the VC. The introduction of elections may have increased his mandate relative to the PS. Also, the upper levels of government emphasized the VC's power when it mandated elections. Both are consistent with our interpretation.

of elections are statistically different for the two types of leaders. The results in Table 3 show that the introduction of elections changed the characteristics of the VC and had no effect on those of the PS.

#### **4.4 The Effect on Popular and Unpopular Policies**

Table 4 presents the results of estimating equation (1) on popular, unpopular and placebo policies. We begin by examining two popular policies, which were discussed earlier in Section 2. The first is local public goods expenditure funded by villagers. The MoA requests villages to keep records of all expenditures for irrigation, schools, electricity, roads (within the village), sanitation and the environment. The records are separated according to the source of financing, which is important because elected leaders can only directly affect the amount of money raised from village sources. Therefore, our measure of interest is the total annual public investment for each village that is financed by village sources, measured in 10,000 constant RMB.<sup>39</sup> Note that approximately 70% of all expenditure on public goods in our sample are paid for by villagers.

Column (1) shows that the introduction of elections increased expenditures by 166,040 RMB. The estimate is statistically significant at the 5% level. To assess the magnitude, note that the sample average is 94,600 RMB, so the increase induced by elections is substantially larger than the sample mean. To interpret this magnitude, it is important to recall that public investment was near zero prior to the introduction of elections. This estimate suggests that elections were followed by large spurts of expenditure to address high latent demand.<sup>40</sup>

The second popular policy is the amount of village land rented to enterprises, which as we discussed earlier was a source of corruption and highly unpopular amongst villagers.<sup>41</sup> Data for the use of village land is reported in the NFS. Villages in our sample use approximately 96% of arable land (approximately 51% of total village land) for household farming. Approximately 75% of the remaining arable land is leased out to enterprises. Since this practice was not widespread, we restrict the sample for this estimate to villages that ever used any arable land for non-household farming prior to the introduction of the first election. This reduces the sample to 103 villages (in 28

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<sup>39</sup>During the period of our study, China had a fixed exchange rate and 1 RMB was roughly equal to 1/7 USD.

<sup>40</sup>Figure 2, which we discuss in the next section, is consistent with these dynamics.

<sup>41</sup>Note that Brandt and Turner (2007) proxies for corruption with this measure.

provinces). Column (2) shows that the introduction of elections reduced the amount of land leased to enterprises by 61.6 mu on average.<sup>42</sup> The estimate is statistically significant at the 10% level. The sample mean is 111 mu. Thus, this is an economically significant effect. This result is consistent with electoral accountability reducing practices commonly perceived to be corrupt.

Next, we examine two policies which are uncontroversially unpopular amongst villagers. The first is the One Child Policy, which was mandated by the central government and widely despised by the population. To control fertility, birth quotas were assigned top-down from the central government all the way down to local governments. To enforce the policy, the village government needed to monitor pregnancies, lead women to abortion clinics and impose fines and social pressure on households who violated the policy. Strict enforcement of the One Child Policy led to severe problems in infanticide and abandonment of female children in rural areas in the early 1980s. The central government responded to this by allowing some rural households to have a second child under limited conditions, such as if the first child is a girl or if the household head is disabled. These exemptions began in 1981, and had to be officially granted by village officials, who were under pressure from the upper government to keep village fertility low (Qian, 2017). Each exemption is recorded in village records.

Column (3) examines the number of One Child Policy exemptions given per year. It shows that on average, elections increased the number of exemptions by about one per every ten observations. The estimate is statistically significant at the 5% level. The sample mean is one per every two villages. Thus, this is a large effect. This result suggests that electoral incentives made officials shift effort from enforcing the One Child Policy to helping villagers circumvent it.

The second unpopular policy is the permanent expropriation of land from the village. This occurs, for example, when the upper level of government needs land to build or expand a highway, or to construct an airport. These eminent domain instances are extremely unpopular with villagers since they result in a permanent loss of land with compensation that is rarely equal to the net present value of the future stream of production. Village officials are supposed to help implement these expropriations by advertising the benefits of the new status (e.g., proximity to infrastructure) as

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<sup>42</sup>1 mu is 1/15th of a hectare. We keep the indigenous unit of measurement because average land plot size is very small.

well as ensuring an uncontroversial eviction of households and reallocation of the remaining village land. Thus, even though the village government has no direct authority over land expropriation, their actions can greatly affect the expropriation costs to the upper government.<sup>43</sup> Instances of land expropriation are recorded in the NFS.

In column (4), the dependent variable is a dummy variable which equals one if some land was permanently expropriated from a village in a given year. The coefficient shows that elections reduce the probability of expropriation by 1.3 percentage-points, which is substantial since the sample mean is 2% (unsurprisingly, this is an infrequent event). The estimate is statistically significant at the 10% level. This result suggests elected village officials are less compliant with vertically mandated expropriations.

Taken together, the findings in columns (1) to (4) are consistent with our framework in showing that elections improved the implementation of policies that were popular with villagers, and moderated the implementation of policies that were unpopular with villagers. The bottom row presents the p-value for the joint significance of the estimates in columns (1)-(4). It shows that they are jointly different from zero at the 1% significance level.

Finally, we address the concern that our findings are driven by changes in the upper government's treatment of villages rather than local electoral accountability. To address this, we examine two placebo policies which were popular with villagers, but over which the village government had little discretion. If upper levels of government wanted to improve conditions of villages in which they had introduced elections, it would be logistically much easier to transfer funds to villages through these policy mechanisms than through those examined in columns (1)-(4).

The first placebo policy is special aid in the form of direct transfers to households with specific characteristics. In our context, these mostly target households with elderly individuals below the poverty line who have no living adult children. The second policy is the transfer of funds from the upper government to the village earmarked for specific public investments (e.g., planting trees). The central government funds these transfers and they are implemented by the county government.

Columns (5) and (6) show that the introduction of elections had no effect on the two placebo

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<sup>43</sup>In practice, political scientists have observed that village officials and villagers can stop or postpone land expropriation by engaging in protests and by submitting petitions (O'Brien, 1994).



policies. The coefficients are small in magnitude and statistically insignificant. These results go against the concern that the results in columns (1) to (4) are a consequence of changes in upper government attitude towards villages as elections are implemented.

The results in Table 4 are consistent with the second prediction of the organizational view of local elections. Introducing elections delegates authority to villagers, rendering VCs accountable to them. This presents the autocratic government with a clear trade-off. Popular policies become better implemented as elections address moral hazard, but unpopular policies weaken as officials face re-election incentives.

## **4.5 Robustness**

The main concern for interpreting the difference-in-differences estimates as support for the core tradeoff predicted by the model is endogenous timing – i.e., the introduction coincides with another factor which affects the outcomes of interest. It is difficult to think of an omitted factor that would generate all of the wide array of relationships we estimate. Nevertheless, to be cautious, we can categorize several specific omitted variable concerns.

