Essays on Development and Finance

by

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Abstract

This thesis is a collection of three empirical essays on economic development and finance.

Chapter 1 examines how politicians influence the lending decisions of government owned-banks, particularly whether government resources are used to achieve electoral goals. Theories of electoral competition predict how politicians may allocate resources to win elections: distributing more resources prior to election years, and targeting these resources towards "close" races. I find strong evidence of manipulation in agricultural lending by government banks. More credit is lent just prior to election years. Moreover, this spike is most pronounced in districts in which the previous election was close. I document that these distortions are costly: repayment rates vary with the electoral cycle, while output does not.

Chapter 2 tests theories of public and private ownership of banks. In 1980, the government of India nationalized some private banks while leaving similar banks in private hands. Using a regression discontinuity design, I find that government owned banks grew less quickly and lent more to agriculture. These differences manifest themselves in outcomes across credit markets in India as well. Villages whose banks were nationalized received a substantial increase in agricultural and total credit, at lower interest rates, than villages whose banks were not. Strikingly, the additional credit had no effect on real agricultural outcomes, and may have hurt employment in trade and services.

Chapter 3 investigates the economics of manumission, a process whereby a slave purchases her own freedom. Using newly collected data from Louisiana, I first paint a qualitative and quantitative portrait of manumission. I then answer the question of whether slaves purchasing their freedom paid above market prices. Legal changes following the Louisiana Purchase allow me to conclude that manumission laws were quite important in determining the terms at which manumission agreements were struck: when slaves lost the right to sue for self-purchase at market price, there was a precipitous drop in the number of manumissions, while prices paid increased.

Thesis Supervisor: Esther Duflo
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To my mother and Michael

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Contents

Chapter 1

Fixing Market Failures or Fixing Elections? Agricultural Credit in India

Summary 1 *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.*

1.1 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 interests 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

[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.

and defaulting on loans.

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 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. 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 1.3, I discuss competing theories of political redistribution, and their testable predictions. Section 1.4 develops the empirical strategy and presents the main results of political capture. In Section 1.5, I establish that these political manipulations are socially costly: increases in government agricultural credit do not affect agricultural output. Finally, Section 1.6 concludes.

1.2 Banking and Politics in India

1.2.1 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 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.

1.2.2 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

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

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

[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.

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% 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).

1.3 Theories and Tests of Redistribution

1.3.1 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

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

(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 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.

1.3.2 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

[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 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: resources will be targeted towards "swing" districts, or politicians will reward their supporters disproportionately. 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. 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

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

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, 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.

1.4 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.

1.4.1 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 coalition that formed a majority. 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 1.1 gives summary statistics. Because district boundaries or district name had changed, I was unable to match all districts in all years.

[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] 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.

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 is zero over the period studied. As a robustness check, I demonstrate in an appendix (Section 1.7) 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 1.7 also presents tests for stationarity and serial correlation.

1.4.2 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:

$$y_{dst} = \alpha_d + \gamma_{rt} + \delta Rain_{dst} + \beta E_{st} + \varepsilon_{dst} \tag{1.1}$$

[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.

[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.

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 rather than levels, as well as using the Generalized Method of Moments technique developed by Arellano and 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:

$$E_{sdt} = \alpha_d + \gamma_{rt} + \delta Rain_{dst} + \beta^0 S^0_{st} + \varepsilon_{dst} \tag{1.2}$$

Table 1.2 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 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 1.3 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

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

[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.

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 1.4 expands these results by tracing out how lending comoves with the entire election cycle. This requires a straightforward extension of equations 1.1 and 1.2. 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:

$$y_{dst} = \alpha_d + \gamma_{rt} + \delta Rain_{dst} + \beta_{-4}S_{st}^{-4} + \beta_{-3}S_{st}^{-3} + \beta_{-2}S_{st}^{-2} + \beta_{-1}S_{st}^{-1} + \varepsilon_{dst} \tag{1.3}$$

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 1.4 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

[19] See Khemani (2004).

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 1.3 are presented in the appendix. Estimated relationship using changes (Table 1.A4), as well as with the Arellano-Bond estimator in levels and changes (1.A5 and 1.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.

1.4.3 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 1.4.2 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 1.3. 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:

$$y_{dst} = \alpha_d + \pi M_{dst} + \beta_{-4} S_{st}^{-4} + \beta_{-3} S_{st}^{-3} + \beta_{-2} S_{st}^{-2} + \beta_{-1} S_{st}^{-1} + \varepsilon_{dst} \tag{1.4}$$

The estimates are reported in column (2) of Table 1.5. 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 1.4.

The model in equation 1.4 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 1.3 and the results in section 1.4.2: 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 $S_{st}^{-4}, ... S_{st}^{-1}$, 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 1.1, 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 1.5). 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 1.1, 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:

$$\begin{aligned} y_{dst} &= \alpha_d + \beta_{-4}S_{st}^{-4} + \beta_{-3}S_{st}^{-3} + \beta_{-2}S_{st}^{-2} + \beta_{-1}S_{st}^{-1} + \pi^+ M_{dst}^+ + \pi^- M_{dst}^- \\ &\quad + \theta_{-4}^+(M_{dst}^+ * S_{st}^{-4}) + \theta_{-3}^+\left(M_{dst}^+ * S_{st}^{-3}\right) + \theta_{-2}^+\left(M_{dst}^+ * S_{st}^{-2}\right) + \theta_{-1}^+\left(M_{dst}^+ * S_{st}^{-1}\right) \\ &\quad + \theta_{-4}^-(M_{dst}^- * S_{st}^{-4}) + \theta_{-3}^-\left(M_{dst}^- * S_{st}^{-3}\right) + \theta_{-2}^-\left(M_{dst}^- * S_{st}^{-2}\right) + \theta_{-1}^-\left(M_{dst}^- * S_{st}^{-1}\right) + \varepsilon_{dst} \end{aligned} \tag{1.5}$$

Standard errors are again clustered at the state level. Results are presented in the third column of Table 1.5. 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 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

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

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 1.5, 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 1.2 and 1.3 graph the information from the level and growth regressions of equation 1.5 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 1.2 gives the relationship for a notional "swing" district ($M_{dst} = 0$), while Figure 1.3 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 1.A3, 1.A7, and 1.A8 give the results for this approach when estimated in changes, as well as with the Arellano-Bond estimator. Figures 1.4, 1.5, and 1.6 give the results from differenced version of equation 1.3.[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.

1.5 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.

1.5.1 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 loans to supporters of politicians?

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

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 1.6). The results, from equation 1.3 are presented in Table 1.7. 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.

1.5.2 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 1.4.2 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 1.8 presents the first stage of the regression, which is equation 1.3, 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 1.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.

1.5.3 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 1.5.2 (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

[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).

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 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 1.6, 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 discrete break in the determination of 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:

$$y_{v,d} = \alpha_d + \beta * Nat_v + \pi_{1v,1980} + \pi \left(K_{v,1980}\right)^2 + \pi \left(K_{v,1980}\right)^3 + \varepsilon_{c,d} \tag{1.6}$$

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 1.6.[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 clustered by bank.[27]

The first stage relationship between credit and nationalization is obtained by estimating equation 1.6, using the share of public credit in 1992 as the dependent variable. Results are presented in Table 1.9. 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 1.9 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

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

[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.

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, 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 1.10 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 1.10 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.

1.6 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 on lending by 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.

1.7 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 (2001). 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 1.A1. Columns 3 and 4 give tests for stationarity in levels. I use the panel test derived in Levin, Lin, and Chu (2002). 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 1.3 using change in credit, rather than level of credit is:

$$\Delta y_{dst} = \gamma_{rt} + \delta\Delta Rain_{dst} + \beta_{-4}S_{st}^{-4} + \beta_{-3}S_{st}^{-3} + \beta_{-2}S_{st}^{-2} + \beta_{-1}S_{st}^{-1} + \varepsilon_{dst} \tag{1.7}$$

Note that the equation is not strictly analogous to equation 1.3: 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 1.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 1.7 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 1.A3 presents the results of equations based on equation 1.5, using growth in real credit as the dependent variable.

$$\begin{aligned}\Delta y_{dst} &= \beta_{-4} S_{st}^{-4} + \beta_{-3} S_{st}^{-3} + \beta_{-2} S_{st}^{-2} + \beta_{-1} S_{st}^{-1} + \pi^{+} M_{dst}^{+} + \pi^{-} M_{dst}^{-} \\ &\quad + \theta_{-4}^{+} (M_{dst}^{+} * S_{st}^{-4}) + \theta_{-3}^{+} \left(M_{dst}^{+} * S_{st}^{-3}\right) + \theta_{-2}^{+} \left(M_{dst}^{+} * S_{st}^{-2}\right) + \theta_{-1}^{+} \left(M_{dst}^{+} * S_{st}^{-1}\right) \\ &\quad + \theta_{-4}^{-} (M_{dst}^{-} * S_{st}^{-4}) + \theta_{-3}^{-} \left(M_{dst}^{-} * S_{st}^{-3}\right) + \theta_{-2}^{-} \left(M_{dst}^{-} * S_{st}^{-2}\right) + \theta_{-1}^{-} \left(M_{dst}^{-} * S_{st}^{-1}\right) + \varepsilon_{dst}\end{aligned} \tag{1.8}$$

again, the results are very similar to the results in Table 1.5. Figures 1.4-1.6 present the coefficients in 1.7 and 1.8.

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 and 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 1.A5 and 1.A7, and the results for changes are presented in Tables 1.A6 and 1.A8. 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 1.A5 and 1.A6, and at the bottom of Tables 1.A7 and 1.A8. 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, 1.3 and 1.8 are very similar to the preferred specification.

1.8 Data Appendix

The unit of observation throughout the study varies. Section 1.4 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 operate 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 1.5.2. 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, Tehsil name, and state name, to villages in the Bank Branch data set

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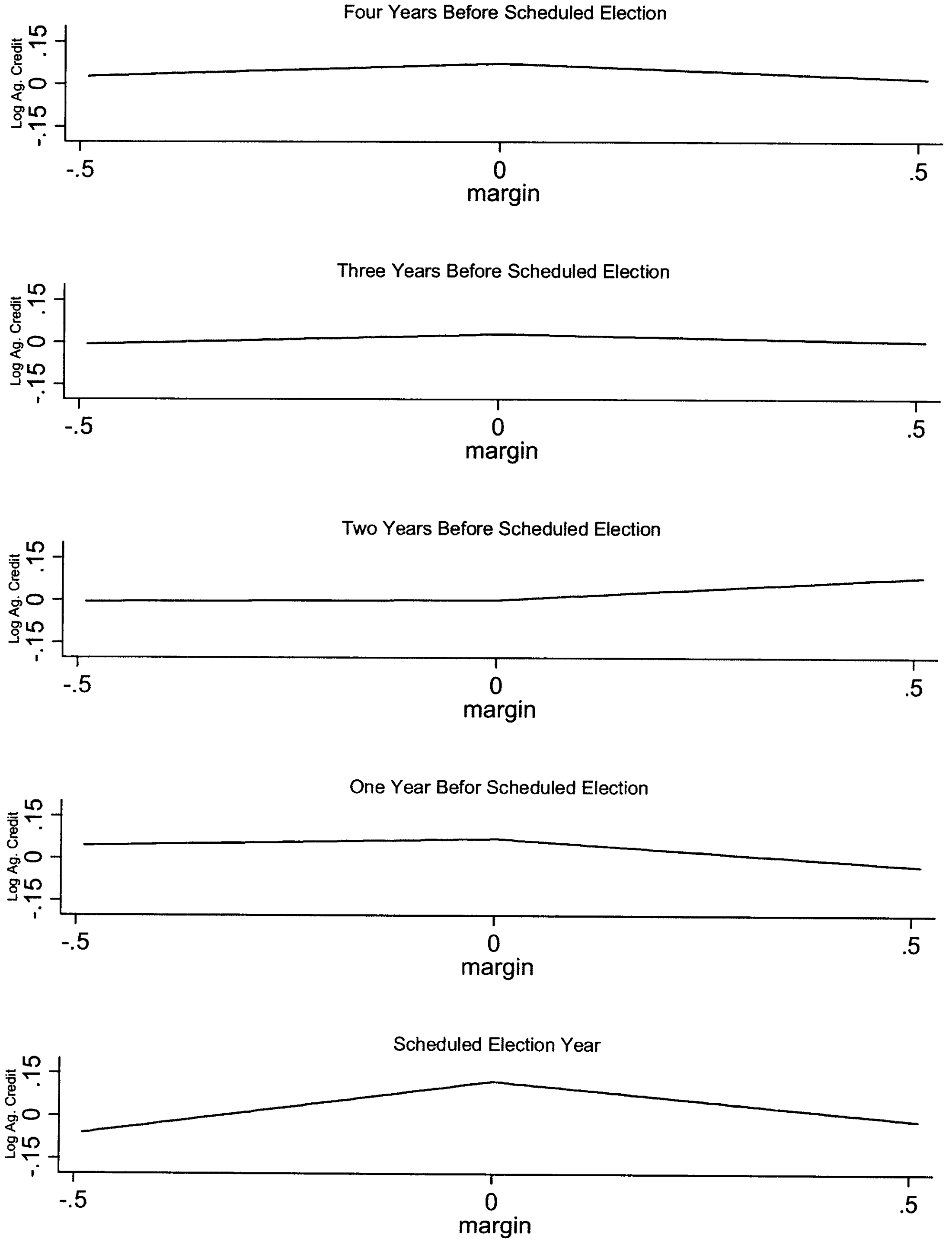
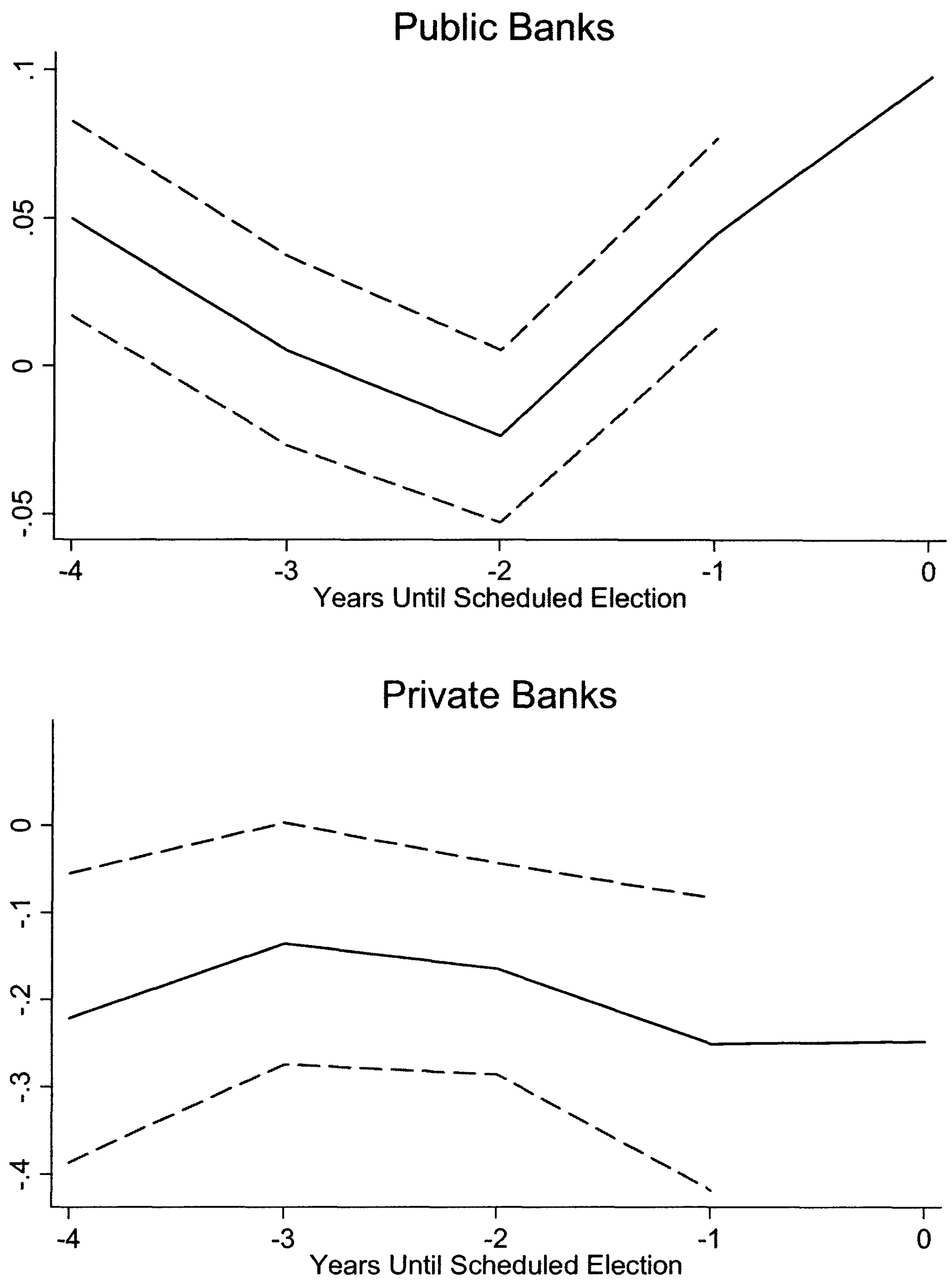
Figure 1.1: Targeted Lending Levels Over the Election Cycle



