Successful Social Programs over Local Political Cycles*

Yao PAN[a] Jing YOU[b]

[a] *Aalto University & GWU*

[b] *Renmin University of China*

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Abstract: We identify the effect of the relative timing of program introduction to local elections on service delivery. Exploring randomized provision of a credit program in China and variations in local political cycles, we find villages introducing the program before elections experience higher take-up rates, better targeting of the poor, and improved welfare, all of which are achieved without compromising the program's financial sustainability. Examining implementation phase-by-phase shows better-designed program practices and greater efforts made by local politicians are plausible contributors to enhanced program impacts. These findings are consistent with incentives to implement well rather than buying votes under election pressure.

Keywords: Microfinance; Political Cycle; Heterogeneous Impact; Randomized Controlled Trial

JEL Classification: D14; D72; G21; I38; O13; O16; Q14

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1. INTRODUCTION

A social program may achieve great success in one case but not in another, and variation in a program's service delivery can also be substantial across different contexts.[1] To better design policy interventions and make projections for program scale-ups, it is crucial to understand the factors influencing program effectiveness and study where and how a social development program will work best (Buera, Kaboski & Shin, 2016; Hanna & Karlan, 2016). Most of the existing studies have focused on incentivizing client households, generally in the form of modifying project design details such as pricing, targeting, and ways to deliver service. For example, requiring individual or joint liability in microfinance (Attanasio et al., 2015), varying the level of subsidy in bednet provision (Cohen & Dupas, 2010), providing cash, vouchers or food in food assistance programs (Hidrobo et al., 2014), etc. Some recent studies have emphasized incentivizing hired agents such as field workers and teachers instead, by linking compensation with their performance (Glewwe, Ilias & Kremer, 2010; Muralidharan & Sundararaman, 2011; Duflo, Hanna & Ryan, 2012; Ashraf, Bandiera & Jack, 2014; Cai et al., 2015) and by changing the level of monitoring (Nagavarapu & Sekhri, 2016). While long being believed to play a crucial role in program delivery, the incentives for program designers, who decide on the manner in which the program is implemented (e.g. program terms, how to manage and monitor the program, what resources to put into the program, etc.), are seldom studied empirically due to identification and data challenges.[2]

Using a unique government-implemented village fund program in China, this paper is the first to analyze the effect of the relative timing of a program's introduction to local (village) election cycles on a program's performance and to identify the underlying motivations of incumbent politicians.[3] On the one hand, the launch of the program provides opportunities for village leaders to engage in undesirable policy distortions for re-election purposes such as offering loans in exchanges for votes, targeting swing voters and over-lending to risky borrowers (hereafter referred to generally as "vote-buying" behaviors), leading to over

[1]For instance, an education-based conditional cash transfer program can raise secondary school attendance rates by as high as 30 percentage points or have no detectable effect at all (García & Saavedra, 2017). Similar heterogeneity in program performance has also been found for other social interventions such as health information campaigns (Dupas & Miguel, 2016) and microfinance programs (Banerjee, Karlan & Zinman, 2015; Karlan et al., 2016; Buera, Kaboski & Shin, 2016).

[2]Admittedly, household attributes (e.g. education, wealth, risk attitude, etc.) and local conditions (e.g. culture, ethnic fragmentation, infrastructure, etc.) are also important factors to account for program performance differences. However, these are often difficult to modify and are not the focus of this study.

[3]Unlike other Chinese government officials who are often appointed by government one level higher, villagers' opinion has been largely accounted for in village cadre selection, with anonymous voting becoming an integral part of the selection process. This feature and the resulting career incentive right before election at least partially resembles democratic governments

and/or inefficient use of the funds.[4] On the other hand, incumbent village leaders may also have incentives to better design the program to suit local needs and make greater efforts in program implementation if they want to demonstrate their competence in the current position prior to elections (hereafter referred to as "implementing-well" behaviors). Disentangling these two effects has been a great challenge for empirical economists as politicians' actions are rarely observed.

The village fund program provides an ideal setting for studying variations in service delivery and their underlying mechanisms. As the central government only provided general guidelines without specific rules, local village councils had great autonomy regarding program design and implementation. Using funds allocated by the central government and contributed by households in the form of the participation fee (which enables them to borrow later), this program provided production loans to poor rural households to improve their access to credit. In practice, the program was implemented by the village council under the lead of village Party secretary, the most important village figure under the current Chinese political system.[5] Unlike most decentralized programs studied in the literature in which local governments are only responsible for delivering a "fixed" service designated by the central government,[6] in the actual implementation of the village fund program, villages are also responsible for service design, such as the composition of management teams, eligibility criteria, and loan terms, based on local conditions. As a result, there were tremendous variations in program practice across villages, leading to a setting that is analogous to implementing a credit program separately in different local contexts.

Our study design and data have several unique advantages in analyzing electoral manipulations and distinguishing political incentives. First, we rely on two-year panel data from a randomized controlled trial (RCT) to causally evaluate the average borrowing performance of the village fund program in all treatment villages (benchmark). Our studied sample consists of 1351 households from fifty poor rural villages, among which thirty were randomly selected to roll out the village fund program in 2010. Second, the election cycle varies considerably across villages for village Party secretaries, who are elected jointly by villagers and Communist Party members for a tenure of three years. With a careful check of potential determinants of the political cycle and of household and village balances, we argue that the observed differences in election cycles are largely arbitrary. As the election cycle was predetermined in each village, the simultaneous launch of the village fund program

[4]Baland & Robinson (2007) and Khemani (2015), for instances, have shown the association between vote buying and under-provision of public services.

[5]The Organic Law of the Villagers' Committees of the People's Republic of China insists the "leadership core" role played by Party branches in village governance. According to O'Brien & Han (2009), village Party secretaries are usually considered the village "number one," i.e. the top power holder.

[6]Therefore, most studies on decentralized service delivery only focus on targeting outcomes.

in all treatment villages in 2010 created exogenous variation in the relative timing of the program introduction to the local political cycle and enables us to examine whether such timing matters for successful service delivery. Third, detailed village and administrative data allow us to track, phase by phase, the manner in which the program is implemented in each village, the allocative efficiency and financial sustainability of the funds, and the investments in other simultaneously implemented projects, all of which are helpful in distinguishing between the implementing-well story and the vote-buying story.

Using household survey data, we find the program's impact on borrowing varies greatly across villages that launched the program at different points in time of their village Party secretary election cycles. Although on average treatment villages experienced higher likelihood of borrowing, households in villages that introduced the program one year before village Party secretary elections (hereafter referred to as "Before Villages") were 15.1 percentage points more likely to borrow from the fund and were 11.6 percentage points more likely to borrow from any source, compared with households in post-election treatment villages (hereafter referred to as "After Villages"). A simple decomposition exercise shows village Party election cycles can explain 8% of the total cross-village variation in program take-up rates.

In addition to borrowing outcomes, we also find better performance in "Before Villages" over a broader range of performance indicators using administrative data extracted from the village fund monitoring system. In particular, "Before Villages" achieved better targeting of the poor, a lower violation rate of the borrowing rules set locally, and a higher official performance score that ranges between zero and 100. We do not find any significant difference in terms of default rate and program profitability. These results are inconsistent with the vote-buying story in which credit opportunities are offered to potentially riskier borrowers and the program's financial sustainability is compromised.

A closer examination of the implementation phase by phase shows better program practices adopted and greater efforts made by the management team in "Before Villages." Interestingly, the initial program participation in the period immediately after information campaigns, which required financial contribution to the village fund in the form of a participation fee, did not differ between "Before" and "After villages," indicating equally high demand for the village fund loan on the first impression. This result is inconsistent with the argument that the better program performance was due to households' pre-electoral loan demand changes, indicating the importance of supply factors in achieving performance differences. For later implementation phases, while we find no difference in the demographic composition of fund management committees, the committees in "Before Villages" set lower interest rates and longer loan lengths. They also attended more management training sessions and were more likely to enforce group liability. Program participants were also more satisfied with the fund committee's service.

The better implementation of the village fund program in "Before Villages" did not come at the cost of underinvestment in other projects but appeared to be welfare-improving. In particular, households in "Before Villages" experienced higher levels of agricultural income, agricultural productive asset value, and food consumption, compared with households residing in "After Villages." It is worth noting that some of the above mentioned initial good practices in program implementation persisted over time, giving rise to possible long-term benefits. Taken together, these findings provide supporting evidence for the implementing-well story.

This paper contributes to the extensive literature on delivery of anti-poverty programs in general, and of credit interventions in particular. As noted by Banerjee, Karlan & Zinman (2015), credit programs generally have low take-up rates and often lack transformative impacts. While design elements and local implementation modalities are believed to cause considerable variation in program performance,[7] existing studies have only investigated a limited set of program details one at a time, including advertising content, interest rate, and loan maturity for program take-up (Karlan & Zinman, 2008; Bertrand et al., 2010; Karlan & Zinman, 2018), and joint-liability for food consumption (Attanasio et al., 2015). Our study is the first to show how political incentives generated by local election cycles affect multiple features of the loan product supplied that encourage take-up on the part of households. Moreover, we show that a program implemented by a motivated leader is more likely to deliver a transformative effect. All these improvements are achieved with the same budget and general guidelines, and without compromising financial sustainability. Taken together, our findings highlight the importance of motivating program designers in the successful delivery of development programs.

Our paper also makes a twofold contribution to the literature on the political cycle. First, we are the first to empirically distinguish between implementing-well and vote-buying incentives, and to show how pre-electoral policy manipulations can be welfare-enhancing. While both types of incentives can coexist, existing observations of pre-electoral manipulations are generally consistent with the vote-buying story. They lead to no welfare gains and sometimes can even be detrimental to development (Cole, 2009; Baskaran, Min & Uppal, 2015; Labonne, 2016). One possible explanation for this behavioral and welfare difference is that previous literature has focused on political cycles at the national or state levels, while

[7] Program practices are crucial for effective implementation of development programs in general. The meta analysis of García & Saavedra (2017), for example, have linked variation in program characteristics with heterogeneity in impact and cost-effectiveness for conditional cash transfer programs.

we study political cycles at the lowest administrative level (the village). Implementing-well incentives are likely to be dominant in local elections as local politicians' efforts as well as misconduct are more visible to villagers who live close by and, with only earmarked grants and no tax revenue, their ability to increase spending is limited. Second, unlike temporary and cyclical manipulations by incumbent politicians documented in previous studies, we show the possibility that the better implementation of a new program can persist over time. This difference is a likely result from the fact that our study focuses on the implementation of a new program, which includes the setting of all initial terms, rather than the manipulation of an existing policy instrument that is often easy to change and revert. In our case, some terms of the initial setting of the program may be "sticky" and costly to change after election, especially for changes unfavorable to farmers such as raising the interest and shortening the loan length.[8] The persistence in some program practices is important as it opens up the possibility for sustainable welfare impacts in post-election periods.

