Fixing Market Failures or Fixing Elections? Agricultural Credit in India (Job Market Paper)

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December 17, 2004†

Abstract

How vulnerable are economic interventions to capture by politicians, how are captured resources used, and how costly are the resulting distortions? This paper answers these questions in the context of the credit market in India. Integrating theories of political budget cycles with theories of tactical electoral redistribution yields a compelling framework to test for the presence of capture. I find that government-owned banks are subject to substantial capture: the growth rate of agricultural credit lent by public banks is 5-10 percentage points higher in election years than in years after an election, and in election years more loans are made to districts in which the ruling state party had a narrow margin of victory (or a narrow loss) in the previous election. This targeting does not occur in non-election years. This paper then shows that politically motivated loans are economically costly. They are less likely to be repaid. Nor are they put to good use: election year credit booms do not measurably affect agricultural output. Finally, I measure whether the average agricultural loan was beneficial, using variation induced by the 1980 bank nationalization: agricultural credit in villages with nationalized bank branches grew more than twice as quickly than in villages with private branches over the 1980s. However, this additional credit had no effect on measured agricultural outcomes.

*Department of Economics, MIT. I thank Abhijit Banerjee, Esther Duflo, and Sendhil Mullainathan for guidance, Abhiman Das and R.B. Barman of the Reserve Bank of India for substantial support. I also thank Abhiman Das for performing calculations on disaggregated data at the Reserve Bank of India. In addition, I thank Victor Chernozhukov, Ivan Fernandez-Val, Francesco Franco, Andrew Healy, Andrei Levchenko, Rema Hanna, Petia Topalova, and participants of the MIT development lunch and finance lunch workshops for comments. Gautam Bastian and Samantha Bastian provided excellent research assistance. Financial support from a National Science Foundation Graduate Research Fellowship is acknowledged. Errors are my own.

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1 Introduction

There is substantial disagreement among economists about how much governments should intervene in markets, and especially credit markets. Advocates of a large government role ("social banking") have argued that intervention can help overcome market failures, increase growth rates, and reduce poverty, particularly in developing countries. Opponents worry that even if the intervention is intended benevolently, public actors may be inefficient, or worse, captured by politicians or interest groups, and intervention will result in more harm than benefit. Political capture can take the form of private enrichment (bribes, loans to politicians, etc.), redistribution towards supporters (patronage), or manipulation for political gains (eg., political budget cycles).

This paper tests for politically induced distortions in the Indian banking sector, and measures their cost. Government planning and regulation were a key component of India's post-independence development strategy, particularly in the financial sector. Three government policies stand out. First and foremost, the government nationalized most private banks, in 1969, and again in 1980. Second, both public and private banks were required to lend at least a fixed percentage of credit to agriculture and small-scale industry. Finally, a branch expansion policy obliged banks to open four branches in unbanked locations for every branch opened in a location in which a bank was already present.

The three policies had a substantial effect on India's banking system, making it an attractive target for government capture. The branch expansion policy increased the scope of banking in India to a scale unique to its level of development: in 2000, India had over 60,000 bank branches both public and private), located in every district across the country. Nationalized banks increased the availability of credit in rural areas and for agricultural uses. Burgess and Pande (2004), and Burgess, Pande, and Wong (2004) show that the redistributive nature of branch expansion led to a substantial decline in poverty among India's rural population. However, these government policies also made public sector banks very attractive targets for capture: public banks did not face hard budget constraints, were subject to political regulation, and were present throughout India.

This paper presents evidence that government-owned banks in India serve the electoral in-

terests of politicians.[1] I show that the amount of agricultural credit lent by public banks is substantially higher in election years. Politicians target this credit to meet electoral goals: in election years, more loans are made in districts in which the ruling state party had a narrow margin of victory (or a narrow loss) in the previous election. This targeting is not observed in off-election years, or among private banks. Politically motivated loans are shown to be economically costly. They are unlikely to be repaid on time. The agricultural lending booms do not appear to affect agricultural output. In fact, political interference may be so costly that even the average agricultural loan from a government bank is not put to productive use: I demonstrate that a substantial increase in government credit in villages whose banks were nationalized did not have any effect on observable agricultural outcomes.

This paper contributes to three literatures. A relatively recent body of empirical work evaluates how government ownership of banks affects financial development and economic growth. In a cross-country setting, La Porta, Lopez-de-Silanes, and Shleifer (2002) demonstrate that government ownership of banks is prevalent in both developing and developed countries (in 1995 the average government held 42% of the equity of the ten largest banks), and that government ownership of banks is associated with slower financial development and slower growth. In a related study (Cole, 2004), I evaluate theories of bank ownership using a policy experiment in India that nationalized banks in some areas while leaving others in private hands. I find that nationalization initially increased the rate of financial development during a decade of financial repression; in a liberalized environment, however, government ownership of banks hindered financial development. Nationalized banks charge lower interest rates, lend more to agriculture, and make more bad loans. Finally, government ownership of banks slowed the development of the trade and service sectors.

Two other recent papers use loan-level data sets to explore the behavior of public sector banks. Sapienza (2004) finds that Italian public banks charge interest rates approximately 50 basis points lower than private banks, and finds a correlation between electoral results and interest rates charged by politically-affiliated banks. Khwaja and Mian (2004) find that Pakistani politicians enrich themselves and their firms by borrowing from government banks and defaulting on loans.

[1] There is no shortage of tales of politicians enriching themselves at the expense of public banks. Khwaja and Mian (2004) document substantial looting in Pakistani government banks. However, in this paper, I am primarily interested in how political incentives affect allocation of resources to the voting population.

The second literature is on political budget cycles. A large body of work documents, and proposes explanations for political budget cycles in both developing and developed countries (reviews of this literature can be found in Alesina and Roubini (1997) and Shi and Svensson (2004)). Relative to the literature, this paper provides a particularly clean test of cyclical manipulation. First, because Indian state elections are not synchronized, I can exploit within-India variation in the relationship between electoral cycles and credit, and thus rule out macroeconomic fluctuations as a possible explanation for cycles. Second, the interpretation of observed cycles for agricultural credit is particularly clear. There is no reason to think that agricultural lending in India, ostensibly unrelated to the political process, should exhibit political cycles. In contrast, one may observe cycles in government spending for a variety of reasons. Politicians are elected because they seek to change policies. Alternatively, if they become more effective over their tenure, and additional experience would affect their ability to spend or borrow, one may observe budgetary cycles unrelated to political goals.

Most closely related to the present work is a very recent paper by Serdar Dinc (2004), which examines lending of public and private sector banks in a cross-country setting. Dinc finds that in election years, the growth rate of credit of government-owned banks is about 3 percent higher than in non-election years, while private banks' loan portfolios grow about 3 percentage points slower. Dinc demonstrates that bank credit responds differently to both inflation and exchange rates in election years compared to non-election years. However, because public sector banks are typically larger than private sector banks, and tend to lend to large, state-owned firms, macroeconomic shocks in election years could affect public and private banks differently. An advantage of this paper is the ability to control for macroeconomic shocks.

Finally, this paper provides a compelling test of theories of politically-motivated redistribution. Reaching as far back as Wright (1974), this literature ties government spending to electoral goals, and in particular attempts to distinguish between patronage (politicians aiding their supporters), and strategic allocation (politicians attempting to woo undecided voters). Studies of cross-sectional redistribution typically face several hurdles. First, they often rely on cross-sectional variation, with limited sample sizes. In contrast, the sample used in this paper contains 412 districts in 19 states. Over the eight years for which data are available (1992-1999), these states collectively witnessed a total of 32 elections. The panel-setting allows the inclusion of dis-

trict fixed-effects (or estimation of first differences), which rules out spurious correlation due to time-invariant cross-sectional variation. Second, it can be difficult to distinguish tactical political redistribution from broader programmatic goals: if the left-wing party aids the poor, is that "politically motivated redistribution" or simply an outcome of the political process? This paper uses agricultural credit from ostensibly independent public banks, which are supposed to make loans according to commercial merit. Finally, typical vehicles of targeted political largesse, such as bridge or road construction, experience only limited variation across time or space. In contrast, there are over 45,000 public sector bank branches in India, which collectively issue hundreds of millions of loans. The size and number of loans granted by each branch varies continuously over time.

The combination of cross-sectional and time-series analysis represents a significant methodological improvement in tools used to identify electorally-motivated redistribution. There are several reasons, unrelated to tactical distribution, that could explain a cross-sectional relationship between electoral outcomes and redistribution. There are other explanations, again unrelated to political goals, that could explain time-series variation. However, none of these reasons could explain why we would observe a cross-sectional relationship in election years, but not in off-election years.

A second substantive contribution of this paper is to identify the costs of tactical redistribution. If political intervention simply shifts credit from one group to a second group, but both groups use it efficiently, then reducing the scope for intervention has implications for equity, but not aggregate output. On the other hand, if the targeted credit is not productively employed, the costs of redistribution may be substantial. A similar question can be asked about cycles: are observed spending booms squandered on projects with little return, or are the funds put to good use? It is even possible that the threat of an upcoming election causes politicians to behave *more* closely in line with the public interest. The answers to these questions are essential to understanding whether tactical redistribution is merely a minor cost of the democratic process, or is so costly that it may be desirable to substantially circumscribe the latitude of governments to intervene in the economy.

Finally, the setting studied here is particularly attractive for testing theories of capture and redistribution. Public sector banks are vulnerable to capture, and loans can be targeted in ways

that many other government expenditures cannot. The Indian constitution induces exogenous election cycles, and private sector banks can serve as a control group. Very good data are available for both electoral outcomes and credit.

This paper proceeds as follows. In the next section, I briefly describe the context of banking and politics in India, including the mechanisms by which politicians may influence banks. In Section 3, I discuss competing theories of political redistribution, and their testable predictions. Section 4 develops the empirical strategy and presents the main results of political capture. In Section 5, I establish that these political manipulations are socially costly: increases in government agricultural credit do not affect agricultural output. Finally, Section 6 concludes.

2 Banking and Politics in India

2.1 Banking in India

Formal financial institutions in India date back to the 18^{th} century, with the founding of the English Agency House in Calcutta and Bombay. Over the next century, presidency banks, as well as foreign and private banks entered the Indian market. In 1935, the presidency banks were merged to form the Imperial Bank of India, later renamed the State Bank of India, which became and continues to be the largest bank in India. Following independence, both public and private banks grew rapidly. By March 1, 1969, there were almost 8,000 bank branches, approximately 31% of which were in government hands. In April of 1969, the central government, to increase its control over the banking system, nationalized the 14 largest private banks with deposits greater than Rs. 500 million. These banks comprised 54% of the bank branches in India at the time. The rationale for nationalization was given in the 1969 Bank Nationalisation Act: "an institution such as the banking system which touches and should touch the lives of millions has to be inspired by a larger social purpose and has to subserve national priorities and objectives such as rapid growth in agriculture, small industry and exports, raising of employment levels, encouragement of new entrepreneurs and the development of the backward areas. For this purpose it is necessary for the Government to take direct responsibility for extension and diversification of the banking services and for the working of a substantial part of the banking system."[2]

In 1980, the government of India undertook a second wave of nationalization, by taking control

[2] Quoted in Burgess and Pande (2004).

of all banks whose deposits were greater than Rs. 2 billion. Nationalized banks remained corporate entities, retaining most of their staff, with the exception of the board of directors, who were replaced by appointees of the government. The political appointments included representatives from the government, industry, agriculture, as well as the public.

In a related paper (Cole (2004)), I examine the effect of ownership on banks. Comparing nationalized banks whose deposits in 1980 were just above the cutoff to private banks whose deposits were just below, I find that nationalization slowed the growth of banks. Nationalized banks also suffered greater financial losses than private banks. I also estimate the effect of nationalization on real economic outcomes, by comparing villages whose branches were nationalized to those whose branches were not. Using a regression-discontinuity design, I find that during the period of financial repression (the 1980s), nationalization caused substantially faster financial development, while in the liberalized 1990s, nationalization slowed financial development. Census data from 1991 suggest that nationalization also led to slower growth in employment in service and trade industries.

2.2 Politics in India

India has a federal structure, with both national and state assemblies. The constitution requires that elections for both the state and national parliaments be held at five year intervals, though the elections are not synchronized. Most notably, the central government can declare "President's rule" and dissolve a state legislature, leading to early elections. Although this is meant to occur only if the state government is nonfunctional, state governments have been dismissed for political reasons as well. Additionally, as in other parliamentary systems, if the ruling coalition loses control, early elections are held.

The Indian National Congress Party dominated both state and national politics from the time of independence until the late 1980s. Since then, states have witnessed vibrant political competition. In the period I study, 1992-1999, a dozen distinct parties were in power, at various times, and in various states. The sample I use (including most states, for the period 1992-1999), contains 32 separate elections in 19 states. These elections are generally competitive: over half of the elections were decided by margins of less than 10 percent.

State governments have broad powers to tax and spend, as well as regulate legal and economic

institutions. While members of state legislative assemblies ("MLAs") lack formal authority over banks, there are several means by which they can influence them. First and foremost, the ruling state government appoints members of the "State Level Bankers Committees," which coordinate lending policies and practices in each state, with a particular focus on lending to the "priority sector" (agriculture and small-scale industry).[3] The committees meet quarterly, and are composed of representatives from the State Government, public and private sector banks, and the Reserve Bank of India. Their membership typically turns over when the state government changes.

Governments also directly influence banks. Harriss (1991) writes of villagers in India in 1980: "It is widely believed by people in villages that if they hold out long enough, debts incurred as a result of a failure to repay these loans will eventually be cancelled, as they have been in the past (as they were, for example, after the state legislative assembly elections in (1980)."[4] A former governor of the Reserve Bank of India has lamented that the appointment of board members to public sector banks is "highly politicized," and that board members are often involved in credit decisions.[5] Nor are state politicians hesitant to promise loans during elections. For example, the Financial Express reports:

> Two main contenders in the Rajasthan assembly elections...are talking about economic well-being in order to muster votes. No wonder then that easier bank loans for farmers, remunerative earnings from agriculture on a bumper crop as well as uninterrupted power supply appear foremost in the manifestoes of both the parties.[6]

Adams, Graham, and von Pischke (1984) describe why agricultural credit is a particularly attractive lever for politicians to manipulate: the benefits are transparent, while the costs are not. This makes it hard for opposition politicians to criticize efforts by those in power.

Focusing on agricultural credit makes sense within the context of India: the majority of the Indian population is dependent on the agricultural sector. Agricultural lending plays a substantial role in the Indian economy: in 1996, there were approximately 20 million agricultural loans, with an average size of Rs. 11,910 (ca. $220). Although agricultural credit comprises only about 17%

[3] See for example, "Master Circular Priority Sector Lendings," RPCD No. SP. BC. 37, dated Sept. 29, 2004, Reserve Bank of India.

[4] p. 79, cited in Besley (1995), p. 2173.

[5] Times of India, June 2, 1999.

[6] Financial Express, November 30, 2003.

of the value of public sector banks' loan portfolios, its importance in the share of loans is large: approximately 40% of loans made by public sector banks are agricultural loans.[7]

The amount of agricultural credit lent by banks is orders of magnitude larger than the amount of money spent on campaigns in India. Each legislative constituency receives, on average, about Rs. 50 - 80 million in credit ($1-$1.6 million). While campaign spending is difficult to measure (campaign spending limits are difficult to enforce, and money spent without authorization of a candidate does not count against the sum), the level of legal campaign limits is informative: between 1992 and 1999, the legal limit ranged from Rs. 50,000 (approximately US $1,000) to Rs. 700,000 (ca. $14,000), or less than 1% of the amount of agricultural credit. (Sridharan (1999)).

3 Theories and Tests of Redistribution

3.1 Political Cycles

The first theories of political cycles in the economy involved monetary policy: Nordhaus (1975) proposed a model in which an opportunistic government exploits myopic voters, who rely on recent economic outcomes as an indicator of government performance. Voters are "fooled" when the government makes sub-optimal intertemporal allocation decisions, in order to increase chances of re-election. A second set of models posits that political cycles may be observed, even in the absence of any distortionary behavior by politicians. In partisan models (such as Hibbs' (1977)), different political parties' preferences for inflation vs. employment will lead to economic cycles coincident with elections. Alesina (1987) extends this result to a model with rational expectations.

More recent theories incorporate frictions into the political process. Alesina and Roubini (1997) describe how a setting with unobservable competence and rational voters can induce politicians to increase spending prior to elections. These models have been criticized, however, because in equilibrium, more competent politicians induce greater distortions than less competent politicians. Persson and Tabellini (2000) and Shi and Svensson (2002) develop models in which politicians face moral hazard: they may undertake hidden effort (perhaps unobservable borrowing) as a substitute for competence prior to election in order to improve economic performance.

