Fixing Market Failures or Fixing Elections? Agricultural Credit in India

Shawn Cole*

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Abstract

How vulnerable are economic interventions to political capture, 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 amount of agricultural credit lent by public banks is 5-10 percentage points higher in election years than in years following 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. Politically motivated loans are costly: they are less likely to be repaid, and election year credit booms do not measurably affect agricultural output.

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1 Introduction

While there is limited evidence that government intervention in markets may improve welfare, there is also convincing evidence that government institutions are subject to political capture. However, less is known about the economic and political implications of capture: How does capture work? How costly is it? How is redistribution targeted?

This paper presents evidence that government-owned banks in India serve the electoral interests of politicians.[1] I show that the amount of agricultural credit lent by public banks is substantially higher in election years. Combining theories of electoral cycles with targeted redistribution, I demonstrate that more loans are made in districts in which the ruling state party had a narrow margin of victory (or a narrow loss), than in less competitive districts. This targeting is not observed in off-election years, or in private bank lending. Political interference is costly: defaults increase around election time. Moreover, agricultural lending booms do not affect agricultural investment or output.

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. A related study (Cole 2006) exploits a natural experiment to measure the effects of bank nationalization in India. I find that government ownership leads to lower interest rates, lower quality financial intermediation, and that nationalization slowed financial development and economic growth.

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 (2005) 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 (2005) 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 (2006). Akhmedov and Zhuravskaya (2004) present a particularly compelling recent example). 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.

Two works are closely related to this present work. A recent paper by Dinc (2005), which examines lending of public and private sector banks in a large cross-country sample. Dinc (2005) finds that in election years, the growth rate of credit from private banks slows, but that the growth rate of government-owned banks grows. This effect is concentrated in developed markets. Bertrand et. al. (2004) study firm behavior in France, and find that firms with politically connected CEOs strategically hire and fire around election years: this effect is strongest in close regions.

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 district 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. Perhaps the threat of upcoming elections simply causes politicians to behave *more* closely in line with the public interest. For example, Akhmedov and Zhuravskaya (2004) demonstrate that politicians pay back wages prior to elections. If political intervention simply shifts resources from one group to another, 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? 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

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 many private banks in 1969 and 1980. Second, both public and private banks were required to lend at least a certain 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 (2005), and Burgess, Pande, and Wong (2005) 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.

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 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.

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 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

[2] Quoted in Burgess and Pande (2005).

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 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

[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.

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, since 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% 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

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

in equilibrium, more competent politicians induce greater distortions than less competent politicians. Persson and Tabellini (2000) and Shi and Svensson (2003) 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 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 Svensson (2006) collect data for 85 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 (2004) 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 (1996) 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

[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.

of obtaining a desired allocation of resources, independent of re-election concerns ("patronage"). Second, they may believe distributing patronage aids in re-election ("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: resources will be targeted towards "swing" districts, or politicians will disproportionately reward their supporters. Snyder (1989) and Dixit and Londregan (1996) 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 re-election.

Several 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] Third, 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

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

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, 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 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. Table 1 gives summary statistics.

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 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.[13] 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 of the panel is relatively short. For this reason, I 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

[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.

[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] 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.

maturation of less than a year. The median and mean rate of real agricultural credit growth for public banks is zero over the period studied. In a previous version of this paper (available on request) I show that the results are robust to estimation in changes, as well as to estimated in a dynamic panel setting, using the GMM technique developed by Arellano and Bond (1991).

4.2 Political Cycle Results

4.2.1 The Amount of Credit

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.[14] 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 1 if the state s had an election in year t. Standard errors are clustered at the state level.[15]

While the constitution mandates elections be held every five years, the timing is subject to some slippage: in the sample, one fourth of elections (10 out of 37) occur before they are scheduled. The typical cause of an early election is a change in the coalition leadership. 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:

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

Because elections are required after four years without an election, S^0_{st} is a powerful predictor of elections. In the first-stage regression, the estimated coefficient is .99, with a standard error of

[14] 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.

[15] 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.

.01. This first stage explains 86% of the variation in election years, because early elections are not common.[16]

Do elections affect credit? Table 2 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.[17] This effect of elections on agricultural credit is not due to aggregate annual 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.[18] Nor is there any systematic relationship, in the OLS, reduced form or IV, between elections and non-agricultural credit. 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. 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, and the confidence intervals around the point estimates are relatively large.

Table 3 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.

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

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

[16] 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, this approach provides relatively less power. I therefore "reset" the instrument after an early election.

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

[18] See Khemani (2004).

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 3 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. Cycles are not observed in non-agricultural lending, though the point estimates are negative and consistent with a smaller cycle.

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 rules out this possibility: private sector banks issue only approximately ten percent of credit in India, and are underweight in their exposure to agricultural credit. Thus, an eight percent 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. Thus, while public bank lending may crowd out private credit, there is still a large aggregate effect.

4.2.2 The Type of Credit

Table 4 investigates how the nature of lending varies over the political cycle. I first examine quantity. An increase in lending could be due to changes on the extensive margin, with banks lending to additional borrowers, as well as the intensive margin, with banks making larger loans. I find weak evidence for both. The off-election cycle dummies are negative for both average loan size, and average agricultural loan size, but they are small in magnitude, and typically not statistically distinguishable from zero. Only the average size of an agricultural loan exhibits cyclical variation, increasing by almost six percent in an election year. There is more action in the number of agricultural loans from public banks: the count increases by five percent from two years prior to the election to the election year. There is no systematic variation in the number or average size of loans in private banks.

Second, there is some evidence of systematic variation in interest rates. At the aggregate level, the average interest rate for all loans by all banks is very slightly higher in election years (ten basis points, off an average of approximately 15%), though this difference is significant only at the ten percent level. The interest rate on agricultural credit appears to be flat over the cycle, in aggregate and for public sector banks. Interestingly, however, private sector banks seem to charge higher rates for agricultural loans in non-election years, with a difference of up to 50 basis points between peak and trough years. It may well be that, in election years, private banks lower the interest rate they offer on agricultural loans in order to attract borrowers who might otherwise find credit on more favorable terms from public sector banks.