In addition, our research connects to a growing literature on the local political determinants of decentralized development program delivery.[9] To improve delivery efficiency, many developing countries have resorted to decentralized implementation of public projects without devolving of financing authority (Bardhan, 2002; Bardhan & Mookherjee, 2006*a*).[10] With the potential to curb local capture and enhance accountability, electoral incentives are widely believed to be important for the effective decentralized delivery of services (World Bank, 2004; Bardhan & Mookherjee, 2006*a*; Mansuri & Rao, 2012). Consistent with the role of political competition, in their pioneer work on decentralized land reform implementation in India, Bardhan & Mookherjee (2010) have documented that the likelihood of carrying out land reform is higher in villages with closer electoral contests and in election and pre-election years. De Janvry, Finan & Sadoulet (2012) have shown that a decentralized educational conditional cash transfer program in Brazil is more successful in reducing school dropout rates in municipalities with first-term mayors, who face re-election pressure, compared with those in their last (second) term. However, these works only focus on the delivery of the studied programs per se, but do not examine possible costs of these electoral maneuvers such as over and inefficient use of the program fund and underinvestment in other projects.[11] With rich household-level and administrative-level data, our research

[8]Karlan & Zinman (2008) and Karlan & Zinman (2018), for example, have shown that loan demand is more elastic for interest increases than interest decreases.

[9]This is part of a broad literature on the determinants of accountability and capture of local governments. Bardhan & Mookherjee (2006*b*) and Bardhan (2016) provide comprehensive reviews of this literature.

[10]These practices differ from those outlined in the traditional fiscal federalism literature with financial devolution.

[11]Camacho & Conover (2011) have documented more extensive manipulation in the implementation of the Census of the Poor, based on which the household eligibility for national social programs is determined, in municipalities with more competitive mayor elections. Such manipulation made non-poor households eligible

complements the literature by addressing these issues, which helps distinguish between performing-well and vote-buying incentives, and aids in evaluating comprehensive welfare implications.

The rest of this paper is structured as follows. In Section 2, we outline the village fund program and village Party selection procedure in China. Section 3 describes the experimental design, discusses attrition and treatment-control balance, compares key demographic and socio-economic indicators between villages with different election cycles, and describes the supplementary administrative data used in this study. The main estimation results on program performance are provided in Section 4. Section 5 checks the implementation heterogeneity in each program phase to explore the underlying mechanisms. Section 6 briefly discusses welfare effects, and section 7 concludes.

2. PROGRAM AND POLITICAL CONTEXTS

2.1 The program

While China has experienced dramatic economic growth in the last few decades, the growth is unbalanced across regions, resulting in persistent poverty in some rural areas. As stated in the government's 13th Five-Year Plan, China aims to eliminate extreme poverty by 2020. To achieve this goal, China's central government has launched a series of poverty-alleviation programs in targeted poor areas. The village fund program is one such intervention designed by the Chinese State Council Leading Group Office of Poverty Alleviation and Development (CPAD) to improve poor rural households' access to credit.

Lack of formal access to credit has been a chronic issue in rural China, especially for the rural poor. Prior to the introduction of the village fund program, Rural Credit Cooperatives (RCCs) were the main formal lending source for rural households. RCCs have been part of the national banking system since it was transformed from local branches of the People's Bank of China (i.e., the current central bank) in the 1950s. While the RCCs aim to support business and agricultural production, most of the funds are allocated to township and village enterprises (TVEs). According to The People's Bank of China (2012), household loans only accounted for 34.9% of the overall outstanding loan balance in 2009. Furthermore, given the goal of profit maximization set during the institutional reform in 2003 and the requirement of collateral in loan application, the RCCs have largely excluded the poor (Li,

for subsidies, leading to an increase of the National Health and Social Security budget.

Gan & Hu, 2011; He & Ong, 2014).[12]

In addition to the RCCs, the Chinese government also launched a subsidized loan program in 1986 as part of its main poverty alleviation strategies over the last thirty years. However, its focus is on developing rural enterprises and supporting local infrastructure investment. Therefore, the funds allocated to help individual households are limited. According to an official report by the Agriculture Department of China's Ministry of Finance, only 0.7% of households in targeted areas ever borrowed from the subsidized loan program in 2001, and this number was even lower for the poor (0.6%).[13] In addition to the limited scale, the household component of the program was unsuccessful as such loans were rarely paid back. The default rate ranged between 57.3% to 70.4% in 2002-2009.[14]

The village fund was designed to be a self-sustaining program to meet the loan demand of those excluded from the formal banking system. It has been the only government-implemented financial intervention targeting the poorest of the poor. Villages can apply to set up the fund if they are either on central or provincial governments' official "poor villages" list or out of the list but with a net annual income per capita that is lower than the county mean. Upon approval, the central government invests 150 thousand Yuan to each village fund with its fiscal budget on poverty alleviation.[15] This program's budget size is considerable compared to past poverty alleviation projects implemented by the government in general (including irrigation, road construction, safe drinking water, etc.), and is unprecedented for loan interventions in particular. In 2009 (one year prior to the introduction of the village fund program in our study), 50.7% of all poor villages participated in at least one government poverty-alleviation project. Among participating villages, the average fiscal budget for development programs was 334 thousand Yuan, of which only 38 thousand Yuan was allocated to subsidized loans.[16]

In addition to government funding, households also need to contribute to the fund in the form of a participation fee, which is fully refundable when they quit. Participation is on a voluntary basis for both poor and non-poor villagers, and a typical entry fee is 200 Yuan.[17] It enables households to submit loan applications but does not necessarily lead to actual

[12]Based on household data collected in Hubei province, Li, Gan & Hu (2011) show that the failure to meet the income requirement is the most important self-reported reason for both not applying for RCCs loans and loan application rejections.

[13]Available in Chinese at http://nys.mof.gov.cn/zhengfuxinxi/bgtDiaoCheYanJiu_1_1_1_1_2/200806/t20080619_47086.html. Accessed on January 14, 2020.

[14]Source: China Rural Poverty Monitoring Report (2010), CPAD.

[15]The village fund program in China shares several common features with the Thailand's Million Baht Village Fund program studied by Kaboski & Townsend (2012), including the fixed amount of fund for each village.

[16]Source: China Rural Poverty Monitoring Report (2010), CPAD.

[17]This fee can be waived for poor households.

borrowing. All participants in a village vote to elect the village fund committee, which then sets specific management details including loan terms and default penalty.[18] According to the central government's practice guideline, the village fund lends to individuals within small groups consisting of five to seven program participants on a rotating basis. Group members are responsible for each other's debts. There is no collateral requirement. Loans are often made discrete and small (1,000-5,000 Yuan each),[19] with poor and female participants endowed with priority to loan allocation. The guideline also asserts that loans can only be used for income-generating activities, with a strong emphasis on agricultural production. However, these suggestions are general and vague, leaving ample room for local adjustments. According to the guidelines provided in the Decree No.103 [2009] of the CPAD, villages were required to formulate their own implementation plan according to local conditions in order to encourage take-up and to ensure program sustainability. As a result, the actual practice varied considerably across villages.

Following the initial trial conducted by the World Bank in Sichuan and Henan provinces, the village fund program has spread all over China. By the end of 2009, the Fund had reached the size of 170 million Yuan. 740,000 households (of which 370,000 were poor households measured by their income against the national poverty line) in 9,003 villages from 940 counties had participated in the Fund.[20] To understand how the program was operated locally and to assess the impact of the village fund, the CPAD supported (with extensive cooperation with local governments) an RCT of this fund in 2010 in villages where the fund had not been introduced. Eventually, the CPAD aims to expand the program to all of their listed poor villages as a main policy intervention.

2.2 Election and motivation of village Party secretaries

The village fund program was implemented by the village government under the lead of the village Party secretary, the most important village official under the current Chinese political system (Unger, 2002; Bislev & Thøgersen, 2012). They are state agents entitled

[18] The village fund committee consists of a management board and a supervisory board. The management board is led by a managing director and consists of three to five members including an accountant and a cashier. They are responsible for the approval, distribution, and recovery of loans under the monitoring of the supervisory board that is led by the supervisory director and consists three persons. While both the managing and supervisory directors are core members of the committee, the former has larger influence on the implementation process and is often considered the head of the village fund committee.

[19] The size of the loan is considerable compared with the production scale of poor Chinese households. According to China Rural Poverty Monitoring Report (2010), the average household production expenditure for the poor was 4,401 Yuan in 2009.

[20] The the coverage rate of this fund among all government-listed poor villages was 6% in 2009. Later, the program coverage rate reached 15% in 2013.

to salaries in practice, but not official civil servants on the regular state payroll.[21] Unlike full time officials in higher levels of governments who are often career bureaucrats, village Party secretaries are usually part-time cadres and part-time producers, who are local residents actively engaged in farming activities (White, 1992; Zhang, 2018).[22] Traditionally, they were appointed by the government one level above (township) for a tenure of three years. At the end of their term, reselection decisions are made based on their performance (Whiting, 2006; Edin, 2003).[23] Common performance indicators include industrial performance, agricultural output, Party building activities, education, and family planning (Whiting, 2006). Promotions to higher level government positions are rare in China.[24]

Over the last 15 years, China has adopted a new system (i.e. "two recommendations and one election") to augment villages' input in the selection of Party secretaries. This system was first introduced by some pioneer regions in the early 2000s (Chen, 2014*b*).[25] In general, this procedure has three steps. First, a meeting of Party members proposes a tentative list of candidates via anonymous nomination. Each village then organizes anonymous voting, in which all adult residents are eligible to vote. Next, those who win at least 50 percent of the votes are included in an official list of candidates to be elected by all party members (Chen, 2014*b*).[26] This new selection procedure was promoted for nationwide implementation during the 17th National Congress of the Communist Party of China in 2007. Under the new system, villagers' evaluations are also important for village Party secretaries' reselection. While there are no term limits, the turnover rate for village Party secretaries is relatively high.[27]

[21]Civil servants are selected via annual national exams or provincial exams. Benefits involved of being a civil servant are substantial, including security of tenure, relatively high wage, improved welfare benefits, etc.

[22]This feature is not unique to China. Village cadres in many other countries, such as Thailand, Philippines, and Uganda, also serve on a part-time basis.

[23]The performance-based reappointment is common for higher levels of governments. Several recent studies have documented the important role that economic growth plays in promotion for provincial leaders, e.g. Li & Zhou (2005) and Jia (2017). In addition, the reappointment evaluation is likely based on average performance, rather than the contemporary year, a fact documented in provincial leaders' reappointment practice (Li & Zhou, 2005).

[24]As pointed it out by Chen (2014*b*), "it was stipulated that Civil servants must not be selected directly from among incumbent village cadres." Even though promotion cases do exist occasionally, they do not follow a particular timeline but highly depends on availability of vacant positions.

[25]A better known and studied step for China towards democracy was the introduction of elections for village head during the village self-governance movement in 1989 (e.g. Zhang et al., 2004; Luo et al., 2007; Shen & Yao, 2008; Martinez-Bravo et al., 2017). The political system studied in this paper, i.e. "two recommendations and one election," applies to village Party secretaries.

