

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 almost four 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

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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.¹ 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.² In contrast to existing studies, our study addresses 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 to discipline 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.³ Citizens are well

¹See 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.

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

³A 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 have weak incentives 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 to study 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) for around one billion rural residents. Elections were introduced at the village level as a low-cost device to address monitoring difficulties and concerns of ineffective local governance.⁴ In the nearly four decades after this reform, dramatic economic growth transformed the central government from one of the poorest to one of the richest in the world. At the same time, starting in the mid-2000s, the autonomy of elected village officials has progressively eroded. In short, the Chinese context allows us to investigate whether the introduction of elections posed the hypothesized tradeoffs to the autocrat, as well as to examine the co-evolution of the autocrat's resources and her preference for vertical supervision versus local autonomy.

A key challenge for our study is the limited data available in this, or any other, autocratic context. One of the main empirical contributions is therefore to collect a large new panel dataset to document changes in village political economy over forty years for over 200 representative Chinese villages.⁵ The *Village Democracy Survey* (VDS) records village administrative data and includes information about the timing of elections, policies, elected leaders and their *de facto* power. These are the only existing data that document the history of village elections, the characteristics of village government officials and their power for a nationally representative panel of villages going back to the early reform era.

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

⁵The 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.

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 characteristics. This suggests that timing was quasi-random, which is consistent with the descriptive literature.⁶ 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.⁷ 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. They show that due to electoral accountability, improving the performance of local officials comes at the cost of weakened vertical control.

Four 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 (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. Second,

⁶Please see the discussion in Sections 2 and 4.1.

⁷See Section 4.4 for a more detailed discussion.

the introduction of elections reduced the age of VCs, while having 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. Third, VC 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. Finally, respondents whose impressionable ages overlap with the introduction of elections in their region display higher trust toward local officials. This suggests that elections succeeded in improving the standing of local officials.

There are several caveats for our preferred interpretation. The first one 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 there may have been a change in upper government policy towards villages that hold elections, rather than an increase in electoral accountability to the voters. We address this by examining upper government transfers to villages as placebo policies. 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). 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 progressive loss of autonomy of elected local governments, which began in the early 2000s. County governments, which are responsible for the management and supervision of villages, began implementing intrusive managerial practices that increased vertical control over village governments. For example, in many counties, fiscal control over village spending was tightened and county officials began to visit the villages with higher frequency. The organizational view of local elections predicts specific patterns in the process of re-centralization across villages. County governments oversee villages under their responsibility and must meet the financial and organizational costs of any managerial practice they adopt. Therefore

re-centralization should depend on the bureaucratic capacity of counties as well as the costs of projecting vertical control over villages. Hence, we expect greater re-centralization as counties increase revenues; and within these counties, less re-centralization for more remote villages.

We examine the temporal and spatial patterns in the loss of VC autonomy measured as the *de facto* power of the VC and the implementation of policies which increased county-level oversight of village governments. We find that villages in counties with faster growth in bureaucratic capacity (proxied by county revenue over GDP) experienced faster reduction in autonomy, with the magnitude of the reduction declining in the distance between the village and the county seat. These specifications control for village and year fixed effects.⁸ These results suggest that at the current level of bureaucratic capacity in many counties, supervision frictions continue to render local autonomy worthwhile for distant villages, which may explain why elections have not been abrogated *de jure*.

We consider and provide evidence against a number of plausible alternative mechanisms. For example, distant rural villages may be allowed more autonomy because they matter less for the county government, either in fiscal terms or in terms of career prospects for the county PS. We control for several measures of fiscal importance and economic development of the village, as well as age of the PS and show that our results are robust. Similarly, county revenues divided by GDP may reflect the size or the level of economic development of the county, which could itself be a driver of re-centralization. However, controlling for population, GDP, geographical size or number of villages does not affect our coefficients of interest. Neither does controlling for indicators of economic development such as rural population share or GDP growth. We discuss these and other checks of alternative mechanisms in Section 5.3.

This study sheds new light on the forces leading to decentralization and re-centralization in autocracies. We accomplish this by developing a coherent conceptual framework and highlighting the central role of agency frictions and bureaucratic capacity.⁹

We build on several literatures. The fact that informational asymmetry introduces a basic tension

⁸We also control for province trends when the sample size allows.

⁹Other forces such as changes in national leadership doubtlessly contribute to the rise and fall of elections. However, independently of national leadership priorities, bureaucratic capacity has to be strong enough to allow effective vertical rule over villages. In this sense, bureaucratic capacity should be seen as a necessary enabler of vertical control.

between top-down authority and local autonomy has been recognized in many contexts.¹⁰ We are the first to provide evidence that this organizational tension can explain ostensibly surprising autocratic institutions, such as local elections.¹¹ 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).¹²

A growing number of empirical studies provide important evidence on local governance in developing countries.¹³ 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.¹⁴ We focus on the crucial importance of organizational frictions, and are the first to propose a framework that rationalizes the motivations of the central government.¹⁵ 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).

This paper is organized as follows. Section 2 summarizes the political discourse and implemen-

¹⁰The 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 by local power-brokers, as in Bardhan and Mookherjee (2006).

¹¹At the theoretical level, Myerson (2015) makes a related point, except in his work the lack of commitment of the autocrat is also a crucial friction.

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

¹³In 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.

¹⁴For other examples of economic studies of Chinese local elections, see Zhang, Fan, Zhang, and Huang (2004), Gan, Xu, and Yao (2007), Luo, Zhang, Huang, and Rozelle (2010), and 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). 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.

¹⁵Brandt and Turner (2007) pioneers the application of moral hazard insights to local elections in China, but it does not enquire on the reasons for the introduction or undermining of elections.

tation 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 and Conceptual Framework

2.1 The Argument

We borrow from the insights of organizational economics to explain local elections in an autocracy, and thus the conditions under which local elections may be introduced or undermined.

While their first priority is to stay in power, autocrats usually have a vision for the country which is reflected in a number of policies they want implemented. These policies are ultimately implemented by local officials, which are those in contact with the population. These tasks require effort. In the absence of well-targeted incentives and supervision, local officials shirk from their tasks and instead pursue other objectives such as personal rent-seeking.¹⁶ How well a vertical system of control deals with this moral hazard depends crucially on the quality of information that reaches the upper government and its ability to process it to provide appropriate incentives.