4.2.3 What Determines the Size of the Cycle?

What determines the size of the lending cycles? In this subsection, I consider how the size of the electoral cycle varies with fixed district characteristics. One natural line of inquiry is to examine whether the quality of corporate governance of the banks in a district is relevant: bank with professional managers, or managers who are able to resist political pressure, may be less likely to engage in costly cycles. Unfortunately, no measure of the quality of corporate governance of banks is available. Instead, I use the share of loans late in a given district in 1992, as a proxy for the quality of the banks' corporate governance.

I estimate slightly modified versions of equations 1 and 2: in addition to the dummy for election year (E_{st}) or scheduled election year (S^0_{dt}), I include an interaction term between the district characteristic C_{dt} and the election indicator. The main effect of the district characteristic is of course captured in the district fixed effect

$$y_{dst} = \alpha_d + \gamma_{rt} + \delta Rain_{dst} + \beta E_{st} + \chi \left(E_{dt} * C_{dt} \right) + \varepsilon_{dst} \tag{4}$$

Table 5 presents the results. The first row gives the main election effect without the interaction. The regressions presented in columns (1) and (2) use actual elections, while those in (3) and (4) use scheduled elections. The second two rows interact election with measures of loan default. The point estimates on χ are negative, but insignificant. The mean value of Share of Agricultural Loans Late is .1, with a standard deviation of.1. Thus, taking the point estimates at face value, comparing a a district with 30% default to one with 10% default, the size of the cycle would be approximately two percentage points smaller in the region with higher default rates.

Most theories of political cycles require asymmetric information between politicians and voters. Shi and Svensson (2006) present a model in which the share of informed voters affects the size of the observed election cycles: since informed voters are not fooled by manipulation, the greater the share of informed voters, the smaller the incentive to manipulate. The authors test this finding in the cross-country setting, and find strong support for it. Akhmedov and Zhuravskaya (2004) find similar results in Russia: regions with higher levels of voter awareness, greater education, and more urbanization experience smaller cycles. No measures of voter awareness are available in India at the district level, however, I consider whether the latter two are correlated with the size of the cycle.

The share of population that is rural strongly affects the size of the cycle. Note that this is not a mechanical affect due to the fact that there level of agricultural credit is greater in districts with greater rural populations, since the dependent variable is estimated in logs. The average rural population share is .78, with a standard deviation of .15. Thus, a one standard deviation increase in the share of rural population increases the size of the cycle by approximately two percentage points.

I also find results consistent with previous findings on education. Cycles are significantly smaller in areas with higher literacy, and in which a higher share of the population has graduated from primary school. (These same results hold for other schooling levels.)

4.3 How is Redistribution Targeted?

In this subsection, I examine whether agricultural credit varies with the average margin of victory enjoyed by the current ruling party in closest election in each district, M_{dst}.I assign to M$_{dst}$ the margin of victory of the ruling party in the years immediately following the election. For years just prior to the election, the ideal measure would be poll data indicating the expected margin of victory. Lacking that, I use the realized margin of victory of the ruling party in the upcoming election for M_{dst} in the two years prior to the election.[19]

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:

$$y_{dst} = \alpha_d + \pi M_{dst} + \beta_{-4} S_{st}^{-4} + \beta_{-3} S_{st}^{-3} + \beta_{-2} S_{st}^{-2} + \beta_{-1} S_{st}^{-1} + \varepsilon_{dst} \tag{5}$$

The estimates are reported in column (2) of Table 6. 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.

The model in equation 5 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}^{+}

[19] In scheduled election years, the margin of victory of incumbent party is used. The margin of victory of the majority party is used in scheduled election years -4 and -3. In scheduled election years -2 and -1, the ruling party is again defined as the incumbent party, but their margin of victory is assigned using the upcoming election results. To the extent that politicians know in which districts the race will be competitive, this should be a valid proxy for expected competitiveness.

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 6). 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 slightly negative slope for positive margins of victory indicates that districts in which the average margin of victory is greater than zero received slightly less credit. The slope of the lines are not statistically distinguishable from zero.

The second panel in Figure 1, for the year three years prior to the next scheduled election, continues to indicate 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. 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:

$$\begin{aligned} y_{dst} &= \alpha_d + \beta_{-4}S^{-4}_{st} + \beta_{-3}S^{-3}_{st} + \beta_{-2}S^{-2}_{st} + \beta_{-1}S^{-1}_{st} + \pi^+ M^+_{dst} + \pi^- M^-_{dst} \\ &\quad + \sum_{k=-4}^{-1} \theta^+_k \left(M^+_{dst} * S^k_{st}\right) + \sum_{k=-4}^{-1} \theta^-_k \left(M^-_{dst} * S^k_{st}\right) + \varepsilon_{dt} \end{aligned} \tag{6}$$

Standard errors are again clustered at the state-year level. Results are presented in the third column of Table 6. Once the margin of victory is included, the estimated size of the cycle increases, to approximately 10% at the minimum, three 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 -.34, while the coefficient on M^-_{dst} is .43. 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) an election by 15 percentage points will receive approximately 5-6 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) 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:

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

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 6, but substituting $\pi^A M_{dst}^A$ for $\left(\pi^+ M_{dst}^+ \ + \pi^- M_{dst}^-\right)$,with analogous replacements for the interaction terms, resolves this measurement error problem. The estimated equation is thus:

$$\begin{aligned} y_{dst} &= \alpha_d + \beta_{-4} S_{st}^{-4} + \beta_{-3} S_{st}^{-3} + \beta_{-2} S_{st}^{-2} + \beta_{-1} S_{st}^{-1} + \pi^A M_{dst}^A \\ &\quad + \theta_{-4}^A (M_{dst}^A * S_{st}^{-4}) + \theta_{-3}^A \left(M_{dst}^A * S_{st}^{-3}\right) + \theta_{-2}^A \left(M_{dst}^A * S_{st}^{-2}\right) + \theta_{-1}^A \left(M_{dst}^A * S_{st}^{-1}\right) + \varepsilon_{dst} \end{aligned} \tag{7}$$

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 6 in another way. They trace credit for both public and private sector banks, over the election cycle. 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 grows sharply prior to an election, increasing 10 percentage points between the year two years prior to the election and election time. Predicted credit from private banks is flat over the cycle. 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 (2005) document that loans made by public sector banks to firms controlled by politicians are much more likely to end up in default. In this section, we ask this question of loans given to the general population.