[26]According to the Internal Statistical Report of the CPC, around 4% of all rural villagers were members of the Communist Party of China in 2012.

[27]According to the 2011 wave data from the nationally representative China Health and Retirement Longitudinal Study (CHARLS), village Party secretaries' turnover rates for the overall sample and for the restricted sample consisting only the five provinces covered by our RCT are 24.3% and 25.2%, respectively. These rates are relatively high compared with the turnover rates for U.S. House members, which is often less than 10%.

Benefits of being a village Party secretary include government subsidies and ego rents. While the level of subsidy is only moderate, it is often supplementary rather than being the main source of income.[28] In fact, most Party secretaries are farmers themselves while serving the village, and the additional source of income makes these positions attractive for villagers in general. Regardless of improved migration opportunities and rising wages that are often higher than subsidy incomes, village Party secretaries' positions are still considered attractive opportunities as they do not require separation of families and are well respected by villagers.

How does the local election cycle affect the implementation of the village fund program? Vote-buying behaviors may exist in the following two forms. First, implementing the program prior to election provides incumbents the opportunity to use loan offers in exchange for votes. Second, according to the political business cycle theories, incumbent politicians have an incentive to engage in opportunistic pre-electoral manipulation of economic policies in order to increase their chances of being re-elected (Drazen, 2000).[29] The earlier opportunistic business-cycle theory features irrational voters who give more importance to the recent past than they do to the distant past, which in turn leads to expansionary policies and temporary increases in economic activities prior to elections (Nordhaus, 1975). More recent rational expectation models drop this irrationality assumption but focus on information asymmetries about the incumbent leader's competence instead. As voters believe competent politicians are able to manipulate economic outcomes more than incompetent ones, competent politicians want to use electoral policy expansion as a signal to reveal their type (Rogoff & Sibert, 1988; Rogoff, 1990). In an alternative model with rational voters, even though incumbents have no information advantage regarding their competency (it is unknown to everyone instead), they still have incentives to use policy instruments to appear as competent as possible (Lohmann, 1998). Regardless of differences in model setups, these models all focus on policy distortions that we categorized generally as vote-buying behaviors.

Nevertheless, the traditional literature on the political cycle does not consider possible pre-election changes in incumbents' effort levels, which can also make the incumbents appear

[28] For example, the annual subsidy for village Party secretaries was around 10,200 Yuan in 2013 for Hubei province, which is 1.15 times of the provincial average net income per capita (CPC's Hubei Provincial Research Department, 2015). This compensation is much lower for ultra-poor areas in both absolute and relative terms. In Mizhi, an ultra-poor country in Shaanxi province, the annual subsidy ranged from 3,700 to 5,100 Yuan in 2010, which was lower than the average annual net income of 5,209 Yuan (Hu & Bai, 2011).

[29] This theoretical prediction has been empirically tested extensively. The literature includes evidence of increases in electricity service to election-holding constituencies in India (Baskaran, Min & Uppal, 2015) and pre-electoral shifts in government spending towards investment in Columbia (Drazen & Eslava, 2010). In terms of economic performance, Labonne (2016) have documented increases in employment levels in the two pre-electoral quarters in Philippine.

competent. Admittedly, the implementing-well incentives are likely to be limited or even absent in national- or state-level elections given that effort levels are hardly observed by voters, and program visibility is a key determinant of incumbents' pre-electoral expansion decisions.[30] However, the implementing-well incentives are arguably greater at the local level, where politicians' efforts are more visible through daily interactions with villagers. Due to the coexistence of both types of incentives, whether a village fund program introduced prior to election achieves better performance is left for empirical investigation.

3. EXPERIMENT, DATA, AND IDENTIFICATION ISSUES

We begin this section by outlining the experimental design, discussing attrition and treatment-control balance, and describing the supplementary administrative data used in this study. We then go on to show the balance of key demographic and socio-economic indicators across village Party election cycles.

3.1 Experimental design and data

We collaborated with the CPAD to sample and implement an experiment to evaluate the effectiveness of the village fund. Under the consideration of geographic balance, the survey team selected 50 ultra-poor villages among areas where it was planned to implement the village fund program. Sample villages covered Shandong, Henan, Hunan, Gansu, and Sichuan provinces. The sample covers five out of the eleven ultra-poor cluster areas designated by the State Council. Among these villages, 30 were randomly selected to implement the program, while keeping the other 20 villages for control. The general guideline provided by the State Council requires access to village fund services being restricted to farmers permanently residing in the program village only, limiting any program spillovers from treatment to control villages. With the help of local village councils, we made a list of all households in each village in descending order according to their relative economic status in the village. We then used systematic sampling to randomly select 30 households for interview in each village. The baseline survey conducted in August 2010 successfully interviewed 1500 households.

Immediately upon finishing the baseline survey, the State Council transferred funds to all treatment villages. Figure A.1 shows a typical timeline of implementation of the village

[30]Consistent with the prediction of Rogoff (1990)'s model, several empirical studies have documented pre-election expansion in spending on highly visible areas such as transfers and infrastructure investment (Kneebone & McKenzie, 2001; Gonzalez, 2002; Akhmedov & Zhuravskaya, 2004).

fund program as suggested by the State Council's general guideline. This timeline is not unique to our experiment and is applicable to village fund programs implemented elsewhere. We generally divide program activities into four phases. In the preparation phase, the village council organizes advertising campaign meetings to publicize the program. After the campaign meetings, households can choose whether to participate or not. Next, all participants elect fund management committee members through anonymous voting. The elected fund committee members attend management trainings (conducted by the research team), which cover practice guidelines, accounting practices and usage of the official online reporting system, and then set specific loan terms based on their local conditions. In the last phase, participants form borrowing groups on a voluntary basis and decide the order in which to borrow. Potential borrowers then submit individual loan applications, which will be reviewed and either approved or rejected by the fund committee. The survey team went back in July 2012 to conduct a follow-up survey, while the program was still in place.

The attrition rate is relatively low. 1351 out of 1500 (90.1%) baseline households were repeatedly interviewed in July 2012. To show attrition is not likely to bias our results reported below, we first regress an attrition dummy on the treatment dummy allowing errors to be clustered at the village level and find no difference in the likelihood of attrition between treatment and control villages. In addition, we show attrition households are similar to the panel households in terms of key outcome indicators in borrowing, income, and welfare. We report detailed attrition analysis results in Appendix Table A.1.

To check the balance of baseline household characteristics, we regress each of these characteristics on a treatment dummy. As reported in Appendix Table A.2, we do not find statistically significant differences between treatment and control households in any demographic and socioeconomic indicators. However, given the limited number of villages included in our study, we are only able to detect treatment-control differences that are large enough using the method outlined above (i.e. limited statistical power). Some statistically insignificant differences may be economically significant. For example, treatment villages tend to have higher agricultural income and a lower value of productive assets. To account for these economically significant baseline differences, we control for baseline values of outcomes in relevant post-treatment regression analysis when applicable. In terms of baseline borrowing behavior, Table A.2 shows a high overall borrowing rate: more than half of households in our sample reported taking at least one loan since 2009. However most of these loans were informal without interest, mostly likely to come from friends and relatives. This fact highlights the lack of formal borrowing opportunities for poor households in rural China.

In addition to survey data, our analysis also uses administrative village-level program indicators for treated villages. These indicators were extracted in June 2012 from the village fund monitoring system managed by CPAD. One concern about using administrative data is that village politicians have incentives to overreport program performance indicators and such incentives would be stronger for village Party secretaries in the year before election. However, the overreporting issue is likely to be mild in our administrative data given intensive monitoring by higher levels of government. While we cannot fully rule out the possibility of overreporting, the administrative data provide important supplementary performance evidence on aspects that are not feasible to study using survey data, such as targeting the poor, violations of rules set by village fund committees, default behaviors, financial sustainability of the program, etc.

Although the elected fund committee was in charge of the actual delivery, village Party secretaries still had significant influence power in the way the program was implemented. In fact, they served as the head of the fund committee in 24 out of 30 treatment villages, and as the supervisory director (the second most important figure in the fund committee) in an additional two villages.[31] Many village government officials also played important roles in the village fund committee, which was formed via anonymous voting. On average, government officials represented 73% of core members of fund committees, including managing directors (head of village fund) and supervisory directors. The overlap in members between village government and the village fund committee was not necessarily a result of manipulation in voting results of the latter, as village government officials were among the most respected and educated in the villages. This overlap ensures the Party secretaries' influence over the implementation of the fund even when they themselves were not on the committee.

3.2 Balance across village Party secretary selection cycles

While the village fund program was simultaneously launched in all treatment villages, the local election cycle varied across villages. As the year of election is pre-determined in each village at the time of the introduction of the program, it creates exogenous variation in the relative timing of the introduction of the village fund program to the election cycle and enables us to examine whether such timing matters for successful implementation of the program. Even though the election cycle is unlikely to be changed by the implementation of the program, we cannot fully rule out this possibility. To avoid the potential problem of endogenous choice of the election year, we use planned elections to define the relative

[31] At least one fund committee member was government official in the remaining four villages.

timing of the introduction of the program to local political cycles.[32,33]

Our ability to attribute program performance variations to the local election cycle and its associated political incentives relies on the crucial assumption that no local factors affect political cycle and program delivery simultaneously. Otherwise, performance differences should be attributed to these local factors rather than political cycles. To validate this assumption, we go back in time to examine potential determinants of local political cycles. According to historical versions of The Constitution of the Communist Party of China, village Party secretaries served for one-year terms until 1973, when their terms were extended to two years. The official announcement made by the General Office of the Communist Party of China on July 6, 1993 further extended their terms to three years.Thus, the village Party secretary re-selection cycle potentially depends on the following factors: (1) the year of establishment of the village Party committee, (2) how fast each village followed the term extension policies, and (3) the existence of unexpected shocks that delay or advance the re-selection.

First, variations in the establishment year of existing village Party committees can be broken down into two parts, the initial introduction of Party branches following the founding of the People's Republic of China and the formation of new branches later as a result of village merging. The former had little influence on the election cycle studied here, as village Party committees were first established during the period with yearly Party secretaries' re-selection.[34] The latter, however, may play a role in explaining variations in village election cycles. To achieve economies of scale, rural China experienced several rounds of village-merging activities from the 1990s. Small villages were either merged together based on their proximity, or merged with larger villages nearby.[35] As a result, the total number of administrative villages in China declined from 804,153 in 1991 to 594,658 in 2010. The single most important criteria in merging decisions is the village population, with no other rules explicitly specified by the central government.[36] Although village merger can affect

[32]We calculate the planned election year using total years of tenure of the incumbent village Party secretaries by the year of the baseline survey.

[33]Unfortunately, our data do not have information regarding the timing of the actual election. Thus, we cannot use planned year as an instrument for the actual election year as in many other political cycle studies.

[34]According to the news of the Communist Party of China website, following the establishment of PRC in 1949, the coverage of village Party branch expanded drastically, reaching 99.9% by 1957. http://cpc.people.com.cn/GB/164113/10112555.html. Accessed on January 14, 2020.