There are two potential solutions to this problem. The first one is to invest in the vertical system of bureaucratic supervision, which we call bureaucratic capacity. This improves the collection and processing of local information to provide correctly targeted incentives to local officials. The problem with this solution is that it is costly in money and time.¹⁷ 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 locality 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 carries a cost: local officials facing elections have little incentive to implement unpopular policies mandated by the upper government. Thus, the delegation of authority

¹⁶This is a classic moral hazard issue, as in Holmstrom (1979).

¹⁷Current 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.

creates a tension between the better use of local information versus a loss in vertical control.¹⁸

The organizational framework indicates that the desirability of local elections is linked to bureaucratic capacity. When bureaucratic capacity is low, autocrats benefit from introducing elections because the system of vertical control can only extract little effort from local officials. Introducing elections in such circumstances should improve local official performance for popular policies. As bureaucratic capacity improves, the benefits of local autonomy diminish. Better vertical control allows upper governments to detect and intervene when elected officials are shirking, and thus extract more effort for both popular and unpopular policies. A direct implication of this logic is that autocratic regimes should curtail the autonomy of elected local governments as bureaucratic capacity increases.

2.2 Local Elections in China

Policy implementation has been a perennial challenge for the central government of China.¹⁹ 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.²⁰ In this study, we focus on the tension between the villages and the county, the level of government that supervises them. We often refer to the upper government to subsume all levels above the village.

During the early reform era (beginning in 1978), governance problems were particularly acute within 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 upper government lacked the bureaucratic capacity to hold village officials accountable, which led to ineffective and contentious governance.

Two issues, in particular, were a common source of discontent amongst villagers. First, the

¹⁸This trade-off has long been identified in the organizational economics literature in the context of a firm. See for instance, Aghion and Tirole (1997), Dessein (2002) and Mookherjee (2006).

¹⁹Meng, 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.

²⁰There have been showcase elections at other levels. We do not discuss them for brevity.

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. 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 determine village investment needs and cajole the villagers to make contributions. National policymakers and villagers widely complained that village governments shirked from this effort.²¹

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.²² 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.²³ 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? 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 government could not afford expanding bureaucratic capacity to better supervise local officials.²⁴ While proponents emphasized the benefits of making local officials accountable to the villagers, opponents of the reform feared

²¹Several studies document the paltry provision of public goods and widespread corruption. See, for example, Shen and Yao (2008) and Luo, Zhang, Huang, and Rozelle (2010).

²²Consistent 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.

²³The comparative literature has studied the debate surrounding the introduction of village elections. White (1992), Kelliher (1997) and O'Brien and Li (2000) summarize the main issues in the policy debate and contain rich descriptions of the implementation process of elections.

²⁴See O'Brien (1994) for a discussion of the tight budgetary constraints under which this discussion was taking place.

disarray in local governance and a loss of vertical control.²⁵

Local elections were ultimately enacted as a reform to improve local governance while saving on costs. The 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 did not modify the standing of the village Party branch, the responsibilities of the village government, and the extant fiscal arrangements. 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.²⁶ 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 issues and are probably selected for qualities that have been long observed by their fellow villagers.²⁷ 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.

While innovative regional governments experimented with elections in the early 1980s, the re-

²⁵White (1992), describing the contemporaneous policy debate, 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 villages 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. It is worth noting that opponents did not foresee an improvement in performance of popular policies and advocates did not accept that vertical control would weaken. Neither of the sides was thus ultimately correct in predicting all the effects of this reform. See Kelliher (1997) and O’Brien and Li (2000) for further qualitative analysis of the policy debate.

²⁶For 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 subsequent 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.

²⁷There 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).

form was formally 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 (O'Brien and Li, 1999). By the late 1990s, local elections had been implemented in the vast majority of villages.²⁸

While elections are now universally adopted, the authority, autonomy and scope of elected village governments has progressively eroded in recent years. This process started with the 2003 *Tax and Fee Reform*, which made it illegal for village governments to use *ad hoc* fees to fund public investment. This shortfall was to be made up with direct financing from the upper government.²⁹ While the stated purpose of these reforms was to reduce the tax burden on rural households, they also eliminated village fiscal autonomy.

The *Tax and Fee Reform*, which was a national policy implemented on all villages, was an increase in vertical control that was conspicuously promoted by the central government. In its wake, county governments have also adopted a number of managerial practices that further limit the autonomy of village governments. For instance, Oi, Babiarz, Zhang, Luo, and Rozelle (2011) document that many counties have implemented a policy of account oversight (*shuang daiguan*) where villages need explicit permission from county authorities to access funds earmarked for the village.

Besides exerting fiscal control, county governments have also resorted to other practices of direct supervision. He and Wang (Forthcoming) examines a new policy under which university graduates are allocated to villages for three-year terms in exchange for Party membership or a position in the state bureaucracy afterwards. Another prominent example is the cadre-in-residence

²⁸This oscillating relationship in which experimentation from the bottom turns into national policy and is then imposed top-down is typical of policy reform in modern China. Roland (2000), Qian (2003) and Cai and Treisman (2006) among others attribute this policy determination system to reasons ranging from optimal design to political expediency. Heilmann (2008) presents a full account of this process and summarizes it as "In the process of policy conception, implementation and revision governments at various levels thereby become participants, sometimes leaders and sometimes followers, of reform initiatives."

²⁹The 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). 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).

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 government. Therefore, they serve both as a source of information and as a tool of intervention in village affairs which further curtails the autonomy of elected village bodies.³⁰

It is important to note that both the introduction of village elections and the *Tax and Fee Reform* were centrally mandated policies to be implemented in every village. In contrast, the managerial practices and policies that some county governments have adopted since 2003 are not national policy. While county governments operate under the supervision of officials higher up in the hierarchy and must respect national policies, they decide how to manage each village under their responsibility and must meet the financial and organizational costs of any managerial practice that they adopt. As noted, there has not been an official revocation of village elections, which continue to take place. However, as these governance changes spread throughout rural China, the authority, autonomy and relevance of elected village officials are progressively undermined.