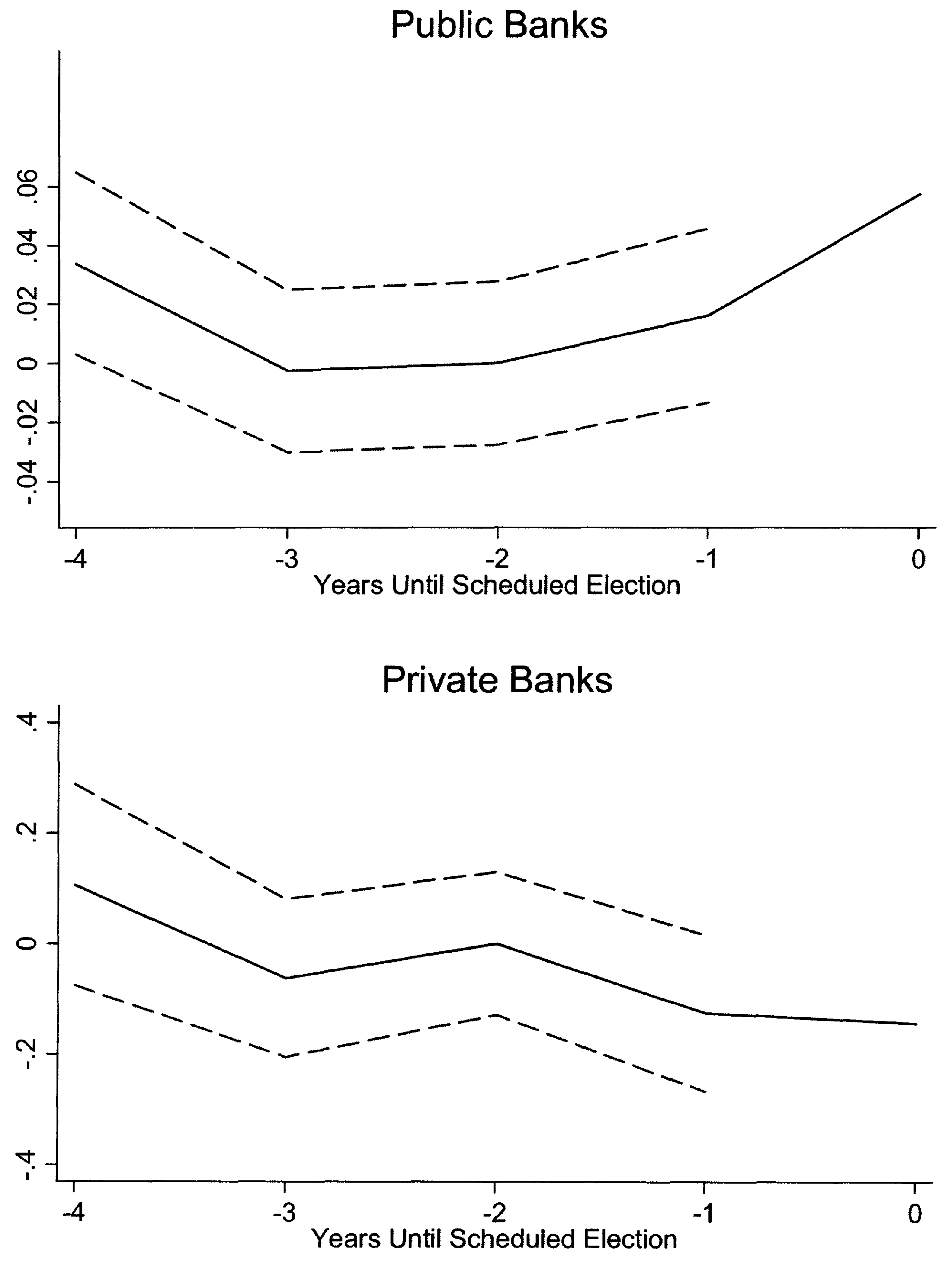
Note: The panels in the figure graph the predicted relationship between agricultural credit levels from public sector banks and political support of the state majority party. Each panel gives the relationship for a different year in the electoral cycle.

Figure 1.2: Cycles in Level of Credit, Swing District



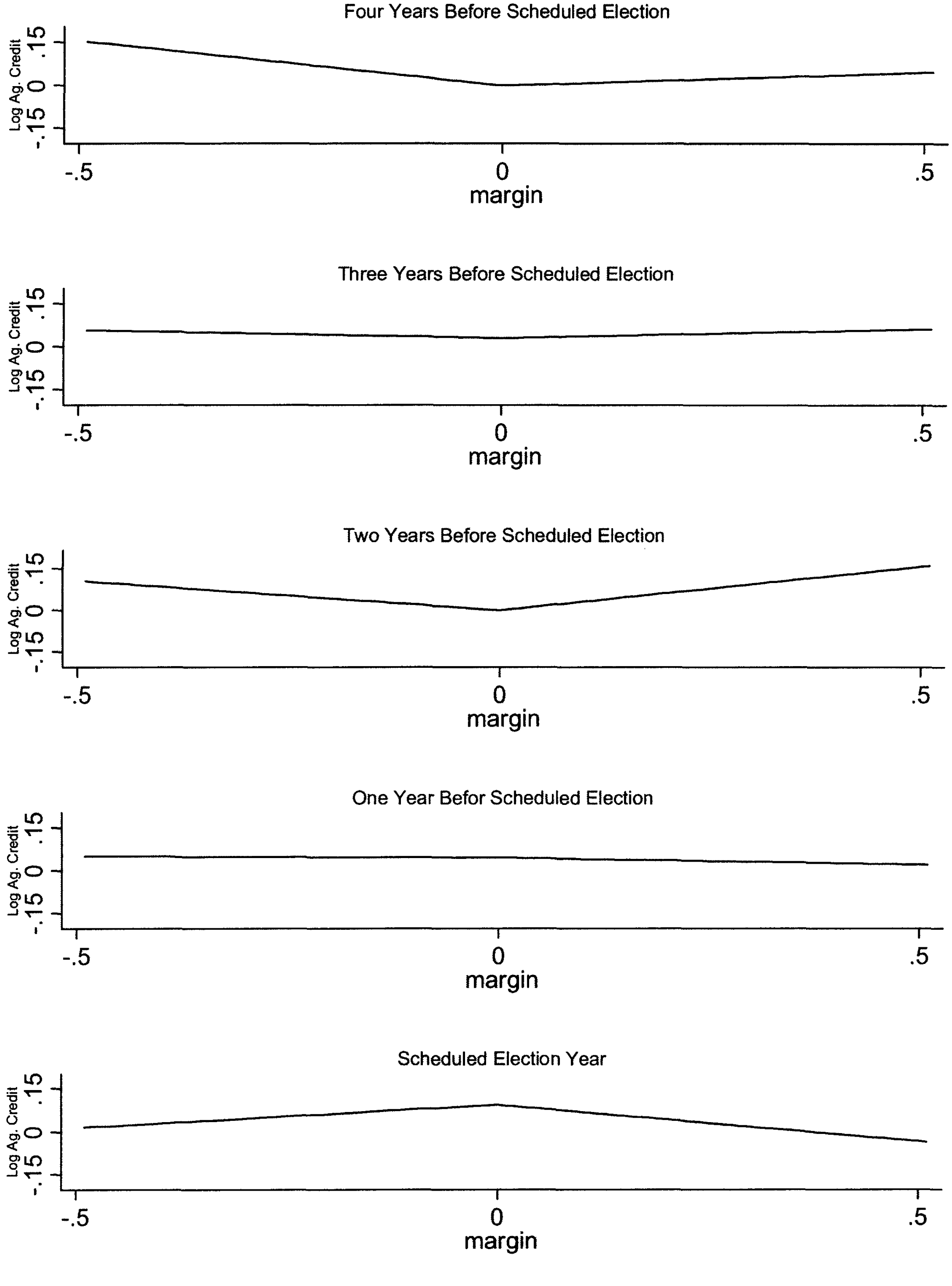
Note: Predicted agricultural credit for a notional district in which the margin of victory in the previous election was zero. Dotted lines give the 95 percent confidence interval.

Figure 1.3: Cycles in Level of Credit, Non-Swing District



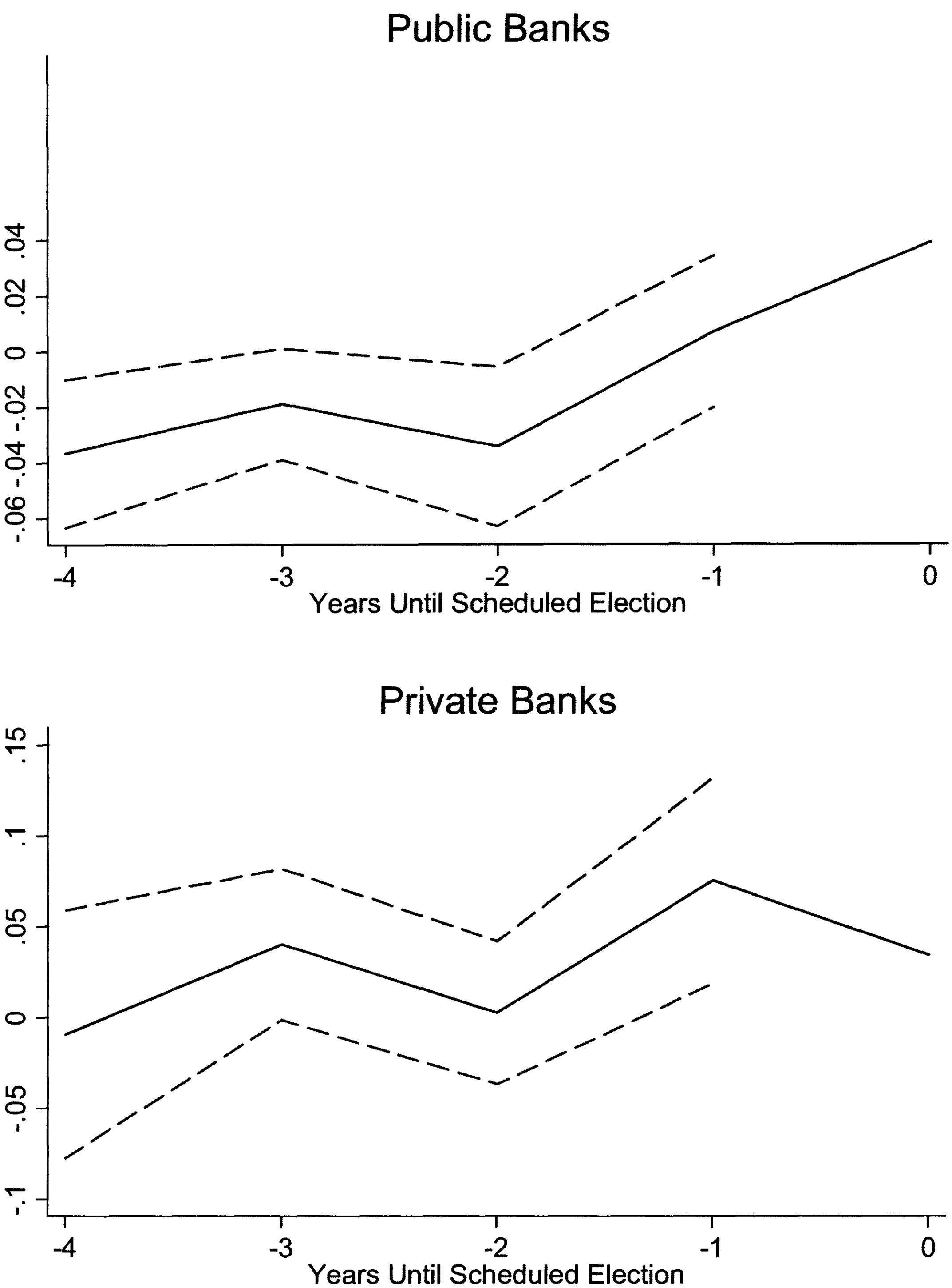
Note: Predicted agricultural credit for a notional district in which the margin of victory in the previous election was fifteen. Dotted lines give the 95 percent confidence interval.

Figure 1.4: Targeted Lending Growth Over the Election Cycle



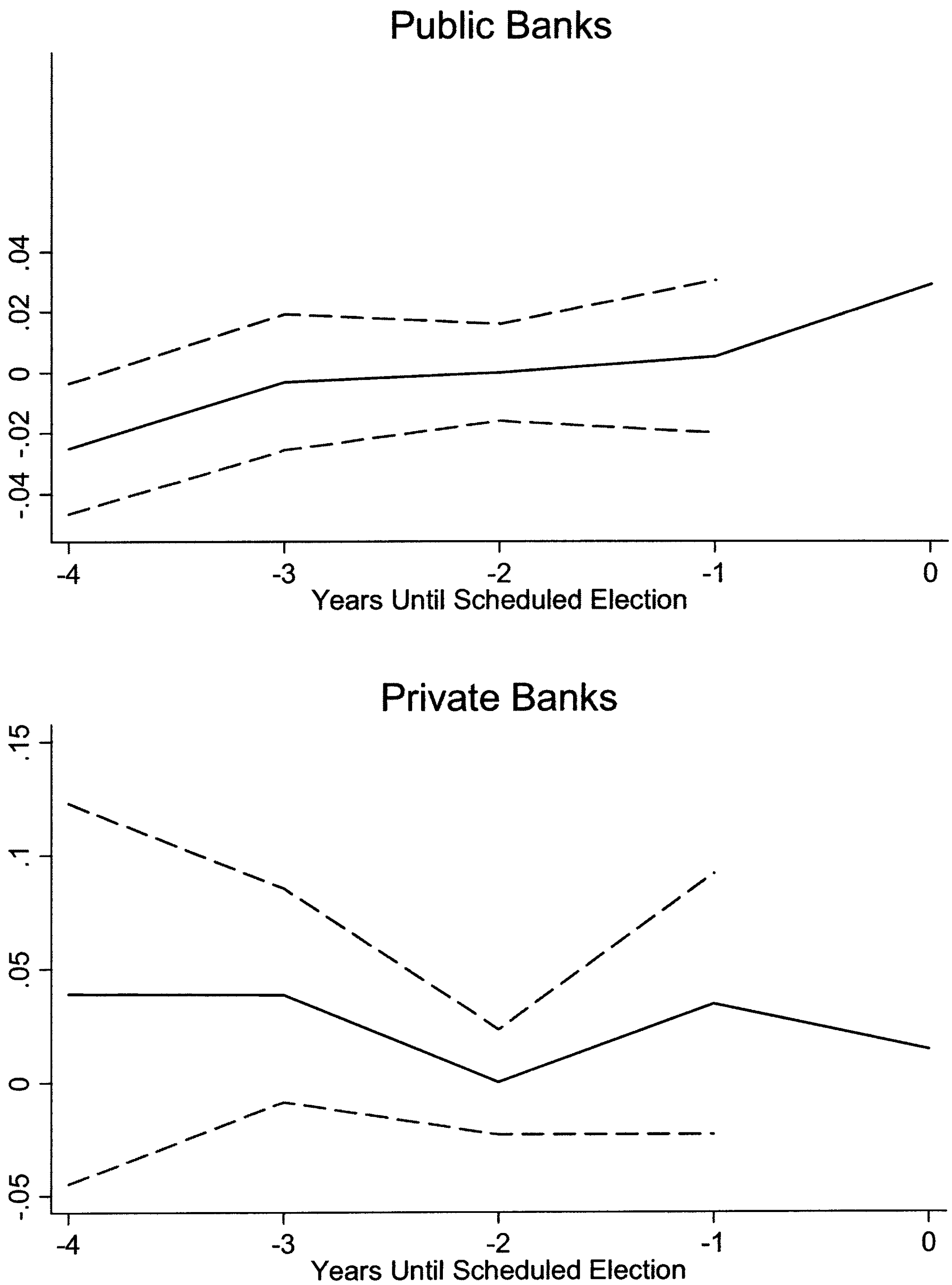
Note: The panels in the figure graph the predicted relationship between agricultural credit growth from public sector banks and political support of the state majority party. Each panel gives the relationship for a different year in the electoral cycle.

Figure 1.5: 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 1.6: 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 fifteen. Dotted lines give the 95 percent confidence interval.