[35]An alternative merging method available for remote and isolated villages is to relocate the entire village to merge with another village in more developed areas. Our sample villages did not experience such relocations as they were still ultra-poor and located in remote areas during the study period.

[36]We confirm the importance of population size and irrelevance of other factors in the comparison of characteristics between villages established after 1990 and those established earlier using the CHARLS data. Among various demographic and socioeconomic factors, we only find difference in the total number of households (and population), with later established villages being significantly larger (Appendix Table A.3).

the exact years of election, there is no reason to expect it to affect local political cycles in a systematic way. While the data do not allow us to identify merged villages and directly test this conjecture, the fact that the number of households are balanced between "Before" and "After villages," documented later in this subsection, indicates that political cycles do not differ between merged and non-merged villages. Nevertheless, we directly control for the number of households as a proxy for merged villages in the empirical analysis.

Second, given the general passivity of rural villages in implementing reforms in China noted by Unger (2002), villages had little control over the timing of term adjustments. One factor potentially leading to variations in the timing of introducing term extensions is the bargaining between the central and provincial government.[37] However, the province that study vilalges belong to have no statistical power in explaining variations in election cycles across villages in our sample.[38] Therefore, the variation that occurs in village political cycles is unlikely to be a result of the potential bargaining between central and provincial governments.

Third, as noted in The Constitution of the Communist Party of China, elections can be advanced or delayed in special conditions. Anecdotal evidence collected by us points to two general conditions that warrant the adjustment of re-selection timing. One valid condition is the coincidence of re-selection with other duties demanding immediate attention, such as urgent tasks assigned by higher level governments[39] or natural disasters, both of which are out of control of the affected villages. The other condition is the lack of guarantee of a fair election. Kinship ties are important factors that could influence the election procedures and outcomes.[40] Majority rule can produce dominance of one clan or a struggle between several clans, both of which are potential causes of election delays. To test if the kinship structure affects local election cycle in these two ways, we regress the election year separately on the each of the following two measures of kinship distribution: the share of households belonging to the largest clan in a village, and whether the largest two kinship clans are close in size (difference<10% of households). According to results reported in Appendix Table A.4, kinship ties have little influence on local political cycles. The lack of impact of kinship ties on election timing is not surprising, as changes in election timing are in fact uncommon in China. According to additional information collected during our interviews

[37]Martinez-Bravo et al. (2017), for example, show substantial cross-province variations in introducing election for village head, another important village official. Within-province implementation was top-down and rapid.

[38]We regress election year on province fixed effects and find that the adjusted R-squared is 0 (Table A.4 Row 1). In addition, none of these province dummy variables are statistically significant.

[39]These tasks are unlikely to be village-specific. For example, a village in Hebei province delayed the re-selection in June 2018 as it coincided with the re-evaluation of ultra-poor county status in Hebei.

[40]The role of kinship ties in elections has been discussed by O'Brien & Han (2009) and Martinez-Bravo et al. (2017).

with Party secretaries of twenty-three ultra-poor villages designated by the State Council in Guizhou and Sichuan provinces, none of these villages ever experienced advanced or delayed elections from 2000 to 2019.

Even though we are not able to pin down the exact causes of variations in the election cycle across villages due to data constraints, we argue that the observed differences in the election cycle at the baseline survey were largely arbitrary. We provide two pieces of supporting evidence for this hypothesis. First of all, the number of villages that implemented the program one year before the election year, during the election year and one year after are 11, 8 and 11, a roughly one-third split. More importantly, we show villages with different election cycles are not systematically different in terms of demographic composition and socioeconomic conditions. In particular, we test the household balance between villages that implemented the program one year before or during the election year, and villages that implemented it one year after. We report these test results in Appendix Table A.5. Households are statistically balanced in terms of these key characteristics in general, except for a higher fraction of male-headed households and a higher formal borrowing rate in "Before Villages" than in "After Villages."[41] As the total number of villages is even smaller when restricting to treatment villages, the previous concern of large minimal detectable size also applies here. To account for both observed and statistically undetectable baseline differences, we include baseline values of an outcome in our regression analysis of heterogenous program impact using 2012 data when applicable.

4. PROGRAM DELIVERY

In this section, we first show the impact of introducing the program before village Party secretary elections on the household program take-up rate and overall borrowing. We then proceed to examine if local political cycles affect other program performance indicators using the village-level administrative data.

4.1 Program take-up and overall borrowing

To better evaluate the relative importance of the local political cycle in successful service delivery, we first examine the standard average impact of the village fund program and use

[41] Note that the number of households did not differ between "Before" and "After villages." Therefore, these two types of villages received the same intensity of the credit treatment, calculated as the total fund transfer amount, which was the same for every village regardless of its size, divided by the number of households in a village.

the results as a benchmark. Specifically, we estimate the average intent-to-treat parameters of the following equation:

$$Y_{ij} = \alpha + \beta T_j + X_j + Z_{ij} + Y_{ij}^{baseline} + e_{ij}, \tag{1}$$

where Y is a borrowing outcome for individual i residing in village j and T is a binary variable that is 1 if respondent i lives in a village with access to the village fund. We cluster standard errors at the village level j, the unit of randomization. We include baseline village and household characteristics, X and Z respectively, to increase the precision of our estimates. We also control for baseline values of Y when applicable. We focus on delivery indicators of the actual provision of loans rather than the program's ultimate welfare effects as the latter often depend on other conditions outside the governments' control. We will return to the discussion of welfare implications in Section 6. As we examine the impact of the timing of program introduction relative to local political cycles on various borrowing and welfare indicators, we make the following adjustments for multiple hypothesis testing. For each table that reports a "family" of indicators (e.g. borrowing), we construct an index as the average of the z-scores of each indicator within the table. In addition, for each of these indices, we report the standard p-value as well as the adjusted p-value for multiple hypothesis testing across the indices based on the stepdown procedure proposed by Romano & Wolf (2005).

In addition to common village characteristics, the set of village controls also includes pre-intervention access to credit measured by whether the village is a "credit village," and the share of households having been rated as "credit households." The title "credit village" is rated by township government according to village management/performance of formal loan programs, including RCCs and bank loans. A "credit village" receives more formal loan opportunities with higher loan caps than elsewhere. "Credit households" are rated by village governments based on their financial status and past repayment history. They usually enjoy higher chances of being approved for a loan and/or a higher cap of formal loans than non-credit households. Our household controls include household size and the age, gender, ethnicity, and literacy of the household head.

As shown in Table A.6, the village fund program had a substantial positive impact on borrowing outcomes. Households in treatment villages were 24.3 percentage points more likely to take loans from the village fund. Note that 0.7% of households reported borrowing from the program in control villages, suggesting either very limited contamination in control villages (possible enrollment via relatives or friends in treatment villages) or the presence of measurement errors (misreporting). While the village fund program crowded out the demand for informal loans, it still increased the overall households' borrowing rate.

As shown in Column 2, households in treatment villages were 7.8 percentage points more likely to take any loan regardless of loan sources. This effect amounted to a 16.4% increase in the likelihood of borrowing, given the average borrowing rate of 47.5% in control villages.

Having shown the program's average impact in the benchmark case, we now turn to the question of whether such impacts vary with the timing of program introduction relative to village Party secretary election cycles. For the purpose of the analysis, we restrict our sample to treatment villages and incorporate political cycle variables into the regression. We choose this specification in order to be consistent with the regressions used to show the difference in program implementation later in this paper.[42] In particular, we estimate the following equation:

$$Y_{ij} = \alpha + \gamma Before_j + \delta During_j + X_j + Z_{ij} + e_{ij}, \tag{2}$$

where $Before$ is a binary variable that equals 1 if village j launched the village fund program one year before the village Party secretary election; $During$ is a binary variable that is 1 if village j launched the program in the year of village Party secretary election (hereafter referred to as "During Villages"); and all other notations are the same in Eq. (1). The coefficient of interest, γ, captures the additional program impact in "Before Villages" compared with the default group, i.e. "After Villages." Standard errors are clustered at the village level.[43] In order to address the multiple hypothesis testing issue, we again construct a borrowing index and report both its standard p-value and p-value adjusted in the same manner as described for the benchmark case.

We do not focus on the During-After comparison because our data do not allow us to gauge which event happened first in During Villages, the introduction of the program or the election of the Party secretary, even though both took place in the same year. Thus, the magnitude of δ would be difficult to interpret. In addition, even for villages implementing the program right before the election but still within the same year, it took time to form village fund committees, and for committee members to acquire necessary training and to pin

[42]Unfortunately, an alternative specification that uses all 50 study villages and regress outcomes on a treatment dummy, political cycle dummies, and the interactions of these two is not feasible as many key performance indicators are only available for treatment villages.

[43]One potential concern with our choice of cluster-robust standard errors is that standard asymptotic tests can over-reject when the number of clusters is not large enough. Nonetheless, according to the simulation results reported by Cameron, Gelbach & Miller (2008), cluster-robust standard errors performs reasonably well with 30 clusters. In Appendix Table A.7, we also report results for all within-treatment household regressions (i.e. with 30 village clusters) using a wild cluster bootstrap-t procedure as recommended by Cameron, Gelbach & Miller (2008) for few clusters. Our results are largely unchanged using this alternative bootstrap method.

down the way the fund would be run and managed locally. As a result, the first loan was unlikely to be given out before the election. There is little room for villagers to factor in the implementation of the village fund program in their voting decisions for village Party secretaries.

According to regression results reported in Table 1 Column 1, households in "Before Villages" were 15.1 percentage points, or a substantial 68.9%, more likely to borrow from the fund, compared with households in "After Villages" (take-up rate = 21.9%). An investigation of the reasons for not borrowing among participants indicates more loan applications, rather than a higher loan approval rate, likely contributes to the improved take-up rate in "Before Villages."[44] The improved take-up rate was achieved without crowding out borrowing from other sources (Column 3 and 4). As a result, the impact difference on overall borrowing from any sources was also dramatic: there was an additional 11.6 percentage points increase in the program's impact on overall borrowing in "Before Villages." Introducing the program prior to election also has a positive and statistically significant impact on the aggregated borrowing index.

Our results indicate that the timing of program introduction is one of the key determinants of the take-up rate of the village fund program. We perform a simple decomposition exercise to get an idea of how much of the variation in program take-up rate across villages can be explained by local political cycles and other local village conditions.[45] For each program village, we estimate the village-specific treatment effect for program take-up rate according to eq. 1 using households from a particular treatment village and from all twenty control villages. We then regress the estimated village-specific program take-up rate on the two political cycle dummies and baseline village characteristics used previously as controls. A high R-squared of 0.49 shows these variables together can explain almost half of the overall variation in program take-up rate across all program villages. Next, we perform a Shapley and Owen decomposition of R-squared to further break down the share of explained variance into contributions of each characteristic. The two political cycle variables account for 16% of all explained variations, which amounts to 8% of the overall cross-village variations in program take-up rate. Even though baseline village characteristics, including measures of village size and pre-intervention development level, explain a remarkable 41% of cross-village variations in program take-up, they are almost impossible to change in the short term. Unlike these pre-determined "background" variables, the timing of program introduction and the associated political incentives are relatively easy to adjust by the central

[44] A lack of need for the village loan was the most common reason for program participating households not to borrow from the village fund (72.22%). Only 0.46% of households reported the reason as application being rejected by the loan committee, indicating a high approval rate overall.