2.3 Predictions

The logic of elections as organizational delegation of accountability can explain both the introduction of elections in the 1980s and their progressive undermining after 2002 because the intervening period saw a large increase in bureaucratic capacity as the economy grew.³¹ 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.³²

³⁰The county government can intervene in the village in several ways. On the one hand, it can give direct orders to the village government, likely through the village PS, or the student- and cadre-in-residence. On the other hand, it can take tasks away from the village government and bring them under the direct control of the county. Villages have experienced the former tactic more frequently than the latter.

³¹Government 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 *China Statistical Yearbooks*.

³²Expenditure 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 an approximation provided by a central government official interviewed by the authors.

In the rest of the paper, we use village-level data to explore five precise predictions from our conceptualization of local elections as a delegation of 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 tradeoff 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.

Fourth, villagers exposed to the introduction of elections should have a better opinion of local officials, since they witnessed the actions and attitudes of local officials who were accountable to the population.

Finally, constraints in bureaucratic capacity at the county level should drive the erosion of village autonomy from 2002 onwards. More specifically, counties with better fiscal positions should re-centralize more, but this effect should be weaker for remote villages where direct supervision is more costly.

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. Since the introduction of local elections dates back to the 1980s, the empirical analysis requires a long panel of villages with detailed data of village policies and political-economic conditions. A survey of past outcomes would face recall bias and would be difficult to administer on a nationally

representative sample given China's linguistic heterogeneity.³³

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 and 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 data in several instances where the NFS data only contains village-level aggregated variables.³⁴

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 governance and 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. Because the analysis in this paper relies exclusively on data from administrative records, recall bias does not play a role in the interpretation of our results. We describe the variables in more detail as they become relevant for our analysis.

³³In 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.

³⁴For 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.

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 re-centralization 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 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.³⁵ The geographic coverage of our data is mapped in Figure 1.

In the analysis, we supplement the VDS data with variables from the NFS and other additional sources when necessary. Online Appendix Table A1 summarizes the main variables and their sources.

4 The Introduction of Elections

4.1 Descriptive Statistics and Timing of the Reform

Innovative provincial governments began experimenting with elections in the early 1980s. Elections were formally codified by the central government in the 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

³⁵The villages were chosen by random sampling stratified at the province level in 1986. There are 31 provinces in China at the end of our sample period. The two excluded provinces are Tibet and Chongqing. Tibet is excluded because it is subject to different political and economic policies. Chongqing is a city-municipality that is excluded because it did not achieve provincial status until 1997 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).

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 the introduction of elections, which is characteristic of reforms in rural China (Unger, 2002).

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 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 the upper 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 supports the notion that the rapid rollout within provinces led to a quasi-random introduction of elections, uncorrelated with omitted variables which would confound our interpretation. We present additional evidence against omitted variable bias after we discuss the estimation and main results.

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

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

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, γ_{pt} ; village fixed effects, δ_v ; and calendar-year fixed effects, ρ_t . We cluster the standard errors at the village level, since that is the level of variation for the introduction of elections.³⁶

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 also control for province-year fixed effects, which are more stringent and force the regression to only exploit within-province variation.

Interpreting β 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 provide additional evidence to support this assumption when we discuss robustness.

³⁶We show that the results are similar if we estimate wild-bootstrapped standard errors clustered at the province level. See the Robustness Section 4.5.

4.3 The Effect on Leaders

In this section, we first investigate whether the newly elected VCs have a meaningful degree of autonomy and power over village affairs. This is far from obvious due to the continued presence of an 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 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 to 3 range from zero to four, since there are up to four policies.

The sample means in Panel A indicate that the VC, on average, unilaterally signs 1.1 policies (row 1), while the PS unilaterally signs 0.63 (row 3). 1.46 policies are signed by both the VC and the PS (row 2). 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), and reduced the number of policies over which the PS is the sole signatory (row 3). The estimates in rows 1 and 3 are statistically significant at the 5% or higher level and are economically meaningful. For example, the estimate in row 1 implies that the introduction of elections increased the number of policies signed unilaterally by the VC by 22% of the sample mean.

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 VCs 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.³⁷

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 of quality of the office holder as the dependent variable. We find 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) examines age, and 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, which are all 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 of elections are statistically different for the two types of leaders. These findings indicate that the introduction of elections had significant effects in changing the type of elected village leaders and no commensurate effect on unelected leaders.

³⁷Note 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 and legitimacy 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.

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 2.2. 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.³⁸ Note that approximately 70% of all expenditure on public goods in our sample are from village sources.

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.³⁹

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.⁴⁰ 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 provinces). Column (2) shows that the introduction of elections reduced the amount of land leased to enterprises by 61.6 mu on average.⁴¹ The estimate is almost statistically significant at the 10%

³⁸During the period of our study, China had a fixed exchange rate and 1 RMB was roughly equal to 1/7 USD.

³⁹Figure 2, which we discuss in the next section, is consistent with these dynamics.

⁴⁰Note that Brandt and Turner (2007) proxies for corruption with this measure.

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

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 in a top-down fashion 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 a rise 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 was a girl or if the household head was 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 in every ten observations. The estimate is statistically significant at the 5% level. The sample mean is one 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 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.⁴² 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 almost statistically significant at the 10% level. This result suggests that 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. The bottom row presents the p-value for the joint significance of the estimates in columns (1) to (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) to (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 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

⁴²In 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).

government attitude towards villages as elections are implemented.

The results in Table 4 are consistent with the second prediction of the organizational argument. 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 trade off predicted by our argument 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. 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 waves 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.⁴³ 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

⁴³These data are reported by *China Statistical Yearbooks*.

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 observed features of the village. Nevertheless, 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).⁴⁴ 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.⁴⁵ 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 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.⁴⁶ To maximize the statistical precision of our estimates, we use the principal component of these three measures as our social capital proxy. Column (7)

⁴⁴These characteristics are measured in the first year that data are available.

⁴⁵We 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 interaction 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.

⁴⁶To measure the size of the kinship groups, the VDS recorded surnames from the village roster.

shows that the resulting coefficients are similar to the baseline.⁴⁷

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 sum, while the estimates vary in precision, the coefficients are statistically similar across columns (1) to (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 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.⁴⁸ 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 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

⁴⁷The 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). The results are also robust to controlling for distance to the county seat interacted with year fixed effects. However, since the distance variable is only available for a subset of villages we do not report the results in this table. These additional results are available upon request.