Table 1.1: Summary Statistics for Political Lending

Panel A: Summary Statistics for Lending Cycle Regressions (19 states)	Mean	Std. Dev	N
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 1.2: 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 1.3: 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	19	19	17	19	19	17	19	19	17
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	19	19	17	19	19	17	19	19	17
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. 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 1.4: 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 1.5, 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 * Four Years until Election			0.163	
			(0.117)	
Positive Margin * Three Years until Election			0.220	
			(0.161)	
Positive Margin * Two Years until Election			0.430 **	
			(0.199)	
Positive Margin * One Year until Election			0.080	
			(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) * Four Years until Election				0.329 **
				(0.137)
Absolute(Margin) * Three Years until Election				0.462 ***
				(0.147)
Absolute(Margin) * Two Years until Election				0.611 ***
				(0.165)
Absolute(Margin) * One Year until Election				0.149
				(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 1.5, 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 * Four Years until Election			1.487	
			(1.047)	
Positive Margin * Three Years until Election			-0.201	
			(0.913)	
Positive Margin * Two Years until Election			0.410	
			(0.931)	
Positive Margin * One Year until Election			0.142	
			(0.936)	
Negative Margin * Cycle Dummy				
Positive Margin * Four Years until Election			0.687	
			(0.905)	
Margin * Three Years until Election			1.074	
			(0.882)	
Margin * Two Years until Election			1.340	
			(0.936)	
Margin * One Year until Election			1.611	
			(1.410)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) * Four Years until Election				1.075
				(0.967)
Absolute(Margin) * Three Years until Election				-0.148
				(1.068)
Absolute(Margin) * Two Years until Election				-0.318
				(1.114)
Absolute(Margin) * One Year until Election				-0.111
				(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 1.6: 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 1.7: 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 1.8: Agricultural Credit and Agricultural Output

	Years Until Next Scheduled Election			
	Four	Three	Two	One
Panel A: Real Log Agricultural Credit				
Agricultural Credit, All Banks	-0.016	-0.035 *	-0.038 **'	-0.028 ***
	(0.018)	(0.019)	(0.013)	(0.020)
Agricultural Credit, Public Banks	-0.029	-0.046 **	-0.052 **'	-0.042 **
	(0.021)	(0.022)	(0.013)	(0.021)
Agricultural Credit, Private Banks	-0.386	-0.142	-0.212	-0.202
	(0.570)	(0.286)	(0.368)	(0.233)
Panel B: Real Log Agricultural Output, Reduced Form				
Real Log Agricultural Output	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 agricultural credit, while in Panel B the dependent variable is log 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

	Total Credit	Gov't Credit	Private Credit
Bank Credit	-0.42	-0.15	0.08
	(0.93)	(1.11)	(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 1.9: 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 **	0.185 ***
	(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 1.10: 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 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 1.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 $\rho=-.5$.

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 1.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 Banks				
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 Banks				
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 1.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 * Four Years until Election			0.143	
			(0.153)	
Positive Margin * Three Years until Election			0.171	
			(0.148)	
Positive Margin * Two Years until Election			0.294	
			(0.200)	
Positive Margin * One Year until Election			0.054	
			(0.171)	
Negative Margin * Cycle Dummy				
Negative Margin * Four Years until Election			-0.412 ***	
			0.123	
Negative Margin * Three Years until Election			-0.187	
			0.126	
Negative Margin * Two Years until Election			-0.376 ***	
			0.109	
Negative Margin * One Year until Election			-0.205 *	
			0.112	
Absolute Margin * Cycle Dummy				
Absolute(Margin) * Four Years until Election				0.386 **
				(0.169)
Absolute(Margin) * Three Years until Election				0.275 **
				(0.128)
Absolute(Margin) * Two Years until Election				0.285 *
				(0.155)
Absolute(Margin) * One Year until Election				0.007
				(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 1.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 * Four Years until Election			0.452	
			(0.543)	
Positive Margin * Three Years until Election			0.120	
			(0.269)	
Positive Margin * Two Years until Election			0.115	
			(0.203)	
Positive Margin * One Year until Election			-0.141	
			(0.396)	
Negative Margin * Cycle Dummy				
Positive Margin * Four Years until Election			-0.316	
			(0.230)	
Margin * Three Years until Election			-0.363	
			(0.255)	
Margin * Two Years until Election			-0.155	
			(0.293)	
Margin * One Year until Election			-0.226	
			(0.352)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) * Four Years until Election				0.446
				(0.324)
Absolute(Margin) * Three Years until Election				0.044
				(0.187)
Absolute(Margin) * Two Years until Election				-0.224
				(0.342)
Absolute(Margin) * One Year until Election				-0.381
				(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 1.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 1.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 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. 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 1.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 1.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 * Four Years until Election			0.233 **	
			(0.115)	
Positive Margin * Three Years until Election			0.381 **	
			(0.188)	
Positive Margin * Two Years until Election			0.582 ***	
			(0.174)	
Positive Margin * One Year until Election			0.339 *	
			(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) * Four Years until Election				0.385 ***
				(0.109)
Absolute(Margin) * Three Years until Election				0.363 ***
				(0.140)
Absolute(Margin) * Two Years until Election				0.417 ***
				(0.141)
Absolute(Margin) * One Year until Election				0.065
				(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 1.A7: Arellano-Bond Estimates of Credit Level Targeting (cont.)

Panel B: Private 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	0.07	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 1.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 * Four Years until Election			0.137	
			(0.132)	
Positive Margin * Three Years until Election			0.444 **	
			(0.212)	
Positive Margin * Two Years until Election			0.536 ***	
			(0.202)	
Positive Margin * One Year until Election			0.426 **	
			(0.213)	
Negative Margin * Cycle Dummy				
Negative Margin * Four Years until Election			-0.320 **	
			(0.136)	
Negative Margin * Three Years until Election			-0.150	
			(0.154)	
Negative Margin * Two Years until Election			-0.241	
			(0.163)	
Negative Margin * One Year until Election			-0.216	
			(0.177)	
Absolute Margin * Cycle Dummy				
Absolute(Margin) * Four Years until Election				0.351 ***
				(0.121)
Absolute(Margin) * Three Years until Election				0.358 **
				(0.158)
Absolute(Margin) * Two Years until Election				0.242
				(0.163)
Absolute(Margin) * One Year until Election				0.057
				(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 1.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

Chapter 2

Financial Development, Bank Ownership, and Growth. Or, Does Quantity Imply Quality?

Summary 2 *In 1980, the Indian government, following a simple policy rule, nationalized some banks while leaving others in private hands. Using a regression discontinuity design, I evaluate theories of ownership, and the relative importance of quality versus quantity of financial intermediation on real economic outcomes. Compared to banks that remained private, nationalized banks grew less quickly, and lent more to agriculture and rural areas. These differences manifest themselves in outcomes across credit markets in India. Villages whose banks were nationalized received a substantial increase in agricultural and total credit, at lower interest rates, than villages whose banks were not. Strikingly, this additional credit had no effect on real agricultural outcomes, and nationalization of banks may have hurt development in trade and service industries. These results are consistent with the view that the quality of intermediation matters: loans issued by government banks are substantially more likely to be non-performing, and government-owned banks suffered greater financial losses than private banks.*

2.1 Introduction

Economists and states have long been interested in the relationship between financial development and economic growth, and promoting financial development has been an integral part of many countries' development strategies. While recent influential research has found a positive causal link between financial development and growth, much less is known about the efficacy of government efforts to promote financial development.[1]

Perhaps the most important means governments have used to promote development is public ownership of banks. In socialist countries, proponents of nationalization argued that economic planning required control of the banks (e.g., Lenin, Gershenkron, etc.). But even those who favored market-based systems found reasons to support public ownership of banks: government intervention in rural areas could both mobilize deposits and improve the lives of the poor; credit market failures and lender moral hazard problems were severe enough that regulation alone was felt insufficient; and some feared monopolistic behavior in the industrial credit market could limit entry. Proponents of nationalization succeeded in both developing and developed economies. La Porta, Lopez-De-Silanes and Shleifer (2002) calculate that in countries around the world the average share of equity of the ten largest banks held by governments was 42% in 1995.

This question is directly related to the debate on the merits of state ownership of any enterprise. Advocates believe that government ownership can solve market failures, and enhance equity. Opponents worry that the soft incentives typically faced by public sector employees lead to inefficiency, and that public enterprises are subject to political capture. Finally, it must be noted that public ownership is not the only way to achieve "social" goals: regulation of private banks is an alternative approach. However, if social goals are not contractible, or if regulatory power is limited, government control may accomplish what regulation cannot. Whether the costs of government ownership exceed the benefits is a vital empirical question, yet there is relatively little careful empirical evidence on this issue.

La Porta et. al. (2002) estimate cross-country regressions, finding government ownership

[1]E.g. King and Levine (1993) and Rajan and Zingales (1998). However, Levine (2004), reviewing the empirical literature, cautions that available evidence suffers from "serious shortcomings," and that "we are far from definitive answers to the questions: Does finance cause growth, and if so, how?" (p. 3)

of banks negatively correlated with financial development and growth. Causal interpretation is however made difficult by their finding that government ownership of banks correlated with many other things, such as government intervention in the economy and marginal tax rates. Sapienza (2004) and Mian (2003) use micro-level data to compare public and private sector banks in Italy and Pakistan, respectively. Sapienza finds that public sector banks lend at lower interest rates, and with a bias towards poorer areas, compared to private banks, and that some lending appears to be politically motivated. Mian finds that government-owned banks are more likely than private banks to lend to firms whose directors or executives have political affiliation, and less likely to collect on these loans. These papers do not, however, directly link bank lending behavior to real outcomes.

In a paper closely related to this one, Cole (2004) demonstrates that public sector banks in India are subject to substantial government capture. I show that the allocation of public credit is manipulated to meet the electoral goals of politicians, with lending booms around election years, and targeting of credit to "swing" districts. This manipulation is costly: I show the marginal political loan is less likely to be repaid. Using electoral lending booms as an instrument for agricultural credit, I find that the marginal agricultural loan has no impact on agricultural output.

However, studying lending behavior alone is not sufficient to answer the most important question of how government ownership of banks affects growth. It may be that both theories are right: government ownership leads to capture and inefficiency, but also cures market failures. In this case, the desirability of government banks hinges crucially on the real effects of ownership.

The present paper uses a policy experiment in India to evaluate the effect of government ownership of banks on financial and economic development. In 1980, the government of India nationalized some, but not all, private banks according to a strict policy rule, leaving comparable banks in both public and private hands. Nationalization made banks more responsive to the goal of lending to agriculture, but did not have cause banks to lend more to small scale industry. Nationalized banks grew at a slower rate in the 1990s, and suffered greater financial losses than private banks.

Because the 1980 nationalization induced variation in the share of credit issued by public banks across credit markets in India, the causal effect of nationalization on economic outcomes across credit markets can be determined. Credit markets with more nationalized banks issued a higher share of credit to agricultural and rural borrowers and had lower lending rates, and initially experienced faster financial development. This came at the cost of lower quality intermediation and slower financial development in the 1990s. Most strikingly, despite substantial increases in agricultural credit, there is no evidence of improved agricultural outcomes in markets with nationalized banks. Bank nationalization may have slowed the growth of employment in the more developed sectors of trade and services.

This paper makes three main contributions to the literature. The richness of available data provide a comprehensive picture of the effect of ownership on lending behavior. In particular, I measure whether nationalization caused banks to achieve the "social" goals of the government. Second, because the nationalization occurred according to a strict policy rule, and because public and private banks face identical regulation, differences in lending behavior and outcomes can be attributed to bank ownership, rather than characteristics of the bank (such as size or regulation). Third, by focusing on India, I avoid interpretation problems associated with cross-country regressions. Finally, by combining credit data with real outcomes, this paper demonstrates a causal link between shocks to financial development and real economic outcomes.

This paper proceeds as follows. In the next section, I describe two competing theories of government ownership of banks, including their empirical predictions. Section 2.3I describe the Indian bank nationalization in detail, and describe the data. Section 2.4 examines the effect of bank ownership on bank performance, compares the costs of government assistance to private banks to the cost of assistance to public banks, and describes how nationalization affected sectoral allocation of credit at the bank level. Section 2.5 links bank ownership to real outcomes, by comparing financial development, credit market outcomes, and employment and agricultural investment in towns whose bank branches were nationalized to outcomes in towns whose branches were not nationalized. Section 2.6 concludes.

2.2 Theoretical Framework

The debate on government intervention in the financial sector is an old one, and hardly settled: even the most market-oriented economies substantially regulate the financial industry.

Following La Porta et al., I label the rationale in favor of government ownership of banks the "development" view, and the rationale opposed the "political" view.

The development view argues that government intervention in credit markets can aid both financial and economic development, for a variety of reasons. Intervention may cure market failures such as credit rationing, lead to the funding of socially desirable but privately unprofitable investment, limit the monopolistic behavior of banks, facilitate simultaneous investment in several sectors of the economy, or prevent inefficient risk-taking by private bankers.

The political view argues that greater government control of the financial sector will lead to political capture. There are several ways this capture could manifest itself: politicians may enrich themselves, or reward supporters in inefficient ways. Government banks may undertake socially undesirable projects. Employees may face weak incentives and thus shirk. Opponents of government ownership of banks argue that these costs outweigh any potential gain from intervention.

The development view has found some support in India: Burgess and Pande (2005) study a branch expansion program in India, and find that increases in rural banking led to lower poverty rates, and greater diversification of economic activity. They do not, however, distinguish between government and private banks, both of which expanded into rural areas.

The Indian experience provides an attractive environment in which to test theories of ownership, because a rich set of credit market and real outcomes are observed. Sections 2.4 and 2.5 will paint a detailed portrait of the effect of nationalization on bank performance, and village-level outcomes for financial development, the credit market, employment, and agricultural investment. Before proceeding, I briefly describe what the competing models predict for the variables observed.

These predicted effects of government ownership of banks (relative to private ownership of banks) are summarized in Table 2.1. The development view is ambiguous about the speed at which banks would grow: if public sector banks fund projects that have a high social return but low private return, it could slow the growth rate of banks. Alternatively, if nationalization cures market failures, the size of the credit market could increase. The political view predicts that government banks, because they are inefficient or fund projects with low returns, will grow less quickly than private banks, *ceteris paribus*. However, if government banks face a soft budget constraint, they may grow more quickly.

The cost to the government of making good depositors of failed private banks can be substantial (Dinc and Brown, 2004), as can the cost of recapitalizing public banks. The development view argues risk-taking by private banks is more costly to society than poor screening and monitoring by public sector bank employees facing weak incentives. The political view predicts the opposite.

Predictions for financial development follow a similar pattern. Government banks could speed up financial development, through expansion into unbanked areas and low minimum balance requirements for depositors. Or, public banks may squander scarce capital for political projects, slowing down financial development.

The predictions for interest rates and sectoral allocation of credit are similar under both theories: government intervention may lead to lower interest rates, and targeting of credit towards "key" sectors. Under the political view, however, these results will obtain only if they give some private benefit to politicians.

Loan-making institutions face a fundamental agency problem: in both public and private banks, because the officer making a decision on loans is not the residual claimant, it is difficult to provide her with correct incentives.[2] The social view of banking predicts that the quality of intermediation should not vary with ownership. The political view predicts that loan default rates will be higher in government owned banks, both because loans may be allocated to the wrong projects,and because lending officers may face weaker incentives.

Perhaps the most fundamental test of the two theories is the effect of ownership of banks on real outcomes. Particularly given the focus on agricultural credit in India, the development view suggests that more agricultural investment should be observed in areas with nationalized banks. Finally, the development view predicts nationalized banks may increase the overall speed of economic development, including employment in industry, the service sector, and trade. The political view does not.

Neither view is likely to be always and everywhere correct. Nor do the theories always predict different patterns in the data. However, there are enough opposing predictions for the

[2]Banerjee, Cole, and Duflo (2004) demonstrate that lending officers in government banks in India may be reluctant to make risky loans, because they fear prosecution for corruption. Brickley, Linck, and Smith (2003) provide evidence that the organizational form of large private banks limits their ability to lend in specific environments.

measured outcomes that it is reasonable to expect to be able to distinguish the two.

2.3 Indian Bank Nationalization and Data

2.3.1 Bank Nationalization

Formal banking has a long history in India. The 18^{th} century founding of the English Agency Houses in Bombay and Calcutta was followed a century later by the entry of foreign banks, the founding of domestic presidency banks, and later joint stock banks. In 1935, the presidency banks were merged together to form the Imperial Bank of India, which was subsequently renamed the State Bank of India. Also that year, India's central bank, the Reserve Bank of India (RBI), began operation. By the time of independence, there were over fifty banks operating over 1,500 bank branches in India.

The post-independence development strategy of India involved planning and government intervention in almost all aspects of the economy. In particular, the Indian government felt that banks were not lending enough to those who needed credit most. Following independence, the RBI was given broad regulatory authority over commercial banks in India. In 1959, the State Bank of India acquired the state-owned banks of eight former princely states. These state banks comprised 31% of branches in India in June, 1969. The largest government intervention occurred in July 1969, when the government nationalized all banks whose nationwide deposits were greater than Rs. 500 million. This resulted in the nationalization of 14 banks, or 54% of the branches in India at that time.

Prakash Tandon, a former chairman of the Punjab National Bank (nationalized in 1969), describes the rationale for nationalization as follows:

> Many bank failures and crises over two centuries, and the damage they did under 'laissez faire' conditions; the needs of planned growth and equitable distribution of credit, which in privately owned banks was concentrated mainly on the controlling industrial houses and influential borrowers; the needs of growing small scale industry and farming regarding finance, equipment and inputs; from all these there emerged an inexorable demand for banking legislation, some government control and a central banking authority, adding up, in the final analysis, to social control

and nationalization.[3]

Between 1969 and 1980, new private banks were founded, and the growth rate of private bank branches exceeded that of public bank branches. By April 1, 1980, private banks accounted for approximately 17.5 percent of all branches in India. In April of 1980, the government undertook a second round of nationalization, placing under government control the six private banks whose nationwide deposits were above Rs. 2 billion, or a further 8 percent of bank branches, leaving approximately 10 percent of bank branches in private hands. The share of private bank branches stayed fairly constant between 1980 to 2000.

Nationalized banks remained corporate entities, retaining most of their staff, with the exception of members of the board of directors, who were replaced by appointees of the central government. The political appointments included representatives from the government, industry, agriculture, as well as the public.

The breadth and scope of the Indian banking sector is perhaps unmatched by any other country of comparable income. Indian banking has been remarkably successful at achieving mass participation. Between 1970 and the present, over 58,000 bank branches were opened in India. These new branches, as of March 2003, had mobilized over 9 trillion Rupees in deposits, and indeed bank branches founded since 1970 represent the overwhelming majority of deposits in Indian banks.[4] This rapid expansion is attributable to a policy which required banks to open four branches in unbanked locations for every branch opened in banked locations.