The first concern is that despite controlling for province time trends, our baseline results may be partly driven by cross-province variation in timing, which is determined by provincial leaders for potentially endogenous reasons. We address this issue in three ways. First, to control for the province-level timing of the decision to introduce village elections, we add a dummy variable that indicates whether any village in a given province has introduced elections (Table 5 column (2)). The results are similar to the baseline estimates, which are displayed in column (1) for comparison. Hence, our results are not an outcome of province-level variation in the timing of the introduction of the reform, which is the main source of endogeneity concerns. A second way of accounting for province-level factors is to control for province-year fixed effects in place of province-year trends. This allows the influence of province characteristics to vary flexibly over time. Column (3) shows that this stringent set of controls reduces the precision of our estimates, which is natural given that elections are introduced in a wave within each province. However, the coefficients are very similar in magnitude to the baseline specification. Finally, we control for province-level variables such

as per capita GDP, per capita agricultural GDP, and per capita government expenditure in public goods.<sup>44</sup> The coefficients with these controls in column (4) are similar to our baseline.

The second 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 which affect the outcomes of interest through channels other than elections. The strongest evidence against this concern is in Table 1, which we discussed earlier. It shows that the timing of the introduction of elections is uncorrelated with the observed features of the village. Nevertheless, one may still be concerned that the timing of the introduction of elections is correlated with pre-conditions that affect our outcomes of interest through channels other than elections. Given the anecdotal evidence on the delay of elections for villages with a history of non-compliance to unpopular central government policies, we directly control for baseline measures of village-level policy outcomes (public goods financed by villagers, land leased out to enterprises, upper government land expropriation, One Child Policy).<sup>45</sup> To capture the relevant variation, we calculate the principal component of these four time-invariant variables. To allow the effects to vary flexibly over time, we interact the principal component with the full vector of year fixed effects. The interaction controls account for the influence of these variables over time in a fully flexible manner, and it also controls for the influences of all of its correlates over time. Column (5) of Table 5 shows that the coefficients with these additional controls are very similar to our main estimates. In column (6), we repeat this exercise with measures of the base year annual growth in the four policy variables.<sup>46</sup> The results are similarly robust.

We also consider the possibility that several other village-level factors could potentially confound the effect of elections on our outcomes of interest. These factors include whether a village is a suburb of a city (a dummy variable for being in a suburb interacted with year fixed effects), whether the *Tax and Fee Reform* had been implemented (a dummy variable which takes the value of one if the reform has been introduced), and the level of village social capital (a proxy for social

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<sup>44</sup>These data are reported by *China Statistical Yearbooks*.

<sup>45</sup>These characteristics are measured in the first year that data are available.

<sup>46</sup>We calculate average growth using the first three years of data for each of the four policy variables. Then, we calculate the principal component of the four time invariant average growth variables, and interact it with year fixed effects. The controls are the interactions variables of the average growth principal component and year fixed effects. For both the levels and growth in the pre-election characteristics, our results are very similar if we controlled for the interactions of each base year measure and year fixed effects (in lieu of the principal components and year fixed effects). These results are available upon request.

capital interacted with year fixed effects). To proxy for the latter we follow Tsai's (2007) work in using the presence of a lineage group (i.e., an ancestral hall, family tree), village temple or a large kinship group to proxy for social capital.<sup>47</sup> To maximize the statistical precision of our estimates, we use the principal component of these three measures as our social capital proxy. Column (7) shows that the resulting coefficients are similar to the baseline.<sup>48</sup>

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

In summary, while the estimates vary in precision, the coefficients are statistically similar across columns (1)-(8). These results imply that our main findings are highly unlikely to be biased by omitted variables.

In column (9), we re-estimate the baseline, but cluster the standard errors at the province level to address the concern that they may be correlated within provinces. Since there are 29 provinces, we estimate wild bootstrapped standard errors to address the possibility of small sample bias (Cameron, Gelbach, and Miller, 2008). The results are similar to the baseline in column (1).

We can also provide evidence for the parallel trends assumption by examining pre-trends. The data allow us to do this for public goods expenditures funded by villagers, One Child Policy Exemptions, special aid transfers from the upper government and public goods expenditures funded by transfers from the upper government. Figure 2 presents the year-by-year coefficient estimates of the introduction of elections.<sup>49</sup> The reference group comprises observations that are three years before the first election. This figure shows that the coefficients are approximately zero for the years before the first election for all outcomes (i.e., no pre-trends). They become positive the year of the first election for public goods expenditure funded by villagers and One Child Policy exemptions. For

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<sup>47</sup>To measure the size of the kinship groups, the VDS recorded surnames from the village roster.

<sup>48</sup>The results are very similar if we control for the individual measures interacted with year fixed effects (in lieu of the principal component interacted with year fixed effects). They are available upon request.

<sup>49</sup>These estimates as well as their standard errors are presented in Appendix Table A.2

the placebo policies, the coefficients remain near zero after elections are introduced. These results support the identification assumption.

Finally, as an additional sensitivity check, we conduct random permutation tests to demonstrate that even though election timing and the key outcome variables may be infrequent events, our results are highly unlikely to be driven by coincidence.<sup>50</sup>

#### **4.6 Correlates of Re-election Probability**

This section investigates whether re-election patterns are consistent with our assumption of which policies are popular and unpopular and whether the estimated effects of the introduction of elections on policies reflect electoral accountability. Table 6 examines a dummy for the incumbent remaining in office as the dependent variable. The explanatory variable in each regression is one of the six policies we examined earlier. In each case, we use the average of the policy outcome for the previous term (which is usually three years). These regressions should not be interpreted as causal, but can provide a useful falsification test for our framework. Note that the sample size for these estimates is smaller because it is restricted to election years.

The correlations in columns (1) and (3) show that VCs who provide more public goods and One Child Policy exemptions are re-elected at a higher rate. The standardized coefficients show that the magnitude of the effects is notable, especially for One Child Policy exemptions. A one standard deviation increase in exemptions increases the re-election probability by 0.22 standard deviations. The estimates are statistically significant at the 1% level. The estimates for land leased and land expropriation in columns (2) and (4) are very statistically imprecise. This is most likely due to the low frequency of these events and the small sample size.<sup>51</sup> The p-value at the bottom of the table for the joint significance of the estimates in columns (1)-(4) shows that they are jointly different from zero at the 1% level.

Reassuringly, we find no relationship between the implementation of the placebo policies and re-election probabilities in columns (5) and (6). The estimates are small in magnitude and statistically insignificant.

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<sup>50</sup>See the Appendix.

<sup>51</sup>However, it is worth noting that Brandt and Turner (2007) finds that in the cross-section, villages with less land leased out present higher re-election rates.

These re-election correlations are consistent with our interpretation that at the margin, elections reflect the preferences of villagers, who demanded more public goods and disliked the One Child Policy.

## **5 The Undermining of Elections**

### **5.1 Main Results**

In Section 2.2, we described how from the early 2000s, a sequence of reforms have progressively reduced the scope and autonomy of village governments, transferring authority upwards to the upper governments. In Section 2.3, we posit that this is because economic growth over the 1980s and 1990s increased budgetary slack, which was invested in improving bureaucratic capacity. This process of recentralization was designed to be more flexible than the introduction of elections two decades earlier. Elections were introduced as a blanket reform meant to be equally implemented on all villages (this is not to say that the end results were uniform). In contrast, the recentralization process has put a number of policy tools in the hands of county governments – the relevant level of Communist Party rule over rural areas. These regional governments choose when, how and to which villages these policy tools apply.