I estimate the reduced form relationship between agricultural credit default rates and the electoral cycle. I use three measures of default rate: the log volume of late credit, the share of loans late, and the share of credit late. Loans are coded as late if they are past due by at least six months. (Summary statistics are given in Table 1). The results, from equation 3 are presented in Table 7. There is a large cycle in the volume of late agricultural loans: the amount increases 16% in government-owned banks in scheduled election years relative to the trough two years prior to the election. This likely occurs for several reasons. First, of course, credit is increasing in election years, so one would naturally expected the volume of loans to increase. However, this increase is more than proportional: the size of the credit cycle is 8%, while the size of cycle in late loan volume is 15%. This is likely due to several factors: even if no political considerations did not affect the screening and monitoring of lending, the marginal borrower is likely less creditworthy than the average borrower. Second, politicians may pressure loan officers to be more lax in collecting in times of election. (Support for this latter explanation is found in newspaper articles in which politicians urge banks to extend or forgive loans prior to elections.)

The third row of each panel gives the faction of agricultural loans marked as late in each year of the electoral cycle. Election years still generate the highest share of late loans, but there is an immediate drop following elections. This may seem puzzling, since one might expect loans made at the peak of the election cycle to show up as late one or two years following the election. Unfortunately, it is not possible to separate out the term structure of individual loans. Again, these effects may be driven by political pressure to forgive loans following elections. The fact that this cycle does not appear when the value-weighted share of agricultural loans (line four of each panel) is used is consistent with this interpretation.

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.[20] However, the fact that the volume of loans in default increases with electoral cycles supports a strong presumption that the political loans are costly.

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 agricul-

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

tural productivity, crop prices or idiosyncratic shocks will almost surely bias any estimate. The lending booms documented in Section 4.2 suggest an instrument for the efficacy of politically-induced lending: the electoral cycle induces a supply shock uncorrelated with other confounding factors.[21]

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 (revenue and yield) at the district level. The data set was initially assembled by Dinar et al (1998) for the time period 1957-1987. It has been supplemented by Rohini Pande. I use two measures of agricultural output. The first is log aggregate agricultural revenue, at the district. One difficulty with the data is that missing observations are relatively common. Thus, it is not possible to calculate $\text{logrevenue}_{dt}=\log\left(\sum_{i\in Crops} p_{i,dt} * q_{i,dt}\right)$ for all districts. It would not be correct to replace missing quantities with zero, as that would induce substantial, potentially non-random variation in measured revenue. I therefore calculate revenue, using for each district only the set of crops for which there are no missing values from 1992 to 1999. To measure yield, I take the average yield of all crops ($\text{y}_{c,dt}$) in a district, weighted by acres planted, a_{cdt}. Thus, $\text{yield}_{dt} = \frac{1}{\sum_{i\in crops} a_{c,dt}} \sum_{i\in Crops} \alpha_{c,dt} * y_{c,dt}$. Because the frequency of missing data is relatively high (some states have output for only one or two years), the size of the sample shrinks considerably, to 106 districts, over 8 years, located in only six states.[22] Because the number of states is low, I use year, rather than region-year, fixed effects, when estimating equation 7.

Panel A of Table 8 presents the reduced form relationships between credit, output, and the electoral cycle. The coefficients on θ^A_{-k} are included in the regressions but suppressed from the table for notational simplicity. As in the full sample, the electoral cycle dummies and margin of victory variables serve as powerful predictors of agricultural credit. The first line of Panel A gives

[21] The observation that politicians hire additional police prior to elections is used by Levitt (1997) to measure the effect of the size of the police force on crime.

[22] The states, are, however, among the most important in India: Rajasthan, Gujarat, Maharashtra, Andhra Pradesh, Madyha Pradesh, and Karntaka.

the results for public banks only. However, since I am unable to determine which agricultural output is financed by public vs. private banks, the relevant variable of interest for the structural equation is aggregate agricultural credit. The second row of Panel A gives the relationship, and again electoral variables predict credit. The null hypothesis that the electoral coefficients β, θ and π do not affect credit can be rejected at less than .1% level.

The next two rows give the reduced form relationship between agricultural revenue, and output, and the electoral cycle. While β_{-1}, the dummy on S_{dt}^{-1} is negative and significant for revenue, there is no systematic relationship between the electoral cycle and revenue. The point estimates on β_{-4} and β_{-2} are positive, but statistically indistinguishable from zero. The reduced-form relationship for output is similar: only β^{-2} is statistically significant from zero, and there is no pattern between credit and electoral cycles.

In panel B, I estimate the structural relationship between yield and credit, and output and credit:

$$y_{dt} = \alpha_i + \beta * credit_{dt} + \gamma_t + \varepsilon_{dt},$$

using the electoral variables as instruments for credit. The OLS relationship between yield and output, and credit, is given in the first column of panel B.

For both measures of output, the point estimate of the effect of credit on output is very close to zero. Unfortunately, the estimates are quite imprecise, with large standard errors. Nevertheless, there is no systematic relationship between credit and output.

A previous version of this paper conducted the same exercise, using state-level data on agricultural output. State-level agricultural data are available for fourteen states. I found that while credit varied with the electoral cycle, output did not. The IV estimates were similarly imprecise.

Thus, while credit does go up in election years, there is no evidence that agricultural output does so.