These models all generate a similar, testable prediction: policy outcomes will co-move with electoral cycles. In particular, the models that focus on strategic behavior by politicians predict

[7] "Basic Statistical Returns," Table 1.9, Reserve Bank of India, 1996.

pro-growth manipulation of policy levers (such as expansionary monetary policy, spending or borrowing), followed by contraction and/or tax increases after elections.

These models have received extensive empirical testing. In surveys, both Drazen (2000) and Alesina and Roubini (1997) argue that the evidence of cycles in monetary instruments is weak, while evidence of fiscal cycles is more robust. Shi and Svenson (2002) collect data for 60 countries, and find that fiscal cycles are characteristic of both developed and developing countries. They find that fiscal cycles are more pronounced in countries in which institutions protecting property rights are weaker and voters are less informed.

The robust relationship between elections and budget deficits need not, however, imply that politicians behave opportunistically. Lower tax collection or increased spending could differ systematically prior to elections for reasons other than political manipulation. Spending increases may be attributable to the fact that politicians, who seek to implement programs, learn on the job. On average, a year just before an election will have politicians with a longer tenure than a year just after an election, since the politician will have served, at a minimum, almost an entire term in office.

These concerns are less applicable to agricultural credit. First, political goals should not affect the amount of agricultural credit issued by public sector banks. The most significant factor influencing farmers' agricultural credit needs is probably weather, which is inarguably out of the politicians' control. Second, because I focus on state elections, the possibility that state-specific agricultural credit moves in response to national economic shocks (such as interest rates or exchange rate adjustments) can be ruled out.

Of course, if there are large cycles in state government spending in India, agricultural credit could covary with elections for reasons unrelated to government interference in banks. Khemani (2003) tests for political budget cycles in Indian states. She finds no evidence of political cycles in overall spending or deficits. She does find evidence of small decreases in excise tax revenue, as well as evidence of other minor fiscal manipulation prior to Indian state elections.

The models discussed above typically involve policy instruments that affect the entire economy. Political cycles involve intertemporal trade-offs, and are thought to be inefficient because politicians behave opportunistically to reallocate resources intertemporally in ways the voters would oppose. Agricultural credit affects a subset of the population, benefitting some at the expense of

others. One might then ask, if politicians are buying votes with agricultural credit, why would they pay in one or two years, rather than over the entire election cycle?

Certainly if voters consider credit a feature of the economy, rather than a "bribe," then the standard analysis would hold. Resource constraints of the bank limit how much banks can lend to agriculture, meaning politicians meddling with banks face intertemporal constraints similar to the fiscal budget constraints.[8] An alternative cause for temporally concentrated redistribution would be a fixed cost of interference. If there is a fixed cost to inducing bad loans (such as a positive probability of being caught by the anti-corruption authorities no matter how small the manipulation[9]) politicians may concentrate largesse.

In summary, models of political cycles predict lending booms around elections.

3.2 Politically Motivated Redistribution

Agricultural credit is a means of redistribution: by law, agricultural credit is lent at rates substantially lower than non-agricultural loans. Moreover, default rates are extremely high, especially for public sector banks. Redistribution comes in many forms. In a paper on redistributive politics, Dixit and Londregan (1997) distinguish between "programmatic" and "pork barrel" redistribution. The former, which includes programs such as Social Security and public education, represents society's preferences towards equality and social opportunity. This type of redistribution evolves slowly over time. "Pork barrel" redistribution, on the other hand, is clearly a cost of the democratic process. (Examples include giving government jobs to supporters of politicians or building unnecessary weapons systems in key congressional districts.) Politicians may engage in pork-barrel redistribution for two, not mutually-exclusive reasons. First, they may simply use it as a means of obtaining a desired allocation of resources, independent of re-election concerns ("patronage"). Second, they may believe distributing patronage aids in reelection ("tactical redistribution").

The methodology in this paper tests for both patronage and tactical redistribution. Models of patronage predict that areas in which the ruling party enjoys more support will receive a disproportionate amount of resources, since politicians reward their supporters irrespective of electoral goals. Models of tactical redistribution predict resource allocation will follow one of two patterns:

[8] While public sector banks faced soft budget constraints in the 1980s, they hardened considerably in the 1990s, as the central government compelled banks to conform to international capital adequacy norms.

[9] The Central Vigilance Commission (CVC), India's anti-corruption authority, is officially charged with ensuring that bankers make only commercially sound loans.

resources will be targeted towards "swing" districts, or politicians will disproportionately reward their supporters. Snyder (1989) and Dixit and Londregan (1997) develop models in which either pattern may be observed, depending on model parameters. Cox and McCubbins (1986) argue that risk-averse politicians will tend to target tactical redistribution towards their core supporters to maximize their chance of reelection.

Three recent studies investigate the question of tactical redistribution using cross-sectional variation. Dahlberg and Johanssen (2002) study a grant project in Sweden, in which the incumbent government enjoyed control over which constituencies received the grant. They find strong evidence that money was targeted to districts in which swing voters were located. In contrast, Case (2001), examining an income redistribution program in Albania, finds that the program favored areas in which the majority party enjoyed greater support. Finally, Miguel and Zaidi (2003) examine the relationship between political support and educational spending in Ghana, and find no evidence of targeted distribution of educational spending at the parliamentary level.[10] Finally, two recent papers investigate whether government grants from the center to the state are politically motivated. (Dasgupta, Dhillon, and Dutta, 2003, and Khemani, 2004).

Empirically distinguishing between the theoretical models is difficult for several reasons. First, data on purely tactical spending is rarely readily available. The usual vehicles through which tactical resources are distributed, such as public works projects, may not vary much over space or time. Sample sizes may be small: the three papers cited above use a single cross-section with relatively small sample sizes (115, 47, and 199, respectively). It is not obvious what types of spending can be characterized as tactical, rather than programmatic. In the cross-section, both patronage and some types of tactical redistribution towards supporters will generate the same relationship. Moreover, cross-sectional relationships may be driven by omitted variables, such as per-capita income.

This work overcomes these problems: the sample size is large, comprising 412 districts in 19 states; thirty-two election cycles are observed over an eight-year period. Credit data are comprehensive, well-measured, and vary continuously. In the absence of political pressure, agricultural credit should vary primarily only with rainfall, or with fixed agricultural characteristics, such as quality of soil. Because I have eight years of data, I am able to include a district fixed-effect,

[10] Miguel and Zaidi (2003) also use a regression discontinuity design to look for patronage effects: they find none.

which controls for all unobserved time-invariant determinants of credit disbursal at the district level. Alternatively, I can estimate the effects in changes rather than levels.

Most importantly, the cross-sectional and time-series component taken together allow for a much more powerful test of both political cycles and tactical redistribution. The political budget cycle literature predicts that politicians and voters care more about allocation of resources prior to elections, than in other periods. Thus, observed distortions, such as patronage, or targeting swing districts, should be larger during election years than non-election years. This test thus has the power to distinguish between models of patronage unrelated to electoral incentives, and models that predict a positive relationship between support and redistribution simply as a result of electoral incentives: the former would not vary with the electoral cycle, while the latter would. Moreover, while either cycles or cross-sectional variation could be caused by reasons other than electorally-motivated manipulation, it is very unlikely that the cross-sectional relationships would change over the electoral cycle for any reason other than tactical redistribution.

4 Evidence

I begin with a brief description of the data (details are available in the data appendix), and then develop the empirical strategies, and present results for political lending cycles and tactical targeting of credit.

4.1 Data

Unless otherwise indicated, the unit of observation in this section is the administrative district, roughly similar to a U.S. county. The data, collected by the Reserve Bank of India ("Basic Statistical Returns") are aggregated at the district level, and published in "Banking Statistics." This aggregation is based on every loan made by every bank in India.[11]

Election data for state legislative elections are available at the constituency level from 1985-1999. These data, from the Election Commission of India, include the identity, party affiliation, and share of votes won, for every candidate in a state election from 1985 to 1999. The majority party is identified as the party that won the majority of seats in the most recent state election. If

[11] Banks were allowed to report loans smaller than Rs. 25,000 (ca. $625) in an aggregated fashion until 1999, at which point loans below Rs. 200,000 (ca. $5,000) were reported as aggregates.

the majority party did not field a candidate, I define the margin of victory for the majority party to be the negative of the vote share of the winning candidate. If the majority party candidate ran unopposed, I define the margin of victory to be 100. For states in which no single party won a majority, print media searches identified the majority coalition initially in power. All members of parties aligned with the majority coalition were coded as "majority."[12] Because credit data are observed at the district level, vote shares are also aggregated to the district level. I therefore use as a measure of ruling party strength, M_{dst}, the average margin of victory of the ruling party in a district. The median district has 9 legislative assembly constituencies.

The credit dataset used in the analysis contains information for 412 districts in 19 states, giving a total of 3,296 observations.[13] Table 1 gives summary statistics. Because district boundaries or district name had changed, I was unable to match all districts in all years.

A case could be made for conducting the analysis at the level of the electoral constituency, rather than the district: the number of observations would increase substantially, and identification of political variables would be tighter. However, it is not currently possible to match the credit data to constituencies. Moreover, credit may cross constituency boundaries: the district of Mumbai has 34 constituencies and 1,581 bank branches.[14]

While the specification includes district fixed-effects and region-year fixed effects, rainfall varies substantially over time within regions. I thus include annual rainfall.

One limitation of this data set is that the time dimension is relatively short. For this reason, I will focus on standard panel estimation, using log credit as the dependent variable. This is a reasonable approximation: a large share of agricultural credit is short-term loans, with maturation of less than a year. The median and mean rate of real agricultural credit growth for public banks

[12] The theoretical models of redistribution derived below were motivated by a two-party system. While India has many parties, I am careful to code all members of the ruling coalition as Majority Party. Moreover, Chhibber and Kollman (1998) document that while India often had more than two parties at the national level, in local elections, the political system closely resembled a two-party system.

[13] The included states are Andhra Pradesh, Assam, Bihar, Gujarat, Haryana, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Rajasthan, Tamil Nadu, Uttar Pradesh, West Bengal, Arunachal Pradesh, Himachal Pradesh, Meghalaya, Mizoram, Nagaland, and Tripura. States were included if credit and electoral data were available from 1985-2000. Many studies of India focus on the larger states (the first twelve in the given list), which contain the vast majority of the Indian population. The results in this paper are robust to focusing only on those states. Punjab and Jammu and Kashmir were not included because they did not experience normal election cycles over the sample period.

[14] Matching credit data to constituencies would require substantial effort. However, identifying credit "leakages" outside the targeted constituency would allow a test of the electoral impact of additional credit, using a methodology similar to Levitt and Snyder (1997). I leave this for future research.

is zero over the period studied. As a robustness check, I demonstrate in an appendix (section 7) that the results are robust to estimation in changes, and present the key results in a dynamic panel setting, estimated with the standard GMM technique developed by Arellano and Bond (1991). Section 7 also presents tests for stationarity and serial correlation.

4.2 Political Cycle Results

The simplest approach to test for temporal manipulation is to compare the amount of credit issued in election years to the amount issued in non-election years. I include district fixed-effects to control for time-invariant characteristics in a district that affect credit.[15] Region-year fixed effects (γ_{rt}) control for macroeconomic fluctuations. Finally, I include the average rainfall in the previous 12 months in district t ($Rain_{dst}$). Formally, I regress:

$$y_{dst} = \alpha_d + \gamma_{rt} + \delta Rain_{dst} + \beta E_{st} + \varepsilon_{dst} \tag{1}$$

where α_d is a district fixed-effect, and E_{st} is a dummy variable taking the value of one if the state, s had an election in year t. Standard errors are clustered at the state level.[16] The appendix replicates the key results for estimation in changes, as well as by means of the Generalized Method of Moments technique developed by Arellano-Bond (1991).

Elections in India are, however, endogenously determined: in my sample, one fourth of elections (10 out of 37) occur before they are scheduled. If parties in power call early elections when the state economy is doing particularly well, one may observe a spurious correlation between credit and election years. Following Khemani (2004), I use as an instrument for election year a dummy, S^0_{st}, for whether five years have passed since the previous election. (The superscript on S_{st} denotes the number of years until the next scheduled election). The first stage is thus:

$$E_{sdt} = \alpha_d + \gamma_{rt} + \delta Rain_{dst} + \beta^0 S^0_{st} + \varepsilon_{dst} \tag{2}$$

Table 2 presents the results from the first stage regression. Because elections are required after four years without an election, S^0_{st} is a powerful predictor of elections. The estimated coefficient

[15] The Reserve Bank of India divides India into six different regions. All results presented here are robust to using year, rather than region*year fixed effects. State*year fixed effects would of course be collinear with the election variables. Results are also robust to including or excluding rainfall.

[16] Results are robust to clustering by state. Serial correlation is less of a concern here than in a standard differnce-in-difference settings, because the election cycle dummies exhibit only weakly negative serial correlation.

is .99, with a standard error of .01. This first stage explains 86% of the variation in election years, because early elections are not common.[17]

Do elections affect credit? Table 3 gives the results from OLS, reduced form, and instrumental variable regressions. I focus initially on aggregate credit and agricultural credit. For agricultural credit, there is clear evidence of electoral manipulation: both the IV and reduced form estimates indicate that the lending by public sector banks is about 6 percentage points higher in election years than non-election years.[18] This effect of elections on agricultural credit is not due to region-level shocks, which would be absorbed by the region-year fixed effect, nor can it be attributed to budgetary manipulation, since state governments did not spend more in election years.[19] Nor is there any relationship, in the OLS, reduced form or IV, between elections and non-agricultural credit. This effect is precisely estimated for credit from public banks. The IV and OLS estimates are relatively similar, suggesting that the endogeneity of election years should not be a large concern.

Interestingly, no relationship between credit and elections is observed for private banks: the point estimate on the scheduled election dummy for private agricultural lending is -.02, and statistically indistinguishable from zero. Unfortunately, because private sector banks are smaller, operate in substantially fewer districts, and have more volatile agricultural lending, their usefulness as a control group is limited: in particular, the confidence interval for private sector banks cannot rule out that they covary with elections in the same manner as public sector banks.

Table 4 expands these results by tracing out how lending comoves with the entire election cycle. This requires a straightforward extension of equations 1 and 2. Define S_{st}^{-k}, k=0,...4, as dummies which take the value 1 if the next *scheduled* election is in k years for state s at time t. For example, if Karnataka had elections in 1991, 1993, and 1998, S_{st}^{-4} would be 1 for years 1992 and 1994, and 1999, while S_{st}^{-3} would be 1 in 1995 only, and S_{st}^{0} would be 1 for year 1998 only.

[17] The results reported here are robust to an alternative instrument which uses information on elections only prior to 1990. Denoting t_s the first election after 1985 in state s, this instrument assigns elections to years t_s,$t_s + 5$, $t_s + 10$, and $t_s + 15$. However, because the cycle results resemble a sine function, I gain more power when I "reset" the instrument after an early election.

[18] Because the left hand side variable is in logs, the coefficients may be interpreted approximately as percentage effects.

[19] See Khemani (2004).

The following regression gives the reduced-form estimate of the entire lending cycle:

$$y_{dst} = \alpha_d + \gamma_{rt} + \delta Rain_{dst} + \beta_{-4} S_{st}^{-4} + \beta_{-3} S_{st}^{-3} + \beta_{-2} S_{st}^{-2} + \beta_{-1} S_{st}^{-1} + \varepsilon_{dst} \quad (3)$$

The IV equivalent would use the S_{st}^{-k} as instruments for E_{st}^{-k}, where E_{st}^{-k} is defined as the *actual* number of years until the next election. (Because the IV and reduced form estimates are virtually identical, throughout the rest of the paper, only the latter are reported). Each row in Table 4 represents a separate regression. Panel A gives sectoral credit issued by all banks, Panel B by public banks, and Panel C by private banks. The results indicate that agricultural credit issued by public banks is lower in the years that were four, three, and two years prior to an election than in the years before an election or election years. The difference, of up to 8 percentage points is substantial given that the average growth rate of real agricultural credit issued by public sector banks was 0.5% over the sample period. The standard deviation of the change was 20%). Cycles are not observed in non-agricultural lending, nor among private sector banks. The estimates imply that the cumulative distance from the credit "peak" in an election year to the "trough" three years before the election, is approximately 8.1% of total level of credit, a substantial amount. Results from alternative specifications of equation 3 are presented in the appendix. Estimated relationship using changes (Table A4), as well as with the Arellano-Bond estimator in levels and changes (A5 and A6) are very similar.

While cycles are not observed for private banks, the standard errors on the cycle dummies are much larger than those for public sector banks, and cycles in private banks cannot be ruled out. Could it be that increased public sector lending simply crowds out private sector lending in election years, while private banks pick up the lending slack in the years between elections? The relative size of the two bank groups provides a ready answer: private sector banks issue only approximately ten percent of credit in India, and are underweight in their exposure to agricultural credit. Thus, a 8% decline in the amount of agricultural credit issued by public sector banks would have to be met by an almost doubling of the amount of agricultural credit issued by private sector banks, an amount far beyond the confidence interval of the estimated size of a cycle for private banks.