The organizational view of elections can parsimoniously explain the differences in the design of these two processes: lack of bureaucratic capacity meant that upper governments did not have the information or the ability to tailor the degree of autonomy to each village and were thus simply tasked with rolling out elections as fast as possible for everyone. However, once bureaucratic capacity improved, village-specific tailoring became feasible and allowed heterogeneity in the recentralization process.

The organizational logic also predicts specific patterns in this heterogeneity. As a county's bureaucratic capacity increases, it is better able to figure out the needs of a village, to scrutinize the village government, and to intervene in case elected officials are deviating from the county's preferences over village affairs.<sup>52</sup> Hence, increases in bureaucratic capacity at the county level

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<sup>52</sup>As discussed in Section 2.3, the county can intervene by issuing a direct order to the village government through the Party Secretary or by taking direct responsibility for a task.

should lead to reductions in the autonomy of elected village bodies. At the same time, the county's costs of collecting information and intervening vary across villages within the county. It follows that both county-level resources and the costs of supervising each village should matter for the reduction in the autonomy of a village.

In this section, we use the third wave of the VDS, collected in 2019, to investigate whether these expected patterns are present. More specifically, we examine whether counties with more revenue growth engage in greater reductions of the VCs autonomy, and whether these reductions are larger for villages geographically closer to the county seat – i.e., whether the interaction effect of county government revenues and travel distance to the county seat on VC autonomy is positive. County-level data for government revenues are available for 1990-2017.<sup>53</sup>

Our first measures of VC autonomy are the signatories over important policies that we examined earlier.<sup>54</sup> In columns (1)-(4) of Table 7, we estimate the following triple difference equation

$$Y_{vpt} = \beta_1 G_{vpt} \times P_t + \beta_2 G_{vpt} \times P_t \times D_{vp} + \beta_3 G_{vpt} + \beta_4 G_{vpt} \times D_{vp} + \gamma_p t + \delta_v + \rho_t + \varepsilon_{vpt}, \quad (2)$$

where the outcome for village  $v$  in province  $p$  in year  $t$ ,  $Y_{vpt}$ , is a function of the triple interaction of the revenues for the county that supervises village  $v$  (normalized by the county's GDP),  $G_{vpt}$ , a dummy variable which takes the value of one if it is later than 2002,  $P_t$ , and the log travel distance between the village and the county seat,  $D_{vp}$ ; and all of the lower order interactions and fixed effects. As in the earlier estimates, we control for province time trends.<sup>55</sup>  $P_t$  captures the change to the national Party leadership, which began the recentralization process.<sup>56</sup>  $\beta_1 + \beta_3$  captures the effect of increasing county revenues after 2002 on the autonomy of a village very close to the county seat (i.e., the distance is zero). We expect  $\beta_1 + \beta_3 < 0$ .  $\beta_2 + \beta_4$  captures how this reduction in autonomy

<sup>53</sup>They are reported by Provincial Statistical Yearbooks. Travel distance between each village and the county seat was provided to the authors by the MoA.

<sup>54</sup>The sample spans 1990-2017, when county government revenues are available. Signatory data are collected in all three waves of the VDS.

<sup>55</sup>Note that we observe one village per county in this sample, which is slightly smaller than the first two waves of the VDS (where there were a few counties in which we observed more than one village). Hence, there is no separate subscript for the county.

<sup>56</sup>The *Tax and Fee Reform*, the first significant step in the recentralization process, was approved in 2003. The results are very similar if we use 2003 to define the Post indicator variable. They are available upon request.

varies with distance from the county seat. Since we hypothesize that distant villages maintain more of their autonomy, we expect  $\beta_2 + \beta_4 > 0$ .

Column (1) examines the number of relevant policies for which the VC is the sole signatory as the dependent variable. At the bottom of the table, we show that  $\widehat{\beta_1 + \beta_3} = -0.244$ . The p-value at the bottom of the table shows that the joint estimate is statistically significant at the 5% level. This implies that post-2002, VC unilateral signatures are decreasing in county government revenues for villages that are very close to the county seat. Since the standard deviation of county revenues is 2.92 and a one standard deviation in the dependent variable is 1.27, these coefficients imply that a one standard deviation increase in revenues is associated with a 0.56  $((-.244 \times 2.92)/1.27 = -0.56)$  standard deviation decrease in VC signatures. This is consistent with an economically significant loss of VC autonomy.

The joint coefficient,  $\widehat{\beta_2 + \beta_4} = 0.072$ , is positive and statistically significant at the 5% level, as reported at the bottom of the table. This implies that the reduction in VC signatures as county revenues increase is smaller in villages that are further away from the county seat. To assess the magnitude, consider two villages, one village which is near the county seat and the other village which is one standard deviation further away (the standard deviation in log distance to the county seat in our sample is 0.997). The estimate implies that in the recentralization period, when county revenues increase by one standard deviation, the distant village would experience a 0.165 standard deviation smaller reduction in VC signatures  $((0.072 \times 2.92 \times 0.997)/1.27 = 0.165)$ . Hence, a one standard deviation in distance ameliorates the reduction in VC signatures by 30% (relative to the  $-0.56$  decrease in autonomy for a village near the county seat) associated with a county's increase in revenue. Both of these results are consistent with our hypotheses on the erosion of village autonomy.

Column (2) examines the number of policies that both the VC and PS sign. The joint coefficients  $\widehat{\beta_1 + \beta_3}$  and  $\widehat{\beta_2 + \beta_4}$  have the opposite signs as in column (1) and are statistically significant at the 5% level. Column (3) examines the number of policies that the PS unilaterally signs. The joint coefficients are small in magnitude and statistically insignificant. These estimates suggest that the reduction in VC's unilateral power is mostly driven by an increase in the policies signed by both leaders. This is consistent with anecdotal accounts of the Party asserting its supervisory role over

elected leaders during this period.

In column (4), the dependent variable is the average index measure of VC power. The joint coefficients at the bottom of the table have the same signs as those in column (1) and are almost statistically significant at the 10% level.

These results support the hypothesis that VC autonomy declines as county revenues increase. The post-2002 period has thus resulted in a partial undoing of the increase in VC power brought about by the introduction of elections documented in Table 2. At the same time, distance from the county seat plays a relevant role, most likely due to the county's costs of collecting and acting on relevant information. These frictions make it useful to allow autonomy in geographically distant villages at current levels of bureaucratic efficiency, and helps explain why elections have not been abrogated.