⁴⁸These estimates as well as their standard errors are presented in Appendix Table A.2.

that even though election timing and the key outcome variables may be infrequent events, our results are highly unlikely to be driven by coincidence.⁴⁹

4.6 Policy Implementation and Re-Election Probabilities

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.⁵⁰ The p-value at the bottom of the table for the joint significance of the estimates in columns (1) to (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.

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.

⁴⁹See the Online Appendix.

⁵⁰However, 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.

4.7 Citizen Satisfaction

A key motivation for the introduction of elections was to increase citizens' satisfaction with local cadres and therefore the political stability of the regime. We now directly examine the association between self-reported citizen attitudes towards local officials and exposure to village elections.

This descriptive analysis uses the 2012 *China Family Panel Survey* (CFPS).⁵¹ The CFPS contains a question about the trust respondents place on local government (village and county) cadres. This variable ranges from 0 (no trust) to 10 (very trustworthy). The sample mean is 5.223 with a standard deviation of 2.45. The CFPS data are at the household level and contain county identifiers, but no village identifiers. We are able to link these households to twelve counties within our main sample. Note that the VDS records the date of the first election in each county.

Our analysis is based on the insight from several disciplines that political attitudes are predominantly formed during ages 18-25, the so-called “impressionable years” (Krosnick and Alwin, 1989; Giuliano and Spilimbergo, 2014). We hypothesize that individuals 25 years of age or younger when elections were first introduced in their counties were more “exposed” to cadres that were accountable to villagers, which should influence their current views of local cadres. Those who were older at that juncture had less malleable political attitudes and hence their views of local cadres should be less affected by the introduction of elections. Since the effect may differ depending on age within the impressionably aged group, we further split this group and create a dummy variable for individuals who were 18-25 years old and another one for individuals who were younger than 18 when elections were first introduced in their county.⁵²

We regress trust on the two age category dummy variables, while controlling for a number of demographic variables that are commonly believed to affect political attitudes (gender, dummy variables for the level of education attained, marital status, log income, birth year and its squared term) and county fixed effects. Table 7 column (1) shows that those who were in their most impressionable age ranges when the elections were first introduced report much higher trust towards

⁵¹See the online Data Appendix for details.

⁵²To avoid confounding the estimates with the influence of the undermining of elections that began in 2002, we exclude those who are born after 1977. These individuals would have been 25 or younger (still impressionable) when elections began losing relevance.

government cadres relative to individuals older than 25 when elections were introduced. The coefficients are positive and statistically and economically significant for both age groups. This result is aligned with the fourth prediction of our argument, namely that local elections should improve citizen satisfaction towards the local government.⁵³

A potential concern with this result is that county-cohort-specific trust towards the government may change for other reasons. For example, differential regional economic growth over time may disproportionately benefit younger cohorts. In addition, our control group includes individuals who were in their most impressionable age during the Great Famine (1969-61) and the Cultural Revolution (1966-76). The existing literature and conventional wisdom suggest that these forces affect trust in all dimensions (Meng, Qian, and Yared, 2015; Thaxton, 2008; Walder, 2019; Sapienza, Toldra-Simats, and Zingales, 2013; Walden and Zhukov, 2020). Thus, to address these concerns, we take advantage of the fact that the CFPS, besides reporting trust towards local cadres, also reports trust towards four other groups: “most other people”, neighbors, strangers, and doctors. Columns (2) to (5) show that being of an impressionable age when elections were first introduced does not affect trust towards these social groups. The estimates are statistically zero and the standardized coefficients also show that they are much smaller in magnitude compared to the estimates for trust towards government cadres. These null results indicate that our finding for trust towards cadres is very unlikely to be confounded by these other factors.

5 Undermining Elections

In Section 2.2, we describe how from the early 2000s, the scope and autonomy of village governments have been progressively curtailed. 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. The national-level surge in bureaucratic capacity is reflected on the capacity of county governments, which are directly responsible for managing villages. However, the increase in capacity across counties was very heterogeneous. Counties operate under the supervision of officials higher up in the hierarchy and must respect national policies. At the same time, county

⁵³Unfortunately, we cannot not examine trust in the central government because this question is not included in the CFPS.

governments have discretion over how to manage each village under their responsibility and need to meet the financial and organizational costs of any managerial practices adopted.

In this section, we leverage the decentralized nature of county managerial practices to trace the influence of county bureaucratic capacity as a driver of re-centralization. More specifically, we use the third wave of the VDS, collected in 2019, to investigate whether counties with more revenue growth engage in greater limitations of VC’s autonomy, and whether these autonomy losses are larger for villages geographically closer to the county seat, where the costs of direct control are smaller.

5.1 Estimation

This analysis focuses on the post-2002 period since re-centralization began with the 2003 *Tax and Fee Reform*, which was approved in the wake of Hu Jintao’s ascension to power in 2002. Our baseline estimating equation is the following.

$$Y_{vpt} = \beta_1 G_{vpt} + \beta_2 G_{vpt} \times D_{vp} + \gamma_{pt} + \delta_v + \rho_t + \varepsilon_{vpt}, \quad (2)$$

where Y_{vpt} is the outcome for village v in province p in year t ; G_{vpt} is a measure of county bureaucratic capacity; and D_{vp} is the log travel distance between the village and the county seat. As in the earlier estimates, we control for province-time trends, village and year fixed effects.⁵⁴

When the outcome is a measure of VC autonomy we expect to find $\beta_1 < 0$, since higher county capacity would lead to a reduction in the autonomy of a village located close to the county seat. We expect $\beta_2 > 0$, as this reduction in autonomy is likely to be mitigated for more remote villages, for which vertical control is more difficult and/or expensive.

Measuring County Bureaucratic Capacity Tax revenues as a share of GDP is the standard measure of bureaucratic capacity in the state capacity literature (e.g., Dincecco, 2011; Besley and Pers-

⁵⁴Note 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.