4.3 Is Loan Distribution Targeted?

In this subsection, I examine whether agricultural credit varies with the average margin of victory enjoyed by the current ruling party in the previous election in each district, M_{dst}. Since section 4.2 demonstrated that credit varies over the election cycle, I continue to include the indicators for election cycle, S_{st}^{-k}. The simplest model of patronage would posit that greater support for the majority party leads to increased credit. The most straightforward test for this would be to simply include the average margin of victory of the ruling party in the previous election, M_{dst} in equation 3. A positive coefficient would provide suggestive evidence that areas with more support receive more credit. (Unless explicitly noted, I continue to include γ_{rt} and $Rain_{dst}$ but suppress them in the exposition for notational simplicity). The regression is thus the following:

$$y_{dst} = \alpha_d + \pi M_{dst} + \beta_{-4} S_{st}^{-4} + \beta_{-3} S_{st}^{-3} + \beta_{-2} S_{st}^{-2} + \beta_{-1} S_{st}^{-1} + \varepsilon_{dst} \tag{4}$$

The estimates are reported in column (2) of Table 5. For public sector banks, the coefficient on M_{dst} is relatively precisely estimated at zero. (The standard deviation of M_{dst} is approximately 15 percentage points). This provides strong evidence against a model of constant patronage, in which the majority party rewards districts that voted for it while punish districts that voted for the opposition: a model of patronage would imply a positive π, something the estimate can rule out. For private sector banks, there is a very large positive relationship. The estimate does not represent a robust relationship, but rather a problem with the data: some districts have only one or two private banks, whose agricultural credit is very low, but varies substantially over time. Estimated coefficients for private sector banks are always not robust to dropping outliers; this is the case for π in regression 4.

The model in equation 4 is very restrictive: it would not detect tactical distribution towards swing districts, since it imposes a monotonic relationship across all levels of support. If politicians target lending to "marginal" districts, then $\frac{\partial y_{dst}}{\partial M_{dst}} < 0$ when $M_{dst} < 0$, and $\frac{\partial y_{dst}}{\partial M_{dst}} > 0$ when $M_{dst} > 0$. I therefore define $M_{dst}^{+} \equiv M_{dst} * I_{M_{dst}>0}$, and $M_{dst}^{-} \equiv M_{dst} * I_{M_{dst}<0}$, where $I_{M_{dst}>0}$ is an indicator function taking the value of 1 when $M_{dst}>0$, and 0 otherwise. ($I_{M_{dst}<0} = 1$ when $M_{dst} < 0$, and 0 otherwise). If credit is in fact allocated linearly according to support for the politician, then the coefficients on M_{dst}^{+} and M_{dst}^{-} would both be positive.

The second generalization is motivated by the discussion in section 3 and the results in section

4.2: if politicians induce a lending boom in election years, then perhaps they will differentially target credit in different years of an election cycle. To allow for that, I interact the variables M^+_{dst} and M^-_{dst} with the election schedule dummies $S^{-4}_{st}, ...S^{-1}_{st}$, thus allowing a different relationship between political support and credit for each year in the election cycle.

This approach can perhaps be best understood by looking at Figure 1, which graphs how levels of credit vary both across time and with the margin of victory, M_{dst}. (The regression on which the graph is based is given below in equation 5). The top-most graph gives the predicted relationship four years prior to the next scheduled election (and therefore one year after the previous election): the slight upside down V-shape indicates that districts in which the average margin of victory is close to zero received the most credit. The slope of the lines are not statistically distinguishable from zero.

The next panel in Figure 1, for the year three years prior to the next scheduled election, indicates a relatively flat relationship: credit did not vary with previous margin of victory. The same holds for two years before the election and one year before the election.[20] In a scheduled election year, however, there is a pronounced upside-down V shape: the predicted amount of credit going to very close districts is substantially greater than credit in districts that were not close.

The graph is based on the following regression:

$$\begin{aligned} y_{dst} &= \alpha_d + \beta_{-4}S^{-4}_{st} + \beta_{-3}S^{-3}_{st} + \beta_{-2}S^{-2}_{st} + \beta_{-1}S^{-1}_{st} + \pi^+ M^+_{dst} + \pi^- M^-_{dst} \\ &\quad +\theta^+_{-4}(M^+_{dst} * S^{-4}_{st}) + \theta^+_{-3}\left(M^+_{dst} * S^{-3}_{st}\right) + \theta^+_{-2}\left(M^+_{dst} * S^{-2}_{st}\right) + \theta^+_{-1}\left(M^+_{dst} * S^{-1}_{st}\right) \\ &\quad +\theta^-_{-4}(M^-_{dst} * S^{-4}_{st}) + \theta^-_{-3}\left(M^-_{dst} * S^{-3}_{st}\right) + \theta^-_{-2}\left(M^-_{dst} * S^{-2}_{st}\right) + \theta^-_{-1}\left(M^-_{dst} * S^{-1}_{st}\right) + \varepsilon_{dst} \end{aligned} \tag{5}$$

Standard errors are again clustered at the state level. Results are presented in the third column of Table 5. Once the previous margin of victory is included, the estimated size of the cycle increases, to approximately 12% at the minimum, two years prior to an election. The relationships shown are statistically significant: the coefficient on previous margin of victory during an election year (M^+_{dst} and M^-_{dst}) are different from zero at the 1% level. The coefficient on M^+_{dst} is approximately -.272, while the coefficient on M^-_{dst} is .373. This implies a substantial effect: the standard deviation of the margin of victory is approximately 15 percentage points: thus, a district in which the ruling party won (or lost) the previous election by 15 percentage points will receive approximately 4-5

[20] The regression, which gives standard errors, is described below.

percent less credit than a district in which the previous election was narrowly won or lost.

The relationship between previous margin of victory and amount of credit in a year k years before a scheduled election is given by the value of the parameters $\pi^+ + \theta^+_{-k}$. A test of the hypothesis $\left(\pi^+ + \theta^+_k\right) = 0$, for k=-4, -3, -2, and -1 indicates that the slopes in the off-election years are not statistically indistinguishable from zero. The same holds for tests of $\pi^- + \theta^-_{-k}$, for k=-4, -3, -2, and -1 . Thus, targeting of credit towards marginal districts appears in election years only. Nor is there any evidence of a patronage effect. A patronage effect would show up if π^- or π^+, or the respective sums of main effect and interaction ($\pi^- + \theta^-_{-k}$ and $\pi^+ + \theta^+_{-k}$) were positive.

The coefficients on the interaction terms (θ^+_{-k} compared to θ^-_k) and the main effects (π^+ compared to π^-) are roughly equal in magnitude, but opposite in sign. (Indeed the test that $\pi^+ + \theta^+_{-k} = -\pi^- - \theta^-_{-k}$ cannot be rejected for any k, for both the credit level and credit growth regressions.) This suggests a useful restriction. Recall that M_{dst} measures the average margin of victory in the district: while results across constituencies within a district are highly correlated, M_{dst} does introduce some measurement error. For example, the following two districts would have identical values of M_{dst}: a district in which the margin of victory was 0 in every constituency; a district in which the majority party won half the constituencies by a margin of 100%, and lost the other half by 100%. I therefore define "Absolute Margin," AM, as follows:

$$AM_{dst} = \sum_{c=1}^{k_d} \frac{1}{N_d} |M_{cdst}|$$

where M_{cdst} is the margin of victory in constituency c in district d in state s in the most recent election in year t, and N_d is the number of constituencies in a district. Estimating equation 5, but substituting πAM_{dst} for $\left(\pi^+ M^-_{dst} + \pi^+ M^-_{dst}\right)$,with analogous replacements for the interaction terms, resolves this measurement error problem. Because electoral outcomes within a district are indeed correlated, the results are very similar, and again suggest targeting in an election year, but no relationship in off-years.

Figures 2 and 3 graph the information from the level and growth regressions of equation 5 in another way. They trace credit for both public and private sector banks, over the election cycle (Again, the equivalent Figures for credit growth are given in the appendix tables). Figure 2 gives the relationship for a notional "swing" district ($M_{dst} = 0$), while Figure 3 gives the same relationship for a notional district whose margin of victory was 15 percentage points in the previous

election. Public sector lending exhibits a swift decline after an election, dropping 10 percentage points below the election level two years prior to the election, before returning to zero. Appendix Tables A3, A7, and A8 give the results for this approach when estimated in changes, as well as with the Arellano-Bond estimator. Figures 4, 5, and 6 give the results from differenced version of equation 3.[21] The results reported here are robust to using year, rather than region-year, fixed effects, as well as to restricting the sample to the major states of India. As a final robustness check, I estimated quadratic specifications, but found no strong evidence of non-linearities.

The time-series and cross-sectional evidence of manipulation of public resources supports the idea that credit is used by politicians to maximize electoral gains, rather than reward core supporters. Are the credit booms around elections simply bad loans to friends of politicians that will not be repaid, or is it only when the threat of a re-election looms that politicians ensure that the banks are fulfilling their legal obligation to provide credit to the poorer sections of society? Even if the additional credit is "good" credit, it is very difficult to imagine that the socially optimal allocation of agricultural credit is coincident with the electoral cycle

The cross-sectional data give support to an even stronger presumption that the observed patterns are inefficient. Surely districts whose population are strongly in favor (or opposed to) the incumbent majority party do not need relatively less agricultural credit in *election years* than districts that are more evenly split. Even if the additional credit generated by political competition is welfare-improving, it is not at all obvious why it should be targeted towards districts with electorally even races.

5 Is Redistribution Costly?

What are the real effects of this observed distortion? I begin this section by investigating whether the electoral cycle affects the rate of default among agricultural loans. I then test directly whether more government credit from public banks leads to greater agricultural output.

5.1 Is the marginal political loan more likely to default?

In a study on Pakistan, Khwaja and Mian (2004) document that loans made by public sector banks to firms controlled by politicians are much more likely to end up in default. What about

[21] This difference equation is given in the appendix.

loans to supporters of politicians?

To answer this question, I estimate the reduced form relationship between agricultural credit default rates and elections. I use two measures of default rate: the proportion of loans coded as late by at least six months, and the share of credit, weighted by loan size, coded as late by at least six months. (Summary statistics for all the variables used in this section are presented in Table 6). The results, from equation 3 are presented in Table 7. The equation is estimated in both levels and changes. For public sector banks, the share of value of agricultural loans in default appear to comove with elections. The level and growth rate of bad loans is between 2-3 percent lower after election years than before. (The unconditional average share of credit in default is 20% for public sector banks, with a standard deviation of 16%. The average change in defaulting credit is 0%, with a standard deviation of 14%. The values for the variable "number of loans bad" are very similar.) Somewhat puzzlingly, there is evidence of a relationship for private sector banks, which experience lower levels and growth rates of bad loans in the year before an election.

The analysis suggests that higher default rates are observed during election years.[22] This interpretation suggests that lending booms are costly. The default rate could be higher because the marginal borrower, absent any political consideration, is more likely to default, or because politicians cause banks to lend to borrowers who are even more likely to default than the marginal borrower the bank would choose if it merely wanted to increase credit, without lending specifically to a designee of a politician. I cannot distinguish between these two hypotheses.

A second plausible interpretation of the drop in late loans after an election may be that banks write the bad political loans off their books. Indeed, press accounts give evidence that politicians promise to forgive loans after elections.[23]

There is no compelling reason to accept either of these explanations, given the lack of precise information about the time it takes for a loan to be marked in default, and the process by which banks write off loans.[24] However, the fact that loan default rates comove with electoral cycles gives rise to a strong presumption that the marginal political loan is more likely to default than

[22] The measure of default used is one that classifies loans as non-performing if repayment is more than six months late. Since many agricultural loans are made to purchase inputs, and to be paid back after harvest, default would be detected within a year.

[23] Harriss (1991) cites an example of this. However, I note that while the Indian press is rife with accounts of politicians promising to increase agricultural lending, it is harder to find examples of loan forgiveness.

[24] Examining bank loan write-offs would help solve these problems, but these data are not available at the state level.

the average loan.

5.2 Lending Booms and Agricultural Output

Perhaps the best way to evaluate the cost of cycles is to measure whether the loans are put to productive use. That is, does credit affect agricultural output? This question cannot be answered by measuring correlations between credit and agricultural output: omitted factors, such as agricultural productivity, crop prices or idiosyncratic shocks will almost surely bias any estimate. The lending booms documented in 4.2 suggest an instrument for the efficacy of politically-induced lending: the electoral cycle induces a supply shock uncorrelated with other confounding factors.

If additional loans lead to greater investment and output, then the costs of intervention may be limited to sub-optimal allocation amongst farmers seeking credit. On the other hand, if the additional credit has no effect on agricultural output, this suggests that either the loans are used for very inefficient investment in agriculture, or they are simply consumed by the borrowing population.

To answer this question, I use data on agricultural output (Agricultural Net State Domestic Product). I was not able to obtain district level agricultural output data for a time period that overlapped with my credit variable; therefore, analysis in this section is conducted at the state level. The union of electoral, agricultural, and credit data is available for fourteen major states over the eight-year period 1992-1999.

Panel A of Table 8 presents the first stage of the regression, which is equation 3, with log agricultural credit as the dependent variable, run at the state level. The estimated lending cycle using state aggregates is very similar, both qualitatively and quantitatively, to the cycle estimated using district level data. One difference is that the point estimates for private-sector banks at the state level are negative; in this smaller sample, the standard errors are very large. Estimates for changes, rather than levels, are provided in Appendix Table A4.

Panel B gives the results for the reduced form relationship between agricultural output and the election cycle. Since much of Indian agricultural crops are annual (e.g., rice and wheat), increased agricultural credit could have an almost immediate effect on output. However, there is no relationship between credit and output: the point estimates for agricultural output in off-years are actually positive. Though the standard errors are sufficiently large that negative effects

cannot be ruled out, the joint hypothesis that the four coefficients on $S^{-4}, S^{-3}, S^{-2,}$ and S^{-1} from the agricultural output regression are equal to the point estimates of the coefficients from the regression of all bank credit on $S^{-4}, S^{-3}, S^{-2,}$ and S^{-1} can be rejected at the 5 percent level. Finally, the instrumental variable estimates give a negative effect of public credit on output, but the estimates are very imprecise, and not statistically different from zero.

Thus, while credit does go up in election years in Indian states, there is no evidence that agricultural output does so. The reduced form estimates provide some evidence against the possibility that an additional Rupee of credit leads to an additional Rupee of output.

5.3 The Impact of the Average Agricultural Loan: Evidence from Bank Nationalization

The previous sections suggest that the marginal, political loans do not affect agricultural output, and these marginal loans may be more likely to default. However, this does not prove the case against government intervention in the agricultural sector. If the *average* loan is extremely beneficial, then the benefits of government ownership of banks may outweigh the costs. In this final section, I use bank nationalization itself as an instrument for agricultural credit, using a methodology developed in Cole (2004). While this test's counterfactual (public credit vs. private credit) is different than the one in section 5.2 (less vs. more public credit), it is an important one, for theory and policy,[25] since private sector banks did not appear to be influenced by political considerations. In particular, I test a hypothesis that should be easily met if public agricultural credit has positive effects: does a more than two-fold increase in the level of agricultural credit provided to a village affect agricultural outcomes.

In 1980 the Indian government nationalized six private banks according to a strict cutoff rule, taking control of only those whose deposits were greater than Rs. 2 billion, and leaving many banks of similar size in private hands. Because banks in India had hundreds of branches located throughout the country, nationalization effectively "assigned" some villages public banks, and some villages private banks. I show that, controlling for village conditions in 1980, villages whose banks were nationalized experienced substantially faster credit growth over the 1980s. Comparing

[25] A third counter-factual is investegated by Burgess and Pande (2004). They find that a village with a branch is better off than a village without a branch. (They do not consider the question of bank ownership, but since the vast majority of branches in India are public, it is reasonable to interpret their results as the effect of increasing the number of public bank branches).

agricultural outcomes in the two villages gives a measure of the effects of this increase in credit. (I limit analysis to villages that had only one private branch prior to the 1980 nationalization).

To estimate the effect of public agricultural credit on outcomes, I compare villages whose branch belonged to a parent bank close to the cutoff (the six banks above the cut-off, and the 20 below), because they are most comparable. This approach yields a sample of 1,513 villages: 46% of whose branches were nationalized. Table 6, Panel C gives summary statistics.

One concern is that village level outcomes may be correlated with the size of the parent bank (if, for example, larger parent banks place branches in larger villages). However, the cutoff rule employed by the government induces a non-linear jump in the whether a branch in a village is nationalized. This suggests a regression-discontinuity design: a polynomial function of size in 1980 of the parent bank controls for any correlation between bank size and 1980 village characteristics. Since the cut-off rule induces a jump, I can then also include a dummy for whether the bank is nationalized. This gives the following equation:

$$y_{v,d} = \alpha_d + \beta * Nat_v + \pi_{1v,1980} + \pi \left(K_{v,1980}\right)^2 + \pi \left(K_{v,1980}\right)^3 + \varepsilon_{c,d} \tag{6}$$

where α_d is a district fixed-effect, and $\mathrm{K}_{v,1980}$ is the natural log of the size of the parent bank of the branch operating in village v.