Next, we investigate the actual policies used to curtail the autonomy of elected leaders. We focus on the three policies commonly believed to be the main instruments for recentralization: village *budgetary* account oversight, the student-in-residence program and the cadre-in-residence program.<sup>57</sup> These variables are only recorded in the third wave of the VDS, which was limited to the period 2006-2019.<sup>58</sup>

Using these data, we estimate a double interaction specification.

$$Y_{vpt} = \alpha_1 G_{vpt} + \alpha_2 G_{vpt} \times D_{vp} + \gamma_p t + \delta_v + \rho_t + \varepsilon_{vpt}. \quad (3)$$

Following our earlier logic, we expect  $\alpha_1 > 0$ , since increased revenues should be associated with more assertive county governments, and  $\alpha_2 < 0$ , since reducing village autonomy is more difficult where specific information is more costly to collect. We estimate non-linear Logistic regressions because the outcome variables are binary.<sup>59</sup>

The estimates in columns (5) to (7) are consistent with our hypotheses: the policies meant to assert control over elected leaders are more frequently applied in counties whose resources grow, but

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<sup>57</sup>See Section 2.2.

<sup>58</sup>Earlier waves did not collect these variables since they were not relevant at the time.

<sup>59</sup>The sample size in columns (5)-(7) differs from the one in columns (1)-(4) because it covers fewer years and because Stata omits collinear observations when estimating logit models.



the magnitude of this increase is lower for villages that are geographically distant from the county seat. In column (5), which examines account oversight, the coefficients are statistically significant at the 10% and 1% levels. In column (6), which examines the cadre-in-residence program, the analogous coefficients are significant at the 20% and 1% levels. In column (7), the estimates for student monitors are statistically insignificant, but the signs are the same as for other outcomes, and the magnitudes of the estimates are non-negligible.

Taken together, these results are consistent with our interpretation of the evolution of local elections in China. The upper government prefers direct control when it can afford it, but the frictions associated with physical distance mean that delegating authority to the elected VC remains advantageous for distant villages.

## 5.2 Robustness

The main concern for our preferred interpretation of the recentralization results is that the curtailing of village autonomy may be a response to increased political tensions that are unrelated to the organizational frictions we focus on. To confound our interpretation, dissatisfaction with the regime would need to be correlated with county government revenues and distance to the county seat. This seems unlikely *a priori*. Nevertheless, to be cautious, we control for several proxies of citizen dissatisfaction. The first is the distance to Hong Kong, which has significant political and economic autonomy from Beijing and arguably has been the center of the pro-democracy movement in China in recent years (Cantoni, Yang, Yuchtman, and Zhang, 2019). The second proxy is distance to Xinjiang, where most of the Uyghur minority resides, and has been a source of conflict with the central regime Wen (2020). Finally, we use the number of strikes in each prefecture in 2011.<sup>60</sup> We interact each of these proxies with year fixed effects to allow their effects to be fully flexible over time. We focus on VC signatures because of data limitations.<sup>61</sup>

For brevity, Table 8 presents the joint estimates  $\beta_1 + \beta_3$  and  $\beta_2 + \beta_4$ . Each panel examines a

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<sup>60</sup>Prefectures are the administrative level between a province and a county. These data are reported by the *China Labour Bulletin*, a non-profit organization based in Hong Kong which monitors incidents of collective worker actions across mainland China since 2011. This variable should be interpreted cautiously since it is collected in 2011 and may therefore be endogenous, and because there may be measurement error.

<sup>61</sup>Data for the three curtailing policies are available for a much shorter time horizon. Adding the large number of interacted fixed effects to the baseline results in collinearity.

different dependent variable. The first row in each panel restates the baseline estimate for comparison. The next three rows alternately add the interaction controls. Our estimates are very stable. Thus, it is unlikely that the patterns we observe are confounded by regime retrenchment in the face of demands for openness.

In the last row of each panel, we control for province-year fixed effects in place of province-year trends. The estimates are very similar to the baseline, which means that the baseline results are mostly driven by within-province variation. This allays any concern related to omitted variables that vary by province and year, such as changes in province-level political or economic policies.

## **6 Conclusion**

This paper applies an organizational framework to understand the phenomenon of local elections in autocracies. By introducing local elections, autocrats delegate the monitoring of local officials to citizens in order to harness the specific information possessed by the latter. Better information implies that citizens can hold local officials accountable more effectively than an imperfectly informed vertical system of control. Such delegation can moderate corruption and shirking from local officials in a cost-effective way. However, elected local officials have little incentive to implement vertically mandated policies that are unpopular with citizens. Delegation therefore comes with weakened vertical authority. This reduction in control is problematic for the autocrat. As a consequence, strengthening the vertical control system is an attractive alternative for the local governance problem. Specifically, when resources become available to the autocrat, she will invest in the top-down bureaucratic system. A stronger vertical system is both better informed and more able to directly intervene in villages, and proceeds to progressively limit the autonomy of elected village leaders. This logic can therefore parsimoniously explain why China introduced local elections in the 1980s, when it was one of the poorest nations in the world, and started undermining elected officials in the 2000s, when it had become one of the richest countries in the world and had been visibly investing in the capacity of the vertical control bureaucracy.

Our approach to this subject complements the existing literature on the political economy of autocracies, which has mostly focused on the trade-off between political survival and rent extraction.

However, autocrats often have other policy preferences that are only loosely related to these. For example, the regime in China strongly believed in fertility control and rural education and wanted to impose its vision throughout the country. Implementing any policy requires institutional structures that properly incentivize officials at each level of government, even the lowest one in the villages. Therefore, just as in any large organization, an autocrat who wants the state to perform must grapple with the usual organizational frictions. This study uses newly collected data spanning three decades of China's reform era to show that such frictions can explain at least one important aspect of institutional development and one important aspect of institutional regression.

The organizational view of local elections presents a sobering policy implication. It counters the optimistic view of the introduction of local elections in autocracies as a movement towards political openness prefacing a transition towards a democratic regime. Instead, these institutional innovations may help to strengthen the autocrat's position by serving as a stopgap while she improves her capacity for vertical control. Similarly sobering, viewed through this lens, economic growth allows the autocrat to increase state capacity and tighten authoritarianism. This introduces further skepticism towards the modernization hypothesis as a driving force behind democratic transitions.<sup>62</sup>

It is also important to note that the insights we highlight are not unique to the Chinese experience. For example, the trajectory of local elections under the Suharto regime in Indonesia (1965-1979) exhibits many parallels. During the first decade and half of the regime, local elections played an important role in some regions of Indonesia such as Java. However, starting in 1979, when Indonesia experienced a large increase in state capacity because of the boom in oil revenues, the central regime began to curtail the powers of village governments. As in the Chinese case, elections were kept in place, while the central government severely eroded the *de facto* powers of elected local leaders by simultaneously reducing their ability to raise revenues and substituting them with transfers decided by the central government (Antlov and Cederroth, 1994; Antlov, 2003). The pattern of election introduction followed by recentralization is also present in other autocracies, such as in Vietnam.<sup>63</sup>

Our study opens up several avenues for future research. First, it would be interesting to study

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<sup>62</sup>See Acemoglu, Johnson, Robinson, and Yared (2009) for a recent empirical analysis of aggregate data.