Travel distance between the village and the county seat, D_{vp} , was provided by the MoA. In total, there are 101 of the original sample of 217 villages for which we observe VDS data, county-government statistics and distance measures. These villages are not systematically different from the average village in our original sample in any observables.

son, 2013). Normalizing by GDP avoids mechanically attributing higher levels of capacity to larger states. The measure is meant to capture the capacity of the government to mobilize resources conditional on the amount of economic activity. The analog of this measure to our county-level analysis is G_{vpt} , county government revenue over county GDP.

The numerator of this measure, county government revenue, is reported by the *Fiscal Yearbooks of Chinese Prefectures and Counties*. County government revenues include tax and non-tax revenue, but exclude transfer income from upper levels of government (e.g., tax rebates, targeted subsidies for ethnic minority and remote areas, and disaster relief). We do not include transfers in our measure of county bureaucratic capacity because they are unrelated to the ability of the county government to manage and extract resources from the region under its control.⁵⁵

The main caveat for interpreting the estimates is the concern that county government revenues, county GDP or distance between each village and the county seat are correlated with other factors that can influence re-centralization. We address these potential omitted variables and other concerns after we present the baseline results.

Average county revenues as a share of GDP increase 40% during our sample period, from 3.9% in 2002 to 5.45% in 2017. The standard deviation during this period also increases from 1.76% to 3.92%. The strong increase in revenues over GDP as well as the increased dispersion provide the variation we use in this section.

5.2 Results

Table 8 presents the results. Our first measures of VC autonomy are the signatories over important policies that we examined earlier. Using data collected in the 2019 wave of the VDS, we extended these signatory measures from the original surveys to construct a panel for the period of 2002-2017. The dependent variable in column (1) is the number of relevant policies for which the VC is the sole signatory. The estimate of $\hat{\beta}_1 = -0.182$, which is statistically significant at the 5% level. This indicates that VC unilateral signature power over policies is decreasing in county government revenues. 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

⁵⁵See the Data Appendix for more details.

revenues is associated with a 0.42 $((-.182 \times 2.92)/1.27 = -0.42)$ standard deviation decrease in VC signatures. This is a significant loss of VC autonomy.

The interaction coefficient with log distance to the county seat, $\hat{\beta}_2 = 0.059$, is positive and statistically significant at the 5% level. 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 re-centralization period, when county revenues increase by one standard deviation, the distant village would experience a 0.135 standard deviation smaller reduction in VC signatures $((0.059 \times 2.92 \times 0.997)/1.27 = 0.135)$. Hence, a one standard deviation in distance ameliorates the reduction in VC signatures by 32% (relative to the 0.42 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.

The dependent variable in column (2) is the number of policies that require signatures of both the VC and the PS. The coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$ have the opposite signs as those in column (1) and are statistically significant at the 5% level. Column (3) examines the number of policies that the PS unilaterally signs. The 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.

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 directly managing distant villages. These frictions make it useful to allow autonomy in geographically distant villages at current levels of bureaucratic efficiency, and perhaps helps explain why elections have not been abrogated.

Next, we investigate the implementation of actual managerial practices used to curtail the auton-

omy of elected leaders. We focus on the three practices that we can observe in our data: village budgetary account oversight, the student-in-residence program and the cadre-in-residence program.⁵⁶ These variables are only recorded in the third wave of the VDS, which was limited to the period 2006-2019.⁵⁷

We examine these policy variables as outcomes in a regression analogous to (2). We estimate non-linear Logistic regressions because the outcome variables are binary.⁵⁸ Since all these outcomes correspond to practices that curtail village autonomy, we expect $\beta_1 > 0$ because increased revenues should be associated with more assertive county governments; and $\beta_2 < 0$ because these practices are more costly in more remote villages.

The estimates in columns (4) to (6) are consistent with our hypotheses: the managerial practices meant to assert control over elected leaders are more frequently applied in counties whose resources grow, but the magnitude of this increase is lower for villages that are geographically distant from the county seat. In column (4), which examines account oversight, the coefficients are statistically significant at the 10% and 5% levels. In column (5), which examines the cadre-in-residence program, the analogous coefficients are significant at the 20% and 5% levels. In column (6), 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 large.

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.

5.3 Alternative Explanations

This section explores plausible alternative mechanisms that could explain the results in Table 8.

Career Concerns First, we examine the possibility that our results reflect the career concerns of county Party Secretaries, who are incentivized to exert more control over localities that can

⁵⁶See Section 2.2.

⁵⁷Earlier waves did not collect these variables since they were not relevant at the time.

⁵⁸The sample size in columns (4) to (6) differs from the one in columns (1) to (3) because it covers fewer years and because Stata omits collinear observations when estimating logit models.

determine her chances of promotion. They may allow remote villages more autonomy not because of monitoring difficulties, but because these villages do not matter for their career prospects. To address this directly, we conduct an alternative natural experiment to test for the relevance of career concerns and conduct a horse race of this natural experiment against our main variation.

Promotion incentives decrease with age because county Party Secretaries over the age of 40 are not eligible for promotion and there is a mandatory retirement age (Kou and Tsai, 2014). Thus, we follow existing studies and exploit variation in the age of the county Party Secretary to investigate the effect of promotion incentives (e.g., Jia, Kudamatsu, and Seim, 2015; Persson and Zhuravskaya, 2016). For this exercise, we consulted Party archives to manually collect and digitize biographic information for every Party Secretary that runs a county where a village in our sample is located. See the Data Appendix for the sources.

Table 9 examines policy signatories as dependent variables. For brevity, we focus the discussion on columns (1) to (3), which examine the number of policies with the elected VC as the unilateral signatory. Column (1) presents the baseline main result for comparison. In column (2), we additionally control for the age of the county Party Secretary and its interaction with the distance between county seat and village. Our coefficients of interest are similar to the baseline so our main results are not confounded by the intensity of promotion incentives as proxied by age.

At the same time, we find that the coefficient for age is positive and significant and the interaction with distance is negative. This implies that village autonomy is higher under older county Party Secretaries, and the gradient of distance is flatter. These results are consistent with the stated hypothesis that lower promotions incentives, as captured by age, imply less stringent supervision and less differentiation between closer or distant villages.

In column (3), we use a dummy variable for whether the county Party Secretary is over 40 years of age and thus ineligible for promotion. The estimates have similar signs as in column (2), but are less statistically precise. In sum, the data suggests that there may indeed be a link between promotion incentives and village autonomy, but also that this link does not drive our main results.