This analysis will only be correct if, after controlling for size, banks above the cutoff were similar to banks below the cutoff prior to 1980. In Cole (2004), I test this identification assumption in three ways, showing that conditional on size, nationalized and non-nationalized banks did not experience different growth rates prior to nationalization, had similar balance sheets and levels of profitability, and located in similar villages.

Note also that there is no deterministic relationship between K_v and 1980 level of deposits in a particular village's branch. Of course, the amount of deposits in a village's branch in 1980 may also affect village-level outcomes for reasons unrelated to nationalization. Thus, I include a third-degree polynomial in log deposits in 1981 in equation 6.[26] This strategy compares two villages in the same district in India, with similar amounts of deposits, and looks for a break in the relationship between outcomes and the size of a village's parent bank. Standard errors are

[26] Unfortunately, the earliest data available were for March, 1981, approximately 11 months after nationalization.

clustered by bank.[27]

The first stage relationship between credit and nationalization is obtained by estimating equation 6, using the share of public credit in 1992 as the dependent variable. Results are presented in Table 9. Not surprisingly, nationalization of a village's only bank branch in 1980 has an average effect of increasing the share of credit granted by public banks in 1992 by 100 percent.[28] Because the first stage predicts one for one the share of public credit in 1992, I focus on reduced form results.

An important goal of nationalization was to increase the amount of credit granted to agriculture. Column 2 of Table 9 uses average annual growth rate in log credit from 1981 to 1991 as the dependent variable.[29] Nationalization had a tremendous effect on the growth rate of credit in villages over this time: a village with a public sector bank experienced an annual growth rate approximately 11 percent higher than a village with a private bank. The cumulative effect is an increase over one and a half times the initial level. In the 1990s, when public sector banks faced hard budget constraints, villages with public sector banks grew no more quickly. Nationalization affected more than the level of credit, however. Column 4 gives the results of a regression of the share of agricultural credit granted in 1992 on ownership. On average, nationalized branches provided a 26 percent higher share of credit to agriculture than did private banks.

Nationalization of banks appears to have harmed the quality of intermediation: column 5 indicates that the share of non-performing agricultural credit was 18.2 percentage points higher in villages with public sector banks than in villages with private sector banks.

By 1992, the fraction of credit to agriculture lent by public banks was 26 percent higher than that lent by private banks. Moreover, the overall level of credit in a village branch, was more than twice the level in a village with a private sector bank, after conditioning on 1980 village bank characteristics. What effect did this increase in agricultural credit have on agricultural outcomes? While relatively little data are available at the village-level in India, the 1991 census gives two variables that can be used as measures of agricultural investment: whether a village has a tubewell,

[27] The standard errors are smaller if results are clustered by district. Cole (2004) develops an FGLS model of the error term. The FGLS estimates are close to those reported here.

[28] This relation is not quite tautological: in a few villages, an additional bank branch entered. This was relatively rare, however, and the R^2 of the first stage is nearly 1.

[29] Total credit, rather than agricultural credit is used because the data on agricultural credit are not available for periods before 1992.

and the share of land around a village that is irrigated. Both of these variables are significantly positively correlated with deposits in 1981. Yet, as the first two columns of Table 10 indicate, the estimated effect of nationalization on the presence of a tubewell is zero. The estimate is relatively precise. The same is true for the fraction of land irrigated. The final two columns of Table 10 are devoted to a falsification test: they demonstrate that neither the literacy nor the fertility rates varies by nationalization status.

This approach is used extensively in Cole (2004) to estimate the effect of nationalization on financial development, the quality of intermediation, and industrial development.

6 Conclusion

There are strong theoretical reasons to believe that politicians will manipulate resources under their control in order to achieve electoral success. Yet, compelling examples of this manipulation are rarely documented in the literature. The first contribution of this paper is to develop an improved framework for testing for tactical redistribution. Combining models of time-series manipulation with models of cross-sectional redistribution yields predictions for the distribution of resources across time and space that are very unlikely to be explained by omitted factors. These predictions are tested using data from agricultural credit from public sector banks in India. I find evidence of political lending cycles. Moreover, credit is targeted towards districts in which the majority party just won or just lost the election. This targeting is observed only in election years.

The second contribution of this paper is to measure the cost of these observed distortions. A loan-level analysis demonstrates that election cycles induced credit booms in agricultural credit in election years. However, these booms induced substantially higher default rates. Electoral cycles serve as an instrument for identifying the effect of marginal loans on output, providing evidence that increased levels of credit from public sector banks do not affect aggregate agricultural output at the state level. To answer the more general question about the efficacy of agricultural credit in an environment with political capture, I turn to the 1980 nationalization, in which some private sector banks were nationalized while others were left in private hands. I show that villages whose branch was nationalized experience a substantial increase in credit, and especially agricultural credit, relative to villages whose banks remained private, but that this had no effect on agricultural outputs. The quality of intermediation may explain this: agricultural loans in

villages with government banks were much less likely to be repaid on time.

The third contribution of this paper is to provide a better understanding of why government ownership of banks has negative effects on real economic outcomes. Arguments against government ownership of banks typically rest on two premises: government enterprises are less efficient, and their resources are misused by politicians. This paper provides a clear example of the latter, and suggests that the costs of misuse are so great that additional government credit may have no effect on output. This is a particularly important policy question, since government ownership of banks is very prevalent in developing countries, and financial development may be a key determinant of economic growth.

It is worth noting that these results are not inconsistent with the finding of Burgess and Pande (2004) that rural banks reduce poverty. Their results suggest that the presence of any bank in a village will reduce poverty, but they do not distinguish between public and private sector banks. Of particular relevance to their findings is the result in this paper that government banks suffer substantially higher default rates. Burgess and Pande are agnostic on whether the benefits of rural branch expansion outweighed the cost, precisely because the rural default rates were so high.

This paper also helps interpret tests for redistribution. Previous empirical work has ignored the time series dimension, and may not provide an accurate picture, since redistribution may only occur in periods just before an election. Second, the finding of targeting towards "swing districts" suggests why approaches using regression-discontinuity design (e.g., Miguel and Zaidi (2003)) find no effect of politics on the allocation of goods. If resources are targeted towards swing districts, there will be no discontinuity between a constituency in which the ruling party just won the previous election or just lost it.

The findings reported here are important, in terms of understanding the costs of redistribution. The magnitudes are considerable: the estimated effect of 5-9% higher credit growth rates in election years is substantially larger than the average annual growth rate of credit. Efforts to isolate government banks from political pressure, as is done with many central banks, may reduce these effects. Politicians appear to care more about winning re-election than rewarding their supporters, and they do so by targeting "swing" districts.

7 Appendix: Alternative Specifications

This section explores the robustness of the results reported in the main paper to alternative specifications. I test for stationarity and serial correlation in the time-series, and then present estimates of the main specifications in changes. Finally, I present estimates, for both levels and logs, using the Arellano and Bond (1991).

I begin by testing for serial correlation for levels of credit, and for changes in credit, using a test described in Wooldridge (2002). Under the null of no serial correlation, the time-demeaned residuals u will be serially correlated, with a known autoregressive relationship. The null hypothesis of no serial correlation can be rejected for levels of credit, but not for changes. Results for these tests are presented in Columns 1 and 2 of Table A1. Columns 3 and 4 give tests for stationarity in the levels series I use the panel test derived in Levin, Lin, and Chu (1991). Under the null of non-stationarity, the "T-star" statistic is distributed asymptotically normal. The null of non-stationarity is strongly rejected for all credit level series

A specification analogous to 3 using change in credit, rather than level of credit is:

$$\Delta y_{dst} = \gamma_{rt} + \delta \Delta Rain_{dst} + \beta_{-4} S_{st}^{-4} + \beta_{-3} S_{st}^{-3} + \beta_{-2} S_{st}^{-2} + \beta_{-1} S_{st}^{-1} + \varepsilon_{dst} \tag{7}$$

Note that the equation is not strictly analogous to 3: instead, it measures whether credit growth is higher in election years than non-election years. (This specification is most comparable to Dinc (2004)). The results, presented in Table A2, are very similar to the levels regressions: credit growth is substantially lower in off election years than during an election. This relationship is observed for agricultural credit, but not for other credit, and for public sector banks only. (Time-invariant characteristics are differenced out; however, the results from estimating 7 are nearly identical when a district fixed effect, α_d, is included. A district fixed effect in a changes regression effectively allows for a separate trend in each district.)

Table A3 presents the results of equations based on 5, using growth in real credit as the dependent variable.

$$\begin{aligned}\Delta y_{dst} &= \beta_{-4}S_{st}^{-4} + \beta_{-3}S_{st}^{-3} + \beta_{-2}S_{st}^{-2} + \beta_{-1}S_{st}^{-1} + \pi^{+}M_{dst}^{+} + \pi^{-}M_{dst}^{-} \\ &+\theta_{-4}^{+}(M_{dst}^{+} * S_{st}^{-4}) + \theta_{-3}^{+}\left(M_{dst}^{+} * S_{st}^{-3}\right) + \theta_{-2}^{+}\left(M_{dst}^{+} * S_{st}^{-2}\right) + \theta_{-1}^{+}\left(M_{dst}^{+} * S_{st}^{-1}\right) \\ &+\theta_{-4}^{-}(M_{dst}^{-} * S_{st}^{-4}) + \theta_{-3}^{-}\left(M_{dst}^{-} * S_{st}^{-3}\right) + \theta_{-2}^{-}\left(M_{dst}^{-} * S_{st}^{-2}\right) + \theta_{-1}^{-}\left(M_{dst}^{-} * S_{st}^{-1}\right) + \varepsilon_{dst}\end{aligned} \tag{8}$$

again, the results are very similar to the results in Table 5.

Because of the nature of the data, and the fact that the panel is much wider (412 districts) than it is long (8 years of levels, 7 years of changes), I have focused on standard panel estimation techniques. However, a dynamic panel approach is also possible. As a final test of the robustness of results, I estimate the equations for cycles and targeting, in both levels and changes, using the methodology developed in Arellano Bond (1991). A significant disadvantage of Arellano-Bond in this context is the short length of the panel: the standard model with one lag, using changes as the dependent variable, reduces the effective sample size to five years of data.

The results for levels are reported in Tables A4 and A6, and the results for changes are presented in Tables A5 and A7. One lag of the dependent variable and robust standard errors are used. A necessary condition for the validity of the estimator is that there is no second-order serial correlation in the first-differenced error terms. The p-value of a test that the average serial correlation in second-order residuals is zero is given in the right most columns of Tables 4 and 5, and at the bottom of Tables A6 and A7. The null of no serial correlation cannot be rejected in 30 of the 34 regressions presented in the four tables. The Arellano Bond estimates are very similar to standard panel results, although not always as statistically significant. However, the results for the key equations, 3 and 8 are very similar to the preferred specification.

8 Data Appendix

The unit of observation throughout the study varies. Section 4 uses credit and political data at the district level. The most comprehensive sample includes data from 412 districts, located in 19 states, over the period 1992-1999. Private sector banks do not operation in all districts in India, thus regressions involving private sector banks may have fewer observations.

Credit data come from several sources. Agricultural credit and total credit for the period 1992-1999 are from the Reserve Bank of India's "Basic Statistical Returns-1," published in "Bank-

ing Statistics." These numbers are also aggregated to form the state level agricultural data used in section 5.2. Aggregated data used for estimates of deposit and credit growth over the period 1981-2000 are from the Reserve Bank of India, "Quarterly Handout: Basic Statistical Returns-7."

Rainfall data are from "Terrestrial Air Temperature and Precipitation: Monthly and Annual Time Series (1950-99)," collected by Cort Willmott and Kenji Matsuura, University of Delaware Center for Climatic Research. The data were matched to the centroid of each Indian district using GIS software.

Elections Data are from the Election Commission of India publications. Data for elections in 22 states, between 1985 and 1999. Constituencies were matched to districts using information from the Indian Elections Commission, "Delimitation of parliamentary and assembly constituencies order, 1976." Coalitions data, where necessary, were collected from online searches of the Lexis-Nexis database.

Bank Branch Data are from the Reserve Bank of India, Directory of Commercial Bank Offices in India 1800-2000 (Volume 1), Mumbai. These data include the opening (and closing) date of every bank branch in India, as well as the address of the branch.

Output Data Data on net state domestic product, from 1992-1999 are from the Planning Commission of India. Data on village level outcomes are from the "Primary Census Abstracts" of the 1991 Villages were manually matched by village name, Teshil name, and state name, to villages in the Bank Branch data set.

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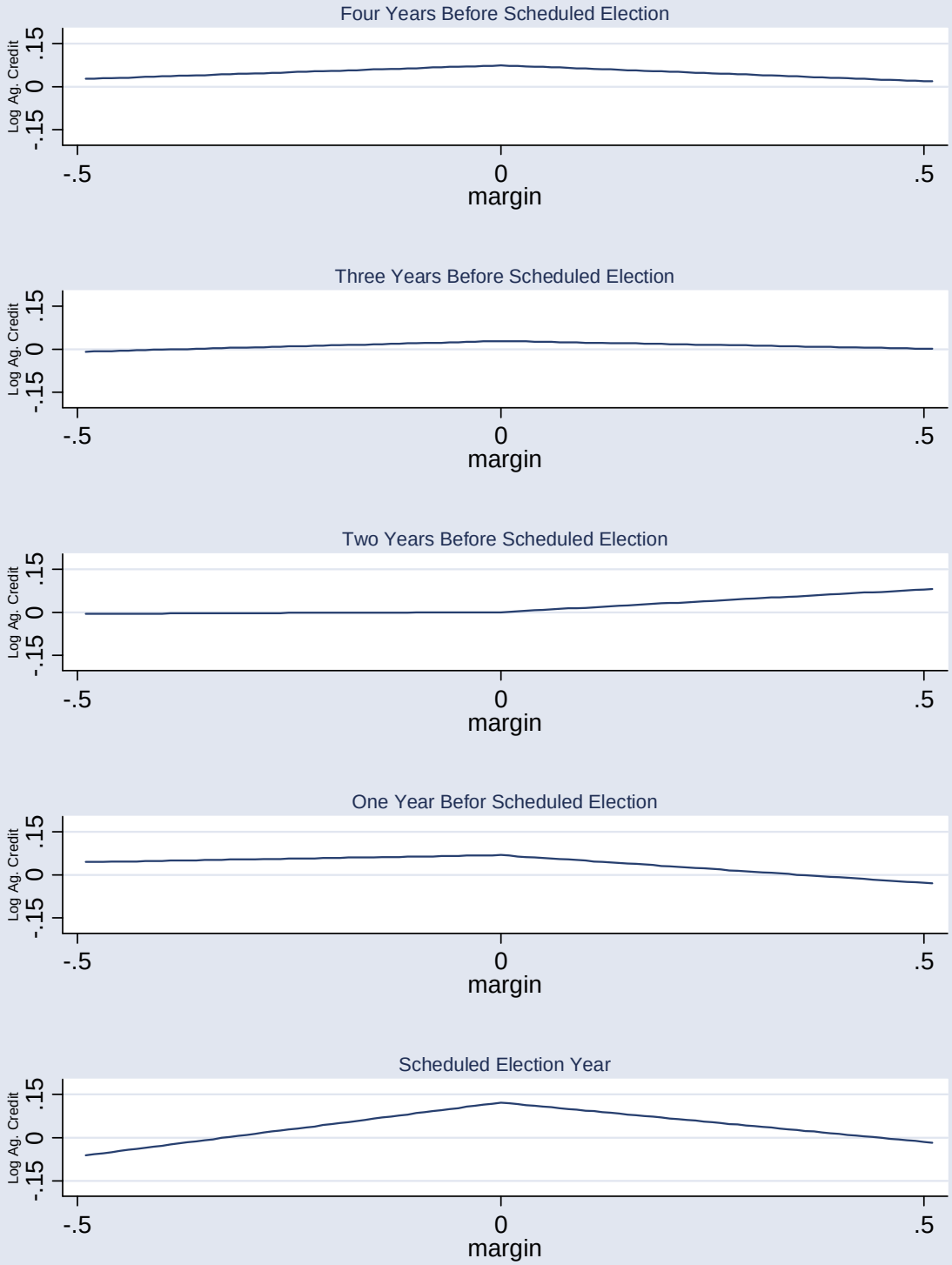
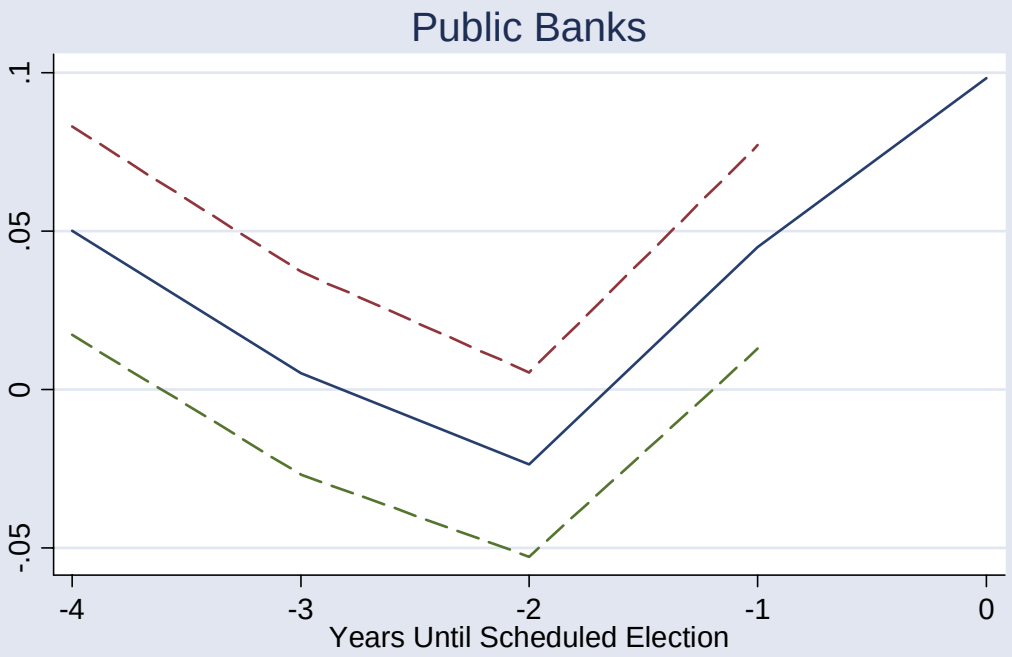
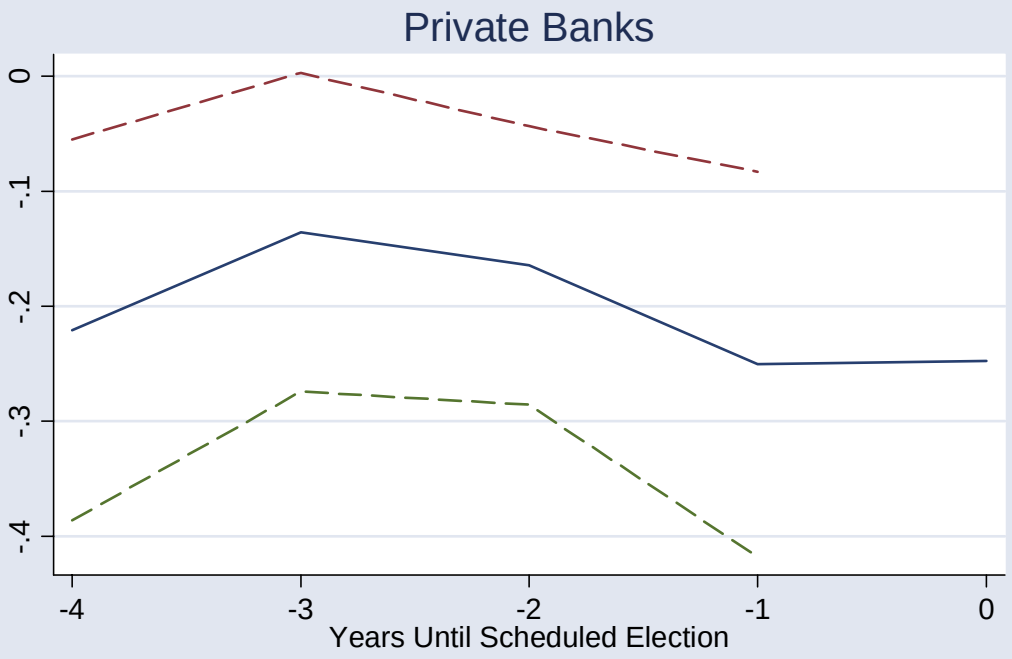
Figure 1: Targeted Lending Levels Over the Election Cycle