[45] We thank Esther Duflo for suggesting this exercise.

government and can be used to potentially boost program take-up.

4.2 Other performance indicators

In addition to borrowing outcomes, the administrative data allow us to investigate possible differences in a broader range of program performance indicators between villages that introduced the fund program before and after their Party secretary elections. In particular, for each of the performance indicators discussed below, we estimate the following village-level regression:

$$Y_j = \alpha + \gamma Before_j + \delta During_j + X_j + e_j. \tag{3}$$

One important factor in evaluating the performance of such a poverty alleviation program is how well the program targets the poor. As targeting requires efforts, election pressure may help the program better meet the poor's agricultural loan needs. The official definition of "poor households" set by the National Bureau of Statistics of China in 2011 were "those with annual expenditure (or income for the ease of data collection) per capita of less than 2,300 Yuan." However, the local poverty line varies substantially across regions and some areas impose additional criteria to define "poor households."[46] While we are unable to identify poor households in our survey sample, the administrative data have information on the share of loan amount lent to poor households in each program village. As shown in Table 2 Column 1, introducing the program one year before the village Party secretary election increased the share borrowed by the poor by 31.3 percentage points, which amounts to a substantial 88% increase compared with implementing the program after the election.

In addition to targeting, we also have information on a few other indicators of program performance: rule violations, loan defaults, and fund profitability. There are three recorded types of violations: households borrow a new loan to repay old ones; the loan amount is larger than the cap set by the committee; and households take new loans before paying back previous ones. Violation rates in "Before Villages" were 29.2 percentage point lower, which amounted to a reduction of more than 100% compared with "After Villages" (Table 2 Column 2). Compared with the six microfinance studies summarized by Banerjee, Karlan & Zinman (2015), the default rate of the village fund program was quite low: there were only 12 cases of default out of more than 2,000 loans taken in the two years of implementation. While take-up rates were much higher in "Before Villages," they did not experience higher default rates than "After Villages." Similarly, we do not find differences in profitability, an

[46]For example, the 2011 poverty line was 4,600 Yuan in Zhejiang province and was 2,665 Yuan in Shaanxi province. Jiangjin county in Chongqing municipality imposed several additional criteria, including no house construction or purchase in the last three years and no children study abroad at the households' own expense.

indicator of the financial sustainability of the village fund program.[47]

In addition to the individual delivery indicators discussed above, we are able to obtain the official program performance score, ranging from zero to 100. The evaluation was based on a comprehensive set of performance dimensions, including program outreach and targeting (24 points), loan quality (25 points), management efficiency (33 points), and program financial sustainability (18 points).[48] The score was calculated automatically in the fund monitoring system using pre-determined weights and scoring criteria on these dimensions, and was directly visible by county-level government officials. The performance score was also sent to provincial and central governments for record-keeping purposes. As shown in Table 2 Column 5, "Before Villages" scored 11 points higher than "After Villages," a result that is consistent with our findings on separate performance indicators.

Does the better program delivery in "Before Villages" simply reflect vote-buying behaviors? Neither a higher take-up rate nor better targeting of the poor provides convincing evidence against this view. In order to secure votes, an incumbent Party secretary may overlend to ineligible riskier borrowers, leading to a higher rate of program take-up. Similarly, both theoretical and empirical research has shown that vote buying tends to target the poor (Brusco, Nazareno & Stokes, 2004; Stokes, 2005; Blaydes, 2006). As directly pointed out by Bardhan & Mookherjee (2012), buying votes from the poor with public services may provide an appearance of successful pro-poor targeting. Nevertheless, vote-buying behaviors, generally defined as distortive manipulations, are expected to result in inefficient use of the fund and / or come at the expense of other programs that are of a long-run nature. The lack of evidence for compromised fund use efficiency measured by default rates and profitability goes against the vote-buying story. To further pin down the underlying political incentives for the observed delivery differences, we turn to program implementation details as well as additional checks including the simultaneous implementation of other programs in the next section.

5. MECHANISMS

To investigate what drives the divergence in program performance, we focus on program villages and test potential differences in campaign efforts, characteristics of fund committee members, committee members' attendance at training sessions, village fund loan terms, and households' evaluation of the program. We will proceed with this analysis by imple-

[47] Profitability is measured as the total interest income minus management costs, if any, and then is divided by the total funding pool including government transfer and households' contribution in the form of participation fees.

[48] Source: The evaluation scheme for village fund program. Program internal memo.

mentation phases. Empirical specifications adopted are analogous to Eq. 3 for village-level outcomes and similar to Eq. 2 for household-level indicators. We then discuss possible alternative contributors to the performance differences, including households' behavioral changes prior to elections, buying votes from other Party members, and better / worse implementation of other simultaneous programs.

5.1 Implementation stages

In the program preparation stage, we check for attendance at campaign meetings and program participation. As shown by Bertrand et al. (2010), advertising strategies have the potential to greatly affect loan take-up. While we do not have detailed information about how campaign meetings were organized, we use attendance by villagers as a proxy for the level of effort that Party secretaries put into mobilizing the program. Overall, the program had reached a large number of households with an average meeting attendance rate of 71%. The high participation rate of 59% in program villages implied a high demand for credit in these villages. Compared with "After Villages," households in "Before Villages" were more likely to attend campaign meetings, suggesting the village council had exerted more effort into mobilizing and advertising the program. However, this did not translate into a statistically significant higher program participation rate as shown in Table 3 Column 4. The similar participation rates indicate that households in "Before" and "After Villages" had equal levels of demand for credit, and were both enthusiastic and interested in borrowing from the village funds when the program was first announced and advertised in 2010. Therefore, the timing of program introduction, i.e. whether the intervention was launched before or after village Party secretary elections, was likely to influence program delivery during later program implementation stages.

In the second phase, all participants elected the committee members for their village funds. The local political cycle could affect how the committee election was organized, leading to differences in committee composition between "Before" and "After Villages." According to Chattopadhyay & Duflo (2004) and Bardhan, Mookherjee & Torrado (2010), committee member characteristics such as gender and ethnicity can affect within-village targeting of public services. Following these studies, we test if the program performance differences were driven by differences in demographic composition of core village fund committee members with the largest decision power, including the managing director and the supervisory director. According to Table 4, the average age of core fund committee members in "After Villages" was 52. Most of these members (97%) were male and 18.2% of them were ethnic minorities. The "quality" of the members was measured by the attendance of postcompulsory high school education, and 53% of core members had done so in the default group. Regression results show little difference in these committee member characteristics between “Before” and “After Villages.”

We next focus on training attendance of the elected fund committees and loan terms set by them. As shown in the first two columns of Table 5, fund committees of “Before Villages” attended significantly more training sessions conducted by the Renmin University of China research team in our experiment, regardless of whether we measure training attendance in person-times or in person-days. The substantial (around 100%) increase in training attendance compared with the control mean suggested the committees made dramatically greater effort in program implementation. In addition to training, the loan terms set by the committee may also be different, contributing to the take-up rate variations. As shown in the last three columns of Table 5, compared with “After Villages,” the annual interest rate set by the fund committee was 1.42 percentage points lower in “Before Villages,” making the village fund loan more appealing to potential borrowers given the downward-sloping loan demand curve.[49] A similar difference also existed for loan length: it was 2.06 months longer in “Before Villages.” Karlan & Zinman (2008), for instance, have shown that longer loan maturity increases loan demand. For the loan amount cap, we do not find any difference between “Before” and “After Villages.” In sum, the greater effort made by the fund committee and the more favorable loan terms in “Before Villages” are potential contributing factors to the higher loan take-up rates in these villages.

The performance measures for the last phase, loan application and borrowing, are derived from participants’ responses to questions regarding practices and their subjective rating to fund committees’ services. According to Table 6, compared with “After Villages,” participating households in “Before Villages” were more likely to meet other members in their borrowing groups frequently and be responsible for each other’s loans, a practice promoted by the central government. They were also more likely to be satisfied with the fund committees’ service and to rate the loan decision fair, even though the impact on the fairness of the loan was not statistically significant. Given that the regression sample is limited to participants, these results can only be interpreted as suggestive evidence, rather than precise estimates, of better program practices in “Before Villages.”

To sum up, while there is no significant difference in initial program participation and in the composition of core fund committee members, the fund committees in “Before villages” set lower loan interest rates and longer loan lengths. They also attended more training sessions,

[49]The downward sloping demand curve for loan has been widely documented in the literature in experimental settings (e.g. Karlan & Zinman, 2008; Bertrand et al., 2010; Karlan & Zinman, 2018). Consistent with Bertrand et al. (2010), we do not find the resulting increase in loan demand is via reduction in the likelihood of borrowing from other sources.

better followed the group responsibility rule outlined in the State Council's guidelines, and provided more satisfactory services. These are important factors that potentially contribute to the improved program delivery in "Before Villages." These findings reveal a consistent pattern of better program practices in multiple implementation phases, providing support for the implementing-well story.

5.2 Alternative explanations

In addition to incumbents' incentives, political cycles might also affect households' financial behaviors. The possibility that economic uncertainty delays investors' irreversible investment decisions has long been documented both theoretically and empirically (e.g. Bernanke, 1983; Romer, 1990; Hassler, 2001). Political events such as elections are an important source of economic uncertainty. Nevertheless, previous studies only argue for electoral impact on delaying households' irreversible investments in such items as homes and consumer durables and their implications do not apply to the decisions to borrow.[50] Our empirical evidence further supports against the argument that the election cycle affects households' take-up of the village fund. Note that the first financial decision households needed to make immediately after the announcement of the village fund program was to participate or not, and program participation required a non-negligible financial commitment in the form of enrollment fees. If households were to advance/postpone program take-up prior to the election, we would observe a difference in the initial program participation. However, as shown earlier in Section 5.1, the program participation rates do not differ between "Before" and "After Villages." Therefore, the previously documented difference in program delivery is unlikely to be a result of households adjusting their financial behaviors prior to election.

As the current selection procedure for village Party secretaries involves both village-wide and within-Party elections, candidates not only have incentives to please villages, but also want to satisfy their fellow Party members. A loosening of lending requirements and an easing of application procedures for Party members would also result in higher program take-up and overall borrowing rates in villages with election pressure. However, this effect is likely to be slight at best, and not dominant. As village Party members are potential election competitors for the Party secretary position, bribery behaviors are unlikely to exist on a scale large enough to affect village-level borrowing. In addition, Party members are

[50]Riem (2016), for instance, has shown that electoral uncertainty reduces firms' add-on investments which face a high degree of irreversibility. Canes-Wrone & Park (2014) further show that uncertainty involved with election also encourages households to delay home-related investment that are costly-to-undo, leading to a pre-election decline in housing markets.

generally richer (Morduch & Sicular, 2000). Lending more to Party members indicates a smaller share borrowed by the poor, which is inconsistent with the better results from targeting the poor that were found earlier for villages with election pressure.