The Economic Importance of Villages A related concern is that villages near the county seat are more economically important. Remote villages are typically poorer and thus economically periph-

eral from the perspective of the county government. Thus, increased county bureaucratic capacity may increase monitoring of nearby villages not because of an increase in capacity, but because the economic importance of nearby villages may be correlated with our proxy of county capacity. By the same token, remote villages may keep their autonomy because they remain unimportant, not because of monitoring costs.

We address this by controlling for the interactions of county government capacity and several measures of the economic importance of the village: all transfers from the village to the upper government, taxes (a subset of transfers), total manufacturing production, total manufacturing profits and the official economic rank assigned to the villages. The last measure is the index used by the government for poverty alleviation and other earmarked policies.⁵⁹

Our results are very robust to the inclusion of these additional controls. Therefore, our main results are not driven by the economic importance of villages. See Appendix Table A3.

County Size and Prosperity Using the economic size of the county in the denominator of G_{vpt} introduces the concern that size itself may be driving the results. It is possible that a county that has to deal with a larger physical area, larger population or a larger number of villages has less effective capacity to project onto villages. These alternative dimensions of the size of the county may be a source of omitted variable bias in our estimates on re-centralization.

We address such concerns by controlling for different dimensions of county size (total county population, total geographic area of the county, the number of villages within the county, as well as county GDP itself) and their interactions with log distance as controls.⁶⁰ Our results are very robust. This allays concerns that our main results may be confounded by other aspects of county size. See Appendix Table A4.

Another possibility is that county governments may have found other ways of appeasing the population, which allows them to remove village autonomy. Thus, the removal of village autonomy would not be driven by changes in bureaucratic capacity of the county, but rather that elections

⁵⁹Economic rank ranges from 0 (poorest) to 8 (most developed) and is the official designation of the economic well-being of the village. To avoid endogeneity (e.g., economic growth in a village “earns” the village more or less autonomy), the new controls are measured as the average over 1998-2002, before re-centralization begins. The data are reported by the NFS village survey.

⁶⁰For time-invariant controls, we control for the interaction with log distance and year fixed effects.

would no longer be necessary as a method of appeasement. Given the often mentioned relationship between social acquiescence and economic progress (i.e., economic development buys social peace), one way of operationalizing this concern is to ask whether regions with higher income or higher income growth lost more autonomy. Thus, we control for measures of economic well-being such as per capita GDP, rural population share and a five year moving average of GDP growth, and their interactions with log distance between the county seat and the village. The results change little with these controls. See Appendix Table A4.

The physical size of the population or area of a county is just one way to measure “size.” Effective “size” can be reduced by transportation infrastructure or increased by difficult terrain. To address this, Appendix Table A5 controls for several measures of transportation infrastructure in the county (the lengths of railway, highway, state roads, provincial roads, all normalized by total area) and ruggedness/hilliness of the terrain. Since a county can invest in its organizational capacity by building physical infrastructure, we control for infrastructure prior to the re-centralization period (2000-2002).⁶¹ Since these controls are time invariant, we control for their interactions with year fixed effects and their triple interactions with year fixed effects and log distance between the village and county seat. Our main results are very robust.

Urbanization The period that we study is one of rapid urbanization, which varied across regions. This raises the question of whether our results spuriously capture the influence of urbanization. For example, central control is generally considered to be much stronger in urban areas than rural counties. Rural counties near rapidly expanding urban areas may thus centralize as a result of changing political culture rather than an increase in bureaucratic capacity. Similarly, one would expect the diffusion of pro-centralization political culture to begin in villages near the county seat before remote villages.

The estimates in Appendix Table A4 address this by controlling for several measures of urbanization in the rural counties that we study: rural population share, county area and the number of villages. Rural population share measures the degree of urbanization within the county. County area

⁶¹The infrastructure data are reported by the *Historical county population census data with maps*, published by the All China Marketing Research Co. Ltd. (2005). We compute the ruggedness measure using ArcGIS.

and the number of villages reflects the expansion of nearby large cities. Much urbanization in China during this period is driven by large cities, which have the same administrative status as a rural county. As these cities expand geographically and absorb nearby villages, the administrative areas and the number of villages of the adjacent rural counties will mechanically shrink.⁶² The robustness of our results to controlling for these measures indicate that our findings are not confounded by urbanization within the county or in nearby large cities.

Political Stability A final concern for our preferred interpretation of the re-centralization results is that the curtailing of village autonomy may be a response to increased political tensions that are unrelated to organizational frictions. 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 2011.⁶³ We interact each of these proxies with year fixed effects to allow their effects to be fully flexible over time.

Each panel of Appendix Table A6 examines a 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

⁶²Note that there are no cases of county seats being absorbed by expanding urban areas in our sample.

⁶³Prefectures 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. Stromberg, Qin, and Wu (2017) and Campante, Chor, and Li (2019) are recent studies that use these data.

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 policies or generalized dissatisfaction with the regime.

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.

While our framework suggests that increased bureaucratic capacity was enough to bring about re-centralization, many other forces such as national shifts in priorities may well have played a part in driving the timing and intensity of re-centralization. Therefore, bureaucratic capacity should not be seen as the only determinant of re-centralization. Instead, it is the necessary enabler of the process: national priority changes could hardly have resulted in effective and re-centralized village governance if bureaucratic capacity was at the same level as it was in the 1980s.

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 almost four 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.⁶⁴

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 a 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 re-centralization is also present in other autocracies, such

⁶⁴See Acemoglu, Johnson, Robinson, and Yared (2009) for a recent empirical analysis of aggregate data.

as in Vietnam.⁶⁵

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

⁶⁵See Malesky and Schuler (2013).