Note: The panels in the figure graph the predicted relationship between agricultural credit levels from public sector banks and political support of the state majority party. Each panel gives the relationship for a different year in the electoral cycle.

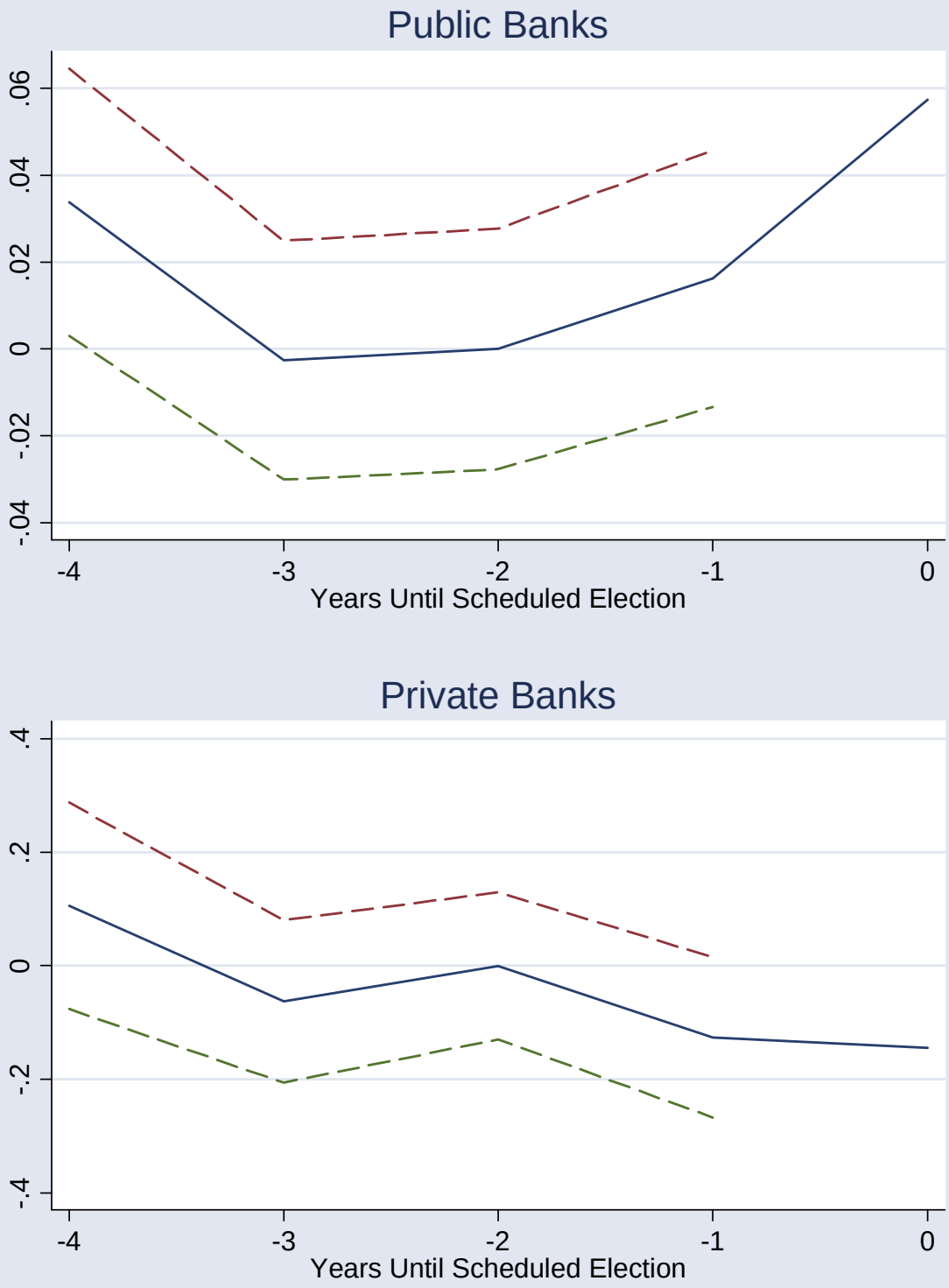
Figure 2: Cycles in Level of Credit, Swing District





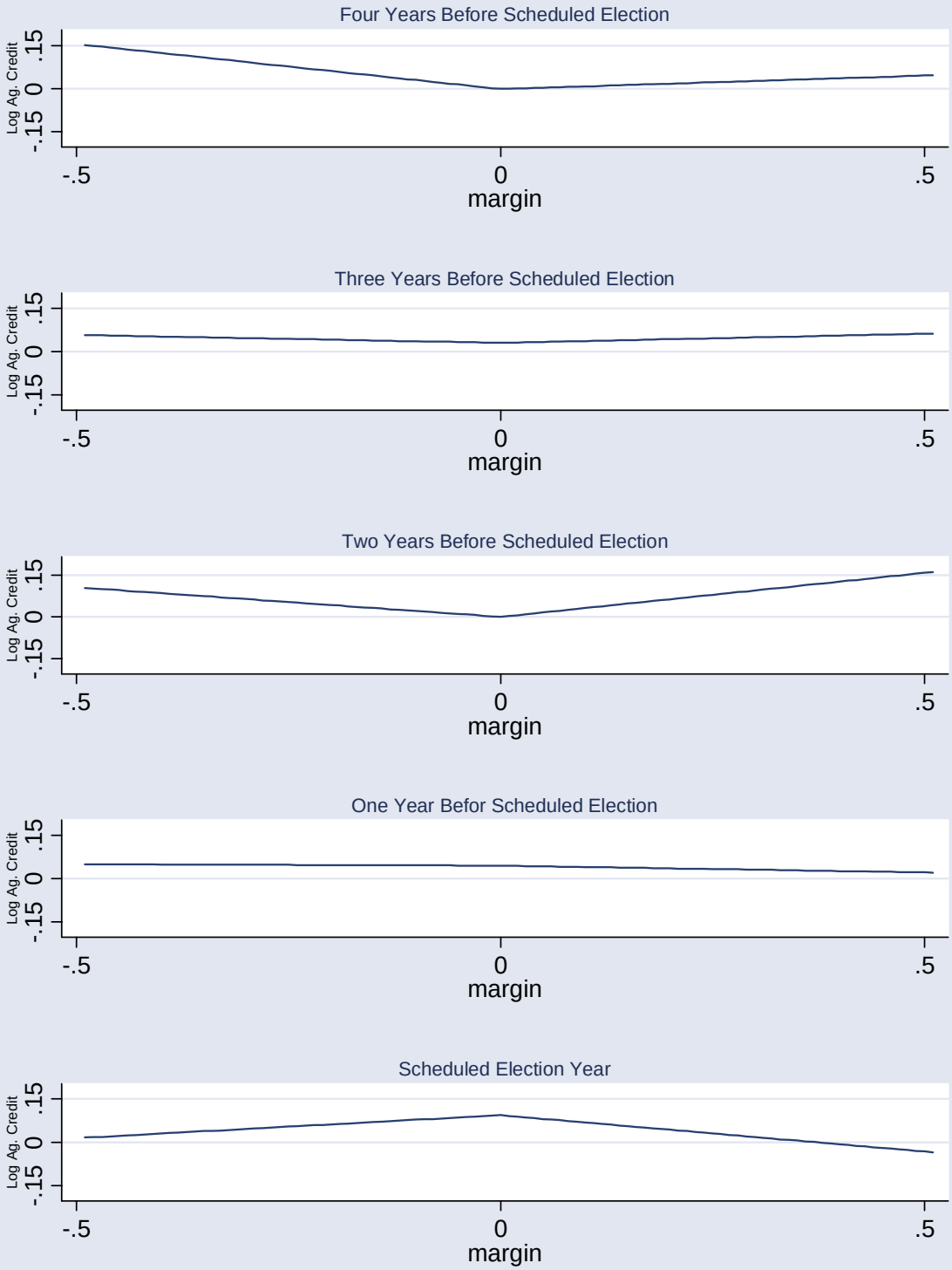
Note: Predicted agricultural credit for a notional district in which the margin of victory in the previous election was zero. Dotted lines give the 95 percent confidence interval.

Figure 3: Cycles in Level of Credit, Non-Swing District



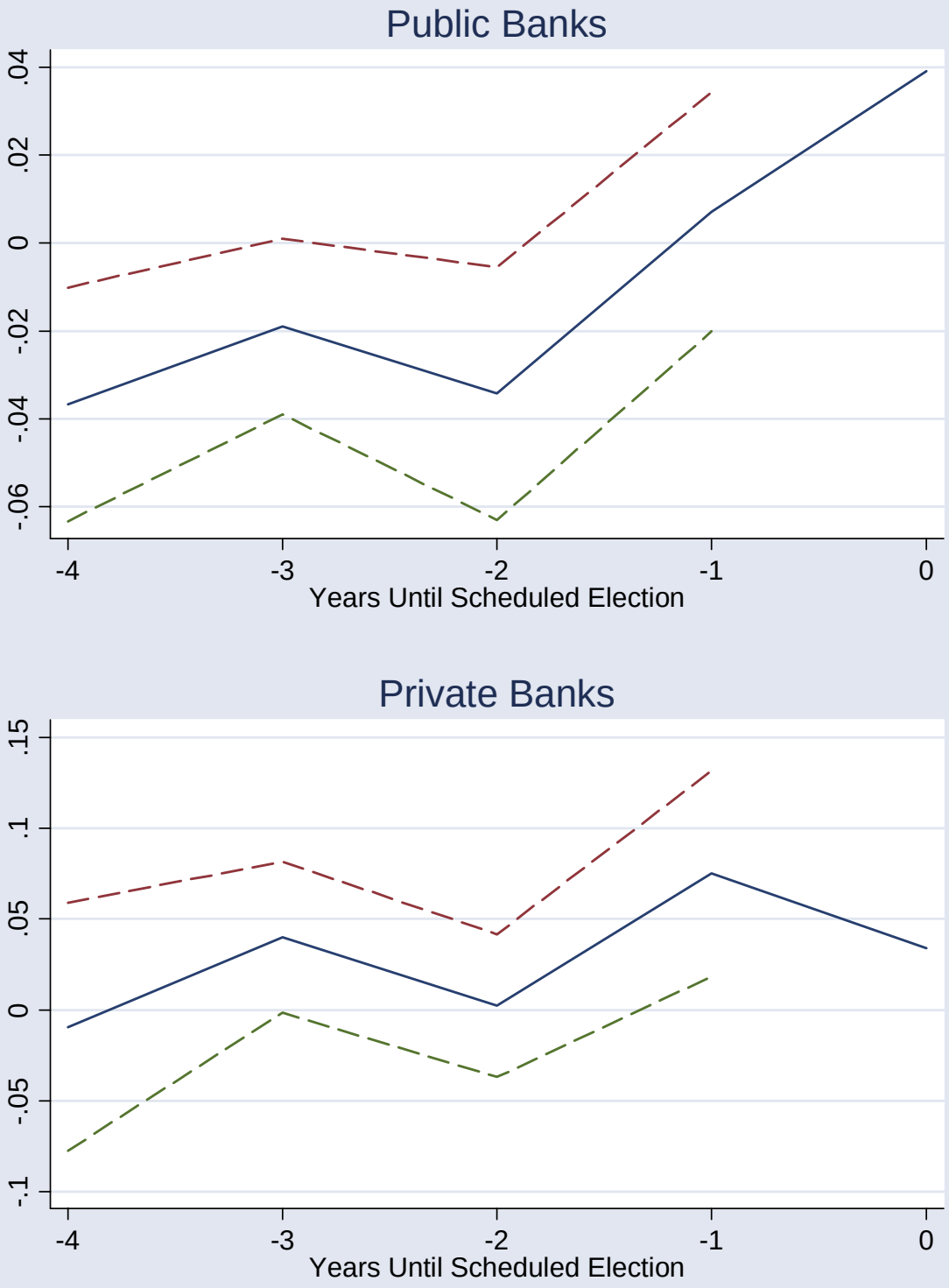
Note: Predicted agricultural credit for a notional district in which the margin of victory in the previous election was zero. Dotted lines give the 95 percent confidence interval.