2.3.2 Data

A major strength of this study is the richness and scope of banking data collected by the Reserve Bank of India. The "Basic Statistical Returns-2" contain information on bank lending. Each year, every bank branch in India is required to provide information on every loan in its portfolio to the Reserve Bank of India. This information includes the size of the loan, interest rate, and performance status, as well as various characteristics of the borrower, including industry (at the three-digit level), rural/urban status, etc.[5] The analyses in this paper are therefore based

[3]Tandon (1989, p. 198).

[4]Statistical Tables Relating to Banks in India, 2003

[5]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.

on a *census*, rather than sample, of loans in India. Geographic identifiers allow for the study of outcomes across three thousand banking markets. The comprehensiveness of the data allows for relatively fine distinctions, as well as confidence that the results presented in Section 2.5 represent general, rather than partial, equilibrium effects. Finally, this is one of very few studies available that examines the sectoral allocation of credit, and the resultant implications for real economic outcomes.

Data on bank branch locations, used to compute the market share of public and private banks in 1980, are from a directory of commercial banks, published by the RBI in 2000, which gives the opening (and closing) date of every bank branch in India, and indicates in which credit market each branch is located.

Finally, annual aggregate deposit and credit data, by branch, are available from 1981-2000. These data are used to evaluate the effect of nationalization on the financial development, and to control for initial conditions when evaluating the outcomes.

Data on bank balance sheets are also from the Reserve Bank of India: various issues of the "Statistical Tables Relating to Banks in India" (1963-1970) and "Banking Statistics" (1972-2000). Village level outcome variables are from the Primary Census Abstracts and Village Abstracts of the 1991 Indian Census.

Appendix Table 2.A1 gives summary statistics for the outcome variables at the village level.

2.4 The Effect of Ownership on Bank Performance

2.4.1 Identification Strategy

A striking result of La Porta et. al. (2002) is that public ownership of banks retards financial development, as measured by credit or deposits per capita, in developing countries. They run cross-country style regressions, and find that an increase in government ownership in banks of 10% leads to .24 percentage points slower annual growth in credit to GDP ratio, from 1970 to 1995. However, they also show that government ownership of banks is positively correlated with government intervention in the economy, and negatively correlated with government efficiency, security of property rights, rule of law, and other factors thought crucial to the determination of economic growth. Indeed, while they find a negative correlation between economic growth and government ownership of banks, this relationship is not distinguishable from zero if a control for either government intervention in the economy, the top marginal tax rate, or share of state owned enterprises in the economy is included in the regression.

Comparing nationalized to non-nationalized banks in India may be a more valid strategy to establish the relationship between financial development and bank ownership: exploiting within-country variation avoids many of the problems of cross-country regressions.

Many of the empirical results below compare private banks to public banks: thus, understanding whether banks differ by nationalization status, for reasons other than ownership, is critical. The Indian nationalization is perhaps unique in its use of a strict policy rule, based on deposits, to select which banks were nationalized. This rules out the possibility that just the better- or worse-performing banks were nationalized. Of course, if the cutoff line were strategically placed to include (or exclude) exceptionally performing banks, comparisons between public and private banks would be suspect. While the Government of India gave no indication of why it chose the particular cutoffs, and contemporary press accounts do not discuss how they were determined,[6] I have found no evidence that the cutoff was based on any factor other than size.

The identification strategy in this paper depends on the claim that the banks nationalized in 1980 were comparable to those that were not. Certainly the banks that were nationalized were larger than those that remained private. Thus, any comparison of the two should condition on size. However, if the nationalized banks differed along additional dimensions, such as profitability or growth rate, then controlling for size would not be sufficient. Fortunately, this can be tested. Table 2.2 compares the average size of deposits, number of branches, profits, deposits per branch, return on assets (profits/deposits) and return on equity of the nationalized and non-nationalized banks. Panel A gives these values for the six nationalized banks, and the 41 private banks as of December 31, 1979. Column (3) gives the p-value for a test of the difference in means. Not surprisingly, the banks that were to be nationalized were larger (both deposits and branches), and had greater profits. However, once variables are scaled by bank size, there

[6] These breakpoints were by no means unambiguous. Indeed, India's Supreme Court initially struck down the 1969 nationalization, on the grounds that nationalizing banks whose deposit base was larger than Rs. 500 million while allowing the smaller private banks to continue operating was an "arbitrary" distinction. Nationalization was only delayed a few months, however, as the government strengthened the legislation (without changing which banks were to be nationalized) to withstand judicial scrutiny.

is no statistically significant difference between the private and nationalized banks: the amount of deposits per branch, the return on assets, and the return on equity for nationalized and non-nationalized banks are indistinguishable. Much of the analysis below will include a smooth control for bank size: columns 4, 5, and 6 each estimate the following regression, where $g(K)$ is a polynomial in the log-deposits of the bank in 1979:

$$y_{b,79} = \alpha + \beta * Nationalized + g(K) + \varepsilon$$

In column 4, $g(K) = K$, while column 5 adds a quadratic and column 6 a cubic term. The p-value of the test $\beta = 0$ is reported for each variable and each specification. A linear control for size renders β insignificant for all variables in the comparison of nationalized banks to non-nationalized banks (Panel A).

Regression discontinuity design is most credible when it focuses on differences just above, and just below, the discontinuity. To do this, I define as "marginal" the group of banks that were closest to the discontinuity. This group includes the five smallest private banks that were nationalized, and the 18 largest private banks that were not. (Eighteen was chosen because the total assets of those 18 are approximately equal to the assets of the five smallest nationalized banks.)

The choice of how many banks to designate as marginal is a trade-off: a larger set gives more statistical power, but renders the largest and smallest banks more dissimilar. A previous version of this paper demonstrates that the results presented here are generally robust to designating different sets of banks as "marginal." (E.g., the four smallest nationalized, and the 12 largest non-nationalized, etc.) These results are available from the author.

Panel B of Table 2.2 tests whether, prior to nationalization, nationalized and non-nationalized banks were different, after controlling for size. Once linear controls are included, there is no detectable difference between any of the variables for either group. In the next section, I will test whether the nationalized and non-nationalized banks grew at a similar rate prior to the 1980 nationalization.

2.4.2 Bank Growth

Standard measures of bank performance, such as return on equity, or return on assets, are of limited value in the Indian context. To determine whether public ownership of banks inhibits financial intermediation, I compare the growth rates of the banks that were just above and below the 1980 nationalization cut-off, using data from the Reserve Bank of India, for the period 1969 to 2000.[7]

In particular, I regress the annual change in bank deposits, credit, and number of bank branches on a dummy for post nationalization (Post_t=1 if the year is between 1980 and 1991), and a dummy for nationalization in a liberalized environment ($\text{Nineties}_t = 1$ if the year is between 1992 and 2000). I split the post-nationalization period into two periods (1980-1991 and 1991-2000) because the former period was characterized by continued financial repression, while substantial liberalization efforts began in the early 1990s. Public and private banks could well behave differently before and after liberalization. Because large banks may grow at different rates than small banks, I include a cubic term in the deposits of the bank as of December 31, 1979, $g(K_{b,80}) = \pi_0 K_{b,80} + \pi_1 K_{b,80}^2 + \pi_2 K_{b,80}^3$. The regression thus measures whether the growth rates of nationalized banks were different from those of non-nationalized banks in three different periods: before nationalization, after nationalization in the 1980s, and the 1990s. The estimated equation is:

$$\begin{aligned} \ln\left(y_{b,t}/y_{b,t-1}\right) &= \alpha + g\left(K_{b,80}\right) + \delta * \text{Nat}_b + \theta_1 * \text{Eighties}_t + \theta_2 * \text{Nineties}_t + \\ &\quad \gamma_1 \left(\text{Eighties}_t * \text{Nat}_b\right) + \gamma_2 \left(\text{Nineties}_t * \text{Nat}_b\right) + \varepsilon_{b,t} \end{aligned} \quad (2.1)$$

The parameters of interest are δ, γ_1 and γ_2. The first (δ) measures whether the banks that were nationalized in 1980 grew at a different rate than non-nationalized banks *before* the 1980 nationalization, while γ_1 and γ_2 test for differential growth rates after nationalization. Standard errors are adjusted for auto-correlation within each bank.[8]

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[7] In 1985, the Lakshmi Commercial Bank was merged with Canara Bank, a large public sector bank, due to financial weakness. In 1993, the New Bank of India (nationalized in 1980) was merged with the Punjab National Bank. Since both the Canara and Punjab National banks were nationalized in 1969, they are not included in the sample.

[8] A more flexible approach would include a bank fixed effect, and omit δ. Doing so leaves estimates in Table 2.3 virtually unchanged.

Table 2.3 presents the results for growth in credit and deposits. As mentioned in section 2.4.1, an identification assumption crucial to this analysis is that prior to nationalization, nationalized and non-nationalized banks were similar. The first line of column 3 in each panel of Table 2.3 reports the estimate of δ for measures of deposit and credit growth rates prior to nationalization. There were no pre-existing differences in bank growth rates prior to nationalization, once size is controlled for.

Following nationalization, the overall rate of growth in deposits and credit slowed substantially for all banks, but there was no differential effect for nationalized and private banks. (The estimated effects of nationalization are -.05 and -.04, not statistically distinguishable from zero.) In the nineties, overall growth in deposits and credit slowed further still. Moreover, in this liberalized environment, nationalization had an effect on growth rates: deposits grew 7% slower, while credit grew 9% less quickly in nationalized marginal banks, relative to non-nationalized marginal banks. These estimates are significant at the ten percent level.

These results may reflect the changing nature of banking in India. During the 1980s, it was relatively difficult for banks to compete: both lending and deposit rates were set by the RBI, and branch expansion was primarily limited to rural, unbanked locations. The 1990s saw the freeing of both lending and deposit rates, and allowed banks to expand where they would find it most profitable.

2.4.3 Bankruptcies, Bailouts, and Bad Loans

Bailouts of both public and private banks have been tremendously costly the world around (Dinc and Brown, 2004). Justification for regulation of private banks is often based on the convex reward function faced by equity holders: negative net-present-value gambles with a large upside may be profitable to bank owners who face limited liability. Concerns about public sector enterprises tends to focus on the potential for political capture. Perhaps the most important problem, faced by both public and private banks, is the distance between equity holders and decision-makers. Loan officers make decisions about substantial amounts of money, and it is very difficult to align the incentives of the bank with the incentives of relatively "low-level" employees (e.g., Berger et. al., 2002).

Comparing public and private banks in India allows for a direct comparison of the relative costliness of failure. Banerjee, Cole, and Duflo (2004) present estimates of the costs of twenty-one private bank failures in India over the period 1969 and 2000, and compare these costs to the cost of bailing out public sector banks. Under the most favorable accounting for public sector banks (which unrealistically assumes they wean themselves completely of government subsidies starting in fiscal year 2004), bailing out public banks cost the government less than did making good the depositors of failed private sector banks. However, a more realistic estimate of the cost of continued recapitalization subsidies to public sector banks would imply that the public sector banks lost a greater portion of their assets to bad loans than did private banks.

2.4.4 Government Ownership and Government Goals

A key goal of nationalization was to shift credit towards agriculture, rural areas, and "small scale industry." The government of India employed two complimentary policies to achieve this goal: nationalization, described above, and "priority sector" requirements, which set sectoral lending targets for credit to agriculture and small scale industries. These requirements applied to both public and private banks.

Banerjee, Cole, and Duflo (2004) evaluate the effect of ownership on bank allocation of credit. We find that government owned banks did indeed lend more to agriculture (the estimated causal effect of nationalization was approximately 8.2%), and more to rural areas (7.3%), and the government (2%), at the expense of credit to trade, transport and finance. Notably, nationalization did not affect the amount of credit lent by banks to Small Scale Industry.

Regulation was less effective than nationalization at achieving agricultural lending goals: private sector banks have consistently fallen short of agricultural targets, while generally meeting overall priority sector requirements. Why the different outcomes? The simple answer appears to be the "bite" of regulation. Both public and private banks that fell short of the priority sector regulations were required to deposit money in government institutions at penalty interest rates. With respect to the agricultural sub-target, however, only public sector banks were required to deposit funds at penalty rates if they fell short: private banks were not.

This begs the question of why the regulations were enforced differentially for public and private banks. While there is no official accounting, contemporary news reports are informative. In 1998 a government report, (the "R.V. Gupta" report, commissioned by the RBI and written by a former Deputy Governor of the RBI), recommended the abolition of agricultural targets. The then minister of agriculture objected, and proposed further increasing agricultural lending limits.[9] Other observers have argued that, during the 1990s, the Reserve Bank of India was intentionally lax in enforcing agricultural regulations, perhaps because it supported their abolition.[10] If the same frictions between regulators and politicians existed in 1969 and 1980, then nationalizations may have been a response by politicians to achieve goals they could not reach through regulation alone.

2.5 The Effect of Ownership on Economic Outcomes

So far, I have demonstrated that nationalized banks grew less quickly than private banks in the 1990s, and described evidence that they lent more to agriculture, rural areas, and the government, at the expense of credit to trade, transport, and finance. This does not, however, necessarily imply that nationalization has had a substantial impact on real outcomes: private banks could have met the growing economy's need for credit, and the differences in sectoral lending could merely represent specialization (or "crowding out") of credit by banks in areas in which they have a comparative advantage. In this section, I exploit the fact that the 1980 nationalization induced variation across credit markets in the share of public banks. This allows for a careful evaluation of the effect of nationalization, in a general equilibrium setting, on financial development, credit markets, and real outcomes.

2.5.1 Identification Strategy and First Stage

Though much of India's banking sector was nationalized in 1969, the banks that remained private grew quickly, and by 1980, there were over 29 private banks in India, operating 4,428 branches. The median private bank in India was large, with 145 branches, and geographically diverse, operating in 118 distinct credit markets. Cities whose branches belonged to banks just above the nationalization cutoff were exposed to more nationalized credit than cities whose branches were just below the cut-off: I exploit this variation to estimate the causal impact of credit on economic outcomes.

[9] "India Ministry Proposes Higher Farm Loan Target," The Hindu, April 28, 1998.
[10] "Agricultural credit: RBI recovers from amnesia," Business Line, May 8, 1998.

The unit of observation in this section is a credit market. The Reserve Bank of India divides India into distinct credit markets, each typically a village, town or city.[11]. The number of banks (in 1980) in a credit market range from zero (in many rural areas) to 972 (Mumbai). The identification strategy in the section is similar in spirit to the one used above. The sample includes all credit markets that had at least one private bank prior to the 1980 nationalization, or 2,928 cities, villages and towns. Of these locations, 1,513 had only one branch, 465 had two branches, 624 had from three to ten branches, and 232 had more than 10 branches.

The most straightforward analysis involves the 1,513 banking markets served by just one branch, belonging to a marginal bank. All of these branches were private prior to the 1980 nationalization. In this case, a standard regression-discontinuity design is suitable:

$$y_{c,d} = \beta * Nationalized_c + g\left(size_{c,80}\right) + h\left(deposits_{c,80}\right) + \delta_d + \varepsilon_{c,t} \tag{2.2}$$

where Nationalized$_c$ is an indicator variable taking the value of one if the branch in city c belonged to a nationalized bank, $size_c$ is the log deposits of the parent bank whose branch was located in city c, and δ_d are district fixed-effects. Note that $size_c$ is the total amount of deposits of all branches of the bank in India, not deposits in the villages's branch. It is of course possible that additional branches opened up between 1980 and 1992, and this banks credit is included as an outcome measure. Throughout the paper, outcomes in 1992 are measured at the credit market level.

It is important to emphasize that nationalization was assigned as a function of India-wide bank deposits, rather than the size of particular branches. This means that some branches in areas with relatively high financial development were not nationalized, and some branches in relatively backward areas that were nationalized.

Since local levels of financial development may affect outcomes independently of bank ownership, I include a three-degree polynomial term in city-specific deposits in 1980, $h\left(deposits_{c,80}\right)$.

A different approach is necessary to include larger towns and cities which had more than one bank branch in 1980. The composition of branches in a city can no longer be parameterized by a single variable, $size_c$, but rather by a distribution function of branches belonging to different

[11]This was done to assist the Reserve Bank of India in determining which locations were banked, and which were unbanked. See Burgess and Pande (2005), p. 6.

parent banks. One way to summarize this distribution would be to use the first average size of parent banks of marginal branches in that district. However, the distribution of banks may matter: a city with two branches, one belonging to a large parent, and one to a small parent, may grow in a different way than a city with two branches belonging to medium-sized banks. As it is not possible to include exact CDFs in a regression equation, I follow Chamberlain (1987) and approximate the density function by dividing banks into four groups: large public banks (the State Bank of India and the set of banks that was nationalized in 1969), large public banks nationalized in 1980, marginal banks (the small public banks nationalized in 1980, and the large private banks not nationalized in 1980), and small banks (all of which stayed private after 1980). Using the same definition of marginal as in Section 2.4.2 gives the following three variables:

$$\begin{aligned}
\text{SmallShare}_{c,80} &= \text{Small banks market share (none nationalized in 1980)} \\
\text{MargShare}_{c,80} &= \text{Marginal bank market share (some nationalized in 1980; others not)} \\
\text{LargeShare}_{c,80} &= \text{Large bank market share (all nationalized in 1980)}
\end{aligned}$$

The omitted category is large public sector banks. To measure the effect of nationalization on outcomes at the city level, I include an interaction term MargNat_c, which is defined as $\text{MargNat}_c = (\text{MargShare}_c)*(\text{Nationalized}_c)$, where Nationalized_c is the share of marginal branches in city c that were nationalized. This gives the following regression:[12]

$$Y_{c,d,92} = \alpha + \pi_s \text{SmallShare}_{c,80} + \pi_m \text{MargShare}_{c,80} + \pi_l \text{LargShare}_{c,80} + \gamma \text{MargNat}_{c,80} + \delta_d + \varepsilon_{c,d,92} \tag{2.3}$$

Finally, even though banks above and below the cutoff are very close in size, outcomes may still vary with the size of the parent of the marginal branch. I control for this with two complementary approaches. The first approach, analogous to equation 2.2, includes the average size of the parent bank of the marginal branches, $avsize_c$.