Another confounding factor is the implementation of other village programs. On the one hand, in order to gain votes, village Party secretaries would also have an incentive to better implement other programs in addition to the village fund. These contemporaneous programs could temporarily boost economic outcomes and affect credit demand. For instance, agricultural programs, such as those concerning irrigation and land improvement, could complement the village fund program by increasing agricultural productivity and profitability, leading to a more active participation. Therefore, the improved delivery of the village fund achieved cannot be fully attributed to better implementation of the program itself. On the other hand, if village government is resource-constrained, an optimal strategy to ensure being re-elected under the generally defined vote-buying story is to prioritize programs with immediate and visible impact such as the village fund program and postpone or marginalize the implementation of other projects with substantial benefits realized in the future, such as infrastructure construction. In this case, better performance of the village fund program does not necessarily benefit the villagers as the long-term cost may outweigh the short-term gain, leading to a net loss.

We provide both qualitative and quantitative evidence to show that political cycles affect little the implementation of other contemporaneous programs. First of all, expenditures on these projects are unlikely to be affected as the tax and fee reform (TFR) in 2003 and the abolishment of agricultural tax (AAT) in 2006 have weakened village finances. Prior to these reforms, villages used to be fiscally autonomous in financing local public goods. According to Tsai (2002), village finance covered most of the expenses for local public service provision, except for specific public projects required by higher level government and covered by project-specific transfers.[51] More specifically, Martinez-Bravo et al. (2017) have shown that village funding contributed to roughly 70% of expenditure on local public goods. The TFR and ATT eliminated the key source of village revenue.[52] As our sample consists of ultra-poor villages located in remote regions, they, unlike more developed villages, had limited opportunities to obtain other sources of income, including revenue from village enterprises,[53] compensation for land requisition by the government as a result of ur-

[51]Village fund is one such project. Other examples include rebuilding the village school as part of a drive to eliminate dilapidated school facilities, re-wiring the village's electrical system, or large-scale inter-jurisdictional infrastructure projects such as road and dam construction project.

[52]According to data from a large-scale nationally representative survey, the China Labor Force Dynamics Survey (CLDS), the fraction of income coming from higher level governments was at least 50% for sample villages in 2011 and the median fraction was 100%.

[53]In our sample, the medium village asset value was merely 100,000 Yuan in 2009.

ban expansion, and rent from collective land leased to investors. As a result, public goods provision with local funding was nearly impossible for poor villages (Chen, 2014*a*).[54] Funding for most contemporaneous projects is likely to come from higher levels of government as part of a larger-scale development plan. The corresponding budget is earmarked for stipulated purposes only and is hard to manipulate. As shown in Appendix Table A.8, we do not find differences in total expenditures on contemporaneous village projects nor itemized expenditure on each type of projects between "Before" and "After Villages."[55]

Second, pre-electoral (non-expense) manipulation on practical details of other programs is expected to be mild given the local context. Unlike the trial-stage village fund program, most other large-scale government development projects were implemented in a standard top-down manner for decentralized projects with clearly specified guidelines and procedures. Therefore, villages were left with limited adjustment room as to how these programs were implemented. While we cannot completely rule out the possibility that the election pressure faced by Party secretaries encouraged them to put more effort into managing other village programs, these changes were probably not large enough to affect borrowing and welfare outcomes. We support this argument with previously documented evidence that socio-economic indicators did not differ between "Before" and "After Villages" in 2009 (Appendix Table A.5). As a result, better performance of the village fund cannot be attributed to better implementation practices of other village projects prior to election.

Third, the greater effort put into the implementation of the village fund program in "Before Villages" does not necessarily crowd out time allocated to other village programs given the part-time nature of the village Party secretary positions with no clearly defined working hours (i.e. without time constraints).[56] Furthermore, as the collection of agricultural tax used to be the most important task for village government and was largely time-consuming, the ATT significantly reduced village governments' workload.[57] In a case study of a village with weak resources, for instance, Li (2008) found the ATT reduced the village government's workload by 42%. Therefore, the better implementation of the village Fund is un-

[54]Yang (2011), for instance, has documented that transfer payment was the only source of post-ATT stable revenue for most villages in Hebei, the amount of which was barely enough to cover village cadres' salaries and basic administration expenses. As a result, very little was left to support public goods provision.

[55]The only exception is energy investment, which is likely to become marginally significant by random chance (p-value=0.098).

[56]Instead, village Party secretaries in "Before Villages" likely shorten their time spent on the farm to accommodate the new village fund task.

[57]As described in Chen (2014*a*), the remaining tasks include "the enforcement of birth control, taking care of disabled elders, issuing certificates to applicants for state subsidies, providing connections and information to urban job seekers, conflict mediation between villagers, making improvements to irrigation systems and infrastructure, offering advice on which crops had the greatest market potential, helping big specialized households and those who aspired to develop family businesses to obtain credit services, building workshops, selling products, and wooing investors (albeit with little success)."

likely to marginalize the management of other projects and result in negative long-term consequences for the village economy. Nevertheless, without Party secretaries' time-use data, we are unable to completely rule out its possibility.

6. DISCUSSION

Given the improved program implementation and delivery in "Before Villages," a natural question is whether such improvements translate into welfare gains. Unlike program take-up and targeting, welfare outcomes depend on many local factors beyond the government's control, and therefore are not accurate measures for the quality of village fund management. Nevertheless, they are still worth exploring, as welfare improvements, or lack thereof, could help us better distinguish the underlying political incentives. As shown by Beaman et al. (2014), households' return to agricultural investments are heterogeneous and those with higher marginal returns self-select into loan programs. Therefore, if the higher take-up rate in "Before Villages" was a result of Party secretaries overlending to farmers with low marginal returns, we would not observe any welfare improvement.

The literature only provides limited evidence linking microcredit access with improved downstream welfare outcomes, partly due to the lack of statistical power to detect effect sizes that are economically meaningful (Banerjee, 2013; Banerjee, Karlan & Zinman, 2015). Given the modest take-up rates, previous studies often generate wide confidence intervals for intent-to-treat effects. This power issue is especially critical for our study with a limited sample size. In addition, income and consumption data are prone to measurement error and reporting biases, which further compromises our statistical power. Therefore, we focus on both economical and statistical significances in interpreting our estimation results on welfare outcomes. We will again first show benchmark results on the average program impact, and then investigate differences between "Before" and "After Villages."

Table A.9a presents benchmark estimation results on different sources of income. Consistent with the program goal, on average, the village fund program increased agricultural income by 1,448 Yuan.[58] Although the effect is marginally statistically insignificant, it is economically significant, representing a 29% increase from the control mean. The village fund program had much smaller effects on wage or business incomes. An interesting pattern emerges in the analysis of heterogenous impact on income: "Before Villages" experienced significantly higher agricultural income and lower business income compared with "After Villages" (Table 7a). This difference suggests borrowers in "Before Villages" were more

[58] Agricultural income is defined as the total production value minus production costs. This result is consistent with those found by Cai, Park & Wang (2017).

likely to use the fund for agricultural production, the main goal of the village fund program emphasized by the central government. In contrast, households in "After Villages" tended to use the fund to support business activities. Given that loan rejection is rare in practice, the difference in loan usage is not due to higher approval rates for agricultural loans. Instead, heavier emphasis on the agricultural focus of the program in campaigns (which would attract more loan applications for agricultural purposes) or better monitoring to limit diversion of loan purposes are plausible explanations for this difference.

Results on asset ownership and food consumption are consistent with the pattern found for agricultural income. As shown in Table A.9b, the program significantly increased the value of productive assets owned by households in treatment villages. However, most of the new investments were in assets not directly related to agriculture such as automobiles. As a result, the program only had a small positive impact on the value of assets closely related to agricultural production that we are unable to detect statistically. The average impacts on the value of consumer durables and food consumption per capital over the last two weeks were also minimal. A closer comparison between "Before" and "After Villages" reveals considerable impact differences (Table 7b). While there was no noticeable difference in the total value of productive assets, the value of assets directly related to agricultural production was 61% higher in "Before Villages." Food consumption per capita over the last two weeks was also significantly higher in these villages. These results are consistent with the previous evidence of increased agricultural income in "Before Villages," reinforcing the fact that fund committees in these villages were more likely to use the fund to support agricultural production following the central government's guideline.

In sum, we find introducing the village fund prior to local elections leads to improved program delivery and enhanced welfare impacts. These findings are in sharp contrast with existing studies on political cycles, which often show pre-electoral manipulations do not have a positive effect on welfare and can be detrimental to development (Cole, 2009; Baskaran, Min & Uppal, 2015; Labonne, 2016). For instance, in a similar context of agricultural credit provision, Cole (2009) has shown an increase in agricultural credit offered by government-owned banks in the year prior to an election or an election year. Unlike our findings of unchanged default rates and enhanced agricultural production, the credit boom found by Cole (2009) leads to increases in default and no significant changes in agricultural output.

Electoral pressure can lead to more successful program performance in our study for the following two reasons. First, our study focuses on the implementation of a new program, which includes setting all initial terms, rather than manipulation of an existing policy instrument that is often easy to change and revert. As noticed by Baskaran, Min & Uppal (2015), manipulation may persist if there are costs to reversing the pre-election increase. In our case, some terms of the initial setting of the program may be "sticky" and costly to change after the election, leading to some persistence instead of purely cyclical changes over time. In addition, the cost can be even greater for changes unfavorable to farmers, such as raising interest and shortening loan length that Party secretaries in "Before Villages" may find appealing to do post-election. Karlan & Zinman (2008) and Karlan & Zinman (2018), for example, have shown that loan demand is more elastic for increases than for decreases in interest. As pointed out by Karlan & Zinman (2008), the more elastic demand for price increases than for decreases is consistent with both the predictions of the prospect theory (Kahneman & Tversky, 1979; Tversky & Kahneman, 1991), in which past experience serves as a reference point and consumers value loss more heavily than gains, and models with transaction utilities (Thaler, 1985, 1999), in which consumers perceive the price increase as unfair.

To test the conjecture that the initial favorable practices can persist over time, we examine differences in loan terms between "Before" and "After Villages" in 2014. We find "Before Villages" had a lower fund annual interest rate and a longer loan length in 2014 compared with "After Villages." Both effects are statistically significant at the 5% level.[59] Unlike the temporary and cyclical policy manipulations by incumbent politicians documented in previous studies,[60] the persistence of (at least some) good program practices opens up the possibility for welfare improvements.