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.

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 (2005) 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 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 "Banking 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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Table 1: Summary Statistics

	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
District Characteristics			
Share of Agricultural Loans Late	0.133	0.104	3296
Share of All Loans Late	0.133	0.072	3296
Percent of Population Rural	0.785	0.149	3296
Share Literate	0.413	0.132	3296
Share Primary Graduates or Above	0.305	0.114	3296
Panel B: Summary Statistics for Targeted Redistribution Regressions (19 states)			
Credit Variables			
Log Real Credit, All Banks	14.293	1.536	3408
Log Real Credit, Public Banks	14.111	1.537	3408
Log Real Credit, Private Banks	11.874	1.851	1777
Log Real Agricultural Credit, All Banks	12.900	1.434	3408
Log Real Agricultural Credit, Public Banks	12.666	1.450	3408
Log Real Agricultural Credit, Private Banks	9.273	2.518	1656
Political Variables			
Election Year	0.206	0.405	3408
Scheduled Election in 4 Years	0.225	0.418	3408
Scheduled Election in 3 Years	0.249	0.432	3408
Scheduled Election in 2 Years	0.248	0.432	3408
Scheduled Election in 1 Years	0.155	0.362	3408
Scheduled Election Year	0.123	0.329	3408
Margin of Victory of Ruling Party	-0.001	0.167	2730
Absolute Value of Margin of Vicotry	0.195	0.114	2730

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: The Effect of Elections on Credit

Panel A: OLS	All Bank Credit	Public Bank Credit	Private Bank Credit
Total Credit	0.019	0.015	0.034
	(0.012)	(0.013)	(0.082)
Agricultural Credit	0.044 ***	0.047 ***	-0.127
	(0.017)	(0.016)	(0.139)
Non-Agricultural Credit	0.012	0.007	0.053
	(0.014)	(0.015)	(0.080)
Panel B: Reduced Form			
Total Credit	0.029 **	0.031 **	0.040
	(0.013)	(0.013)	(0.053)
Agricultural Credit	0.046 ***	0.060 ***	-0.021
	(0.017)	(0.019)	(0.087)
Non-Agricultural Credit	0.021	0.020	0.061
	(0.015)	(0.014)	(0.055)
Panel C: Instrumental Variables			
Total Credit	0.028 **	0.031 **	0.039
	(0.013)	(0.014)	(0.055)
Agricultural Credit	0.046 ***	0.060 ***	-0.020
	(0.018)	(0.020)	(0.092)
Non-Agricultural Credit	0.021	0.020	0.060
	(0.016)	(0.015)	(0.058)
N	1761	1640	1761
States	1761	1640	1761

Notes: Each cell represents a regression. The coefficient reported is a dummy for election year (Panel A), scheduled election year (Panel B), and election year instrumented with scheduled election year (Panel C.) The dependent variable is annual change in log real levels of credit. In addition to the indicated dependent variable of interest, all regressions include district and region-year fixed effects, and a measure of 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.

The first stage of the IV regression in Panel C is: $E_{sdt} = \alpha_d + \gamma_{rt} + \delta Rain_{dst} + \beta^0 S^0_{st} + \varepsilon_{dst}$

S^0_{st} is a dummy variable indicating that five years prior to that year, there was an election. The coefficient on S^0_{st} is .99, with standard error of .01. The R^2 is .86.

Table 3: 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
	(0.015)	(0.014)	(0.014)	(0.016)
Agriculture	-0.023	-0.045 **	-0.061 ***	-0.022
	(0.022)	(0.020)	(0.020)	(0.026)
Non-Agricultural Credit	-0.029 *	-0.024	-0.026 *	0.004
	(0.017)	(0.015)	(0.016)	(0.018)
Panel B: Public Banks				
All Credit	-0.033 **	-0.030 **	-0.040 ***	-0.011
	(0.015)	(0.015)	(0.015)	(0.016)
Agriculture	-0.032	-0.056 **	-0.081 ***	-0.034
	(0.024)	(0.024)	(0.021)	(0.026)
Non-Agricultural Credit	-0.026	-0.022	-0.028 *	0.004
	(0.017)	(0.015)	(0.016)	(0.018)
Panel C: Private Banks				
All Credit	0.022	-0.033	-0.027	-0.156 *
	(0.097)	(0.088)	(0.058)	(0.089)
Agriculture	0.079	0.035	0.014	-0.003
	(0.141)	(0.121)	(0.093)	(0.156)
Non-Agricultural Credit	-0.001	-0.058	-0.045	-0.173 *
	(0.098)	(0.090)	(0.059)	(0.090)

Notes: Each row represents a regression. The coefficients reported are dummies for the number of years until the next scheduled election. The dependent variable is log credit. All regressions include district and region-year fixed effects, as well as annual rainfall.
Standard errors are clustered by state-year.