<sup>63</sup>See Malesky and Schuler (2013).

the detailed processes of local government reform in other autocratic regimes, to further our understanding of institutional development in autocracies. Second, the comprehensive data that we collected will be made public so that researchers can further enrich our understanding of how the Chinese autocrat has governed in the past thirty years, and of the Chinese political economy, more generally.

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Table 1: Correlation between Village Characteristics and Election Timing

Dependent Variable: Year of 1st Election		Coef.	Std. Err.	Obs	R-Square
(1)	Near City	-0.038	(0.773)	217	0.336
(2)	Total Number of Households	0.001	(0.001)	217	0.338
(3)	Median Household Income	0.000	(0.000)	217	0.339
(4)	Median Household Income Growth	3.361	(2.407)	217	0.343
(5)	50/90 Income	-0.118	(4.629)	217	0.336
(6)	50/90 Income Growth	1.664	(5.154)	217	0.336
(7)	Kinship (Surname) Fractionalization	0.599	(1.700)	217	0.336
(8)	Total Village Arable Land	0.000	(0.000)	216	0.346
(9)	Land used for Household Farming	0.154	(2.687)	217	0.336
(10)	Total Public Goods Exp (10,000 RMB)	-0.014	(0.011)	217	0.346
(11)	of which, from Village Sources	-0.017	(0.058)	216	0.358
(12)	of which, from non-Village Sources	-0.003	(0.004)	217	0.336
(13)	Land Leased Out to Enterprises (Mu=1/15 Hectare)	0.000	(0.000)	103	0.426
(14)	One Child Policy Exemptions	-0.484	(0.771)	217	0.337
(15)	Land Expropriation	0.818	(0.690)	217	0.336
(16)	Upper-Government Special Aid	0.070	(0.043)	217	0.337

*Notes :* The sample is a cross-section of villages. Each row is one regression. The dependent variable is the year of the 1st election. The regressors of interest, stated in the table, are measured in the base year (defined as the first year that data are available for each variable). All regressions control for province fixed effects. The sample in row (11) omits one outlier village. The sample in row (13) is restricted to villages that ever leased land to enterprises. Other sample changes are due to missing values in the explanatory variables.

Table 2: The Effect of Elections on VC and PS Signatures

Dep. Var: Policy Signatories			Post 1st Election		Obs.	R-squared
	Mean	Std. Dev.	Coef.	Std. Err.		
A. All Policies						
(1) Sum VC Unilateral	1.104	1.302	0.242	(0.107)	4,340	0.745
(2) Sum VC PS Joint	1.457	1.363	0.063	(0.115)	4,340	0.738
(3) Sum PS Unilateral	0.633	1.156	-0.191	(0.091)	4,340	0.806
(4) Average Index	0.156	0.597	0.127	(0.054)	4,120	0.763
B. Reimbursement						
(5) VC Unilateral	0.549	0.498	0.066	(0.044)	4,098	0.768
(6) VC & PS Joint	0.213	0.409	-0.001	(0.034)	4,098	0.731
(7) PS Unilateral	0.238	0.426	-0.065	(0.033)	4,098	0.814
(8) Index	0.311	0.831	0.130	(0.070)	4,098	0.801
C. Public Investment						
(9) VC Unilateral	0.169	0.375	0.065	(0.032)	3,723	0.759
(10) VC & PS Joint	0.689	0.463	-0.041	(0.034)	3,723	0.805
(11) PS Unilateral	0.142	0.349	-0.025	(0.033)	3,723	0.768
(12) Index	0.027	0.557	0.090	(0.055)	3,723	0.735
D. Land Reallocation						
(13) VC Unilateral	0.316	0.465	0.104	(0.039)	3,269	0.791
(14) VC & PS Joint	0.554	0.497	-0.052	(0.040)	3,269	0.802
(15) PS Unilateral	0.130	0.336	-0.051	(0.035)	3,269	0.792
(16) Index	0.186	0.641	0.155	(0.063)	3,269	0.785
E. Appoint Managers						
(17) VC Unilateral	0.317	0.465	0.068	(0.044)	2,775	0.787
(18) VC & PS Joint	0.388	0.487	-0.015	(0.041)	2,775	0.798
(19) PS Unilateral	0.294	0.456	-0.053	(0.037)	2,775	0.833
(20) Index	0.023	0.782	0.121	(0.071)	2,775	0.814

*Notes:* Observations are at the village and year level. Each row is one regression. In rows (1)-(3), the dependent variables are the sum of policies for which the signatory is only the VC, both, or only the PS. The dependent variable in row (4) is the average across policies of an index for VC power (the average of rows 8, 12, 16 and 20). In rows (5)-(7), (9)-(11), (13)-(15), and (17)-(19), the dependent variables are dummy variables which equal one if the signatory is the VC, PS, or both. The dependent variable in rows (8), (12), (16) and (20) is an index for VC power that takes the values of -1, 0 and 1 for if the signatory is only the PS, both, or only the VC, respectively. All regressions control for province trends, and village and year fixed effects. Standard errors are clustered at the village level. The number of observations varies due to missing values for the dependent variable (not all policies are relevant for all villages).

Table 3: The Effect of Elections on VC and PS Characteristics

	Dependent Variables		
	Education (1)	Age (2)	Party Member (3)
A. Village Chairman (elected)			
<b>Dependent Variable Mean</b>	<b>8.060</b>	<b>42.50</b>	<b>0.785</b>
Post 1st Election	0.517 (0.312)	-2.647 (1.085)	-0.084 (0.053)
Obs	3,260	3,280	3,360
R <sup>2</sup>	0.638	0.493	0.527
B. Party Secretary (appointed)			
<b>Dependent Variable Mean</b>	<b>8.520</b>	<b>44.27</b>	<b>1</b>
Post 1st Election	-0.238 (0.238)	-1.026 (0.969)	
Obs	3,820	2,740	
R <sup>2</sup>	0.600	0.539	
SUR Panel A vs Panel B: p-val	<0.001	0.016	

*Notes:* Observations are at the village and year level. All regressions control for province trends, and village and year fixed effects. Standard errors are clustered at the village level. The number of observations varies due to missing values in the outcome variables for some villages. The p-value for the statistical difference between the coefficients for VC and PS are presented at the bottom of each column.

Table 4: The Effect of Elections on Popular and Unpopular Policies

	Dependent Variables: Policies					
	A. Popular		B. Unpopular		C. Placebo	
	Public Good Expenditures (Villagers, 10,000 RMB) (1)	Village Land Leased to Enterprises (mu) (2)	One Child Policy Exemptions (3)	Dummy for Expropriation of Village Land (4)	Upper- Government Special Aid (10,000 RMB) (5)	Public Good Expenditures (Upper Gov, 10,000 RMB) (6)
<b>Dep. Var. Mean</b>	<b>9.46</b>	<b>111.01</b>	<b>0.55</b>	<b>0.02</b>	<b>1.49</b>	<b>4.28</b>
Post 1st Election	16.604 (8.320)	-61.563 (38.755)	0.097 (0.042)	-0.013 (0.008)	-0.666 (1.241)	-0.807 (1.803)
<i>Standardized Coefficient</i>	<i>0.053</i>	<i>-0.052</i>	<i>0.073</i>	<i>-0.035</i>	<i>-0.005</i>	<i>-0.005</i>
Observations	4,340	1,957	4,340	4,340	4,340	4,340
R <sup>2</sup>	0.103	0.559	0.792	0.094	0.059	0.073
Joint Significance: p-value		<0.001				

*Notes:* Observations are at the village and year level. All regressions control for province trends, and village and year fixed effects. Standardized coefficients are presented in italics. Standard errors are clustered at the village level. 1 mu = 1/15 hectare. In column (2), the sample is restricted to villages that ever leased land to enterprises. The p-value for the joint significance of the coefficients for Post 1st Election in columns (1)-(4) is presented at the bottom of the table.