[12]Equation 2.3 could also be justified asymptotically, similar to a standard regression discontinuity: as the number of banks in the sample grows very large, the cutoff points for "marginal" could be set arbitrarily close to the actual cut-off value for nationalization. The size and other characteristics of just nationalized banks would therefore be nearly identical to non-nationalized banks.

The second approach is based on the fact that $\text{MargNat}_c = (\text{MargShare}_c)*(\text{Nationalized}_c)$. Since the coefficient of interest is the interaction of the two terms, rather than the terms themselves, I include a smooth function of both components of the interaction: MargShare_c, MargShare_c^2, Nationalized_c, and Nationalized_c^2.The reasoning is straightforward. Equation 3 controls for the share of marginal banks. However, if the fraction of marginal banks nationalized, or the square of marginal share or fraction nationalized affect outcomes, then including MargNat_c without the expansion terms (MargShare_c^2, Nationalized_c, and Nationalized_c^2) could lead to a spurious correlation between outcomes and the interaction term. Including a quadratic term in both of these components should account for this correlation, thus giving the interaction term a causal interpretation. In practice, results using either specification are very similar. For the sake of completeness, I report both.

This section answers two related questions. First, how does the elimination of private, through the nationalization of all branches, affect economic outcomes? This can be measured estimating equation 2.2 on the sample of towns that had only one-branch in 1980. The second question, on the effect of nationalizing bank branches in an environment in which public credit may also be available, is answered by estimating equation 2.3 on the "All-India" sample.

It is worth noting that the analysis presented in this paper may provide an upper bound of the benefits of bank nationalization: if public sector banks received government subsidies, but the cost of these subsidies was borne by both public and private sector banks, then public sector banks may appear better than they would have, had the private banks not been present to contribute to the subsidies for public banks. However, given that private sector bank failure imposed substantial cost on the government as well, the size of this bias is likely to be limited. Section 2.4.3 describes evidence that private sector bailouts were almost as costly as public sector subsidies.

The first-stage results are presented in Table 2.4. The dependent variable is share of credit in the town issued by public sector banks (both nationalized and state banks). Column 1 gives the results from equation 2.2, which includes the 1,513 towns and villages that, just prior to the 1980 nationalizations, had one private bank branch and no public branches. Not surprisingly, the nationalization dummy predicts very well the share of credit from public banks, with a point estimate of 1.00 and a standard error of .02. The R^2 of the equation is .97; it is not one because in some villages, additional branches opened after 1980.

Column 2 presents results from equation 2.3 for all cities, which, as of 1980, had at least one branch belonging to a "marginal" bank. As before, if the share of credit had been frozen over time at a level equal to the share of branches in 1980, the coefficient on MargNat would be exactly 1.[13] The point estimate in column 2 is indeed exactly one, with a standard error of .01. The estimate in column 3, which controls for the fraction of marginal banks nationalized, is very close to one, at 1.07. Because the coefficients for the first stage for all three specifications are one, the subsequent analysis presents reduced form, rather than instrumental variable estimates. (In the reduced form, the outcome variable of interest takes the place of Y_c in equation 2.2 and 2.3, respectively).

Each regression includes a district fixed-effect. There are between 208 and 340 districts in the specifications. In this specification, standard errors are clustered by district.[14]

In summary, the first stage is very strong for all three specifications: there is a one-to-one relationship between branch nationalization in 1980 and share of credit issued by public-sector banks in 1992. This stasis is due to the heavy regulations concerning the opening of new branches. District fixed-effects absorb any differences in outcomes that are attributable to unobserved regional variation. The remainder of the paper examines the impact of nationalization on credit market and real outcomes.

[13] Recall that the constant services as the omitted category of large public banks. Thus, the coefficient on the constant should be one. Small banks were not nationalized, so presence of them in a district reduces the share of credit by public sector banks: thus π_s should be -1. Similarly, π_l should be zero, since shifting a bank branch from the omitted category to large public will not affect the share of credit from nationalized banks. Finally, π_m will be 1, while γ will be negative one.

[14] There may be bank-specific effects (independent of whether a bank was nationalized or not) that affect city-level outcomes. Clustering accounts for this possibility for specification 2.2. In specification 2.3, bank-specific effects are less of a concern, since the covariance of any bank-specific effect between two different cities is attenuated by the product of respective shares. Denoting the variance of the bank random effect as σ_ν^2, the contribution to the covariance of two villages, each with three branches of three different banks, would be $\frac{1}{9}\sigma_v^2 + \frac{1}{9}\sigma_v^2 + \frac{1}{9}\sigma_v^2$, or only one-third as large the effect in 2.2. Nevertheless, to ensure conservative inference, the next version of this paper will include estimates and standard errors from a FGLS model. A standard random effects regression of equation 2.2, run on the sample of towns with only one banks, provides the appropriate weights, σ_ν^2 and σ_η^2. These estimates can then be combined with data on the distribution of banks across villages to calculate a weighting matrix for FGLS in equation 2.3.

2.5.2 Financial Development

A major goal of nationalization was to increase the scope and scale of banking in rural areas. One intended effect of nationalization was to mobilize deposits by granting access to bank accounts to a larger portion of the population: one banker said "even if a customer has only 1 Rupee, he may use it to open an account, the public bank should not refuse." The government also sought to increase the growth rate of rural credit by shifting the portfolio allocations of nationalized banks.

The effects of nationalization on financial development are estimated using equations 2.2 and 2.3. As a measure of financial development, I use the annual log growth rate of deposits and credit, in each credit market, over the period 1981 to 2000. Because the effects of nationalization may be different under different regulatory regimes, I consider three time periods: the entire period (1981-2000), the time of "financial repression," (1981-1990), and a time of financial liberalization (1991-2000). Because there are no time-varying regressors, I estimate the equation using cross-sectional growth (e.g., $\log\left(y_{2000}/y_{1981}\right)/19)$), rather than a panel, to avoid potential problems with serial correlation.

Results are presented in Table 2.5. Panel A presents results for the entire time period, 1981-2000. For smaller towns (which had only one branch in 1980), bank nationalization appears to have had no effect on the overall speed of financial development. The impact of nationalization on deposits is precisely estimated at zero; the effect on credit is three percent, though this value is not statistically significant Only equation 2.3, the all-India estimates controlling for average size of the parent banks of marginal branches, gives an effect of nationalization of 1 percent more credit, significant at the five-percent level.

Panel B restricts attention to the growth rate from 1981-1990, and finds a quite different result: towns whose branch was nationalized experienced an annual growth rate of credit approximately 2-3 percentage points higher than areas whose branches were not nationalized. Moreover, credit grew in areas in which branches were nationalized by approximately 11 percentage points *per year* faster in villages, and 4-5 percentage points faster for the all-India measures. Thus, over a nine-year period, the amount of credit increased by a factor of 1.5-2.5 more in cities whose branches were nationalized. These results contrast with the cross-country regressions reported in La Porta et. al. (2002), who find government ownership substantially slows financial development.

The view advanced by La Porta et. al. finds support in the liberalized environment of 1991-2000. In this decade, deposit growth was approximately 1-2% slower, per year, in towns whose branches were nationalized (Panel C). The effect is statistically significant for the all-India estimates, though not for the one-branch towns. The estimated effect on credit growth is larger in magnitude, suggesting nationalization slowed credit growth by 2-4%, though only one specification yields an estimate significant at the 10% level. This slower rate of growth of credit is consistent with a case study of a public sector bank in India, using data from end of the 1990s, which found that loan officers were surprisingly reluctant to increase the credit limit granted to firms, even in the face of inflation, increasing sales or increasing profits (Banerjee and Duflo, 2004), and with evidence reported in Banerjee, Cole, and Duflo (2004) that public sector bankers slow down lending when concerned about anti-corruption activity.

The results suggest that financial liberalization affected the relative ability of public and private sector banks to foster financial development. In the 1980s, public sector banks faced relatively lax budget constraints, enabling them to grow more quickly than private sector banks in the credit markets studied here. In the 1990s, however, the Indian government moved towards adopting Basel norms on bank capital requirements, causing public sector banks to shift assets from credit, because of its relatively high risk-weighting (Nag and Das, 2002). This had an effect on lending at the credit market level: public sector lending growth slowed substantially relative to private sector lending.

The finding that credit markets with nationalized banks experienced faster economic growth than non-nationalized banks is not inconsistent with the fact that nationalized banks grew at the same rate as private sector banks in the 1980s. The numbers are not directly comparable, since the former is a measure of credit-market level outcomes, while the latter takes the bank as the unit of analysis. Moreover, faster growth in rural areas may have come at the expense of growth in urban areas, especially since the interest rate for agricultural lending was, by government mandate, lower than the rate for industrial activity.

2.5.3 Credit Market Outcomes

I now turn to the effect nationalization had on the character of lending in India at the credit-market level. Section 2.4.4 described evidence that nationalized banks lent more in aggregate to agriculture and rural areas, and less to trade, transport, and finance. This does not, however, establish whether the differential bank lending led to differences in the credit markets. Banks could simply have specialized into different lending areas, with nationalized agricultural credit crowding out credit that private banks would have granted in the absence of public banks. Analysis at the credit market level therefore provides a more suitable measure of the impact of bank ownership on outcomes. The identification strategy remains the same as in the previous section.

Table 2.6 presents results for the share of credit lent by banks to agriculture and rural areas. Nationalization had a very substantial impact on credit to agriculture. For the sample of one-branch towns, the share of credit granted to agriculture was 26 percentage points higher in towns whose branch was nationalized than in towns whose branch was not. The average share of granted credit to agriculture in these locations was 38%. For all India, the estimated effect is smaller, but still substantial: a 10% increase in the share of public sector bank led to a more than one percentage point increase in the share of credit going to agriculture. All effects are precisely estimated and significant at the five or one percent level.

Not surprisingly, nationalization had no discernible effect on the share of rural credit for towns with only one branch in 1980: these locations are classified by the RBI as rural, and a full 86% of credit granted in these towns went to rural areas. The effect in the all-India estimates is, however, substantial. Nationalization of 10% of the branches in a city had an effect of increasing the share to rural areas by one percentage point. These estimates are significant at the five and ten percent levels.

Nationalization was thus quite successful in causing banks to focus lending on rural and agricultural areas. This was not the case for another primary goal of nationalization, to increase the flow of credit to "small scale industry." Table 2.7 presents the results for the share of credit to small scale industry, and to all industries. The effect of nationalization on credit to this sector is precisely estimated at zero. Both public and private sector banks by and large met the target of lending 40% to the priority sector (RBI Trends and Progress, 2000). Nor did nationalization affect the share of credit lent to all industry, as indicated by columns 4-6 of Table 2.7.

The discussion in section 2.4.4 suggests that nationalization may have resulted in less credit to trade. However, results presented in Table 2.8 indicate that there was no effect at the level of the credit market: the estimated effect is zero. Crowding out may explain why there is no measurable effect for the all-India estimates.

Advocates of social banking often argue that high interest rates in rural areas, charged either by money lenders or a monopolist bank, limit farmers' ability to invest, and therefore agricultural output.

Interest rates in India are highly regulated, with concessionary rates mandated for various types of loans (small loans, agricultural loans, etc.). To capture the discretionary component of interest rates, I compute a "residual interest rate," which controls for loan characteristics that determine interest rates. I regress the interest rate of each loan on a wide range of control variables: an indicator for whether the borrower is in a small scale industry, borrower industrial occupation dummies (at a three-digit level), district fixed effects, size of loan, an indicator for whether the borrower is from the public or private sector, and dummies indicating whether the loan is given in a rural, semi-urban, or urban or metropolitan area. Aggregating the residuals from this regression, at the credit market level, gives a measure of interest rates that is independent of loan characteristics.

Table 2.9 suggests that when given a chance, public sector banks will lend at a lower interest rate than private sector banks. Nationalization had no effect on interest rates in 1992 among towns with only one branch in 1980 (the point estimate is close to zero, and precisely estimated). This is not surprising, because prior to October 1994, regulation provided banks relatively little latitude to set interest rates, particularly in rural areas. One of the all-India specifications finds lower interest rates for cities whose banks were nationalized (a 10% increase in nationalization would lead to an interest rate eight basis points lower.) However, once lending rates were deregulated, the presence of nationalized banks led to substantially lower interest rates. Columns (4)-(6) present estimates for the effect of nationalization on residual interest rates in 2000. The size of the effect is virtually identical in all three specifications, and significant at the one percent level. A town with a public branch would receive credit at an interest rate of 1.7 percentage points lower than a town with a private sector bank. Note also that this is not attributable to differences in the lending portfolios of public and private banks, since the residual interest rate was calculated conditional on the occupation and size of the loan. This is a substantial difference, given that the interest rate at the time was around 15 percentage points, and is much larger in magnitude than the effect estimated by Sapienza (2004) for Italy, who found that government banks lent at rates approximately 20 to 50 basis points lower than private banks. The lower interest rates charged by public sector banks in the 1990s may have hindered their ability to grow, since the banks earned a lower return on their capital.

Table 2.10 evaluates the quality of intermediation provided by banks, as measured by the share of credit marked as late by more than six months. The first three columns of Table 2.10 use the share of non-agricultural credit that is reported as at least six months late, while columns 4-6 give the effect for agricultural lending.[15] The estimated effect of nationalization is always positive, and usually significant. For non-agricultural credit in the all-India sample, nationalized banks' lending portfolios have a 4-5 percentage points greater share of non-performing loans. For agricultural loans, the effect is even greater: 7 percentage points in the all-India sample, and 18 percentage points in one-branch towns. The combination of higher default rates, and lower interest rates, especially for agricultural credit, contributed to the balance sheet weakness in public sector banks in the 1990s.

The results provide some evidence in support of the development view of government ownership of banks: nationalization resulted in substantially faster financial development in the 1980s, lower interest rates, and shifted credit towards agriculture and rural areas. However, these gains came with two substantial costs: first, areas with public sector banks suffered slower financial development in the 1990s, once financial markets were liberalized. Second, the quality of intermediation provided by government banks was much lower: public sector loans were substantially more likely to default than loans issued by private sector banks.

Strong evidence in favor of the political view is presented in Cole (2004). I demonstrate that there are agricultural lending booms prior to state elections, and that these lending booms are targeted towards districts in which the majority party narrowly won or loss the previous

[15] Measuring loan default is a difficult business, since in the dataset I do not observe actual write-offs. Instead, I use as an indicator of weakness whether the loan is listed as late by six months or more.

election.

Nationalization thus caused an increase in quantity, but lowered quality, of financial intermediation. In the final section below, I investigate how these credit market shocks affected sectoral employment and agricultural investment: were the effects of increased quantity greater or less than the costs of decreased quality?

2.5.4 Real Outcomes

This final subsection examines employment and investment outcomes, using data from the 1991 Indian Census "Primary Census Abstracts (PCA)." The census publishes basic data on all towns and cities in India. Banking locations from the RBI banking directory were manually matched using information on the bank branch addresses. Matches were found for approximately two-thirds of the locales (1,930 out of 2,928).[16] The measures available in the census abstracts are not ideal, but do provide some information on the effects of ownership on employment and investment. They also provide a means of running a falsification test, to ensure that the identification strategy is not picking up other unobserved characteristics. Measures of economic development include share of male workers engaged in the following activities: agricultural laborers, cultivators, household industry, formal manufacturing, trade, and services. A greater share of employees in the latter four sectors will be taken as evidence of greater economic development.[17]

Because agricultural credit was such an important part of the rationale for nationalization, a second data set, the 1991 census village abstracts, was matched to credit markets. These data include information about water supply and irrigation. Seven hundred and one villages were matched to the set of villages with only one branch. I do not estimate equation 3, because very few villages with more than one branch could be matched to village abstracts.

The results for employment in agriculture and small-scale industry are reported in Table 2.11. In one-branch towns in which the bank was nationalized, approximately ten percentage

[16]Much of the difficulty in matching involved name changes: between 1991, when the census was taken, and 2000, when the branch database was collected, many districts, and a substantial portion of "teshils" (the administrative unit that is smaller than a district) changed names. Generally both the teshil name and the town name are necessary to uniquely identify a location.

[17]The only data source that records a measure of income (househodl expenditure) at a disaggregated level is the National Sample Survey. These data do not identify towns.

points more workers were agricultural laborers. There is some evidence that areas which experienced more nationalization have a larger share of cultivators: one of the specifications gives a significant point estimate of one half a percentage point more cultivators for a 10 percent greater share of public sector banks (Cultivators own their own land, while agricultural workers either sharecrop or work for a wage).

Nationalized banks appear to have had little effect in spurring the growth of household industry, with the estimated effect precisely 0 for all specifications, nor did it have any effect on the share of workers involved in formal manufacturing (Table 2.12). These results are consistent with the finding that public sector banks did not lend a greater share to household industry. The average share of workers engaged in household industry is very low, around 2.5 percent of male workers.