Table 4: Loan Characterstics Over the Election Cycle

	Years Until Next Scheduled Election			
	Four	Three	Two	One
Panel A: All Banks				
Log (Average Loan Size)	-0.032	-0.010	-0.032	-0.028
	(0.029)	(0.025)	(0.023)	(0.027)
Log (Avg. Agricultural Loan Size)	-0.028	-0.011	-0.023	-0.058 **
	(0.034)	(0.030)	(0.027)	(0.028)
Log(Number of Loans)	-0.001	-0.019	-0.003	0.019
	(0.021)	(0.017)	(0.019)	(0.023)
Log(Number of Ag. Loans)	0.005	-0.034	-0.038	0.036
	(0.028)	(0.022)	(0.027)	(0.029)
Interest Rate	-0.001 *	-0.001 *	-0.001	-0.001 *
	(0.001)	(0.001)	(0.001)	(0.001)
Interest Rate-Agricultural	0.000	0.000	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Public Banks				
Log (Average Loan Size)	-0.031	-0.014	-0.034	-0.024
	(0.028)	(0.025)	(0.024)	(0.026)
Log (Avg. Agricultural Loan Size)	-0.030	-0.013	-0.027	-0.055 *
	(0.037)	(0.033)	(0.031)	(0.029)
Log(Number of Loans)	-0.003	-0.016	-0.006	0.013
	(0.021)	(0.017)	(0.019)	(0.020)
Log(Number of Ag. Loans)	-0.003	-0.042 *	-0.053 *	0.021
	(0.030)	(0.024)	(0.028)	(0.026)
Interest Rate	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Interest Rate-Agricultural	0.000	0.000	0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Panel C: Private Banks				
Log (Average Loan Size)	0.064	-0.008	-0.034	-0.054
	(0.066)	(0.062)	(0.066)	(0.066)
Log (Avg. Agricultural Loan Size)	0.129	-0.001	0.034	0.070
	(0.139)	(0.134)	(0.098)	(0.158)
Log(Number of Loans)	-0.041	-0.025	0.008	-0.100
	(0.081)	(0.064)	(0.059)	(0.072)
Log(Number of Ag. Loans)	-0.050	0.037	-0.020	-0.073
	(0.094)	(0.091)	(0.052)	(0.091)
Interest Rate	-0.001	-0.001	-0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.002)
Interest Rate-Agricultural	0.004 *	0.003 **	0.005 ***	0.003
	(0.002)	(0.001)	(0.001)	(0.003)

Notes: Each row represents a regression. The coefficients reported are dummies for the number of years until the next scheduled election. The dependent variable is log credit. All regressions include district and region-year fixed effects, as well as annual rainfall. Standard errors are clustered at the state year level.

Table 5: District Characteristics and Cycles in Agricultural Credit

Panel A: Public Banks	**Actual Election Years**		**Scheduled Election Years**	
	Election	Interaction	Scheduled Election	Interaction
	(1)	(2)	(3)	(4)
No Interaction	0.04 **		0.04 **	
	(0.02)		(0.02)	
Quality of Intermediation				
Share of Agricultural Loans Late	0.05 **	-0.09	0.05 **	-0.08
	(0.02)	(0.08)	(0.02)	(0.08)
Share of All Loans Late	0.05 **	-0.14	0.06 **	-0.08
	(0.02)	(0.14)	(0.03)	(0.15)
Population Characteristics				
Percent of Population Rural	-0.02	0.08	-0.05	0.12 **
	(0.05)	(0.06)	(0.04)	(0.05)
Share Literate	0.13 **	-0.22 **	0.18 ***	-0.30 ***
	(0.05)	(0.11)	(0.05)	(0.11)
Share Primary Graduates or Above	0.12 **	-0.25 *	0.15 ***	-0.32 **
	(0.05)	(0.14)	(0.05)	(0.13)

Panel B: Private Banks	**Actual Election Years**		**Scheduled Election Years**	
	Election	Interaction	Scheduled Election	Interaction
District Credit Variables	(1)	(2)	(3)	(4)
No Interaction	-0.14		-0.04	
	(0.14)		(0.08)	
Quality of Intermediation				
Share of Agricultural Loans Late	-0.08	-0.42	0.04	-0.62
	(0.13)	(0.73)	(0.11)	(0.90)
Bad All Loans Count	-0.07	-0.61	-0.09	0.40
	(0.15)	(1.00)	(0.15)	(1.23)
Population Characteristics				
Percent of Population Agricultural	0.20	-0.47	0.02	-0.09
	(0.31)	(0.35)	(0.29)	(0.35)
Share Literate	-0.18	0.09	-0.03	-0.02
	(0.20)	(0.43)	(0.22)	(0.40)
Share Primary	-0.16	0.05	-0.02	-0.07
	(0.16)	(0.48)	(0.18)	(0.41)

Notes: Each row of this table presents two regressions. Columns (1) and (2) present a regression using actual election year, and an interaction between election year and district characteristic, while columns (3) and (4) use scheduled election year for the main effect and interaction. The dependent variable is log agricultural credit, at the district level. All regressions include district and region-year fixed effects, as well as annual rainfall. Standard errors are clustered at the state year level.

Table 6, 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	-0.04 *	-0.07 ***	-0.13 ***
	(0.02)	(0.02)	(0.03)	(0.04)
Three	-0.04 *	-0.07 ***	-0.10 ***	-0.17 ***
	(0.02)	(0.03)	(0.03)	(0.04)
Two	-0.07 ***	-0.06 ***	-0.10 ***	-0.14 ***
	(0.02)	(0.02)	(0.03)	(0.04)
One	-0.01	-0.03	-0.07 **	-0.10 **
	(0.03)	(0.03)	(0.03)	(0.04)
Margin of Victory		-0.051		
		(0.032)		
Abs(Margin of Victory)				-0.51 ***
				(0.10)
Positive Margin of Victory			-0.340 ***	
			(0.083)	
Negative Margin of Victory			0.428 ***	
			(0.104)	
Positive Margin * Cycle Dummy				
Positive Margin * Four Years until Election			0.153	
			(0.103)	
Positive Margin * Three Years until Election			0.143	
			(0.153)	
Positive Margin * Two Years until Election			0.132	
			(0.106)	
Positive Margin * One Year until Election			0.245 **	
			(0.097)	
Negative Margin * Cycle Dummy				
Negative Margin * Four Years until Election			-0.340 ***	
			(0.123)	
Negative Margin * Three Years until Election			-0.289 **	
			(0.134)	
Negative Margin * Two Years until Election			-0.365 ***	
			(0.124)	
Negative Margin * One Year until Election			-0.421 ***	
			(0.146)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) * Four Years until Election				0.41 ***
				(0.13)
Absolute(Margin) * Three Years until Election				0.50 ***
				(0.14)
Absolute(Margin) * Two Years until Election				0.36 ***
				(0.14)
Absolute(Margin) * One Year until Election				0.35 **
				(0.14)
R^2	0.98	0.98	0.98	0.98
N	3408	2730	2730	2730
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 6, 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	0.09	-0.02	-0.06	-0.35
	(0.14)	(0.14)	(0.15)	(0.24)
Three	0.04	-0.04	0.10	-0.20
	(0.11)	(0.12)	(0.12)	(0.22)
Two	0.05	-0.01	-0.02	-0.29
	(0.09)	(0.10)	(0.12)	(0.21)
One	-0.01	-0.10	-0.15	-0.44
	(0.14)	(0.16)	(0.17)	(0.31)
Margin of Victory		0.634 ***		
		(0.236)		
Abs(Margin of Victory)				-0.65
				(0.78)
Positive Margin of Victory			0.590	
			(0.582)	
Negative Margin of Victory			-0.464	
			(0.761)	
Positive Margin * Cycle Dummy				
Positive Margin *			1.353	
Four Years until Election			(0.912)	
Positive Margin *			-1.462	
Three Years until Election			(1.219)	
Positive Margin *			0.909	
Two Years until Election			(0.833)	
Positive Margin *			1.196	
One Year until Election			(1.008)	
Negative Margin * Cycle Dummy				
Positive Margin *			0.620	
Four Years until Election			(0.789)	
Margin *			1.250	
Three Years until Election			(0.986)	
Margin *			0.619	
Two Years until Election			(0.863)	
Margin *			0.435	
One Year until Election			(0.942)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) *				1.58 *
Four Years until Election				(0.82)
Absolute(Margin) *				0.57
Three Years until Election				(1.08)
Absolute(Margin) *				1.49 *
Two Years until Election				(0.84)
Absolute(Margin) *				1.40
One Year until Election				(0.99)
R^2	0.92	0.92	0.92	0.92
N	1656	1393	1393	1393
Number of states	19	19	19	19