Table 5: The Effect of Elections on Popular and Unpopular Policies – Robustness to Controls

Baseline (1)	Province Introduction of 1st Election (2)	Prov-Year FE (3)	Prov Per Capita GDP and Growth (4)	Year FE x Base Year Vars ** (5)	Year FE x Baseline Trend (6)	Year FE x: Near City, Social Capital; Post Tax & Fee (7)	Omit if Ever Merged with Another Village (8)	Wild Bootstrapped P- Values clustered at Province Level (9)
<b>Panel A. Dependent Variable: Policy Signatories (Average Index)</b>								
Post 1st Election	0.127 (0.054)	0.145 (0.063)	0.102 (0.045)	0.125 (0.054)	0.127 (0.055)	0.126 (0.053)	0.142 (0.063)	0.127 [0.008]
Observations	4,120	4,120	3,807	4,080	4,080	4,120	3,280	4,120
R-squared	0.763	0.781	0.788	0.765	0.764	0.766	0.758	0.763
<b>Panel B. Dependent Variable: Public Goods Expenditure from Villagers (10,000 Constant RMB)</b>								
Post 1st Election	16.604 (8.320)	13.676 (9.209)	18.792 (9.371)	15.999 (8.696)	16.051 (8.308)	17.171 (8.324)	17.496 (9.282)	16.604 [0.048]
Observations	4340	4340	4018	4280	4,280	4340	3500	4,340
R-squared	0.103	0.221	0.118	0.122	0.132	0.114	0.104	0.103
<b>Panel C. Dependent Variable: Village Land Leased to Enterprises (mu)</b>								
Post 1st Election	-61.563 (38.755)	-51.738 (37.908)	-57.364 (38.649)	-51.299 (37.221)	-62.595 (40.011)	-59.414 (40.423)	-59.020 (44.206)	-61.563 [0.084]
Observations	1957	1957	1842	1957	1,957	1957	1672	1,957
R-squared	0.559	0.746	0.593	0.670	0.564	0.580	0.614	0.559
<b>Panel D. One Child Policy Exemptions</b>								
Post 1st Election	0.097 (0.042)	0.090 (0.049)	0.100 (0.042)	0.105 (0.042)	0.110 (0.042)	0.100 (0.042)	0.122 (0.046)	0.097 [0.084]
Observations	4,340	4,340	4,018	4,280	4,280	4,340	3,500	4,340
R-squared	0.792	0.812	0.797	0.797	0.795	0.793	0.790	0.792
<b>Panel E. Dummy for Expropriation of Village Land</b>								
Post 1st Election	-0.013 (0.008)	-0.008 (0.009)	-0.015 (0.009)	-0.013 (0.008)	-0.014 (0.008)	-0.011 (0.008)	-0.011 (0.009)	-0.013 [0.068]
Observations	4,340	4,340	4,018	4,280	4,280	4,340	3,500	4,340
R-squared	0.094	0.199	0.105	0.102	0.095	0.103	0.095	0.094

Notes: Observations are at the village and year level. All regressions control for province trends, village and year fixed effects. Additional controls and sample restrictions are stated in the column headings. Column (5) controls for year FE x the principal component of base year measures of pub goods, OCP, land expropriation, land leased out. Column (6) controls for year FE x the principal component of the average growth rate of the same variables for the first three years in the sample. Standard errors are clustered at the village level and reported in parentheses in columns (1)-(8). Column (9) reports p-values in square brackets that are estimated with wild-bootstrap clustered at the province level.

Table 6: The Effect of Popular and Unpopular Policies on Re-election Probabilities

	Dependent Variables: Policies					
	A. Popular		B. Unpopular		C. Placebo	
	Public Good Expenditures (Villagers, 10,000 RMB) (1)	Village Land Leased to Enterprises (mu) (2)	One Child Policy Exemptions (3)	Dummy for Expropriation of Village Land (4)	Upper-Government Special Aid (10,000 RMB) (5)	Public Good Expenditures (Upper Gov, 10,000 RMB) (6)
<b>Dep. Var. Mean</b>	<b>0.479</b>	<b>0.477</b>	<b>0.466</b>	<b>0.466</b>	<b>0.480</b>	<b>0.479</b>
<b>Ind. Var. (Policy*) Mean</b>	<b>8.124</b>	<b>122.8</b>	<b>0.541</b>	<b>0.0162</b>	<b>0.138</b>	<b>3.340</b>
Policy (see column heading)*	0.00067 (0.000)	0.00005 (0.000)	0.22048 (0.066)	-0.06243 (0.223)	-0.00679 (0.012)	-0.00064 (0.001)
<i>Standardized Coefficient</i>	<i>0.080</i>	<i>0.047</i>	<i>0.216</i>	<i>-0.009</i>	<i>-0.023</i>	<i>-0.039</i>
Observations	1,173	566	1,227	1,227	1,171	1,173
R <sup>2</sup>	0.280	0.329	0.287	0.279	0.279	0.278
Joint Significance: p-value	<0.001					

*Notes:* The unit of observation is a village and year in which an election for village committee was held. \*The independent variables are defined as the average value of the corresponding policy in the term before the election. All regressions control for province trends, and village and year fixed effects. Standardized coefficients are presented in italics. Standard errors are clustered at the village level. 1 mu = 1/15 hectare. In column (2), the sample is restricted to the villages that ever leased land to enterprises. The p-value for the joint significance of the coefficients for Post 1st Election in columns (1)-(4) is presented at the bottom of the table.