Figure 4: Targeted Lending Growth Over the Election Cycle



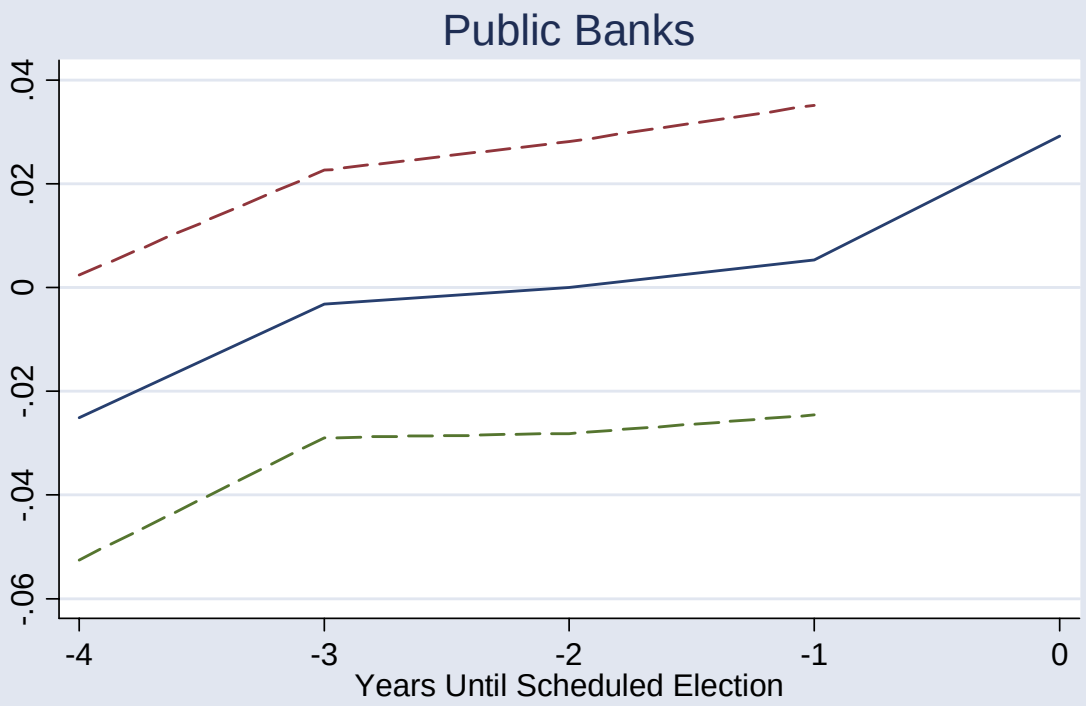
Note: The panels in the figure graph the predicted relationship between agricultural credit growth from public sector banks and political support of the state majority party. Each panel gives the relationship for a different year in the electoral cycle.

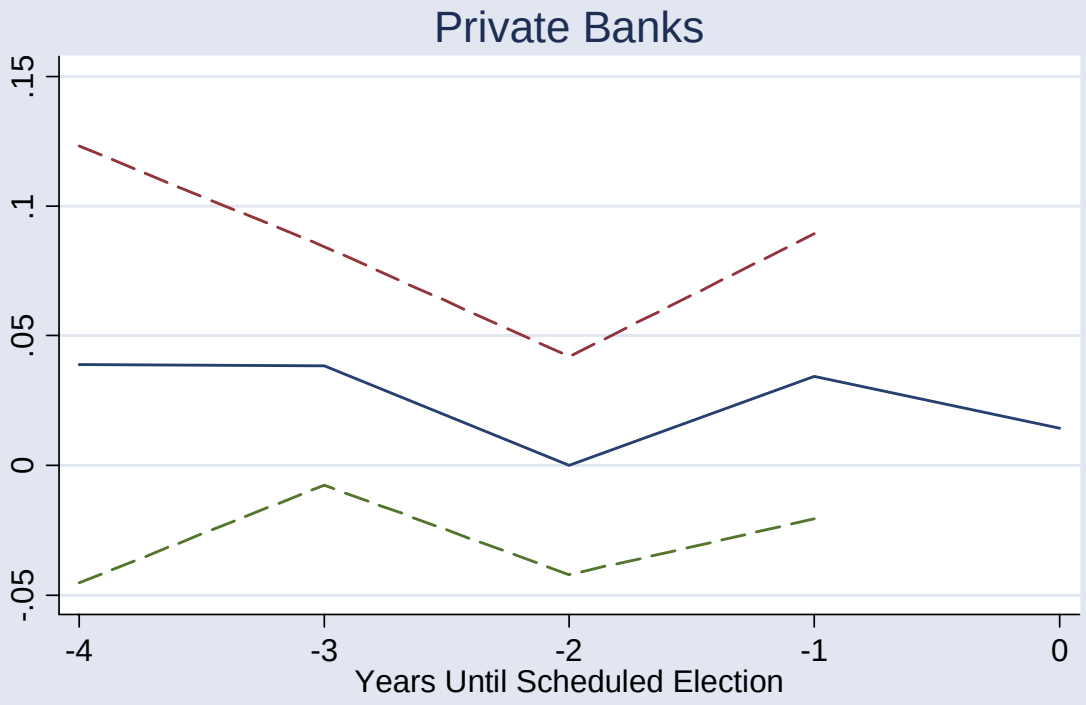
Figure 5: Cycles in Credit Growth, Swing District



Note: Predicted change in agricultural credit for a notional district in which the margin of victory in the previous election was zero. Dotted lines give the 95 percent confidence interval.

Figure 6: Cycles in Credit Growth, Non-Swing District





Note: Predicted change in agricultural credit for a notional district in which the margin of victory in the previous election was zero. Dotted lines give the 95 percent confidence interval.

Table 1: Summary Statistics for Political Lending

	Mean	Std. Dev	N
Panel A: Summary Statistics for Lending Cycle Regressions (19 states)			
Credit Variables			
Log Real Credit, All Banks	14.369	1.472	3296
Log Real Credit, Public Banks	14.181	1.481	3296
Log Real Credit, Private Banks	11.868	1.857	1761
.	.	.	
Log Real Agricultural Credit, All Banks	12.992	1.350	3296
Log Real Agricultural Credit, Public Banks	12.751	1.379	3296
Log Real Agricultural Credit, Private Banks	9.306	2.507	1640
Political Variables			
Election Year	0.207	0.405	3296
Scheduled Election in 4 Years	0.229	0.420	3296
Scheduled Election in 3 Years	0.251	0.433	3296
Scheduled Election in 2 Years	0.248	0.432	3296
Scheduled Election in 1 Years	0.152	0.359	3296
Scheduled Election Year	0.121	0.327	3296
Panel B: Summary Statistics for Targeted Redistribution Regressions (19 states)			
Credit Variables			
Log Real Credit, All Banks	14.475	1.402	2784
Log Real Credit, Public Banks	14.285	1.418	2784
Log Real Credit, Private Banks	11.930	1.881	1521
Log Real Agricultural Credit, All Banks	13.109	1.249	2784
Log Real Agricultural Credit, Public Banks	12.871	1.280	2784
Log Real Agricultural Credit, Private Banks	9.399	2.455	1425
Political Variables			
Election Year	0.210	0.407	2784
Scheduled Election in 4 Years	0.230	0.421	2784
Scheduled Election in 3 Years	0.249	0.433	2784
Scheduled Election in 2 Years	0.248	0.432	2784
Scheduled Election in 1 Years	0.151	0.358	2784
Scheduled Election Year			
Margin of Victory of Majority Party	-0.020	0.156	2784

Notes: The unit of observation is the district-year. The sample used to estimate political cycles only (Tables 4-5) contains data from 412 districts in 19 states, over the period 1992-1999, for a total of 3296 observations. Political data were not available for all districts, so the analysis which includes "Margin of Victory" contains data from 348 districts in 19 states, over the period 1992-1999.

The credit variables are the log value of the amount of credit issued by the specified group of banks (all credit, public credit only, or private credit.) Private banks are not present in all districts: thus, the number of observations is lower.
Margin of Victory is defined as the average share by which the majority party in the state won the district in the previous election. If there was no majority, then all parties in the ruling coalition are coded as "majority" party. Margin ranges from -1 to 1.
Scheduled Election in k years is a dummy indicating whether the next scheduled election will occur in k years.

Table 2: First Stage Estimation for Predicting Election Years

	Election	
Scheduled election Years	0.99	***
	(0.01)	
R^2	0.86	
N	3296	

Note: This table gives the first stage regression of Election Year on Scheduled Election Year. Scheduled Election Year takes the value of 1 if there was no election in the previous four years, and 0 otherwise. Standard errors are clustered by state-year.

Table 3: The Effect of Elections on Credit

		Total Credit			Agricultural Credit			Non-Agricultural Credit		
		All Banks	Public Banks	Private Banks	All Banks	Public Banks	Private Banks	All Banks	Public Banks	Private Banks
Panel A: OLS										
	Election Year	0.019	0.015	0.034	0.044 **	0.047 ***	-0.127	0.012	0.007	0.053
		(0.013)	(0.014)	(0.088)	(0.018)	(0.018)	(0.150)	(0.015)	(0.016)	(0.086)
	R^2	0.99	0.99	0.97	0.98	0.98	0.92	0.99	0.99	0.97
	N	3296	3296	1761	3296	3296	1640	3296	3296	1761
	States	152	152	128	152	152	128	152	152	128
Panel B: Reduced Form										
	Scheduled Election Year	0.029 **	0.031 **	0.040	0.046 **	0.060 ***	-0.021	0.021	0.020	0.061
		(0.014)	(0.014)	(0.057)	(0.018)	(0.020)	(0.093)	(0.016)	(0.015)	(0.059)
	R^2	0.99	0.99	0.97	0.98	0.98	0.92	0.99	0.99	0.97
	N	3296	3296	1761	3296	3296	1640	3296	3296	1761
	States	152	152	128	152	152	128	152	152	128
Panel C: Instrumental Variables										
	Election Year	0.028 **	0.031 **	0.039	0.046 ***	0.060 ***	-0.020	0.021	0.020	0.060
		(0.013)	(0.014)	(0.055)	(0.018)	(0.020)	(0.092)	(0.016)	(0.015)	(0.058)
	R^2									
	N	3296	3296	1761	3296	3296	1640	3296	3296	1761
	States	19	19	17	19	19	17	19	19	17

Notes: The dependent variable is annual change in log real levels of credit. Each column in a panel represents a single regression. In addition to the indicated dependent variable of interest. Panel A gives the OLS relationship between credit and a dummy for election year. Panel B gives the reduced form relationship between credit and scheduled election year. Panel C gives the instrumental variables estimate. In addition to the indicated dependent variable, each regression includes region-year fixed effects, and change in annual rainfall.

The unit of observation is district-year. There are data for 348 districts from 1992-1999, though private banks do not operate in all districts. Standard errors are clustered by state-year.

Table 4: Lending Cycles By Industry and Bank Ownership

	Years Until Next Scheduled Election			
	Four	Three	Two	One
Panel A: All Banks				
All Credit	-0.033 **	-0.029 *	-0.035 **	-0.009
	(0.016)	(0.015)	(0.015)	(0.017)
Agriculture	-0.023	-0.045 **	-0.061 ***	-0.022
	(0.023)	(0.021)	(0.021)	(0.028)
Non-Agricultural Credit	-0.029	-0.024	-0.026	0.004
	(0.018)	(0.016)	(0.017)	(0.020)
Panel B: Public Banks				
All Credit	-0.033 **	-0.030 *	-0.040 **	-0.011
	(0.016)	(0.016)	(0.016)	(0.017)
Agriculture	-0.032	-0.056 **	-0.081 ***	-0.034
	(0.026)	(0.025)	(0.022)	(0.028)
Non-Agricultural Credit	-0.026	-0.022	-0.028	0.004
	(0.018)	(0.016)	(0.017)	(0.020)
Panel C: Private Banks				
All Credit	0.022	-0.033	-0.027	-0.156
	(0.105)	(0.094)	(0.062)	(0.096)
Agriculture	0.079	0.035	0.014	-0.003
	(0.152)	(0.130)	(0.100)	(0.168)
Non-Agricultural Credit	-0.001	-0.058	-0.045	-0.173
	(0.105)	(0.097)	(0.064)	(0.096)

Notes: Each row represents a single regression. The unit of observation is a district-year. The dependent variable is log bank credit in different sectors. The independent variables of interest are a set of dummy variables indicating the number of years until the next scheduled election. Scheduled election year is the omitted category. Panels A and B contain data from 348 districts. Panel C contains data from approximately 180 districts. Data are from 1992-1999.

Standard errors are clustered by state-year.

Table 5, Panel A: Targeted Levels of Credit Over Time and Across Districts

Panel A: Public Banks	(1)	(2)	(3)	(4)
Cycle Dummies:			Unrestricted Margin and Unrestricted Interactions	Abs(Margin) and Abs(Interactions)
Number of Years Until Next Election	Baseline	With Margin		
Four	-0.02	-0.02	-0.05	-0.10 **
	(0.03)	(0.03)	(0.03)	(0.05)
Three	-0.06 **	-0.06 **	-0.09 ***	-0.16 ***
	(0.03)	(0.03)	(0.03)	(0.04)
Two	-0.07 ***	-0.07 ***	-0.12 ***	-0.21 ***
	(0.02)	(0.02)	(0.03)	(0.04)
One	-0.02	-0.02	-0.05 *	-0.06
	(0.03)	(0.03)	(0.03)	(0.05)
Margin of Victory		0.001		
		(0.046)		
Abs(Margin of Victory)				-0.447 ***
				(0.103)
Positive Margin of Victory			-0.272 ***	
			(0.084)	
Negative Margin of Victory			0.373 ***	
			(0.112)	
Positive Margin * Cycle Dummy				
Positive Margin *			0.163	
Four Years until Election			(0.117)	
Positive Margin *			0.220	
Three Years until Election			(0.161)	
Positive Margin *			0.430 **	
Two Years until Election			(0.199)	
Positive Margin *			0.080	
One Year until Election			(0.157)	
Negative Margin * Cycle Dummy				
Negative Margin *			-0.277 **	
Four Years until Election			0.135	
Negative Margin *			-0.298 **	
Three Years until Election			0.133	
Negative Margin *			-0.361 ***	
Two Years until Election			0.136	
Negative Margin *			-0.324 **	
One Year until Election			0.153	
Absolute Margin * Cycle Dummy				
Absolute(Margin) *				0.329 **
Four Years until Election				(0.137)
Absolute(Margin) *				0.462 ***
Three Years until Election				(0.147)
Absolute(Margin) *				0.611 ***
Two Years until Election				(0.165)
Absolute(Margin) *				0.149
One Year until Election				(0.168)
R^2	0.98	0.98	0.98	0.98
N	2784	2784	2784	2784
Number of states	19	19	19	19

Notes: Each column represents a separate regression. Log agricultural credit is the dependent variable. Panel A gives the results for public sector banks. Panel B gives the results for private sector banks. The independent variables of interest are a set of dummy variables indicating the number of years until the next scheduled election, and the average margin by which candidates from the party (or coalition) currently in power in the state won (or lost) in the specific district. Each regression also includes district and region-year fixed effects, and average annual rainfall in the district. Standard errors are clustered by state-year.

Table 5, Panel B: Targeted Levels of Credit Over Time and Across Districts

Panel B: Private Banks	(1)	(2)	(3)	(4)
Cycle Dummies:				
Number of Years Until Next Election	Baseline	With Margin	Unrestricted Margin and Unrestricted Interactions	Abs(Margin) and Abs(Interactions)
Four	0.08	0.07	0.03	-0.13
	(0.16)	(0.15)	(0.17)	(0.29)
Three	0.01	0.04	0.11	0.09
	(0.14)	(0.12)	(0.14)	(0.23)
Two	0.02	0.02	0.08	0.12
	(0.11)	(0.10)	(0.12)	(0.28)
One	-0.06	-0.08	0.00	-0.05
	(0.18)	(0.17)	(0.17)	(0.28)
Margin of Victory		1.242 ***		
		(0.297)		
Abs(Margin of Victory)				-1.480 *
				(0.872)
Positive Margin of Victory			0.688	
			(0.617)	
Negative Margin of Victory			0.355	
			(0.862)	
Positive Margin * Cycle Dummy				
Positive Margin *			1.487	
Four Years until Election			(1.047)	
Positive Margin *			-0.201	
Three Years until Election			(0.913)	
Positive Margin *			0.410	
Two Years until Election			(0.931)	
Positive Margin *			0.142	
One Year until Election			(0.936)	
Negative Margin * Cycle Dummy				
Positive Margin *			0.687	
Four Years until Election			(0.905)	
Margin *			1.074	
Three Years until Election			(0.882)	
Margin *			1.340	
Two Years until Election			(0.936)	
Margin *			1.611	
One Year until Election			(1.410)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) *				1.075
Four Years until Election				(0.967)
Absolute(Margin) *				-0.148
Three Years until Election				(1.068)
Absolute(Margin) *				-0.318
Two Years until Election				(1.114)
Absolute(Margin) *				-0.111
One Year until Election				(1.001)
R^2	0.91	0.92	0.92	0.91
N	1425	1425	1425	1425
Number of states	15	15	15	15

Notes: See Panel A for notes.