Notes: See Panel A for notes.

Table 7: Lending Cycles and Non-Performing Loans

	Years Until Next Scheduled Election			
	Four	Three	Two	One
Panel A: All Banks				
Volume of Late Agricultural Loans	-0.063	-0.099	-0.150 **	-0.127
	(0.087)	(0.067)	(0.067)	(0.098)
Volume of All Late Loans	-0.086	-0.069	-0.032	-0.033
	(0.055)	(0.044)	(0.062)	(0.064)
Share of Agricultural Loans Late	-0.034 **	-0.026 **	-0.017	-0.022 *
	(0.012)	(0.011)	(0.011)	(0.013)
Share of Agricultural Credit Late	-0.022	-0.009	-0.004	-0.006
	(0.011)	(0.009)	(0.010)	(0.011)
Panel B: Public Banks				
Bad Agricultural Loans	-0.074	-0.102	-0.162 **	-0.134
	(0.089)	(0.074)	(0.072)	(0.105)
Bad All Loans	-0.109 *	-0.088 **	-0.043	-0.047
	(0.057)	(0.043)	(0.061)	(0.066)
Share of Agricultural Loans Late	-0.035 **	-0.027 **	-0.019 *	-0.017
	(0.012)	(0.010)	(0.011)	(0.013)
Share of Agricultural Credit Late	-0.025 **	-0.011	-0.008	-0.004
	(0.011)	(0.009)	(0.010)	(0.011)
Panel C: Private Banks				
Bad Agricultural Loans	0.030	0.201 **	-0.102	0.038
	(0.187)	(0.094)	(0.203)	(0.170)
Bad All Loans	-0.060	-0.052	0.065	-0.213 *
	(0.116)	(0.083)	(0.078)	(0.109)
Share of Agricultural Loans Late	-0.015	-0.014	-0.021	-0.040 **
	(0.016)	(0.012)	(0.014)	(0.019)
Share of Agricultural Credit Late	-0.002	0.003	0.008	-0.025
	(0.018)	(0.015)	(0.016)	(0.020)

Notes: Each row in represents a single regression. The unit of observation is a district-year. The independent variables of interest are a set of dummy variables indicating the number of years until the next scheduled election. Panels A and B contain data from 412 districts. Panel C contains data from 180 districts.
Standard errors are clustered by state-year.

Table 8: Lending, Agircultural Investment and Output

	Years Until Next Scheduled Election			
	Four	Three	Two	One
Panel A: Reduced Form				
Agricultural Credit, Government Ban	-0.154 **	-0.179 ***	-0.176 ***	-0.073
	(0.069)	(0.064)	(0.060)	(0.048)
Agricultural Credit, All Banks	-0.120 *	-0.138 **	-0.159 ***	-0.067
	(0.068)	(0.063)	(0.054)	(0.045)
Revenue	0.026	-0.208	0.014	-0.483 ***
	(0.112)	(0.159)	(0.146)	(0.146)
Output (Index)	0.058	-0.217 **	0.030	-0.152
	(0.085)	(0.101)	(0.091)	(0.113)