Second, the level of election and the corresponding political context in our study differ from the existing political cycle studies. While previous works focus on political cycles at the national or state levels, we study political cycles at the lowest administrative level (the village). Implementing-well incentives are likely dominant in local elections as politicians' efforts as well as their misconduct are more visible to villagers who live close by. Moreover, existing observations of pre-electoral fiscal expansions in certain areas are often compensated with contractions in other areas or in post-election periods. Similar fiscal manipulations are not feasible for Party secretaries as village governments have no power to collect tax revenue and development projects are financed solely by earmarked grants. In addition, the intensive monitoring from higher levels of government is also likely to play an important role in discouraging inefficient program manipulations. All of these contextual features limit vote-buying opportunities and encourage Party secretaries to perform well and better implement the program.

[59]The point estimates (standard errors) are 1.47 (0.66) and 2.72 (1.12) for interest rate and loan length, respectively.

[60]The only exception is Baskaran, Min & Uppal (2015), who have shown persistently higher electricity supply in the post-election period.

7. CONCLUSION

This paper shows better delivery of the government-implemented village fund program in China when introduced in the year prior to local Party secretary elections. These include, among others, a higher program take-up rate, better targeting of the poor, and fewer rule violations. The enhanced delivery further leads to increased agricultural income and food consumption.

The improved performance was achieved with similar participation rates of households in the initial stage of the program and comparable demographic compositions of core members in the village fund committees. Instead, choosing more favorable loan terms, attending more training sessions, and better following the practice guideline set by the central government are plausible contributing factors to the enhanced program impact in these villages. Our findings are consistent with the hypothesis that local incumbents have incentives to better implement the program, rather than misuse the service in exchange for votes, under election pressure.

While the results are specific to the local program context in China, they do indicate that political incentives can perhaps improve program performance in other similar contexts as well. An important feature of the village fund program is its decentralized implementation with earmarked grants. This feature both enables local control of the program implementation and prevents manipulation of budgets earmarked for other projects. The decentralized implementation of public projects is common in developing countries aiming for improved delivery efficiency. Similar to China, local governments in many other Asian, African and Latin American countries often lack the power to collect tax and rely on transfers from higher levels of government to implement designated public projects.

Note that the possibility for local politicians' electoral incentives to help a program achieve improved delivery and enhanced welfare is not guaranteed without other, carefully designed, practices. In the village fund program setting, these practices include, but are not limited to, intensive monitoring, which further helps prevent vote buying, and decentralized design of service features that are costly to change after the election. Nevertheless, by demonstrating a case of enhanced program performance with motivated leaders, our results highlight the role of electoral incentives, or incentives of program designers in general, in successful social program delivery. These incentives are equally important, if not more so, than motivating hired agents and client households in policy design.

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Table 1: Program Take-up and Overall Borrowing

	Village Fund Loan (1)	Any Loan (2)	Formal Loan (3)	Informal Loan (4)	Borrowing Index (5)
Before	0.151**	0.116**	-0.016	0.010	0.139**
	(0.062)	(0.045)	(0.017)	(0.057)	(0.056)
During	-0.021	0.041	-0.018	0.042	
	(0.070)	(0.049)	(0.022)	(0.083)	
Base group mean	0.219	0.522	0.054	0.366	0.000
N	780	803	803	803	803
Romano-Wolf p-value					0.000

Note: Table reports additional program impact on borrowing in villages that introduced the program before (or during) the village Party secretary election year, compared with those that introduced the program after the election year. Standard errors are clustered at the village level. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.

Table 2: Official Program Performance Indicators

	Share of Amount Borrowed by Poor (1)	Rate of Violation (2)	Rate of Default (3)	Rate of Profit (4)	Overall Performance Score (5)
Before	0.313**	-0.292***	0.004	-0.007	11.06*
	(0.149)	(0.100)	(0.015)	(0.010)	(6.051)
During	0.025	-0.166	0.024	0.005	-2.852
	(0.164)	(0.110)	(0.016)	(0.011)	(6.640)
Base group mean	0.355	0.272	0	0.026	59.56
N	30	30	30	30	30

Note: Table reports additional program impact on official program performance indicators in villages that introduced the program before (or during) the village Party secretary election year, compared with those that introduced the program after the election year. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.

Table 3: Advertisement and Participation

	Camp. Meeting Attendance (1)	No. of Camp. Meetings Attended (2)	Program Participation (3)
Before	0.139*	0.802**	0.094
	(0.078)	(0.374)	(0.083)
During	-0.105	-0.137	-0.109
	(0.100)	(0.362)	(0.100)
Base group mean	0.704	1.858	0.598
N	705	703	766

Note: Table reports additional program impact on campaign meeting attendance and participation in villages that introduced the program before (or during) the village Party secretary election year, compared with those that introduced the program after the election year. Standard errors are clustered at the village level. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.

Table 4: Loan Committee Composition

	Age (1)	Gender (male=1) (2)	Ethnic Minority (3)	High School (4)
Before	0.292	-0.086	-0.117	-0.141
	(2.390)	(0.071)	(0.153)	(0.172)
During	0.713	0.010	0.165	-0.022
	(2.622)	(0.078)	(0.167)	(0.188)
Base group mean	52.14	0.970	0.182	0.530
N	30	30	30	30

Note: Table reports differences in demographic composition of the fund committee between villages that introduced the program before (or during) the village Party secretary election year and those that introduced the program after the election year. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.

Table 5: Loan Committee Training Attendance and Loan Terms

	Training Attendance		Loan Terms		
	Person-times	Person-days	Annual Interest	Max Length (month)	Max Amount (1000 RMB)
	(1)	(2)	(3)	(4)	(5)
Before	10.99***	20.13*	-1.424*	2.057**	-0.275
	(3.481)	(10.85)	(0.731)	(0.827)	(0.573)
During	-2.037	-2.510	-0.403	0.057	-0.304
	(3.820)	(11.90)	(0.802)	(0.907)	(0.628)
Base group mean	9.636	20.18	10.58	10.36	5.273
N	30	30	30	30	30

Note: Table reports additional program impact on loan terms in villages that introduced the program before (or during) the village Party secretary election year, compared with those that introduced the program after the election year. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.

Table 6: Evaluation by Participants

	Borrowing Group Often Meets	Group Liability	Satisfied with Committee's Work	Loan Decisions are Fair
	(1)	(2)	(3)	(4)
Before	0.187*	0.078*	0.076**	0.035
	(0.102)	(0.041)	(0.036)	(0.023)
During	0.055	0.042	0.055	0.040
	(0.123)	(0.042)	(0.037)	(0.024)
Base group mean	0.368	0.907	0.910	0.961
N	344	349	441	431

Note: Table reports differences in participants' program evaluations between villages that introduced the program before (or during) the village Party secretary election year and those that introduced the program after the election year. Standard errors are clustered at the village level. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.

Table 7: Welfare Impact

(a) Income

	Agriculture Income (1)	Wage Income (2)	Business Income (3)	Income Index (4)
Before	2.252*	-0.677	-2.358**	0.026
	(1.234)	(1.324)	(0.892)	(0.094)
During	0.034	0.065	-1.536	
	(0.799)	(2.013)	(0.740)	
Base group mean	5.018	11.69	3.424	0.000
N	803	803	803	803
Romano-Wolf p-value				0.882

(b) Assets and food consumption

		Productive Assets			
	Consumer Durables (1)	Total (2)	Closely Related to Agriculture (3)	Food Consumption Per Capita (4)	Welfare Index (5)
Before	-0.999	0.211	0.472*	22.54**	0.123*
	(0.723)	(1.416)	(0.232)	(10.45)	(0.064)
During	-0.198	-1.342	-0.002	-4.576	
	(0.763)	(1.229)	(0.395)	(10.02)	
Base group mean	5.080	4.064	0.779	89.90	0.000
N	803	803	803	785	803
Romano-Wolf p-value					0.098

Note: Table reports additional program impact on welfare indicators in villages that introduced the program before (or during) the village Party secretary election year, compared with those that introduced the program after the election year. Incomes and values of assets are measured in 1000 Yuan. Standard errors are clustered at the village level. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.

Appendices

Figure A.1: Program Implementation Timeline

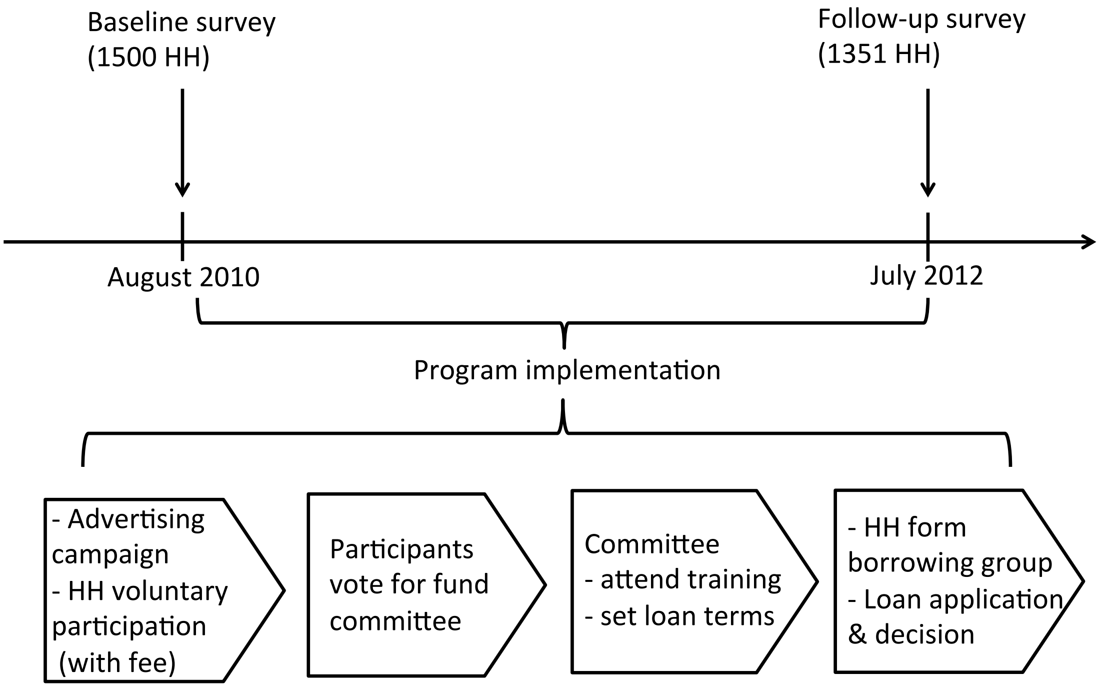

Table A.1: Attrition

(a) Treatment and attrition

	Household Attrition
Treated villages	0.004
	(0.025)
N	1500

(b) Balance of key indicators between attrition and non-attrition households

	Non-attrition		Attrition - Non-attrition		
	Mean	SD	Coeff.	p-value	N
Access to credit					
Any type of loan	0.585	0.493	-0.048	0.334	1500
Formal loan	0.118	0.322	-0.004	0.892	1500
Informal loan	0.531	0.499	-0.042	0.407	1500
Income sources (1,000 RMB)					
Agriculture income	3.169	9.721	-0.664	0.120	1500
Wage income	10.27	14.61	0.531	0.671	1500
Business income	1.139	11.72	1.146	0.365	1500
Assets and food consumption					
Consumer durables (1,000 RMB)	3.981	5.560	-0.054	0.925	1499
Productive assets (1,000 RMB)	3.088	20.72	-0.812	0.420	1427
- of which closely related to ag.	0.741	1.955	-0.075	0.543	1427
Food cons. per capita (RMB)	78.52	56.00	-0.203	0.958	1498