The most substantial deleterious effect of nationalization may have been on employment outside agriculture and manufacturing. The point estimates presented in columns 2-3 of Table 2.13 (the all-India sample) suggest that nationalization led to less employment in trade. The magnitudes are large: a 10% increase in the share of nationalized banks led to .4-.5 percent fewer workers in trade, and .1-.5% fewer workers in services. The all-India estimates for service workers are significant at the one and ten percent level (depending on the specification), and the point estimate for one-branch towns is similar. Nationalization may have had a similar effect on employment in service industries: one of the two all-India specifications gives a negative and significant effect of nationalization on employment in the service sector. These results may be explained by the a finding of Banerjee and Duflo (2004), that public sector loan officers are not successful at selecting the most profitable borrowers.

Unfortunately, no agricultural output data are available at the village level. Instead, the presence of tubewells, and the share of land irrigated are used as measures of agricultural investment. These variables are important to development: improved irrigation has led to substantial increases in output, and decline in output variability. Strikingly, while agricultural credit in villages whose branches were nationalized more than doubled over the period 1980 to1990, relative to villages with private branches, there was no improvement in either of these measures. The results in Table 2.14 indicate that nationalization had no effect on the likelihood a town possesses a tubewell, nor on the share of land under irrigation. The estimates are not as precise as the credit market results, but do rule out substantial impacts: for example, nationalization, which more than doubled agricultural credit, did not affect the probability of having a tubewell by more than 16%, or the share of land irrigated by more than 20%.

Finally, I conduct a falsification exercise, using variables from the census that are unlikely to be influenced by bank nationalization. Results, presented in Table 2.15, indicate that, once the size of the bank is controlled for, nationalization is not associated with literacy rates or a proxy for fertility. (Because actual fertility data are not available, I denote "fertility" as the proportion of children under 6 in the total population). The use of fertility is perhaps not a true falsification test, insofar as it could conceivably respond to bank ownership: however, the identical rate suggests that it is likely that areas with different levels of nationalization are comparable.

The evidence presented here paints a discouraging picture for proponents of government ownership of banks. While nationalization initially spurred financial development, and caused unprecedented amounts of credit to flow to agriculture, this came at a cost of lower quality intermediation. Moreover, the more than doubling of agricultural credit to villages led to no measurable increase in agricultural investment. In the liberalized environment of the 1990s, government ownership of banks hindered, rather than helped, financial development. And nationalization may have had deleterious effects on employment growth in the trade and service sectors.

These results thus help in the interpretation of research by Burgess and Pande (2004), who find that rural branch expansion significantly reduced rural poverty while aiding the diversification of the economy. The findings here suggest that it was not necessary to open government-owned bank branches in rural areas. Had the government imposed the same regulations (requiring expansion into rural areas, and setting lending targets) without nationalizing banks rural areas would likely have achieved the same, or better, outcomes. Perhaps the most compelling evidence against the development view of government ownership of banks is its apparent deleterious effect on employment in trade and service industries: along with manufacturing, these are the industries most associated with economic development.

2.6 Conclusion

If the long-standing debate about public ownership of enterprise has not been settled, there is at least a presumption that private enterprises are likely to be both more efficient and less susceptible to political capture, though less agreement about their ability to achieve "social" goals such as redistribution. The Indian bank nationalization of 1980 serves as a compelling policy experiment to answer these questions with respect to public ownership of banks. In that year, over 2,500 bank branches were nationalized throughout India, while 4,000 branches were left in private hands.

This paper measures the impact of nationalization on bank performance and credit market outcomes. Because banks were nationalized according to a strict policy rule, the measured effects of nationalization can confidently be given a causal interpretation.

Evaluating credit market outcomes alone provides some support for the development view of government ownership of banks: nationalization initially led to impressive financial development. In the decade following nationalization, growth rates in credit and deposits in villages whose branches were nationalized were up to 11 percent higher than those in villages whose branches were not nationalized. Lending rates were also lower.

Nationalization had a substantial effect on the sectoral allocation of credit. The nationalization of a bank branch in a village increased the share of credit going to agriculture by up to 26 percentage points, and to rural areas by approximately 10 percentage points. There were smaller, but still substantial, effects in larger cities as well. Increasing the amount of agricultural credit was described as a primary goal of government intervention. Moreover, nationalization was an effective means of achieving this goal: regulations designed to increase private lending to agriculture were much less effective.

Government intervention came at the cost of lower quality intermediation. Nationalized bank loans were much more likely to default than private bank loans, and the rapid expansion of areas served by public banks in 1980s was followed by slower financial development.

However, the true costs of government ownership of banks can only be understood when credit market outcomes are linked to investment and employment. The most striking fact is that, though villages whose branch was nationalized experienced a more than doubling of agricultural credit relative to villages whose banks remained private, nationalization had no effect on agricultural investment. Nationalization may have also had a harmful effect on employment in trade and services.

Thus, the final analysis rejects the development view of government ownership of banks. These results inform the current debate on financial liberalization. In a liberalized environment, private banks appear to be more efficient, grow more quickly, suffer fewer financial losses, and make fewer bad loans than do public banks. Left to their own devices, private banks will likely shy away from costly "social" lending in which government banks are engaged. However, the reduced amounts of credit may not adversely effect output.

2.7 Bibliography

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Table 2.1: Predicted Effects of Government Ownership of Banks

Outcome	Development View	Political View
Bank Growth		
Soft Budget Constraints	No Prediction	Faster
Hard Budget Constraints	No Prediction	Slower
Cost of Bank Bailouts	Lower	Higher
Financial Development	Speed up	Slow down
Sectoral Allocation of Credit	Meet gov. goals	Meet gov. goals if politically convenient
Interest Rates	Lower	Lower if convenient
Quality of Intermediation	No Different	Lower
Economic Development	Higher	Lower
Agricultural Investment	Higher	Lower

Table 2.2: Comparison of Nationalized and Non-Nationalized Banks

	Panel A: All Banks That Were Private in 1979					
	Mean		P-Value of Tests of Difference including control polynomial:			
	Nationalized	Private	No Control	Linear	Quadratic	Cubic
Deposits	3,922,378	598,220	0.00			
# of Branches	457	101	0.00	0.43	0.80	0.92
Profits	5228	1240	0.00	0.29	0.70	0.90
Deposits / Branch	8565	16940	0.31	0.67	0.97	0.88
Profits / Deposits	0.77	0.46	0.18	0.64	0.53	0.63
Return on Equity	0.0014	0.0040	0.02	0.36	0.77	0.63
Sample Size	6	41				

	Panel B: 5 Smallest Nationalized Banks and 18 Largest Remaining-Private Banks					
	Mean		P-Value of Tests of Difference including control polynomial:			
	Nationalized	Private	No Control	Linear	Quadratic	Cubic
Deposits	3,689,719	732,360	0.00			
# of Branches	420	148	0.00	0.48	0.78	0.78
Profits	4828	1135	0.00	0.66	0.64	0.65
Deposits / Branch	8864	5301	0.00	0.30	0.72	0.73
Profits / Deposits	0.78	0.47	0.24	0.32	0.40	0.41
Return on Equity	0.0014	0.0017	0.44	0.60	0.57	0.58
Sample Size	5	18				

Note: This table compares variables from the balance sheets of banks that were nationalized in 1979 to the banks that were not. Columns 1 and 2 give the means of the two bank groups, while columns 3, 4, 5, and 6 test whether the difference in means is significant. Columns 4, 5, and 6 include a linear, quadratic, and cubic (respectively) polynomial in log bank size. Panel A reports results for all banks that were private in 1979, while panel B reports results for the 5 smallest nationalized, and the 18 largest banks that remained private.

Table 2.3: Growth Rate of Nationalized and Non-Nationalized Banks

	Deposits Growth			
	Small	Marginal	Margional* Nationalized	Large
1970s	0.27 ***	0.26 ***	0.04	0.23 ***
	(0.07)	(0.07)	(0.03)	(0.08)
1980s	-0.09 ***	-0.07 ***	-0.05	-0.07 ***
	(0.02)	(0.01)	(0.03)	(0.00)
1990s	-0.03	-0.04 ***	-0.07 *	-0.07 ***
	(0.04)	(0.01)	(0.04)	(0.00)
N				993
Clusters				40
R^2				0.13

	Credit Growth			
	Small	Marginal	Margional* Nationalized	Large
1970s	0.31 ***	0.30 ***	0.04	0.27 ***
	(0.10)	(0.09)	(0.03)	(0.09)
1980s	-0.08 ***	-0.06 ***	-0.04	-0.05 ***
	(0.03)	(0.01)	(0.04)	(0.00)
1990s	-0.05	-0.03	-0.09 *	-0.09 ***
	(0.06)	(0.02)	(0.04)	(0.00)
N				993
Clusters				40
R^2				0.15

Note: Table 2 compares growth rates of small, marginal, large, and marginal nationalized banks, over the period 1970-2000.

Table 2.4: Nationalization First Stage

	Dependent Variable: Share of Credit Granted by Public Branches in 1992		
	One-Branch Towns	Control for Size	Control for Frac Natz'd
One-Branch	(1)	(2)	(3)
Nationalized	1.00 ***		
	(0.02)		
Parent Size	0.97		
	(3.30)		
Parent Size^2	-0.08		
	(0.23)		
Parent Size^3	0.00		
	(0.01)		
All-India			
Marginal & Nationalized Share		1.00 ***	1.07 ***
		(0.01)	(0.02)
Share of Branches, Marginal Parent		-0.90 ***	-0.57 ***
		(0.03)	(0.08)
Share of Branches, Small Parent		-0.76 ***	-0.77 ***
		(0.14)	(0.15)
Share of Branches, Large Parent		-0.16 **	0.02
		(0.07)	(0.06)
Ave. Size of Marginal Parent Bank		-0.04 ***	
		(0.01)	
Fraction of Marginal Nationalized			-0.15 **
			(0.06)
(Fraction of Marginal Nationalized)^2			0.03
			(0.06)
(Share Marginal)^2			-0.32 ***
			(0.06)
R^2	0.97	0.94	0.94
N	1513	2443	2443

Notes: The dependent variable is share of credit issued by public banks in 1992. The unit of observation is the credit market. Each column represents a regression.

Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.

Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).

Column 2 includes as an additional control the average size of the parent bank of marginal branches.

Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.

All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market ii

Table 2.5, Panel A: Financial Development, 1981-2000

	Deposit Growth, 1981-2000			Credit Growth, 1981-2000		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	0.00			0.03		
	(0.01)			(0.02)		
Parent Size	-2.31			-1.08		
	(1.50)			(2.25)		
Parent Size^2	0.17			0.08		
	(0.11)			(0.16)		
Parent Size^3	0.00			0.00		
	(0.00)			(0.00)		
All-India						
Marginal & Nationalized		0.00	0.00		0.01 **	0.01
		(0.01)	(0.01)		(0.01)	(0.01)
Share Marginal		-0.02 **	-0.04 ***		0.01	-0.03
		(0.01)	(0.02)		(0.01)	(0.02)
Share Small		0.01	0.01		0.04	0.04 *
		(0.01)	(0.02)		(0.02)	(0.03)
Share Large		-0.04 **	-0.05 **		0.03	0.03
		(0.02)	(0.02)		(0.03)	(0.03)
Ave. Size of Marginal		0.00			-0.01 **	
		(0.00)			(0.01)	
Fraction of Marginal Nationalized			0.03 ***			0.00
			(0.01)			(0.01)
(Fraction of Marginal Nationalized)^2			-0.04 ***			-0.01
			(0.01)			(0.01)
(Share Marginal)^2			0.01			0.02
			(0.01)			(0.02)
R^2	0.57	0.48	0.48	0.36	0.28	0.27
N	1513	2443	2443	1513	2443	2443

Notes: The dependent variable is the annual growth rate of deposits or credit. The unit of observation is the credit market. Each column represents a regression.

Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.

Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).

Column 2 includes as an additional control the average size of the parent bank of marginal branches.

Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.

All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.5, Panel B: Financial Development, 1980-1990

	Deposit Growth 1981-1990			Credit Growth 1981-1990		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	0.02			0.11 ***		
	(0.02)			(0.03)		
Parent Size	-4.79 **			-1.85		
	(2.06)			(3.25)		
Parent Size^2	0.35 **			0.15		
	(0.15)			(0.24)		
Parent Size^3	-0.01 **			0.00		
	(0.00)			(0.01)		
All-India						
Marginal & Nationalized		0.02 **	0.03 ***		0.05 ***	0.04 **
		(0.01)	(0.01)		(0.01)	(0.02)
Share Marginal		-0.06 ***	-0.04		-0.01	-0.02
		(0.01)	(0.03)		(0.02)	(0.04)
Share Small		-0.03 **	-0.03 **		0.01	0.02
		(0.01)	(0.01)		(0.02)	(0.02)
Share Large		-0.08 ***	-0.08 ***		0.08 **	0.09 ***
		(0.03)	(0.03)		(0.03)	(0.03)
Ave. Size of Marginal		0.00			-0.02 *	
		(0.01)			(0.01)	
Fraction of Marginal Nationalized			0.04 **			-0.01
			(0.02)			(0.02)
(Fraction of Marginal Nationalized)^2			-0.05 ***			0.00
			(0.01)			(0.02)
(Share Marginal)^2			-0.02			-0.01
			(0.02)			(0.03)
R^2	0.54	0.44	0.45	0.43	0.33	0.32
N	1513	2443	2443	1512	2442	2442

Notes: See Panel A.

Table 2.5, Panel C: Financial Development, 1991-2000

	Deposit Growth 1991-2000			Credit Growth 1991-2000		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	-0.01			-0.04		
	(0.02)			(0.03)		
Parent Size	-0.46			-0.72		
	(1.68)			(3.15)		
Parent Size^2	0.03			0.05		
	(0.12)			(0.23)		
Parent Size^3	0.00			0.00		
	(0.00)			(0.01)		
All-India						
Marginal & Nationalized		-0.01 *	-0.02 **		-0.02 *	-0.02
		(0.01)	(0.01)		(0.01)	(0.01)
Share Marginal		0.02 **	-0.06 ***		0.04 ***	-0.02
		(0.01)	(0.02)		(0.01)	(0.03)
Share Small		0.06 *	0.06 **		0.07	0.07
		(0.03)	(0.03)		(0.04)	(0.04)
Share Large		-0.03	-0.04		-0.06	-0.06
		(0.03)	(0.03)		(0.04)	(0.04)
Ave. Size of Marginal		-0.01 **			-0.01	
		(0.00)			(0.01)	
Fraction of Marginal Nationalized			0.03 **			0.00
			(0.01)			(0.02)
(Fraction of Marginal Nationalized)^2			-0.04 ***			-0.01
			(0.01)			(0.02)
(Share Marginal)^2			0.05 ***			0.03
			(0.01)			(0.02)
R^2	0.48	0.37	0.37	0.35	0.28	0.28
N	1513	2443	2443	1513	2443	2443

Notes: See Panel A.

Table 2.6: Agricultural and Rural Credit

	Agricultural Credit			Rural Credit		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	0.26 **			0.06		
	(0.11)			(0.07)		
Parent Size	-9.10			-9.50		
	(6.06)			(8.45)		
Parent $Size^2$	0.70			0.70		
	(0.44)			(0.61)		
Parent $Size^3$	-0.02 *			-0.02		
	(0.01)			(0.01)		
All-India						
Marginal & Nationalized		0.12 ***	0.08 **		0.10 **	0.12 *
		(0.02)	(0.04)		(0.04)	(0.06)
Share Marginal		-0.20 ***	0.45 ***		0.45 ***	0.41 **
		(0.04)	(0.11)		(0.07)	(0.20)
Share Small		-0.01	0.01		0.21	0.20
		(0.10)	(0.09)		(0.31)	(0.31)
Share Large		-0.94 ***	-0.73 ***		-0.29 *	-0.27
		(0.15)	(0.14)		(0.16)	(0.17)
Ave. Size of Marginal		0.00			0.01	
		(0.01)			(0.02)	
Fraction of Marginal Nationalized			-0.33 ***			-0.20
			(0.08)			(0.17)
$(\text{Fraction of Marginal Nationalized})^2$			0.37 ***			0.19
			(0.07)			(0.16)
$(\text{Share Marginal})^2$			-0.46 ***			0.05
			(0.08)			(0.16)
R^2	0.58	0.54	0.55	0.37	0.68	0.68
N	1513	2443	2443	1513	2443	2443

Notes: The dependent variable is agricultural or rural credit in 1992. The unit of observation is the credit market. Each column represents a regression.

Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.

Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).

Column 2 includes as an additional control the average size of the parent bank of marginal branches.

Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.

All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.7: Industrial Credit

	Small Scale Industry			Industrial Credit		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	0.01			0.01		
	(0.02)			(0.01)		
Parent Size	3.06			5.24 **		
	(1.94)			(2.07)		
Parent Size^2	-0.22			-0.38 **		
	(0.14)			(0.15)		
Parent Size^3	0.01			0.01 **		
	(0.00)			(0.00)		
All-India						
Marginal & Nationalized		0.01	-0.01		0.01	0.00
		(0.01)	(0.02)		(0.01)	(0.03)
Share Marginal		-0.03	-0.14 **		-0.02	-0.40 ***
		(0.02)	(0.06)		(0.02)	(0.11)
Share Small		-0.09 ***	-0.08 **		-0.17 ***	-0.17 ***
		(0.03)	(0.03)		(0.06)	(0.06)
Share Large		0.13 **	0.09		0.21 **	0.10
		(0.06)	(0.07)		(0.09)	(0.09)
Ave. Size of Marginal		0.00			-0.01	
		(0.01)			(0.01)	
Fraction of Marginal Nationalized			0.06			0.09
			(0.05)			(0.07)
(Fraction of Marginal Nationalized)^2			-0.05			-0.08
			(0.04)			(0.07)
(Share Marginal)^2			0.08 *			0.27 ***
			(0.04)			(0.08)
R^2	0.23	0.35	0.35	0.25	0.54	0.54
N	1513	2443	2443	1513	2443	2443

Notes: The dependent variable is small scale industry or industrial credit in 1992. The unit of observation is the credit market. Each column represents a regression.

Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.

Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).

Column 2 includes as an additional control the average size of the parent bank of marginal branches.

Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.

All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.8: Credit to Trade and Services

	Credit to Trade			Credit to Services		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	0.01			-0.01		
	(0.03)			(0.02)		
Parent Size	-0.52			1.26		
	(2.94)			(1.40)		
Parent Size^2	0.04			-0.10		
	(0.21)			(0.10)		
Parent Size^3	0.00			0.00		
	(0.01)			(0.00)		
All-India						
Marginal & Nationalized		0.00	0.00		0.00	-0.01
		(0.01)	(0.01)		(0.01)	(0.01)
Share Marginal		0.02 *	0.05		-0.02	0.02
		(0.01)	(0.04)		(0.01)	(0.04)
Share Small		0.01	0.01		0.01	0.01
		(0.02)	(0.02)		(0.02)	(0.02)
Share Large		-0.18 **	-0.15 *		0.06	0.05
		(0.09)	(0.09)		(0.04)	(0.05)
Ave. Size of Marginal		-0.01 **			0.01	
		(0.00)			(0.00)	
Fraction of Marginal Nationalized			-0.05			0.05 *
			(0.04)			(0.03)
(Fraction of Marginal Nationalized)^2			0.04			-0.03
			(0.03)			(0.03)
(Share Marginal)^2			-0.03			-0.02
			(0.03)			(0.03)
R^2	0.40	0.36	0.36	0.26	0.21	0.21
N	1513	2443	2443	1513	2443	2443

Notes: The dependent variable is small scale industry or industrial credit in 1992. The unit of observation is the credit market. Each column represents a regression.

Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.

Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).

Column 2 includes as an additional control the average size of the parent bank of marginal branches.

Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.

All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.9: Interest Rates

	Interest Rate, 1992			Interest Rate, 2000		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	0.007			-0.017 ***		
	(0.006)			(0.006)		
Parent Size	-0.056			0.720 **		
	(0.409)			(0.319)		
Parent Size^2	0.005			-0.052 **		
	(0.030)			(0.024)		
Parent Size^3	0.000			0.001 **		
	(0.001)			(0.001)		
All-India						
Marginal & Nationalized		-0.001	-0.008 ***		-0.017 ***	-0.019 ***
		(0.002)	(0.003)		(0.002)	(0.003)
Share Marginal		0.011 ***	0.013		0.019 ***	-0.016
		(0.003)	(0.015)		(0.003)	(0.018)
Share Small		0.032 ***	0.035 ***		0.055 ***	0.055 ***
		(0.010)	(0.010)		(0.020)	(0.021)
Share Large		-0.004	-0.008		0.015	0.004
		(0.013)	(0.014)		(0.013)	(0.014)
Ave. Size of Marginal		0.000			-0.001	
		(0.001)			(0.001)	
Fraction of Marginal Nationalized			0.014			0.013
			(0.012)			(0.014)
(Fraction of Marginal Nationalized)^2			-0.007			-0.014
			(0.011)			(0.013)
(Share Marginal)^2			-0.001			0.024 *
			(0.011)			(0.014)
R^2	0.31	0.48	0.48	0.43	0.70	0.71
N	1507	2437	2437	1448	2393	2393

Notes: The dependent variable is the interest rate in 1992 or 2000. The unit of observation is the credit market. Each column represents a regression.

Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.

Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).

Column 2 includes as an additional control the average size of the parent bank of marginal branches.

Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.

All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.10: Quality of Intermediation

	Share of Non-Agricultural Credit Late			Share of Agricultural Credit Late		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	0.038			0.185 ***		
	0.034			0.046		
Parent Size	4.88 **			-0.55		
	(2.47)			(3.89)		
Parent Size^2	-0.36 **			0.08		
	(0.18)			(0.28)		
Parent Size^3	0.01 **			0.00		
	(0.00)			(0.01)		
All-India						
Marginal & Nationalized		0.042 *	0.054 **		0.067 **	0.072 *
		0.022	0.024		0.028	0.037
Share Marginal		-0.03	-0.09		-0.04	-0.28 **
		(0.02)	(0.07)		(0.05)	(0.13)
Share Small		-0.07	-0.07		-0.12	-0.13 *
		(0.06)	(0.05)		(0.08)	(0.08)
Share Large		-0.04	-0.04		-0.12	-0.17
		(0.15)	(0.16)		(0.12)	(0.13)
Ave. Size of Marginal		-0.01			-0.01	
		(0.01)			(0.02)	
Fraction of Marginal Nationalized			0.04			0.10
			(0.05)			(0.10)
(Fraction of Marginal Nationalized)^2			-0.06			-0.11
			(0.04)			(0.10)
(Share Marginal)^2			0.02			0.16 *
			(0.05)			(0.09)
R^2	0.25	0.22	0.23	0.27	0.21	0.21
N	1223	1908	1908	857	1533	1533

Notes: The dependent variable is the share of non-agricultural or agricultural credit late in 1992. The unit of observation is the credit market. Each column represents a regression.

Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.

Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).

Column 2 includes as an additional control the average size of the parent bank of marginal branches.

Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.

All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.11: Agricultural Employment

	Share of Males in Agricultural Labor			Share of Males as Cultivators		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	0.10			0.00		
	(0.07)			(0.09)		
Parent Size	85.52			-61.74		
	(59.40)			(74.19)		
Parent Size^2	-5.78			4.15		
	(4.02)			(5.02)		
Parent Size^3	0.13			-0.09		
	(0.09)			(0.11)		
All-India						
Marginal & Nationalized		0.03	0.04		0.04 *	0.08 **
		(0.02)	(0.03)		(0.02)	(0.04)
Share Marginal		0.05	0.07		0.14 ***	0.23 **
		(0.03)	(0.08)		(0.04)	(0.11)
Share Small		-0.33	-0.34		-0.11	-0.10
		(0.23)	(0.24)		(0.19)	(0.20)
Share Large		-0.38 ***	-0.36 ***		0.27 **	0.32 **
		(0.09)	(0.09)		(0.12)	(0.13)
Ave. Size of Marginal		0.00			-0.02	
		(0.01)			(0.02)	
Fraction of Marginal Nationalized			-0.10 *			-0.12 *
			(0.06)			(0.07)
(Fraction of Marginal Nationalized)^2			0.09 *			0.06
			(0.05)			(0.06)
(Share Marginal)^2			-0.02			-0.11
			(0.07)			(0.09)
R^2	0.62	0.65	0.65	0.51	0.67	0.67
N	716	1303	1303	716	1303	1303

Notes: The dependent variable is share of males in agricultural labor or share of males as cultivators in 1991. The unit of observation is the credit market. Each column represents a regression.
Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.
Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).
Column 2 includes as an additional control the average size of the parent bank of marginal branches.
Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.
All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.12: Industrial Employment

	Share of Males in Small-Scale Industry			Share of Males in Industry		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	-0.01			-0.02		
	(0.02)			(0.04)		
Parent Size	-3.29			14.46		
	(17.41)			(33.04)		
Parent Size^2	0.23			-0.96		
	(1.18)			(2.23)		
Parent Size^3	-0.01			0.02		
	(0.03)			(0.05)		
All-India						
Marginal & Nationalized		0.00	0.00		0.00	-0.01
		(0.01)	(0.01)		(0.01)	(0.02)
Share Marginal		0.00	0.01		-0.06 **	-0.15 *
		(0.01)	(0.03)		(0.02)	(0.09)
Share Small		0.05 *	0.05 **		0.07	0.06
		(0.03)	(0.03)		(0.09)	(0.10)
Share Large		0.00	0.01		-0.05	-0.08
		(0.08)	(0.08)		(0.09)	(0.11)
Ave. Size of Marginal		0.00			0.00	
		(0.00)			(0.01)	
Fraction of Marginal Nationalized			-0.04 *			0.10
			(0.02)			(0.06)
(Fraction of Marginal Nationalized)^2			0.03 *			-0.09
			(0.02)			(0.06)
(Share Marginal)^2			-0.01			0.07
			(0.02)			(0.06)
R^2	0.39	0.39	0.40	0.40	0.58	0.58
N	716	1303	1303	716	1303	1303

Notes: The dependent variable is share of males in cottage industry or share of males in industry in 1991. The unit of observation is the credit market. Each column represents a regression.

Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.

Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).

Column 2 includes as an additional control the average size of the parent bank of marginal branches.

Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.

All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.13: Trade & Service Employment

	Share of Males Employed in Trade			Share of Males Employed in Service		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	-0.03			0.00		
	(0.04)			(0.03)		
Parent Size	-3.37			4.35		
	(28.55)			(24.30)		
Parent Size^2	0.24			-0.29		
	(1.93)			(1.65)		
Parent Size^3	-0.01			0.01		
	(0.04)			(0.04)		
All-India						
Marginal & Nationalized		-0.05 ***	-0.04 *		-0.01	-0.05 ***
		(0.01)	(0.02)		(0.01)	(0.02)
Share Marginal		-0.12 ***	0.13 *		-0.03	-0.12 *
		(0.03)	(0.07)		(0.02)	(0.07)
Share Small		0.08	0.08		0.06	0.07
		(0.13)	(0.12)		(0.08)	(0.09)
Share Large		-0.14 ***	-0.10 **		0.03	0.00
		(0.05)	(0.05)		(0.04)	(0.04)
Ave. Size of Marginal		0.02 *			0.00	
		(0.01)			(0.01)	
Fraction of Marginal Nationalized			0.01			0.07 *
			(0.04)			(0.04)
(Fraction of Marginal Nationalized)^2			0.00			-0.03
			(0.04)			(0.04)
(Share Marginal)^2			-0.18 ***			0.09 *
			(0.06)			(0.05)
R^2	0.42	0.70	0.70	0.44	0.64	0.64
N	716	1303	1303	716	1303	1303

Notes: The dependent variable is share of males in trade or share of males in services in 1991. The unit of observation is the credit market. Each column represents a regression.
Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.
Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).
Column 2 includes as an additional control the average size of the parent bank of marginal branches.
Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.
All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.14: Agricultural Outcomes

	Share of Towns with Tubewell	Fraction of Land Irrigated
	One-Branch Towns	One-Branch Towns
One Branch	(1)	(2)
Nationalized	0.00	0.00
	(0.08)	(0.10)
Parent Size	116.98	80.14
	(164.40)	(62.50)
Parent Size^2	-7.87	-5.42
	(11.12)	(4.19)
Parent Size^3	0.18	0.12
	(0.25)	(0.09)
R^2	0.38	0.79
N	701	636

Notes: The dependent variable is a dummy of whether the town has a tubewell or the fraction of land irrigated in 1991. The unit of observation is the credit market. Each column represents a regression.

Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.

All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Table 2.15: Falsification Exercise

	Fertility Rate			Literacy Rate		
	One-Branch Towns	Control for Size	Control for Frac Natz'd	One-Branch Towns	Control for Size	Control for Frac Natz'd
One Branch	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	-0.01			0.02		
	(0.01)			(0.05)		
Parent Size	2.91			-8.97		
	(10.93)			(45.97)		
Parent Size^2	-0.19			0.63		
	(0.74)			(3.11)		
Parent Size^3	0.00			-0.01		
	(0.02)			(0.07)		
All-India						
Marginal & Nationalized		0.00	0.01		0.00	-0.03
		(0.00)	(0.00)		(0.01)	(0.02)
Share Marginal		-0.01	0.05 ***		-0.02	-0.19 ***
		(0.01)	(0.01)		(0.03)	(0.07)
Share Small		0.00	0.00		-0.05	-0.04
		(0.02)	(0.02)		(0.09)	(0.11)
Share Large		-0.01	0.00		-0.02	-0.06
		(0.01)	(0.01)		(0.06)	(0.06)
Ave. Size of Marginal		0.00			-0.01	
		(0.00)			(0.01)	
Fraction of Marginal Nationalized			-0.01			0.08
			(0.01)			(0.05)
(Fraction of Marginal Nationalized)^2			0.00			-0.06
			(0.01)			(0.04)
(Share Marginal)^2			-0.04 ***			0.12 **
			(0.01)			(0.05)
R^2	0.64	0.69	0.69	0.75	0.77	0.78
N	716	1303	1303	716	1303	1303

Notes: The dependent variable is the fertility rate or literacy rate in 1991. The unit of observation is the credit market. Each column represents a regression.
Column 1 presents results from a regression for villages that had one marginal private bank prior to the 1980 nationalization. A marginal bank was one whose size placed it just above, or just below, the cutoff line for nationalization. The independent variable of interest is a dummy for whether the branch was nationalized. Control variables include a cubic polynomial in the log size of the parent bank of the branch as of 1980.
Columns 2 and 3 present the results for all towns that had at least one marginal private bank in 1980. The independent variable of interest is share of branches in the credit market that were both marginal and nationalized. Control variables include share of branches whose parents belonged to large banks, share of branches whose parents belonged to medium banks, and share of branches whose parents belonged to small banks. (Share belonging to already nationalized banks is the omitted category).
Column 2 includes as an additional control the average size of the parent bank of marginal branches.
Column 3 includes as a control quadratic terms in the share marginal and the share nationalized branches.
All regressions include district fixed effects and a cubic polynomial in the log level of deposits in the credit market in 1980.

Appendix Table 2.A1: Summary Statistics for Political Lending

	One Branch Sample			All India Sample		
	Mean	Std. Dev	N	Mean	Std. Dev	N
Credit Variables						
Share of Credit from Public Banks, 1992	0.492	0.492	1513	0.605	0.431	2449
Share of Credit to Agriculture, 1992	0.381	0.235	1513	0.325	0.223	2449
Share of Credit to Rural Areas, 1992 .	0.864	0.343	1513	0.549	0.497	2449
Share of Credit to Priority Sector, 1992	0.061	0.090	1513	0.090	0.099	2449
Share of Credit to Industry, 1992	0.070	0.119	1513	0.140	0.175	2449
Share of Credit to Trade, 1992	0.094	0.088	1513	0.098	0.080	2449
Share of Credit to Service, 1992	0.053	0.062	1513	0.055	0.061	2449
Residual Interest Rate, 1992	0.003	0.016	1507	0.004	0.020	2443
Residual Interest Rate, 2000	0.005	0.015	1448	0.006	0.025	2399
Share of Non-Agricultural Loans 6 Months Late	0.212	0.232	1259	0.223	0.193	2301
Share of Agricultural Credit 6 Months Late	0.242	0.307	870	0.306	0.278	1876
Financial Development						
Nominal Annual Deposit Growth, 1981-2000	0.165	0.037	1513	0.160	0.032	2449
Nominal Annual Deposit Growth, 1981-1990	0.169	0.055	1513	0.157	0.046	2449
Nominal Annual Deposit Growth, 1991-2000	0.163	0.055	1513	0.164	0.044	2449
Nominal Annual Credit Growth, 1981-2000	0.142	0.048	1513	0.138	0.042	2449
Nominal Annual Credit Growth, 1981-1990	0.168	0.083	1512	0.157	0.071	2448
Nominal Annual Credit Growth, 1991-2000	0.120	0.073	1513	0.122	0.062	2449
Share of Males Employed in:						
Agricultural Labor	0.276	0.156	716	0.161	0.145	1306
Cultivation	0.322	0.172	716	0.197	0.188	1306
Household Industry	0.023	0.034	716	0.022	0.030	1306
Manufacturing	0.092	0.079	716	0.148	0.109	1306
Trade	0.089	0.060	716	0.176	0.106	1306
Services	0.116	0.070	716	0.174	0.099	1306
Fertility	0.154	0.031	716	0.153	0.029	1306
Literacy	0.488	0.142	716	0.572	0.141	1306
Village Census Data						
Indicator for whether town has a tube well	0.307	0.461	701			
Share of land irrigated	0.436	0.390	636			

Notes: This table presents summary statistics of the dependent variables used in the analysis. The unit of observation is the credit market, which is typically a village, town, or city. The first three columns correspond to the sample of towns that had one private branch prior to 1980. Columns 3-6 include all towns and villages that had at least one marginal bank in 1980.

Chapter 3

Capitalism and Freedom: Manumission and the Slave Market in Louisiana, 1725-1820

Summary 3 *I use a rich new dataset of Louisiana slave records to answer longstanding questions about manumission. I examine who was manumitted, by whom, and whether manumisses paid prices above market for their freedom, shedding some light on the debate of the efficiency of slavery. Legal changes after the Louisiana Purchase allows me to conclude that manumission laws were quite important in determining the terms at which manumission agreements were struck: when slaves lost the right to sue for self-purchase at market price, there was a precipitous drop in the number of manumissions, while prices paid increased.*

3.1 Introduction

Contemporary narratives and historical sources provide a general view of manumission in the United States and other slave societies. But little is known of the details, and almost nothing of the economics. How effective was manumission in overcoming the severe agency costs that must be present in any situation of unfree labor? Who was manumitted? By whom? Did slaves pay "fair market prices," or did slave owners exploit their monopoly power? Using a remarkable new data set, this paper will answer these questions within the context of slavery in Louisiana at the turn of the nineteenth century, providing the first real empirical evidence on the economics of manumission.