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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 |
| B. Reimbursement | | | | | | | |
| (4) | VC Unilateral | 0.549 | 0.498 | 0.066 | (0.044) | 4,098 | 0.768 |
| (5) | VC & PS Joint | 0.213 | 0.409 | -0.001 | (0.034) | 4,098 | 0.731 |
| (6) | PS Unilateral | 0.238 | 0.426 | -0.065 | (0.033) | 4,098 | 0.814 |
| C. Public Investment | | | | | | | |
| (7) | VC Unilateral | 0.169 | 0.375 | 0.065 | (0.032) | 3,723 | 0.759 |
| (8) | VC & PS Joint | 0.689 | 0.463 | -0.041 | (0.034) | 3,723 | 0.805 |
| (9) | PS Unilateral | 0.142 | 0.349 | -0.025 | (0.033) | 3,723 | 0.768 |
| D. Land Reallocation | | | | | | | |
| (10) | VC Unilateral | 0.316 | 0.465 | 0.104 | (0.039) | 3,269 | 0.791 |
| (11) | VC & PS Joint | 0.554 | 0.497 | -0.052 | (0.040) | 3,269 | 0.802 |
| (12) | PS Unilateral | 0.130 | 0.336 | -0.051 | (0.035) | 3,269 | 0.792 |
| E. Appoint Managers | | | | | | | |
| (13) | VC Unilateral | 0.317 | 0.465 | 0.068 | (0.044) | 2,775 | 0.787 |
| (14) | VC & PS Joint | 0.388 | 0.487 | -0.015 | (0.041) | 2,775 | 0.798 |
| (15) | PS Unilateral | 0.294 | 0.456 | -0.053 | (0.037) | 2,775 | 0.833 |

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. In rows (4)-(6), (7)-(9), (10)-(12), and (13)-(15), the dependent variables are dummy variables which equal one if the signatory is the VC, PS, or both. 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) | | |
| Dependent Variable Mean | 8.060 | 42.50 | 0.785 |
| Post 1st Election | 0.517 (0.312) | -2.647 (1.085) | -0.084 (0.053) |
| Obs | 3,260 | 3,280 | 3,360 |
| R ² | 0.638 | 0.493 | 0.527 |
| | B. Party Secretary (appointed) | | |
| Dependent Variable Mean | 8.520 | 44.27 | 1 |
| Post 1st Election | -0.238 (0.238) | -1.026 (0.969) | |
| Obs | 3,820 | 2,740 | |
| R ² | 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) | Village Land Leased to Enterprises (mu) | One Child Policy Exemptions | Dummy for Expropriation of Village Land | Upper-Government Special Aid (10,000 RMB) | Public Good Expenditures (Upper Gov, 10,000 RMB) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Dep. Var. Mean | 9.46 | 111.01 | 0.55 | 0.02 | 1.49 | 4.28 |
| 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 ² | 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 | Omit if Ever Merged with Another Village (8) | Wild Bootstrapped P-Values clustered at Province Level (9) |
|---|---------------------|---|---------------------|------------------------------------|---------------------------------|------------------------------|--|--|--|
| | | | | | | | City, Social Capital, Post Tax & Fee (7) | | |
| Panel A. Dependent Variable: Sum of Policies Unilaterally Decided by VC | | | | | | | | | |
| Post 1st Election | 0.242 (0.107) | 0.247 (0.107) | 0.253 (0.128) | 0.218 (0.104) | 0.223 (0.109) | 0.233 (0.109) | 0.234 (0.106) | 0.245 (0.123) | 0.242 [0.016] |
| Observations | 4,340 | 4,340 | 4,340 | 4,018 | 4,280 | 4,280 | 4,340 | 3,500 | 4,340 |
| R-squared | 0.745 | 0.745 | 0.762 | 0.758 | 0.747 | 0.745 | 0.747 | 0.754 | 0.745 |
| Panel B. Dependent Variable: Public Goods Expenditure from Villagers (10,000 Constant RMB) | | | | | | | | | |
| Post 1st Election | 16.604 (8.320) | 14.615 (6.969) | 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 | 4340 | 4018 | 4280 | 4,280 | 4340 | 3500 | 4,340 |
| R-squared | 0.103 | 0.105 | 0.221 | 0.118 | 0.122 | 0.132 | 0.114 | 0.104 | 0.103 |
| Panel C. Dependent Variable: Village Land Leased to Enterprises (mu) | | | | | | | | | |
| Post 1st Election | -61.563 (38.755) | -59.336 (39.091) | -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 | 1957 | 1842 | 1957 | 1,957 | 1957 | 1672 | 1,957 |
| R-squared | 0.559 | 0.559 | 0.746 | 0.593 | 0.670 | 0.564 | 0.580 | 0.614 | 0.559 |
| Panel D. One Child Policy Exemptions | | | | | | | | | |
| Post 1st Election | 0.097 (0.042) | 0.098 (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 | 4,340 | 4,018 | 4,280 | 4,280 | 4,340 | 3,500 | 4,340 |
| R-squared | 0.792 | 0.792 | 0.812 | 0.797 | 0.797 | 0.795 | 0.793 | 0.790 | 0.792 |
| Panel E. Dummy for Expropriation of Village Land | | | | | | | | | |
| Post 1st Election | -0.013 (0.008) | -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 | 4,340 | 4,018 | 4,280 | 4,280 | 4,340 | 3,500 | 4,340 |
| R-squared | 0.094 | 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-bootstraps clustered at the province level.

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

| | Dependent Variables: Indicator for Incumbent Reelection | | | | | |
|---------------------------------|---|--|------------------------------------|--|--|---|
| | 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) |
| Dep. Var. Mean | 0.479 | 0.477 | 0.466 | 0.466 | 0.480 | 0.479 |
| Ind. Var. (Policy*) Mean | 8.124 | 122.8 | 0.541 | 0.0162 | 0.138 | 3.340 |
| 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 ² | 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: Exposure to Elections and Trust in Government Cadres

| | Dependent Variable: Self-Reported Trust (Higher Value is More Trust) | | | | |
|------------------------------------|--|--------------------------|--------------------------|--------------------------|------------------------|
| | Government | | | | |
| | Cadres [0, 10] (1) | Most People [0,1] (2) | Neighbors [0, 10] (3) | Strangers [0, 10] (4) | Doctors [0, 10] (5) |
| Dep. Var. Mean | 5.223 | 0.524 | 6.578 | 1.845 | 6.830 |
| Age First Election in County 18-25 | 0.610** (0.302) | -0.036 (0.071) | 0.083 (0.331) | 0.288 (0.305) | 0.362 (0.289) |
| <i>Standardized Coef.</i> | <i>0.1025</i> | <i>-0.0278</i> | <i>0.0139</i> | <i>0.0542</i> | <i>0.0632</i> |
| Age First Election in County <18 | 0.887* (0.460) | 0.029 (0.105) | 0.050 (0.486) | 0.138 (0.392) | 0.439 (0.441) |
| <i>Standardized Coef.</i> | <i>0.1629</i> | <i>0.0247</i> | <i>0.0093</i> | <i>0.0284</i> | <i>0.0839</i> |
| Observations | 1216 | 1206 | 1214 | 1213 | 1212 |
| R-squared | 0.039 | 0.038 | 0.046 | 0.106 | 0.056 |