Table A.2: Treatment-Control Balance

		Control			Treatment-Control	
	N	N	Mean	SD	Coeff.	p-value
Household demographics						
Head age	1344	539	51.9	11.4	0.648	0.620
Head male	1344	539	0.941	0.237	0.011	0.497
Head illiteracy	1340	537	0.179	0.384	0.001	0.988
Head ethnic minority	1344	539	0.174	0.380	0.001	0.994
Household size	1351	541	4.31	1.53	-0.111	0.586
# of children (age<16)	1351	541	0.815	0.886	-0.039	0.705
# of adults (16≤age<60)	1351	541	2.81	1.38	-0.089	0.528
# of elderly (age≥60)	1351	541	0.688	0.863	0.017	0.841
# of male	1351	541	2.27	0.956	-0.055	0.654
Access to credit						
Any type of loan	1351	541	0.558	0.497	0.044	0.369
Formal loan	1351	541	0.109	0.312	0.014	0.521
Informal loan	1351	541	0.508	0.500	0.039	0.493
Income sources (1,000 RMB)						
Agriculture income	1351	541	2.74	8.71	0.717	0.355
Wage income	1351	541	11.9	16.3	-2.65	0.129
Business income	1351	541	1.53	16.1	-0.656	0.217
Assets and food consumption						
Consumer durables (1,000 RMB)	1350	541	4.41	6.09	-0.711	0.230
Productive assets (1,000 RMB)	1286	510	4.31	29.8	-2.03	0.217
- of which closely related to ag.	1286	510	0.716	1.94	0.041	0.792
Food cons. per capita (RMB)	1349	541	82.3	64.8	-6.39	0.357
Other socioeconomic indicators						
# of members with disability or chronic illness	1351	541	1.01	1.08	0.094	0.439
Distance to bank (km)	1350	541	4.36	3.72	0.824	0.404
Distance to hospital (km)	1338	533	1.07	1.62	0.013	0.965
Distance to town (km)	1350	541	27.1	12.8	4.06	0.339
Village characteristics						
# of Households	50	20	268	165	16.9	0.671
Categorized as credit village	50	20	0.550	0.510	0.017	0.910
% of credit HHs	50	20	0.308	0.309	-0.069	0.435
% of HHs with sanitary latrine	50	20	0.170	0.178	-0.011	0.859
% of HHs with phone	50	20	0.784	0.197	-0.059	0.343

Table A.3: Village Characteristics by Year of Establishment (CHARLS data)

	N	Before 1990 N	Before 1990 Mean	Before 1990 SD	After-Before Coeff.	After-Before p-value
Demographics						
Population (1000s)	293	204	1.97	1.44	1.03	0.000***
# of households	293	204	566	437	223	0.000***
Ethnic minorities (dummy)	293	204	0.426	0.496	0.000	0.994
Village economy						
Net per-capita income (1000s)	280	196	4.27	4.80	-0.022	0.971
% of HHs with formal credit	269	192	0.141	0.192	-0.012	0.636
Enterprise in village (dummy)	291	202	0.460	0.500	0.034	0.594
Infrastructure						
Access to paved road (dummy)	293	204	0.583	0.494	.035	0.580
Hospital (dummy)	293	204	0.779	0.416	0.075	0.142
Kindergarten (dummy)	293	204	0.426	0.496	0.034	0.589
Primary school (dummy)	293	204	0.574	0.496	0.101	0.106
Junior high school (dummy)	293	204	0.103	0.305	0.021	0.604
Senior high school (dummy)	293	204	0.025	0.155	-0.013	0.462
Bank branch (dummy)	293	204	0.127	0.334	0.030	0.495
Supermarket (dummy)	293	204	0.225	0.419	0.044	0.417
Nursing home (dummy)	293	204	0.103	0.305	-0.013	0.732
% of HHs with cellphone	293	204	0.866	0.160	0.020	0.318

Note: Table shows differences in characteristics between villages established on or after 1990 and those established before 1990 using the CHARLS data.

Table A.4: Contributing Factors to Local Political Cycle Variations

Dependent Variable: Election Year	Coef.	Obs.	Adj R-sq
Province Fixed Effects	–	30	-0.00
Faction of household with the largest kinship ties	-0.026	30	-0.04
	(0.763)		
Largest two kinship clans close in size	-0.205	30	-0.02
(difference<10% of HH)	(0.335)		

Note: We assign values of 0, 1 and 2 for the election year variable to villages with (planned) elections in 2010, 2011 and 2012 respectively.

Table A.5: Balance across Villages in Different Political Cycles

	Program Implementation Relative to Village Secretary Reappointment Year			
	Before - After		During - After	
	Coeff.	p-value	Coeff.	p-value
Household demographics				
Head age	0.115	0.957	0.223	0.899
Head male	0.041	0.017**	0.018	0.470
Head illiteracy	-0.068	0.296	-0.018	0.705
Head ethnic minority	0.021	0.885	0.088	0.623
Household size	0.129	0.710	0.091	0.795
# of children (age<16)	-0.031	0.873	-0.045	0.806
# of adults (16≤age<60)	0.017	0.947	0.115	0.622
# of elderly (age≥60)	0.142	0.255	0.021	0.855
# of male	0.071	0.726	0.085	0.705
Access to credit				
Any type of loan	-0.064	0.318	-0.054	0.493
Formal loan	0.051	0.080*	0.012	0.773
Informal loan	-0.095	0.192	-0.068	0.451
Income sources (1,000 RMB)				
Agriculture income	1.86	0.157	0.010	0.989
Wage income	-2.57	0.271	-4.05	0.099*
Business income	-0.939	0.185	-0.417	0.526
Assets and food consumption (RMB)				
Consumer durables (1,000)	-0.266	0.763	-0.721	0.472
Productive assets (1,000)	-0.579	0.667	-1.82	0.101
- of which closely related to ag.	0.198	0.253	0.081	0.760
Food cons. per capita	-0.457	0.958	0.497	0.965
Other socioeconomic indicators				
# of members with disability or chronic illness	0.041	0.854	0.066	0.754
Distance to bank (km)	0.005	0.997	-1.66	0.347
Distance to hospital (km)	0.419	0.268	-0.155	0.554
Distance to town (km)	7.87	0.240	3.06	0.726
Village characteristics				
# of Households	-44.7	0.378	-4.61	0.933
Categorized as credit village	0.000	1.000	-0.261	0.278
% of credit HHs	0.087	0.510	0.038	0.789
% of HHs with sanitary latrine	0.072	0.490	0.008	0.943
% of HHs with phone	0.058	0.529	-0.143	0.163

Table A.6: Average Impact on Program Take-up and Overall Borrowing

	Village Fund Loan (1)	Any Loan (2)	Formal Loan (3)	Informal Loan (4)	Borrowing Index (5)
Treated villages	0.243***	0.078**	-0.017	-0.062*	0.712***
	(0.033)	(0.030)	(0.012)	(0.033)	(0.100)
Base group mean	0.007	0.475	0.081	0.416	0.000
N	1317	1340	1340	1340	1340
Romano-Wolf p-value					0.000

Note: Table reports the average program impact on borrowing in treatment villages (benchmark). Standard errors are clustered at the village level. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.

Table A.7: Wild Cluster Bootstrap-t Method for Within-treatment Specifications

	Before - After		During - After	
Variables	Coeff.	p-value	Coeff.	p-value
Program take-up and overall borrowing				
Village fund loan	0.151	0.020	-0.021	0.828
Any loan	0.116	0.072	0.041	0.536
Formal loan	-0.016	0.448	-0.018	0.548
Informal loan	0.010	0.832	0.042	0.720
Advertisement and participation				
Camp. meeting Attendance	0.139	0.168	-0.105	0.456
No. of camp. Meetings Attended	0.802	0.056	-0.137	0.728
Program participation	0.094	0.348	-0.109	0.356
Income				
Agriculture income	2.252	0.096	0.034	1.000
Wage income	-0.677	0.648	0.065	0.968
Business income	-2.358	0.052	-1.536	0.088
Assets and food consumption (RMB)				
Consumer durables (1,000)	-0.999	0.256	-0.198	0.880
Productive assets (1,000)	0.211	0.884	-1.342	0.380
- of which closely related to ag.	0.472	0.088	-0.002	0.912
Food cons. per capita	22.54	0.088	-4.576	1.000

Note: P-values are calculated using a wild cluster bootstrap-t procedure with 500 repetitions.

Table A.8: Expenditure on Other Village Projects in 2010 and 2011

		By Projects							
	Total	Road & Bridge	Electronic Commu-nication	Energy	Drinking Water	Irrigation	Land Improve-ment	Environ-ment	Sanitation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Before	20.44	30.28	-7.207	4.176*	-5.917	10.73	8.164	-22.82	-0.563
	(88.44)	(42.54)	(8.687)	(2.395)	(6.161)	(14.86)	(50.49)	(17.32)	(0.494)
During	-21.86	-11.31	-10.17	-1.040	-0.996	9.402	6.508	-8.618	0.277
	(100.4)	(48.30)	(9.862)	(2.719)	(6.994)	(16.87)	(57.32)	(19.66)	(0.561)

Note: Table reports village project expenditure differences between villages that introduced the program before (or during) the village Party secretary election year and those that introduced the program after the election year during the two-year implementation of the village fund program, 2010 and 2011. The first column reports total expenditure differences, including the nine specified project types and all other unspecified projects. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.

Table A.9: Average Welfare Impact

(a) Income

	Agriculture Income	Wage Income	Business Income	Income Index
	(1)	(2)	(3)	(4)
Treated villages	1.448	0.526	-0.055	0.047
	(0.926)	(1.015)	(0.685)	(0.052)
Base group mean	5.023	11.00	2.577	0.000
N	1340	1340	1340	1340
Romano-Wolf p-value				0.353

(b) Assets and food consumption

		Productive Assets			
	Consumer Durables	Total	Closely Related to Agriculture	Food Consumption Per Capita	Welfare Index
	(1)	(2)	(3)	(4)	(5)
Treated villages	0.188	1.757**	0.207	4.271	0.046
	(0.455)	(0.762)	(0.180)	(7.140)	(0.041)
Base group mean	4.580	3.877	0.882	92.82	0.000
N	1340	1340	1340	1319	1340
Romano-Wolf p-value					0.353

Note: Table reports the average program impact on income and welfare in treatment villages (benchmark). Incomes and values of assets are measured in 1,000 Yuan. Standard errors are clustered at the village level. Asterisks *, **, and *** denote significant levels of 10%, 5%, and 1% respectively.