Table 6: Summary Statistics for Cost of Political Lending

	Level			Changes			
Panel A: Credit Variables	**Mean**	**Std. Dev**	**N**	**Mean**	**Std. Dev**	**N**	**Number of States**
Percentage of Agricultural Loans Non-Performing, All Banks	0.191	0.142	3273	0.003	0.113	2858	19
Percentage of Agricultural Loans Non-Performing, Public Banks	0.200	0.146	3273	0.004	0.119	2858	19
Percentage of Agricultural Loans Non-Performing, Private Banks	0.153	0.220	1439	0.011	0.161	1218	13
.	.		.	.			
Share of Agricultural Credit Non-Performing, All Banks	0.184	0.146	3273	0.006	0.127	2858	19
Share of Agricultural Credit Non-Performing, Public Banks	0.191	0.152	3273	0.008	0.135	2858	19
Share of Agricultural Credit Non-Performing, Private Banks	0.149	0.235	1439	0.012	0.180	1218	13
Panel B: Agricultural Credit and Output							
Log Real Agricultural Credit, All Banks	16.87	0.67	104	0.000	0.092	91	13
Log Real Agricultural Credit, Public Banks	16.68	0.68	104	-0.011	0.098	91	13
Log real Agricultural Credit, Private Banks	11.53	3.41	104	0.182	0.829	91	13
Change in Log Real Agricultural State GSP	9.65	0.43	101	0.021	0.117	101	13

Panel C: Village-Level Outcomes	**Mean**	**Std. Dev.**	**N**	**Number of States**
Share of Credit in 1992 from Public Sector Banks	0.483	0.492	1581	18
Average Annual Growth Rate, Credit, 1981-1990	0.167	0.084	1561	18
Average Annual Growth Rate, Credit, 1991-1999	0.122	0.074	1565	18
Share of Credit to Agriculture, 1992	0.378	0.235	1581	18
Share of Agricultural Credit that is Non-Performing, 1992	0.242	0.307	857	15
Towns has a Tubewell, 1991	0.307	0.462	716	12
Share of Land Irrigated, 1991	0.437	0.390	645	10
Literacy Rate, 1991	0.155	0.031	731	15
Fertility Rate, 1991	0.487	0.142	731	15

Notes: This table presents summary statistics for the variables that will be used in Tables 7-10.
Panel A gives credit outcomes at the district level, for aggregate credit, public banks, and private banks. A loan is designated as non-performing if the borrower is at least six months late in repayment. The unit of observation is the district-year.
Panel B gives state-level values for levels and changes in agricultural credit and agricultural output.
Panel C gives summary statistics for the 1,581 villages used in the Table 12. Not all villages could be matched to the census.

Table 7: Lending Cycles and Non-Performing Loans

	Years Until Next Scheduled Election			
	Four	Three	Two	One
Panel A: All Banks				
Bad Agricultural Loans (Count)	-0.014	-0.011	0.005	-0.002
	(0.011)	(0.009)	(0.011)	(0.011)
Bad Agricultural Credit (Share)	-0.023 *	-0.030 **	-0.007	-0.016
	(0.012)	(0.011)	(0.011)	(0.012)
Delta Bad Agricultural Loans (Count)	-0.022	-0.002	0.011	-0.010
	(0.019)	(0.009)	(0.019)	(0.011)
Delta Bad Agricultural Credit (Share)	-0.037 *	-0.030 **	0.001	-0.019
	(0.020)	(0.014)	(0.019)	(0.014)
Panel B: Public Banks				
Bad Agricultural Loans (Count)	-0.017	-0.013	0.000	0.001
	(0.011)	(0.009)	(0.011)	(0.011)
Bad Agricultural Credit (Share)	-0.024 **	-0.032 **	-0.010	-0.009
	(0.012)	(0.010)	(0.011)	(0.013)
Change in Bad Agricultural Loans (Count)	-0.026	-0.001	0.007	-0.007
	(0.021)	(0.010)	(0.019)	(0.013)
Change in Bad Agricultural Credit (Share)	-0.036 *	-0.026 *	0.004	-0.010
	(0.021)	(0.015)	(0.019)	(0.016)
Panel C: Private Banks				
Bad Agricultural Loans (Count)	0.001	-0.005	0.013	-0.040 *
	(0.022)	(0.018)	(0.019)	(0.023)
Bad Agricultural Credit (Share)	-0.008	-0.020	-0.016	-0.050 **
	(0.023)	(0.017)	(0.019)	(0.023)
Change in Bad Agricultural Loans (Count)	0.000	-0.029	-0.012	-0.056
	(0.021)	(0.038)	(0.011)	(0.046)
Change in Bad Agricultural Credit (Share)	-0.019	-0.049	-0.037 ***	-0.063 *
	(0.018)	(0.030)	(0.006)	(0.034)

Notes: Each row in represents a single regression. The unit of observation is a district-year. The dependent variable is the share of non-performing loans, or the share of non-performing credit, measured in levels or changes. The independent variables of interest are a set of dummy variables indicating the number of years until the next scheduled election. Scheduled election year is the omitted category. Panels A and B contain data from 412 districts. Panel C contains data from approximately 180 districts. Level estimates include data from 1992-1999, while changes estimates cover the period 1993-1999.
Standard errors are clustered by state-year.

Table 8: Agricultural Credit and Agricultural Output

	Years Until Next Scheduled Election			
	Four	Three	Two	One
Panel A: Real Log Agricultural Credit Growth				
Agricultural Credit Growth, All Banks	-0.016	-0.035 *	-0.038 ***	-0.028 ***
	(0.018)	(0.019)	(0.013)	(0.020)
Agricultural Credit Growth, Public Banks	-0.029	-0.046 **	-0.052 ***	-0.042 **
	(0.021)	(0.022)	(0.013)	(0.021)
Agricultural Credit Growth, Private Banks	-0.386	-0.142	-0.212	-0.202
	(0.570)	(0.286)	(0.368)	(0.233)
Panel B: Real Log Agricultural Output Growth, Reduced Form				
Real Log Agricultural Output Growth	0.023	0.072	0.046	0.004
	(0.067)	(0.072)	(0.052)	(0.021)

Notes: Each row represents a single regression. The dependent variable for the regressions in Panel A is log real growth in agricultural credit, while in Panel B the dependent variable is log growth in state agricultural product. Data are available for 13 states for the period 1992-1999. The dependent variables of interest are dummy variables indicating the number of years until the next scheduled election. The omitted category is election year.

Panel C: IV Estimates of the Effect of Credit on Agricultural Output Growth

	Output Growth	Output Growth	Output Growth
All Bank Credit	-0.42		
	(0.93)		
Public Bank Credit		-0.15	
		(1.11)	
Private Bank Credit			0.08
			(0.20)

Notes: Panel C presents the Instrumental Variables estimates of the effect of credit on agricultural output. Each column represents a regression. Dummy variables indicating the number of years until the next scheduled election serve as an instrument for credit. The first stage is given in Panel A. Data are available for 14 states, for the period 1993-1999.

Table 9: Bank Nationalization and Credit

	First Stage	Credit Growth		Agricultural Credit	
	Share of Credit in 1992 from Public Banks	Annual Rate, 1981-1990	Annual Rate, 1991-2000	Ag. Credit / Total Credit	Share of Agriculture Credit Non-Performing
	(1)	(2)	(3)	(4)	(5)
Nationalized	1.00 ***	0.11 ***	-0.04	0.26 **	0.182 ***
	(0.02)	(0.03)	(0.03)	(0.11)	0.046
K	0.97	-1.85	-0.72	-9.10	-0.55
	(3.30)	(3.25)	(3.15)	(6.06)	(3.89)
K^2	-0.08	0.15	0.05	0.70	0.00
	(0.23)	(0.24)	(0.23)	(0.44)	(0.28)
K^3	1.97E-03	0.00	0.00	-0.02 *	-0.03
	(5.46E-03)	(0.01)	(0.01)	(0.01)	(0.01)
R^2	0.97	0.43	0.35	0.58	0.20
N	1513	1512	1513	1513	857

Notes: This table presents the effect of bank nationalization on credit outcomes, for a sample of 1,513 villages that had a private bank branch prior to the 1980 nationalization. "Nationalized" is a dummy variable indicating whether the branch in a village was nationalized. K, K^2, and K^3 are a polynomial in the log size of the parent bank, as measured by India-wide deposits in 1980. All regressions include district fixed effects, and a third-degree polynomial in the log amount of deposits in each village in 1981.

Column 1 gives the relationship between the share of credit granted by public sector banks in a village in 1992 and whether the branch in that village was nationalized in 1980. Columns (2) and (3) estimate the effect of nationalization on the average annual rate of credit for the periods indicated. Columns (4) and (5) estimate the effect of nationalization on the share of credit in each village that went to agricultural borrowers, and then share of agricultural credit that is non-performing, respectively. Standard errors are clustered by bank.

Table 10: Nationalization and Village Outcomes

	Agricultural Outcomes		Falsification	
	Share of Towns with Tubewell	Fraction of Land Irrigated	Literacy Rate	Fertility Rate
	(1)	(2)	(3)	(4)
Nationalized	0.00	0.00	0.02	-0.01
	(0.08)	(0.10)	(0.05)	(0.01)
K	116.98	80.14	-8.97	2.91
	(164.40)	(62.50)	(45.97)	(10.93)
K^2	-7.87	-5.42	0.63	-0.19
	(11.12)	(4.19)	(3.11)	(0.74)
K^3	0.18	0.12	-0.01	0.00
	(0.25)	(0.09)	(0.07)	(0.02)
R^2	0.38	0.79	0.75	0.64
N	701	636	716	716

Notes: This table presents the effect of bank nationalization on real outcomes, for a sample of approximately 700 villages that had a private bank branch prior to the 1980 nationalization. "Nationalized" is a dummy variable indicating whether the branch in a village was nationalized. K, K^2, and K^3 are a polynomial in the log size of the parent bank, as measured by India-wide deposits in 1980. All regressions include district fixed effects, and a third-degree polynomial in the log amount of deposits in each village in 1981.

Column 1 gives the relationship between the whether a town had a tubewell in 1991and whether the branch in that village was nationalized in 1980. Columns (2) and (3), and (4) estimate the effect of nationalization on the fraction of land irrigated, the literacy rate, and the fertility rate, respectively. Standard errors are clustered at the bank level.

Appendix Table A1: Time Series Properties

	Test For Serial Correlation		Test for Non-Stationarity	
	(1)	(2)	(3)	(4)
	Estimated ρ	Ho: No serial correlation:	"T-Star"	P-Value
Log Real Credit, Levels				
Log Real Credit, All Banks	0.033	0.000	-15.61	0.00
Log Real Credit, Public Banks	-0.027	0.000	-18.27	0.00
Log Real Credit, Private Banks	0.084	0.000	-11.85	0.00
	.			
Log Real Agricultural Credit, All Banks	-0.177	0.000	-23.98	0.00
Log Real Agricultural Credit, Public Banks	-0.197	0.000	-20.14	0.00
Log Real Agricultural Credit, Private Banks	-0.323	0.014	-14.79	0.00
Log Real Credit Growth				
Log Real Credit Growth, All Banks	-0.52	0.53		
Log Real Credit Growth, Public Banks	-0.53	0.41		
Log Real Credit Growth, Private Banks	-0.58	0.17		
Log Real Agricultural Credit Growth, All Banks	-0.57	0.16		
Log Real Agricultural Credit Growth, Public Banks	-0.57	0.14		
Log Real Agricultural Credit Growth, Private Banks	-0.70	0.01		

Notes:

Columns 1 and 2 report results from a test of serial correlation in the dependent variables of interest (Wooldridge, 2002). Column 1 presents the estimate of the auto-correlation parameter of the residuals from a regression of the difference of the dependent variable of interest. Under the null of no serial correlation in the series of interest, the estimated auto-regressive parameter, ρ, from the time-demeaned residuals of the fixed-effects regression is equal to -.5 Column 2 reports the p-value of the test of the hypothesis ρ=-.5.

Columns 3 and 4 report results from the Levin, Chin, and Chu (2002) unit root test for panel data. Under the null of non-stationarity, the "T-star" statistic, given in column (3), is asymptotically normal. The p-value of a test of the null hypothesis that the series is non-stationary is reported in column (4).
The aggregate and public bank credit panels contain 412 districts, while the private bank credit panels contain approximately 180 districts. The are 8 observations per district for the level variables, and 7 per district for the growth variables.

Appendix Table A2: Lending Cycles in Credit Growth By Industry and Bank Ownership

	Years Until Next Scheduled Election			
	Four	Three	Two	One
Panel A. All Banks				
All Credit	-0.025 **	-0.017	-0.019	-0.012
	(0.012)	(0.011)	(0.013)	(0.014)
Agriculture	-0.075 ***	-0.050 **	-0.061 **	-0.059 *
	(0.025)	(0.022)	(0.025)	(0.034)
Non-Agricultural Credit	-0.001	-0.007	-0.004	0.011
	(0.014)	(0.012)	(0.014)	(0.016)
Panel B. Public				
All Credit	-0.021 *	-0.016	-0.019	-0.009
	(0.012)	(0.011)	(0.013)	(0.014)
Agriculture	-0.081 ***	-0.057 **	-0.073 ***	-0.056
	(0.027)	(0.025)	(0.027)	(0.034)
Non-Agricultural Credit	0.004	-0.008	-0.003	0.012
	(0.015)	(0.013)	(0.015)	(0.016)
Panel C. Private				
All Credit	-0.087	-0.078	-0.089	-0.120 *
	(0.068)	(0.067)	(0.056)	(0.068)
Agriculture	0.080	0.149	0.045	0.070
	(0.146)	(0.091)	(0.092)	(0.125)
Non-Agricultural Credit	-0.116	-0.106	-0.099 *	-0.138
	(0.071)	(0.076)	(0.059)	(0.075)

Notes: Each row represents a single regression. The unit of observation is a district-year. The dependent variable is change in log credit in different sectors. The independent variables of interest are a set of dummy variables indicating the number of years until the next scheduled election. Each regression includes region-year fixed effects.

Standard errors are clustered by state-year.

Appendix Table A3, Panel A: Targeted Credit Growth Over Time and Across Districts

Panel A: Public Banks	(1)	(2)	(3)	(4)
Number of Years Until Next Election	Baseline	With Margin	Unrestricted Margin and Unrestricted Interactions	With Abs(Margin) and Abs(Interactions)
Four	-0.04 *	-0.04 *	-0.08 ***	-0.12 ***
	(0.02)	(0.02)	(0.02)	(0.04)
Three	-0.04 **	-0.04 **	-0.06 ***	-0.09 ***
	(0.01)	(0.02)	(0.02)	(0.03)
Two	-0.03 *	-0.03 *	-0.07 ***	-0.09 ***
	(0.02)	(0.02)	(0.02)	(0.03)
One	-0.01	-0.01	-0.03	-0.02
	(0.02)	(0.02)	(0.03)	(0.04)
Margin of Victory		0.013		
		(0.020)		
Abs(Margin of Victory)				-0.189 *
				(0.105)
Positive Margin of Victory			-0.066	
			(0.105)	
Negative Margin of Victory			0.221 ***	
			(0.079)	
Positive Margin * Cycle Dummy				
Positive Margin *			0.143	
Four Years until Election			(0.153)	
Positive Margin *			0.171	
Three Years until Election			(0.148)	
Positive Margin *			0.294	
Two Years until Election			(0.200)	
Positive Margin *			0.054	
One Year until Election			(0.171)	
Negative Margin * Cycle Dummy				
Negative Margin *			-0.412 ***	
Four Years until Election			0.123	
Negative Margin *			-0.187	
Three Years until Election			0.126	
Negative Margin *			-0.376 ***	
Two Years until Election			0.109	
Negative Margin *			-0.205 *	
One Year until Election			0.112	
Absolute Margin * Cycle Dummy				
Absolute(Margin) *				0.386 **
Four Years until Election				(0.169)
Absolute(Margin) *				0.275 **
Three Years until Election				(0.128)
Absolute(Margin) *				0.285 *
Two Years until Election				(0.155)
Absolute(Margin) *				0.007
One Year until Election				(0.135)
N	0.11	0.11	0.12	0.12
R^2	2429	2429	2429	2429
Number of states	19	19	19	19

Notes: Each column represents a separate regression. Annual change in log agricultural credit is the dependent variable. Panel A gives the results for public sector banks. Panel B gives the results for private sector banks. The independent variables of interest are a set of dummy variables indicating the number of years until the next scheduled election, and the average margin by which candidates from the party (or coalition) currently in power in the state won (or lost) in the specific district. Each regression also includes district and region-year fixed effects, and average annual rainfall in the district. Standard errors are clustered by state-year.