Notes: Observations are at the individual level. All regressions control for gender, level of education, marital status, log income, birth year and its squared term and county fixed effects. Huber-White robust standard errors shown in parentheses. The number of observations varies due to missing values in the outcome variables for some villages. Source: *China Family Panel Survey*.

Table 8: The Loss of VC Autonomy

| | Dependent Variables: 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) | Account Oversight Logit (4) | Cadre in Residence Logit (5) | Student Monitor Logit (6) |
| <i>Dep Var Mean</i> | 0.638 | 2.575 | 0.489 | 0.752 | 0.561 | 0.488 |
| Gov | -0.182 (0.076) | 0.203 (0.097) | -0.070 (0.052) | 2.541 (1.528) | 0.923 (0.696) | 0.701 (0.852) |
| Gov x Ln Dist | 0.059 (0.025) | -0.091 (0.031) | 0.029 (0.020) | -0.973 (0.406) | -0.426 (0.197) | -0.227 (0.272) |
| Observations | 1,616 | 1,616 | 1,616 | 564 | 636 | 504 |
| R-squared | 0.594 | 0.646 | 0.634 | | | |
| Sample period | 2002 - 2017 | 2002 - 2017 | 2002 - 2017 | 2006 - 2017 | 2006 - 2017 | 2006 - 2017 |

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. See Table 2 for a description of the dependent variables in columns (1)-(3). The regressions in columns (1)-(3) control for province-year trends, village and year fixed effects. The Logit estimates in columns (4) - (6) control for village and year fixed effects. The standard errors are clustered at the village level.

Table 9: County Official Career Concerns

| | Dependent Variable | | | | | | | | |
|-------------------------|----------------------|-----------------------|-----------------------|------------------------|------------------------|------------------------|---------------------|-----------------------|---------------------|
| | Sum VC Unilateral | | | Sum VC PS Joint | | | Sum PS Unilateral | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | Years of | | | Years of | | | Years of | | |
| | Baseline | Age | I (Age > 40) | Baseline | Age | I (Age > 40) | Baseline | Age | I (Age > 40) |
| Gov x Ln Dist | 0.0586** (0.0252) | 0.0688*** (0.0260) | 0.0655*** (0.0233) | -0.0900*** (0.0308) | -0.0929*** (0.0308) | -0.0870*** (0.0302) | 0.0295 (0.0202) | 0.0299 (0.0189) | 0.0264 (0.0189) |
| Gov | -0.179** (0.0759) | -0.211*** (0.0795) | -0.198*** (0.0710) | 0.199** (0.0969) | 0.212** (0.0968) | 0.191** (0.0946) | -0.0708 (0.0524) | -0.0710 (0.0481) | -0.0626 (0.0486) |
| County PS Age | | 0.0868** (0.0392) | 1.266* (0.657) | | -0.0519 (0.0480) | -0.0944 (0.709) | | -0.00828 (0.0273) | -0.422 (0.621) |
| County PS Age x Ln Dist | | -0.0265* (0.0134) | -0.308 (0.211) | | 0.0110 (0.0160) | -0.0694 (0.213) | | 0.000343 (0.00877) | 0.122 (0.187) |
| Observations | 1,609 | 1,609 | 1,609 | 1,609 | 1,609 | 1,609 | 1,609 | 1,609 | 1,609 |
| R-squared | 0.594 | 0.602 | 0.605 | 0.645 | 0.648 | 0.647 | 0.634 | 0.634 | 0.635 |

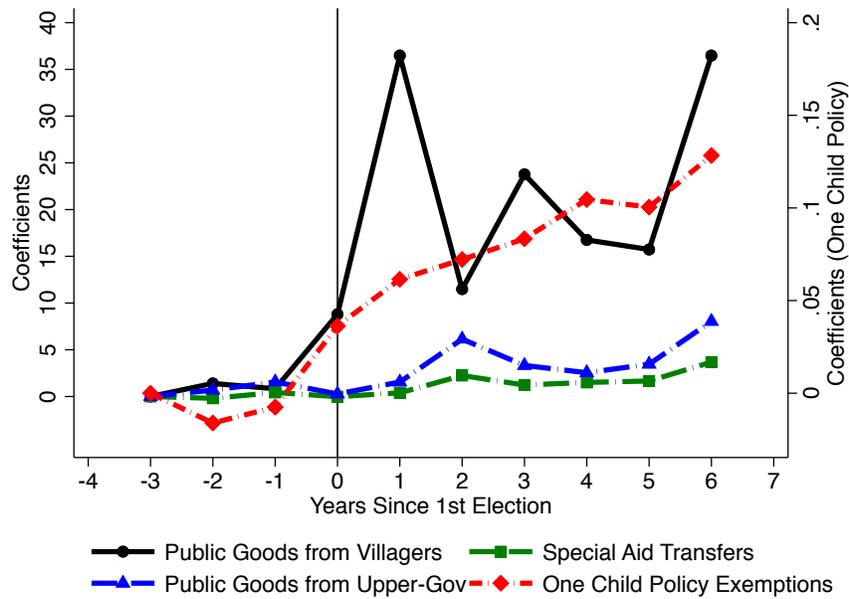
Notes: Observations are at the village and year level. "Gov" is county government revenues divided by county GDP. "Ln Dist" is the log distance to the county seat. County PS Age is the age of the county Party Secretary when on entering office, measured in years or as a dummy variable that equals one if it is greater than forty (see column headings). All specifications control for province-year trends, and village and year fixed effects. The standard errors are clustered at the village level. Source: See Data Appendix.

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.