Table 7: The Loss of VC Autonomy

	Dependent Variable: Proxies for VC Autonomy						
	Policy Signatories			Re-centralization Policies			
	Sum VC Unilateral OLS (1)	Sum VC PS Joint OLS (2)	Sum PS Unilateral OLS (3)	Average Index OLS (4)	Account Oversight Logit (5)	Cadre in Residence Logit (6)	Student Monitor Logit (7)
Dep. Var. Mean	0.900	2.137	0.523	0.111	0.752	0.561	0.488
Gov x Post 2002	-0.316 (0.153)	0.254 (0.140)	0.035 (0.058)	-0.096 (0.041)			
Gov x Ln Dist x Post 2002	0.097 (0.052)	-0.094 (0.046)	-0.007 (0.025)	0.031 (0.014)			
Gov	0.072 (0.099)	-0.014 (0.100)	-0.093 (0.066)	0.049 (0.032)	2.541 (1.528)	0.923 (0.696)	0.701 (0.852)
Gov x Ln Dist	-0.026 (0.032)	0.008 (0.034)	0.023 (0.024)	-0.015 (0.011)	-0.973 (0.406)	-0.426 (0.197)	-0.227 (0.272)
Observations	2828	2828	2828	2812	564	636	504
R-squared	0.523	0.538	0.562	0.591			
Sample	1990 - 2017	1990 - 2017	1990 - 2017	1990 - 2017	2006 - 2017	2006 - 2017	2006 - 2017
Gov x Post 2002 + Gov	-0.244	0.239	-0.058	-0.048			
p-value	0.015	0.036	0.260	0.109			
Gov x Ln Dist + Gov x Ln Dist x Post 2002	0.072	-0.086	0.0158	0.0162			
p-value	0.045	0.027	0.459	0.112			

Notes: Observations are at the village and year level. Gov is county government revenues divided by county GDP in year t. Ln Dist is the log distance from the village to the county seat. Post=1 if year  $\geq$  2002. See Table 2 for a description of the dependent variables in columns (1)-(4). The regressions in columns (1)-(4) control for all of the lower order interactions, province-year trends, village and year fixed effects. Joint coefficients and their p-values are shown at the bottom of the table. The Logit estimates in columns (5) - (7) control for village and year fixed effects. The standard errors are clustered at the village level.

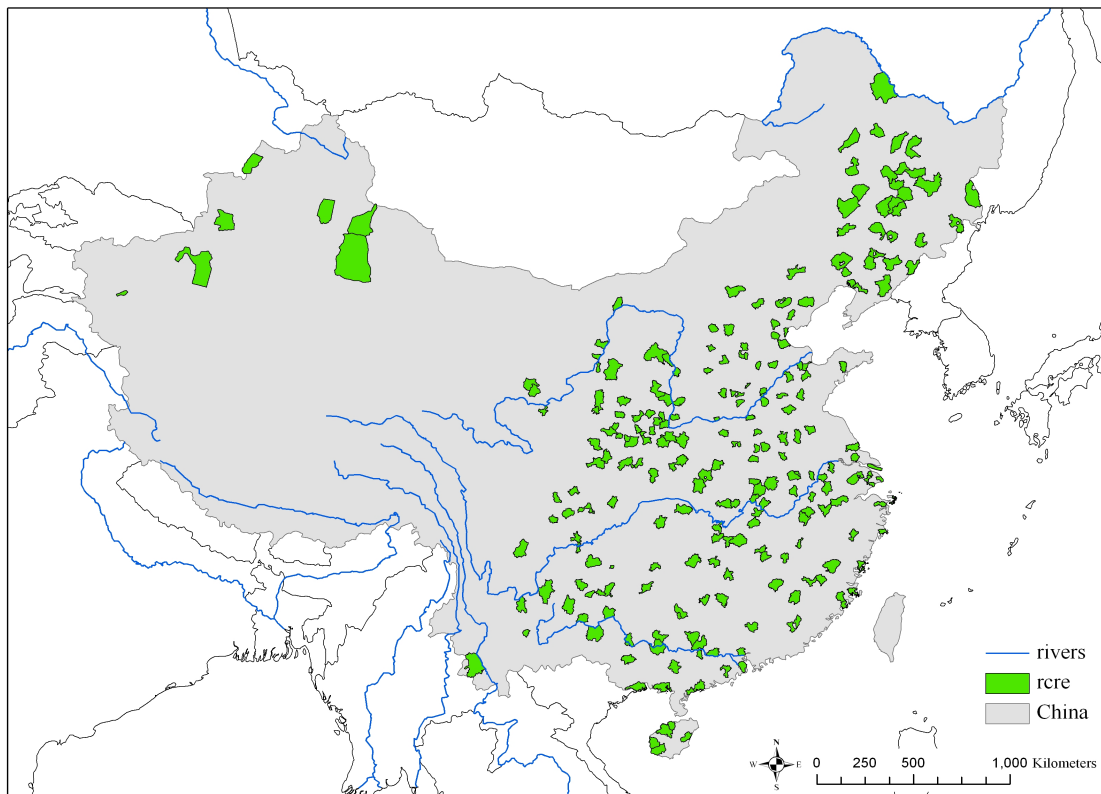
Table 8: The Loss of VC Autonomy – Robustness to Anti-Regime Movements

Dependent Variables: Policy Signatories						
Additional Controls:	Gov x Post 2002 + Gov		Gov x Ln Dist + Gov x Ln Dist x Post 2002		Obs.	R-squared
	Joint Coef.	p-value	Joint Coef.	p-value		
A. Dependent Variable: Sum VC Unilateral						
(1) Baseline	-0.244	0.0148	0.0718	0.0454	2828	0.524
(2) Dist. to Hong Kong x Year FE	-0.230	0.0179	0.0638	0.0621	2800	0.544
(3) Dist. to Xinjiang x Year FE	-0.216	0.0266	0.0620	0.0871	2800	0.558
(4) # Strikes x Year FE	-0.273	0.00676	0.0784	0.0315	2828	0.531
(5) Province x Year FE	-0.274	0.0296	0.0811	0.0643	2828	0.565
B. Dependent Variable: Sum VC PS Joint						
(6) Baseline	0.239	0.0358	-0.0858	0.0272	2828	0.538
(7) Dist. to Hong Kong x Year FE	0.243	0.0296	-0.0875	0.0190	2800	0.544
(8) Dist. to Xinjiang x Year FE	0.230	0.0359	-0.0856	0.0272	2800	0.558
(9) # Strikes x Year FE	0.306	0.00727	-0.101	0.0101	2828	0.553
(10) Province x Year FE	0.267	0.0795	-0.100	0.0387	2828	0.575
C. Dependent Variable: Sum PS Unilateral						
(11) Baseline	-0.0576	0.259	0.0158	0.459	2828	0.563
(12) Dist. to Hong Kong x Year FE	-0.0589	0.241	0.0168	0.401	2800	0.564
(13) Dist. to Xinjiang x Year FE	-0.0603	0.253	0.0156	0.476	2800	0.566
(14) # Strikes x Year FE	-0.0507	0.330	0.0144	0.499	2828	0.567
(15) Province x Year FE	-0.0945	0.126	0.0266	0.277	2828	0.610
D. Dependent Variable: Dep Var: Average Index						
(16) Baseline	-0.0475	0.109	0.0162	0.112	2812	0.592
(17) Dist. to Hong Kong x Year FE	-0.0448	0.118	0.0144	0.132	2786	0.601
(18) Dist. to Xinjiang x Year FE	-0.0378	0.196	0.0134	0.189	2786	0.610
(19) # Strikes x Year FE	-0.0597	0.0349	0.0189	0.0598	2812	0.604
(20) Province x Year FE	-0.0444	0.262	0.0161	0.214	2812	0.636

*Notes:* Observations are at the village and year level. See Table 2 for a description of the dependent and explanatory variables. The table reports joint coefficients from estimating the triple interaction equation (2) in the paper. Additional controls are stated in the row headings. Rows (5), (10), (15) and (20) control for province-year fixed effects instead of province-year trends. The standard errors are clustered at the village level.