Appendix Table A3, Panel B: Targeted Credit Growth Over Time and Across Districts

Panel B: Private Banks	(1)	(2)	(3)	(4)
Number of Years Until Next Election	Baseline	With Margin	Unrestricted Margin and Unrestricted Interactions	With Abs(Margin) and Abs(Interactions)
Four	-0.01	-0.01	-0.04	-0.10
	(0.07)	(0.07)	(0.08)	(0.10)
Three	0.04	0.04	0.01	0.04
	(0.03)	(0.03)	(0.03)	(0.04)
Two	-0.02	-0.02	-0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.07)
One	0.04	0.04	0.04	0.13 *
	(0.03)	(0.03)	(0.03)	(0.07)
Margin of Victory		-0.001		
		(0.044)		
Abs(Margin of Victory)				0.023
				(0.184)
Positive Margin of Victory			-0.131	
			(0.247)	
Negative Margin of Victory			0.222	
			(0.145)	
Positive Margin * Cycle Dummy				
Positive Margin *			0.452	
Four Years until Election			(0.543)	
Positive Margin *			0.120	
Three Years until Election			(0.269)	
Positive Margin *			0.115	
Two Years until Election			(0.203)	
Positive Margin *			-0.141	
One Year until Election			(0.396)	
Negative Margin * Cycle Dummy				
Positive Margin *			-0.316	
Four Years until Election			(0.230)	
Margin *			-0.363	
Three Years until Election			(0.255)	
Margin *			-0.155	
Two Years until Election			(0.293)	
Margin *			-0.226	
One Year until Election			(0.352)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) *				0.446
Four Years until Election				(0.324)
Absolute(Margin) *				0.044
Three Years until Election				(0.187)
Absolute(Margin) *				-0.224
Two Years until Election				(0.342)
Absolute(Margin) *				-0.381
One Year until Election				(0.302)
N	0.13	0.13	0.13	0.13
R^2	1130	1130	1130	1130
Number of states	15	15	15	15

See Panel A for notes.

Appendix Table A4: Agricultural Credit Growth and Agricultural Output Growth

	Years Until Next Scheduled Election			
	Four	Three	Two	One
Panel A. Real Log Agricultural Credit Growth				
Agricultural Credit Growth, All Banks	-0.073 ***	-0.048 *	-0.042 **	-0.062 ***
	(0.016)	(0.026)	(0.018)	(0.024)
Agricultural Credit Growth, Public Banks	-0.088 ***	-0.056 *	-0.055 ***	-0.064 **
	(0.017)	(0.030)	(0.019)	(0.028)
Agricultural Credit Growth, Private Banks	0.160	0.074	0.142	-0.119
	(0.183)	(0.162)	(0.188)	(0.152)
Panel B. Real Log Agricultural Output Growth, Reduced Form				
Real Log Agricultural Output Growth	0.048	0.075	-0.022	0.037
	(0.057)	(0.051)	(0.057)	(0.063)

Notes: Each row represents a single regression. The dependent variable for the regressions in Panel A is log real growth in agricultural credit, while in Panel B the dependent variable is log growth in state agricultural product. Data are available for 13 states, for the period 1992-1999. The dependent variables of interest are dummy variables indicating the number of years until the next scheduled election. The omitted category is election year.

Panel C IV Estimates of the Effect of Credit on Agricultural Output Growth

	Output Growth	Output Growth	Output Growth
All Bank Credit	-0.95		
	(0.72)		
Public Bank Credit		-0.69	
		(0.70)	
Private Bank Credit			0.10
			(0.27)

Notes: Panel C presents the Instrumental Variables estimates of the effect of credit on agricultural output. Each column represents a regression. Dummy variables indicating the number of years until the next scheduled election serve as an instrument for credit. The first stage is given in Panel A. Data are available for 13 states, for the period 1993-1999.

Appendix Table A5: Arellano-Bond Estimates of Credit Level Cycles, By Industry and Bank Ownership

	Years Until Next Scheduled Election				Test of Second-Order Serial Correlation
	Four	Three	Two	One	
Panel A: All Banks					
All Credit	-0.014	-0.021 **	-0.021 **	-0.014	0.15
	(0.010)	(0.009)	(0.009)	(0.010)	
Agriculture	-0.048 **	-0.004	-0.035 **	-0.049 ***	0.33
	(0.019)	(0.017)	(0.017)	(0.019)	
Non-Agricultural Credit	0.000	-0.033 **	-0.027 **	0.002	0.07
	(0.011)	(0.009)	(0.011)	(0.010)	
Panel B: Public Banks					
All Credit	-0.010	-0.019 **	-0.021 **	-0.013	0.11
	(0.011)	(0.010)	(0.010)	(0.011)	
Agriculture	-0.055 **	-0.003	-0.043 **	-0.053 **	0.54
	(0.022)	(0.019)	(0.021)	(0.021)	
Non-Agricultural Credit	0.005	-0.033 **	-0.027 **	0.003	0.09
	(0.011)	(0.011)	(0.012)	(0.012)	
Panel C: Private Banks					
All Credit	-0.057	-0.052	-0.064	-0.077 *	0.19
	(0.056)	(0.039)	(0.053)	(0.044)	
Agriculture	0.098	0.091	0.093	0.016	0.62
	(0.096)	(0.121)	(0.102)	(0.101)	
Non-Agricultural Credit	-0.067	-0.082 **	-0.071	-0.099	0.12
	(0.061)	(0.041)	(0.051)	(0.047)	

Notes: Each row in represents a single regression. The unit of observation is a district-year. The dependent variable is log credit in different sectors. The independent variables of interest are set of dummy variables indicating the number of years until the next scheduled election. Scheduled election year is the omitted category. Panels A and B contain data from 412 districts. Panel C contains data from approximately 180 districts. Data are from 1993-1999. Estimation is conducted by means of the Arellano-Bond (1991) estimator, using one lag of the dependent variable. Column five gives the p-value of the test that the second-order error terms are serially correlated.

Appendix Table A6: Arellano-Bond Estimate of Credit Growth Cycles By Industry and Bank Ownership

	Years Until Next Scheduled Election				Test of Second-Order Serial Correlation
	Four	Three	Two	One	
Panel A: All Banks					
All Credit	-0.016	-0.021 *	-0.022 *	-0.019	0.13
	(0.012)	(0.012)	(0.012)	(0.013)	
Agriculture	-0.063 **	-0.009	-0.067 ***	-0.069 ***	0.48
	(0.021)	(0.020)	(0.020)	(0.022)	
Non-Agricultural Credit	0.003	-0.033 **	-0.012	0.011	0.46
	(0.014)	(0.014)	(0.014)	(0.015)	
Panel B: Public Banks					
All Credit	-0.010	-0.017	-0.023 *	-0.017	0.25
	(0.014)	(0.013)	(0.013)	(0.014)	
Agriculture	-0.067 **	-0.009	-0.084 ***	-0.068 ***	0.18
	(0.024)	(0.023)	(0.023)	(0.025)	
Non-Agricultural Credit	0.010	-0.032 **	-0.012	0.010	0.94
	(0.016)	(0.015)	(0.015)	(0.016)	
Panel C: Private Banks					
All Credit	-0.095 *	-0.102 **	-0.126 ***	-0.064	0.60
	(0.054)	(0.048)	(0.045)	(0.057)	
Agriculture	0.068	0.106	0.141	0.051	0.02
	(0.143)	(0.126)	(0.116)	(0.151)	
Non-Agricultural Credit	-0.120 **	-0.135 **	-0.126 ***	-0.083	0.45
	(0.058)	(0.051)	(0.048)	(0.061)	

Notes: Each row in represents a single regression. The unit of observation is a district-year. The dependent variable is change in log credit in different sectors. The independent variables of interest are set of dummy variables indicating the number of years until the next scheduled election. Scheduled election year is the omitted category. Panels A and B contain data from 412 districts. Panel C contains data from approximately 180 districts. Data are from 1993-1999. Estimation is conducted by means of the Arallano-Bond (1991) estimator, using one lag of the dependent variable. Column five gives the p-value of the test that the second-order error terms are serially correlated.

Appendix Table A7, Panel A: Arellano-Bond Estimates of Credit Level Targeting

Panel A: Public Banks	(1)	(2)	(3)	(4)
Cycle Dummies:			Unrestricted Margin and Unrestricted Interactions	Abs(Margin) and Abs(Interactions)
Number of Years Until Next Election	Baseline	With Margin		
Four	-0.02	-0.02	-0.06 ***	-0.11 ***
	(0.02)	(0.02)	(0.02)	(0.03)
Three	0.01	0.01	-0.03	-0.07 **
	(0.02)	(0.02)	(0.02)	(0.03)
Two	-0.02	-0.02	-0.07 ***	-0.12 ***
	(0.02)	(0.02)	(0.02)	(0.04)
One	-0.02	-0.02	-0.05 **	-0.04
	(0.02)	(0.02)	(0.03)	(0.04)
Margin of Victory		0.058		
		(0.056)		
Abs(Margin of Victory)				-0.301 ***
				(0.106)
Positive Margin of Victory			-0.251 **	
			(0.112)	
Negative Margin of Victory			0.280 **	
			(0.124)	
Positive Margin * Cycle Dummy				
Positive Margin *			0.233 **	
Four Years until Election			(0.115)	
Positive Margin *			0.381 **	
Three Years until Election			(0.188)	
Positive Margin *			0.582 ***	
Two Years until Election			(0.174)	
Positive Margin *			0.339 *	
One Year until Election			(0.189)	
Negative Margin * Cycle Dummy				
Negative Margin *			-0.322 **	
Four Years until Election			0.127	
Negative Margin *			-0.198	
Three Years until Election			0.142	
Negative Margin *			-0.237	
Two Years until Election			0.144	
Negative Margin *			-0.119	
One Year until Election			0.162	
Absolute Margin * Cycle Dummy				
Absolute(Margin) *				0.385 ***
Four Years until Election				(0.109)
Absolute(Margin) *				0.363 ***
Three Years until Election				(0.140)
Absolute(Margin) *				0.417 ***
Two Years until Election				(0.141)
Absolute(Margin) *				0.065
One Year until Election				(0.151)
Test: second-order serial correlation	0.05	0.05	0.17	0.16
N	2088	2088	2088	2088
Number of states	19	19	19	19

Notes: Each column represents a separate regression. The level of agricultural credit is the dependent variable. Panel A gives the results for public sector banks. Panel B gives the results for private sector banks. The independent variables of interest are a set of dummy variables indicating the number of years until the next scheduled election, and the average margin by which candidates from the party (or coalition) currently in power in the state won (or lost) in the specific district. The method of estimation is the Arellano Bond (1991) estimator with one lag of the dependent variable on the right hand side. Robust standard errors are used. A the bottom of the table, the p-value of the test that the second-order error terms are serially correlated is given.

Appendix Table A7: Arellano-Bond Estimates of Credit Level Targeting (cont.)

Panel B: Private Banks	(1)	(2)	(3)	(4)
Cycle Dummies:				
Number of Years Until Next Election	Baseline	With Margin	Unrestricted Margin and Unrestricted Interactions	Abs(Margin) and Abs(Interactions)
Four	0.07	0.07	0.05	-0.18
	(0.13)	(0.13)	(0.16)	(0.24)
Three	0.04	0.04	0.14	0.03
	(0.11)	(0.11)	(0.16)	(0.25)
Two	0.08	0.08	0.15	0.15
	(0.11)	(0.11)	(0.15)	(0.26)
One	-0.06	-0.07	-0.03	-0.22
	(0.13)	(0.13)	(0.17)	(0.26)
Margin of Victory		0.416		
		(0.362)		
Abs(Margin of Victory)				-0.612
				(0.855)
Positive Margin of Victory			1.203	
			(1.004)	
Negative Margin of Victory			-0.322	
			(0.794)	
Positive Margin * Cycle Dummy				
Positive Margin *			0.543	
Four Years until Election			(1.074)	
Positive Margin *			-1.348	
Three Years until Election			(1.375)	
Positive Margin *			-0.766	
Two Years until Election			(1.347)	
Positive Margin *			-0.298	
One Year until Election			(1.432)	
Negative Margin * Cycle Dummy				
Positive Margin *			0.201	
Four Years until Election			(0.851)	
Margin *			0.439	
Three Years until Election			(0.914)	
Margin *			0.463	
Two Years until Election			(0.947)	
Margin *			0.488	
One Year until Election			(1.109)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) *				1.310
Four Years until Election				(0.895)
Absolute(Margin) *				0.097
Three Years until Election				(0.983)
Absolute(Margin) *				-0.138
Two Years until Election				(1.017)
Absolute(Margin) *				0.688
One Year until Election				(1.049)
Test: second-order serial correlation	0.12	0.13	0.11	0.19
N	1041	1041	1041	1041
Number of states	15	15	15	15

Notes: See Panel A for notes.

Appendix Table A8: Arellano-Bond Estimates of Credit Growth Targeting

Panel A: Public Banks	(1)	(2)	(3)	(4)
Cycle Dummies:			Unrestricted and	With Abs(Margin)
Number of Years Until Next Election	Baseline	With Margin	Unrestricted	and Abs(Interactions)
Four	-0.02	-0.02	-0.05 **	-0.10 ***
	(0.02)	(0.02)	(0.03)	(0.03)
Three	0.01	0.01	-0.02	-0.06
	(0.02)	(0.02)	(0.03)	(0.04)
Two	-0.02	-0.02	-0.07 ***	-0.08 *
	(0.02)	(0.02)	(0.03)	(0.04)
One	-0.02	-0.02	-0.06 **	-0.03
	(0.02)	(0.02)	(0.03)	(0.04)
Margin of Victory		0.082		
		(0.063)		
Abs(Margin of Victory)				-0.232 *
				(0.124)
Positive Margin of Victory			-0.241 *	
			(0.135)	
Negative Margin of Victory			0.314 **	
			(0.135)	
Positive Margin * Cycle Dummy				
Positive Margin *			0.137	
Four Years until Election			(0.132)	
Positive Margin *			0.444 **	
Three Years until Election			(0.212)	
Positive Margin *			0.536 ***	
Two Years until Election			(0.202)	
Positive Margin *			0.426 **	
One Year until Election			(0.213)	
Negative Margin * Cycle Dummy				
Negative Margin *			-0.320 **	
Four Years until Election			(0.136)	
Negative Margin *			-0.150	
Three Years until Election			(0.154)	
Negative Margin *			-0.241	
Two Years until Election			(0.163)	
Negative Margin *			-0.216	
One Year until Election			(0.177)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) *				0.351 ***
Four Years until Election				(0.121)
Absolute(Margin) *				0.358 **
Three Years until Election				(0.158)
Absolute(Margin) *				0.242
Two Years until Election				(0.163)
Absolute(Margin) *				0.057
One Year until Election				(0.167)
Test: second-order serial correlation	0.24	0.24	0.13	0.14
N	1726	1726	1726	1726
Number of states	19	19	19	19

Notes: Each column represents a separate regression. The annual growth in agricultural credit is the dependent variable. Panel A gives the results for public sector banks. Panel B gives the results for private sector banks. The independent variables of interest are a set of dummy variables indicating the number of years until the next scheduled election, and the average margin by which candidates from the party (or coalition) currently in power in the state won (or lost) in the specific district. The method of estimation is the Arellano Bond (1991) estimator with one lag of the dependent variable on the right hand side. Robust standard errors are used. A the bottom of the table, the p-value of the test that the second-order error terms are serially correlated is given.

Appendix Table A8, Panel B: Arellano-Bond Estimates of Credit Growth Targeting (cont.)

Panel B: Private Banks	(1)	(2)	(3)	(4)
Cycle Dummies:			Unrestricted and	With Abs(Margin)
Number of Years Until Next Election	Baseline	With Margin	Unrestricted	and Abs(Interactions)
Four	-0.02	-0.02	-0.05	-0.15
	(0.06)	(0.06)	(0.07)	(0.11)
Three	-0.02	-0.02	-0.01	0.05
	(0.06)	(0.06)	(0.08)	(0.13)
Two	-0.07	-0.07	-0.05	0.00
	(0.05)	(0.05)	(0.07)	(0.13)
One	-0.02	-0.02	0.00	-0.04
	(0.07)	(0.07)	(0.08)	(0.13)
Margin of Victory		-0.055		
		(0.178)		
Abs(Margin of Victory)				-0.047
				(0.420)
Positive Margin of Victory			0.169	
			(0.459)	
Negative Margin of Victory			0.388	
			(0.382)	
Positive Margin * Cycle Dummy				
Positive Margin *			0.056	
Four Years until Election			(0.451)	
Positive Margin *			-0.738	
Three Years until Election			(0.635)	
Positive Margin *			-0.692	
Two Years until Election			(0.604)	
Positive Margin *			-1.017	
One Year until Election			(0.637)	
Negative Margin * Cycle Dummy				
Positive Margin *			-0.424	
Four Years until Election			(0.402)	
Margin *			-0.313	
Three Years until Election			(0.447)	
Margin *			-0.355	
Two Years until Election			(0.452)	
Margin *			-0.441	
One Year until Election			(0.534)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) *				0.533
Four Years until Election				(0.405)
Absolute(Margin) *				-0.355
Three Years until Election				(0.486)
Absolute(Margin) *				-0.253
Two Years until Election				(0.489)
Absolute(Margin) *				-0.036
One Year until Election				(0.489)
Test: second-order serial correlation	0.72	0.72	0.76	0.75
N	712	712	712	712
Number of states	15	15	15	15

Note: See panel A for notes