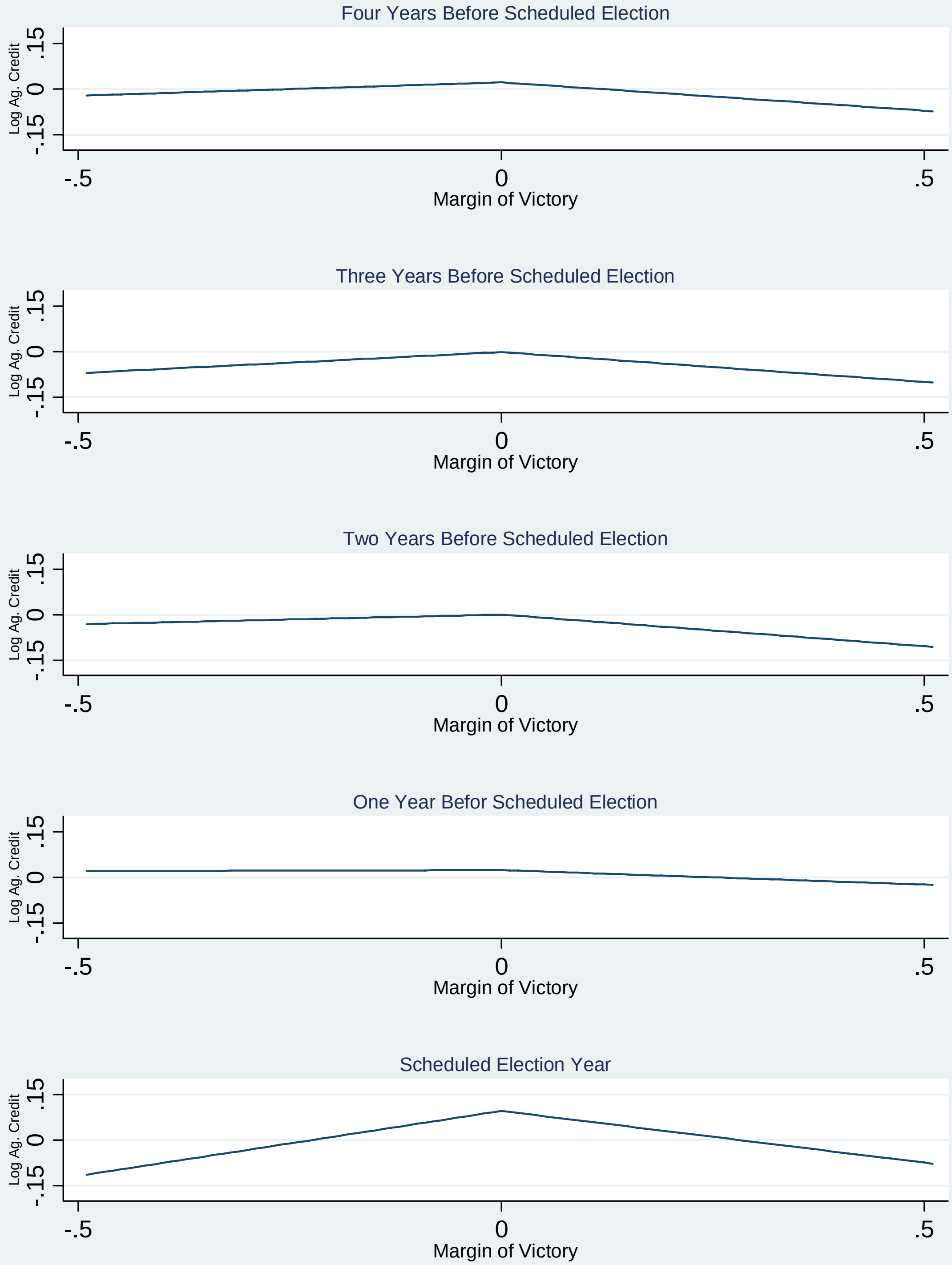
Notes: Each row represents a single regression. Data are available for 106 districts, located within 6 states, for the period 1992-1999. The dependent variables of interest are dummy variables indicating the number of years until the next scheduled election. Standard errors are clustered at the state-year level.

Panel B: Instrumental Variables Estimates of the Effect of Credit

Dependent Variable:		Revenue	Output (Index)
	OLS	0.097	-0.091
		(0.070)	(0.638)
	IV	0.024	0.027
		(0.047)	(0.409)

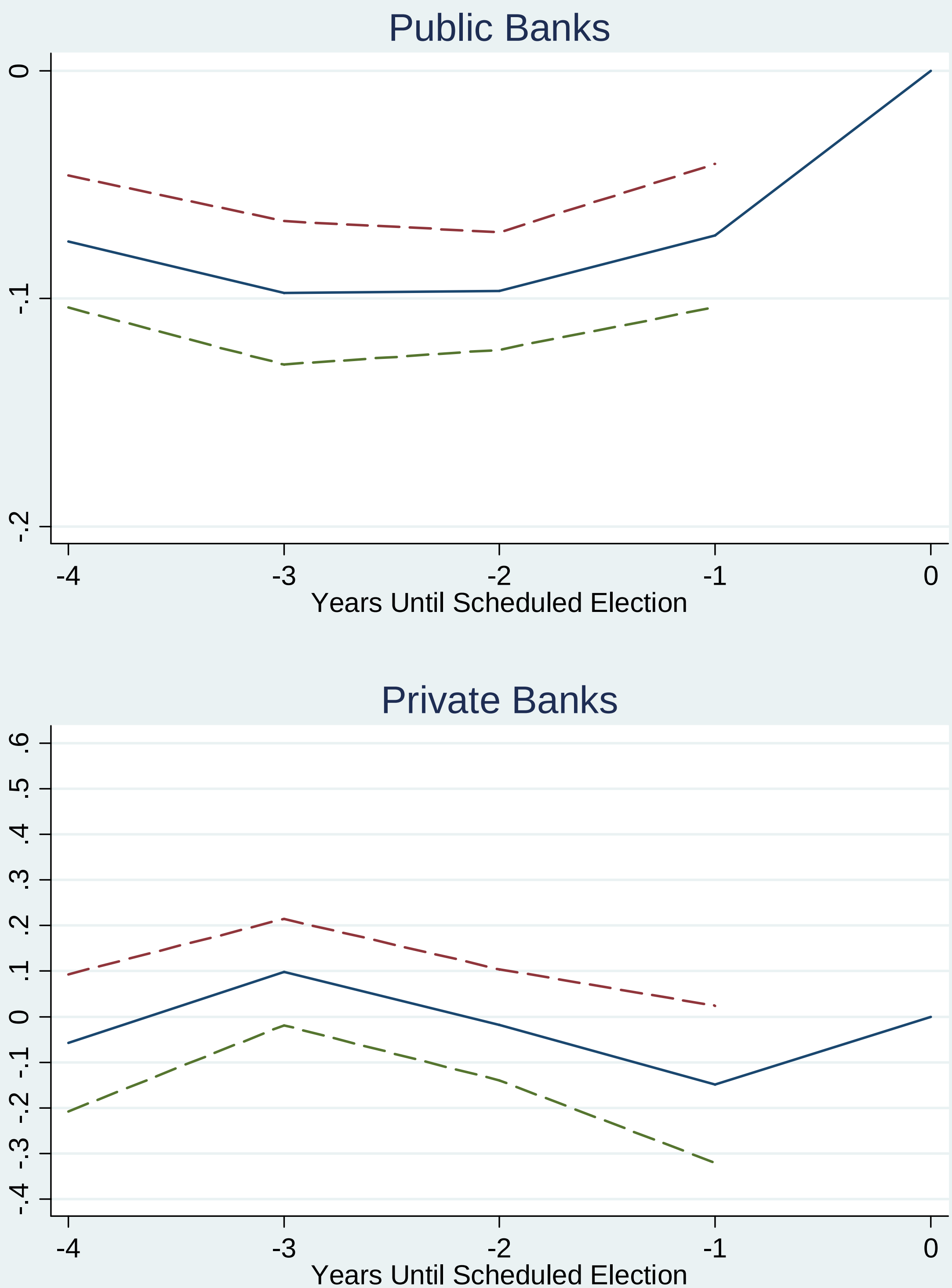
Notes: Each cell represents a single regression. Data are available for 106 districts, located within 6 states, for the period 1992-1999. The dependent variables of interest are revenue (column 1) and output (column 2). The OLS relationship is given in the first row. An instrumental variables estimate is given in the second row. Four dummies for the election schedule, along with the absolute value of the margin of victory enjoyed by the ruling party (interacted with each election cycle dummy) serve as instruments. The null hypothesis that the instruments do not predict aggregate credit can be rejected a the .1% level. All regressions include district fixed effects, year fixed effects, and rainfall.