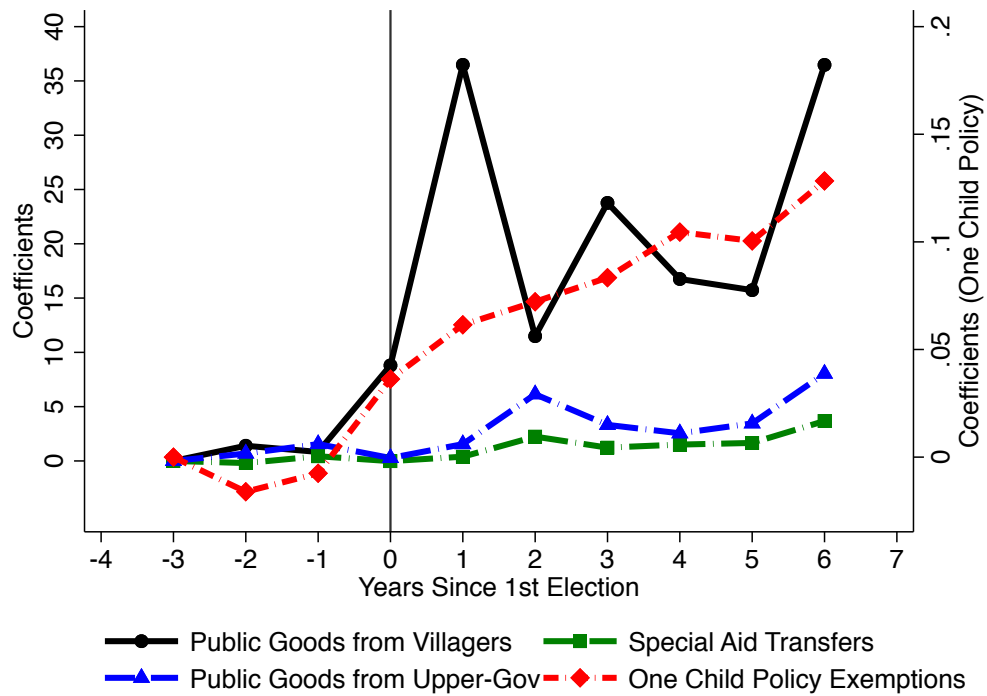


Figure 1: Map of VDS Sample



*Notes:* Villages in the VDS would be points within the counties highlighted in green. For confidentiality reasons, the authors of this paper were not provided GPS coordinates that would allow the mapping of the villages.

Figure 2: The Effect of Elections on Congruent and Incongruent Policies Over Time



*Notes:* The y-axis plots the coefficient for the indicator variable for the number of year since the first election. The estimates for public goods from villagers, public goods from the upper government and special aid transfers are shown on the left y-axis. The coefficients for the One Child Policy Exemption are shown on the right y-axis. Source: Authors' estimation results. See Appendix Table A.2 for the coefficients and standard errors.

## **Online Appendix – Not For Publication**

### **Random Permutation Tests**

To investigate whether our results are driven by spurious correlations, we can also conduct random permutation tests where we permute the timing of elections across villages (while preserving the mean and variance of the sample distribution). We then estimate the baseline equation using the randomly assigned timing. This is done for 1,000 iterations for each outcome variable. Comparing the distribution of the coefficients from the permutations to the main results, we find that the probability that the latter is due to coincidence is near zero for the average index of signatories, public goods expenditure and One Child Policy Exemptions, less than 0.6% for land expropriation and less than 0.7% for land leased out.

Table A.1: Descriptive Statistics

Variable	Source	Obs	Mean	Std. Dev.
Total Public Goods Expenditure (10,000 Constant RMB)	VDS	4,340	13.81	133.23
Irrigation	VDS	4,340	3.31	63.69
Schooling	VDS	4,340	0.02	0.34
Roads & Sanitation	VDS	4,340	4.98	88.25
Electricity	VDS	4,340	0.71	7.75
Environment	VDS	4,340	0.31	12.41
Other	VDS	4,340	2.58	48.04
Total Village Land	NFS	3,612	9,245	14,719
Arable Land (Mu)	NFS	3,612	2,295	2,329
Share of Village Land that is Arable	NFS	3,612	0.51	0.32
Used for HH Farming (Mu)	NFS	3,612	2,215	2,312
Not Used for HH Farming (Mu)	NFS	3,612	79.72	367.26
Leased Out to Enterprises (Mu)	NFS	3,612	60.46	347.61
Median HH Annual Gross Income Growth	NFS	3,084	0.08	0.19
The Number of Village Committee Members	NFS	2,287	4.36	2.36
The Number of Village Party Cadres	NFS	2,295	6.70	3.82
Party Secretary Tenure	VDS	4,340	10.80	7.91
Village Chief: Tenure	VDS	4,340	7.60	6.07
Has Election	VDS	4,340	0.83	0.37
Has Open Nomination	VDS	4,340	0.24	0.43
Years between Election Introductions in Village and Province	VDS	217	5.02	5.07
Years since last election	VDS	1,005	3.10	0.79
VC different from previous term*	VDS	3,688	0.16	0.36
1st Election Changed VC*	VDS	122	0.32	0.47
# of HH in Village	NFS	4,340	416.15	276.17
Near City	NFS	4,340	0.30	0.46

*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.

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

Dummy Var for X Years Since the 1st Election: X=	Dependent Variables:			
	Public Good Expenditures (Villagers, 10,000 RMB)	One Child Policy Exemptions	Upper-Government Special Aid (10,000 RMB)	Public Good Expenditures (Upper Gov, 10,000 RMB)
	(1)	(2)	(3)	(4)
-2	1.398 (2.089)	-0.016 (0.017)	-0.194 (0.743)	0.685 (1.267)
-1	0.829 (4.482)	-0.008 (0.032)	0.433 (1.018)	1.551 (1.574)
0	8.800 (5.143)	0.036 (0.041)	-0.016 (1.040)	0.271 (1.557)
1	36.483 (31.564)	0.061 (0.047)	0.392 (1.454)	1.547 (2.055)
2	11.492 (7.388)	0.072 (0.049)	2.242 (1.858)	6.138 (3.594)
3	23.766 (14.931)	0.083 (0.057)	1.227 (2.069)	3.320 (3.197)
4	16.757 (9.534)	0.105 (0.061)	1.508 (2.381)	2.546 (3.377)
5	15.729 (10.729)	0.100 (0.064)	1.660 (2.831)	3.460 (3.836)
6	36.479 (19.093)	0.128 (0.070)	3.680 (4.028)	8.038 (6.241)
Observations	3,919	3,919	3,919	3,919
R-squared	0.130	0.811	0.062	0.081

Notes: All regressions control for province trends, and village and year fixed effects. Standard errors are clustered at the village level. The sample is a village-level panel balanced across election years (years since the first election).