Figure 1: Targeted Lending Levels Over the Election Cycle



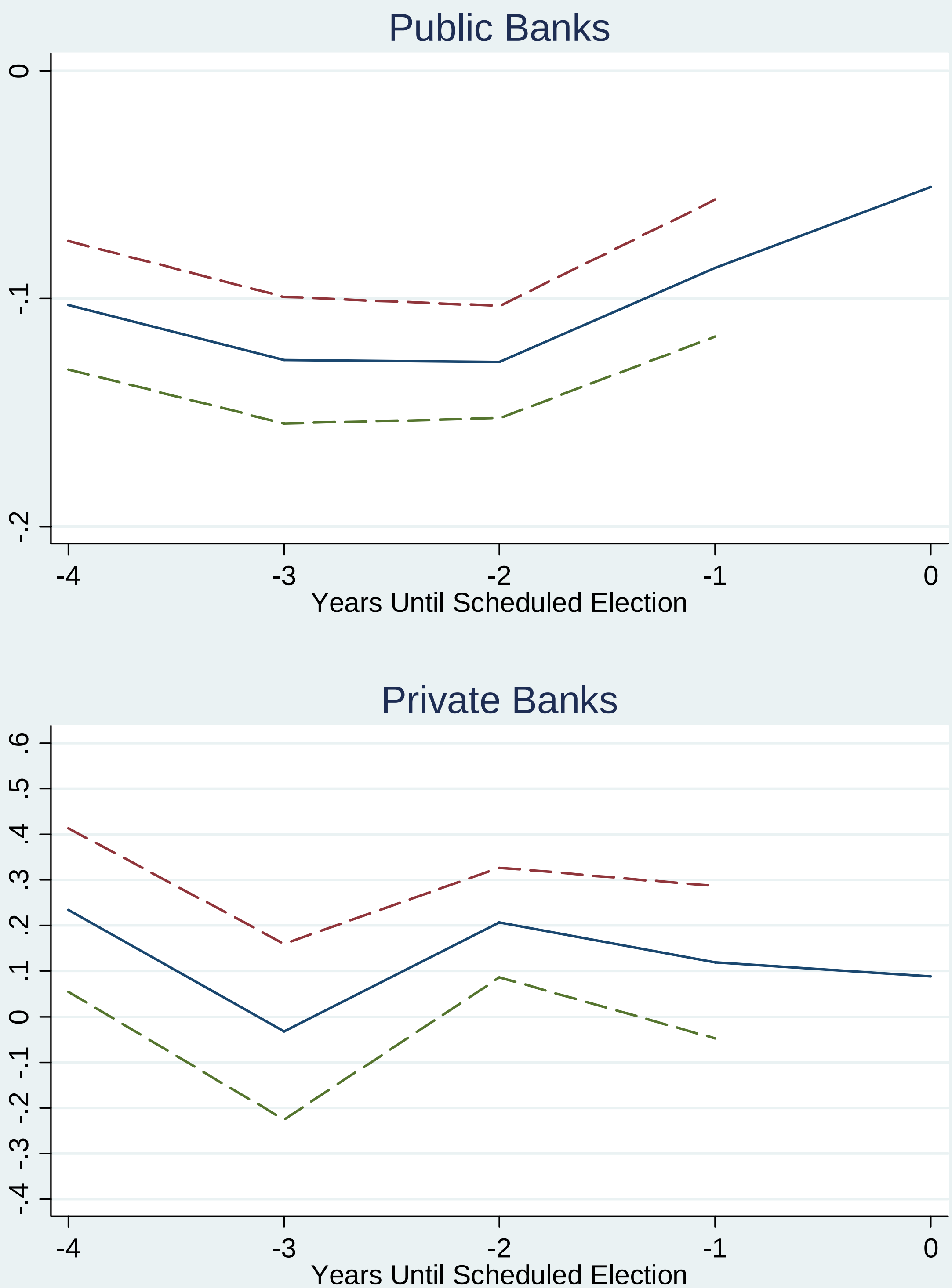
Note: The panels in the figure graph the predicted relationship between agricultural credit levels from public sector banks and political support of the state majority party. Each panel gives the relationship for a different year in the electoral cycle.

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 fifteen. Dotted lines give the 95 percent confidence interval.