Manumission, the freeing of a slave from bondage, has served as a powerful incentive for slaves throughout history. It was common enough in ancient times that economic historians have gone as far as to describe the Roman labor system as a functioning labor market: manumission was common enough that it encouraged slaves to work cooperatively with their owners (Temin, 2004). While generally permitted by U.S. state legal codes until the middle of the nineteenth century , manumission was much less common in the United States than ancient Rome or Greece (Matison, 1948). Fenoaltea (1984) attributes the relative infrequence of manumission to the nature of the work performed by U.S. slaves. He contends that pain ("the lash") can generate greater work effort, but not more care, while positive rewards are effective in eliciting care. Thus, manumission would not be an optimal incentive for difficult manual labor, such as plantation farming, the major occupation of slaves in the South.

Even so, there were many exceptions. A system of "term slavery" in Maryland at the turn of the nineteenth century allowed a slave holder to make a legally binding agreement with a slave to free her after a certain number of years, in return for greater and more reliable work effort. A special court was set up to adjudicate disputes between slaves and slave-holders who had made this agreement (Whitman, 1995). Spanish-ruled territories had a system of coartación (described in detail below), whereby a slave could sue for freedom if she had enough money to compensate her owner.

The incentive schemes designed by slave owners could be complex: Dew (1994) describes the operations of an iron forge staffed by slaves in Virginia in the early nineteenth century. The owner designed an "overwork system" whereby slaves were obliged to provide a standard amount of output per day, and were paid at a piece rate equal to the wages for free labor for output above their quota. Slaves were allowed to use their wages to purchase food and goods (above the standard ration), but there is only one documented example of a slave purchasing his freedom.

The idea that manumission provided a powerful performance incentive was part of the early cliometric debate over the profitability of slavery. In response to the pioneering work of Conrad and Meyer (1956), Moes (1958) argued that the U.S. system of slavery was not efficient, because a system encouraging manumission was:

> to the advantage of the owner because it gave the slave an incentive to work well and in general to make himself agreeable to his master... the idea that slavery is profitable (and therefore likely to be maintained) when slave prices are high does not stand up against this modern notion of opportunity cost but is the result of overlooking the most relevant alternative opportunity: that of allowing the slave to buy himself.

Moes adduced an anecdote about a philanthropist, who reported having agreed to pay the slaves on his plantation for work on Saturdays, in return for their eventual freedom. The philanthropist claimed to have received enough in payments to replace the departed slaves with double their number.

While closer scrutiny by Fenoaltea revealed this story to be apocryphal, the principle could still hold.[1] In Coasian terms, there are at least two reasons why manumission could be more efficient than slavery: first, productivity effects aside, slaves may value their freedom more than masters value their enslavement. Second, to the extent that there are frictions in inducing slaves to exert effort, the prospect of manumission even at market price (or above) could provide a powerful incentive motivating slaves to work. Thus, we might reasonably expect that property rights will eventually be transferred to those who value them most, and that manumission would thus be an important feature of U.S. slave society. Conrad and Meyer's response (1958) to this criticism was agnostic: "... this is clearly an empirical question—an empirical question, moreover, about which neither we nor Moes now have sufficient information to say anything definitive" (p. 188).

A rich new dataset from every surviving document on slavery and manumission in Louisiana from 1770-1820 allows me to paint a qualitative, quantitative, and economic portrait of manumission in Louisiana. I examine whether slaves who purchased their freedom paid above or below market prices. Moreover, a change in the laws regulating manumission following the Louisiana Purchase allows me to explore how legal rights affected the economics of manumission, and better explain why manumission was relatively rare in the U.S.

[1] See footnote 40 of Fenoaltea (1984).

3.2 Evidence and Data

The question of whether manumitted slaves paid the market price for their freedom has puzzled economic historians for over a century. Wergeland (1902), in a survey article on slavery in medieval Europe, notes that data on the price of manumission are almost unavailable. He cites some prices listed in legal codes, which were well below "market value," but points out that these amounts were most likely not actually used in practice. In an early cliometric exercise, Westerman (1955) examined over a thousand Delphic manumissions, from 201 to 53 B.C. The majority were outright grants, whereby a slave purchased her unconditional freedom. He reports that an examination of the manumissive price of over five hundred slaves reveals that slaves paid higher prices for their freedom than the average market price. Westerman ascribes the higher prices to the monopoly power of slave-holder.

Louisiana is a particularly good place to study manumission: it was relatively common, and records are well-preserved. For example, archival copies in Spain have helped fill gaps in the surviving records in Louisiana. Hanger (1997) uses surviving notarial records from Spanish New Orleans to study manumission. She provides some demographic information, along with the annual average price paid from a sample of 700 manumissions over the period 1771-1803, but does not compare these prices to the market prices. Kotlikoff and Rupert (1980) examine jury records of manumissions in New Orleans from 1827-46, and find that free blacks, as purchasers, were involved in a significant proportion of manumissions. However, since price data were typically not recorded in the jury records, they cannot investigate the terms of the manumissions.

I use information from two recent databases: the "Louisiana Slave Database" and "Louisiana Free Database," collected by Hall (2000) and her team. They spent 15 years collecting information from every document available relating to slavery in Louisiana, from the arrival of the Europeans until 1820. Sources include every archive and courthouse in Louisiana, as well as archives in Mississippi, Alabama, Florida, Texas, France and Spain. The databases contain detailed descriptions of over 100,000 individual slave sales or other transactions. Four-thousand and sixty records relate to manumission.[2] Table 3.1 provides some key summary statistics

[2]Duplicate documents were deleted, though if a person appeared multiple times in different documents, the observations were retained.

about the coverage and scope of principal demographic variables of interest.

The various documents were produced in the course of normal transactions, and consist primarily of sale contracts, probate records, manuscripts, and published census records. They provide information about the age, sex, origin, health, character, and skills of slaves and manumitted slaves. In addition, the investigators coded information on the relationship between freer and freed, whether a freed slave is (or potentially is) a child of a master, the illnesses or disabilities of the slave or freed person, and the location.

During the period covered by the databases, Louisiana was ruled by three regimes, with several currencies, and significant currency fluctuations. Fortunately, a large number of government and commercial historical documents list prices in several currencies, allowing Hall to convert all prices into nominal dollars using the appropriate exchange rate. Hall cautions, however, that the earlier price data are less reliable. The present analysis involving price data is restricted to observations from 1770 on.[3]

3.3 Legal Context and the Nature of Manumission in Louisiana

From 1769 until 1803, Louisiana was under Spanish control.[4] Compared to the French code noir it replaced, and U.S. slave codes which followed, the Spanish legal system afforded several specific protections to slaves. Masters did not require any official permission to manumit slaves; mistreated slaves could request resale to another master, and slaves could purchase their freedom by paying their market price to their masters (Baade, 1983, p.52).

This last right, a system of self-purchase called "*coartación*," developed initially in Cuba prior to the first half of the eighteenth century, and spread to varying degrees throughout the Spanish colonies. (Bergad, 1995). The linguistic root of coartación is "*cortar*," which means to cut or limit. This system sought to balance slaves' aspirations for freedom with masters' property rights, by allowing a slave to manumit herself if she paid her master her fair market value. By custom as well as law, if a slave's master refused a slave's request to purchase her freedom, the slave could sue. If there was a dispute about the fair price, each party, as well as the court, would be provided an assessor, and the slave would be freed following payment

[3] See Hall (2001). Including slave sales and manumissions prior to 1770 does not change the results.

[4] The Spanish slave code was not promulgated until 1770 (Baade, 1983).

of the court-determined value to the master. These rights were guaranteed by officials called "*sindicos*," who were charged with protecting the rights of the poor, the Native Americans, and the slaves (Aimes, 1909).

While the right of self-purchase was not codified into Louisiana law (the other two protections were), coartación in Louisiana "was recognized and enforced, nevertheless, in a steady stream of judicial decisions, averaging somewhat less than two cases per year for the thirty-three year period of direct Spanish rule" (Schafer, 1994, p.2).

Hanger reports that approximately one in seven cases of coartación required court supervision to set a fair price, and Baade (1983) provides numerous instances of the courts overseeing slaves' right to sue for self-purchase, including enforcing the price of the court's assessors as the manumissive price. When litigation was required, court costs were paid by the petitioner, but these appear to have been relatively low. I found only one enumeration of court costs, given as 27.5 pesos for a purchase price of 800 pesos, or about 3.5% of the sale price.

Hanger describes the system as efficient:

> In most cases coartación offered advantages to slaveholders, slaves, and the Spanish government alike. All three groups acted according to their best interests... Coartación provided slave owners with incentives that encouraged slaves to work more productively, reduced their provisioning costs, and compensated at the slaves' estimated fair values. Legal manumission also acted as an effective form of social control by holding out liberty to obedient bondspersons and denying it to rebellious ones (p. 43).

Once freed, persons of color in Spanish Louisiana enjoyed a life substantially more free than their counterparts in the US. Berlin (1974) notes that apart from a ban on intermarriage, a requirement that manumitted slaves "respect" former owners, and harsher penalties under the legal code, free blacks in Louisiana enjoyed many of the same rights as white men. A free black militia served as a useful ally to the Spanish Crown, which limited local attempts to further restrict the rights of free blacks.

When American administrators took over Louisiana on December 20, 1803, they worked with local slave-holders to bring Louisiana laws in line with other southern states, including eliminating the right to sue for freedom (Ingersoll, 1991).[5] In 1807, the first legislature of the Territory of Orleans passed a law limiting manumission to individuals over thirty years of age, unless they had saved the life of their slave-holder. Hanger argues that, removed from the moderating influence of the Spanish government, Louisianans were "fearful of slave unrest, and increasingly disposed towards Anglo racial attitudes." (p. 165). From 1807-13, all seeking to manumit a slave were required to appear before the parish court. This became too burdensome, and in 1813 any officer of the peace could serve in the judge's stead (Taylor, 1963).

The laws restricting manumission were only partially effective. Many contained exceptions and exemptions. Kotlikoff and Rupert, for example, report that an 1830 law requiring manumitted slaves to leave the state allowed for an exception if three-fourths of a police jury voted for it; of the 1,770 slaves manumitted in their sample, not one was obliged to leave the state. Moreover, the spirit of such laws was legally evaded by the development of "benevolent slave-holding," whereby a free black (or white) would possess the title to a slave, but allow her to live as a free person. Finally, the laws were often simply ignored. (Matison, 1948)

Efforts to limit the rights of free blacks were initially successful, including the disbandment of the free black battalion. However, fear of British attack in the war of 1812 led Andrew Jackson to call free blacks back into service, and their success, as well as the growing economic strength of the free black population, helped prevent further erosion of the rights of free blacks. Not until the 1830s did the state begin to pass legislation such as registration laws, bringing Louisiana closer to the other Southern states.

It is worth noting how Louisiana law differed from U.S. law: only Spanish Louisiana law allowed a slave to sue for freedom. And while free persons of color in Louisiana enjoyed considerable freedoms an economic opportunity throughout the antebellum period, free blacks outside Louisiana occupied a status much closer to slaves:

> Southern law presumed all Negroes to be slaves, and whites systematically barred free Negroes from any of the rights and symbols they equated with freedom. Whites

[5]The Spanish legacy of manumission may have persisted more in Louisiana than other states. Despite significant curtailment of manumissive rights following the Louisiana Purchase, some steps forward were made. In 1825, Louisiana was one of only three states to explicitly allow slaves to contract for their own freedom. (Matison, 1948) As late as 1860, Louisiana's manumission rate was several times greater than the median rate of southern states. (United States, 1866, page 337).

legally prohibited Negro freeman from moving freely, participating in politics, testifying against whites, keeping guns... In addition, they burdened free Negroes with special imposts, barred them from certain trades, and often tried and punished them like slaves. (Berlin, 1974, pp. 316-317.)

3.4 Who Was Manumitted? How?

The breadth and depth of the Hall database allow me to paint a comprehensive picture of manumission in Louisiana. Unique in scale and scope, the data are particularly well suited for economic analysis, since a large share of observations include both individual characteristics and sale prices of slaves and freed slaves.

Table 3.2 indicates how each of the 4,060 slaves achieved freedom between 1725 and 1820, in both New Orleans and rural Louisiana.[6] The consensus view that manumission was less common in the American period than the Spanish period is correct: the documents in the database suggest there were only 2.3 manumissions for every 100 slave sales in the U.S. period, compared to 6.5 per 100 slave sales in the Spanish period.

In both the Spanish and U.S. regime, the most common path to freedom was an outright grant by a living master or mistress. Why did masters free slaves? It is hard to imagine, in any other economic setting, forfeiture of so much wealth as the gratuitous manumission of slaves in the new world. While one cannot hope to understand all the complexities in the relationship between the owner and slave, the data can speak to what the owners thought was most important. To manumit a slave, the owner was typically prompted or required to give just cause. I manually coded the causes recorded in the manumission documents into the five most common reasons. The data are presented in Table 3.3, broken down according to whether the manumission was by a living grant, or through a will, as well as by time period (Spanish vs. U.S.)

The prevalence of "good service" suggests that an incentive dynamic may have been important in as many as half of all manumissions in which there was no monetary payment. Appeals to the immorality of slavery were surprisingly rare: perhaps those uncomfortable owning slaves

[6]The "Free database" has 4,064 records, but four of the slaves were not actually manumitted.

sold the slaves, rather than freeing them.

While freers typically did not admit to a blood relationship with the slaves they freed, family bonds were often a reason for manumission. Of the 2158 manumissions which were grants of freedom (by living masters or in wills), only 192 stated outright that the freed slave was related to the master. However, the investigators were able to deduce from evidence in the documents and elsewhere that 323 freed slaves were likely children of masters: thus approximately 24 percent of gratuitous manumissions involved relatives of the slave owner. A typical case is perhaps that of Genevieve, who in 1779 was freed along with her brother Nicolas, in Point Coupee, upon the death of their master, Simon Macour. He recognized the children as his in his will: his widowed wife and white son protested, but the slaves were freed nonetheless.

Also notable is the disappearance of "affection," and the sharp drop in relationships, as reasons for manumission in the U.S. period. While manumission by living masters was only slightly less common in the US period (79 percent of U.S. vs. 72 percent of Spanish manumissions), manumission for reasons of affection becomes much less common: affection is cited in 26 percent of Spanish, and only 4 percent of U.S. manumissions. The difference may be attributable to different attitudes towards slaves: the Spanish tradition recognized that "slavery was not the natural condition of men" (Hanger, p. 25) Cuban law, for example, explicitly allowed owners to free slaves in wills, provided such action proceeded "from an honest and laudable motive." (Knight, 1970, pp. 130-1) U.S. laws and attitudes were much less sympathetic. There is a less severe, but still noticeable, decline in the proportion of documents indicating that manumission is granted for service.

The importance of blood relations between the freer and the slave probably also accounts for the skewed age and sex distribution of manumitted slaves. Figure 3.1 graphs the age distribution of manumissions, decomposed by whether the manumission was gratuitous or by purchase (left panel), and by gender (right panel). For comparison purposes, the age distribution of slaves in 1850 is given as a dashed line.[7] Young children were significantly overrepresented among gratuitous manumissions: many were children of the slave-owner. Similarly, prime-aged women were much more likely to be manumitted than men, probably both because their productive value was lower, and they were more likely to have been in intimate relationships with their

[7] The slave population distributions are from the U.S. Census, as reported by Tadman (1989, p.240.)

owners.

Manumissions appeared to follow the seasonal pattern of the slave market. Outright grants of manumission were least common during the summer months of July, August, and September (over the entire time period, approximately 88 manumissions occurred per summer month, while 113 per month occurred during the rest of the year.) Kotlikoff reports a similar lull in slave sales at this time, and attribute it to the fact that summer months were idle months: perhaps slaves seeking to manumit themselves also had less earning opportunity during the slack summer months.

Southern manumission has been described as a largely urban phenomenon, because there were more opportunities for slaves to earn money, and slaves could benefit from association with free blacks, who typically lived in cities. (Matison, 1948, and Goldin, 1976) The data confirm that manumission was more common in urban areas. Though we do not have information about the actual residence of the slaves, 76 percent of manumission records originate from New Orleans (the only urban parish in Louisiana prior to 1820), while only 57 percent of the slave records originate in New Orleans.[8] Of the manumissions by self-purchase, 81 percent were in the parish of New Orleans.

Historical accounts suggest that manumission was most prevalent among skilled workers. While an insufficient share of records contain enough information on skills to make detailed claims, many records indicate either basic skills (such as laborer) or artisan skills (such as tailor). Skill distributions are reported in Table 3.1.

The prevalence of self-purchase or purchase by others is striking: more than 30 percent of manumissions occurred either through self-purchase or purchase by other. Though typically described as an urban phenomenon, manumission by purchase was prevalent in rural areas as well, during both the Spanish and U.S. administrations. Slaves earned money in various ways, such as hiring their own time from their owners and working for others or selling produce of individual vegetable plots. In 1806 Louisiana even passed a law obliging slaveholders to pay slaves for work on Sundays. (Matison, 1948.) This law was upheld in a Louisiana court, with the judge writing that "slaves are entitled to the produce of their labor on Sunday; even the

[8]No population data are available: thus, when appropriate, the number of slave sales can be used to scale the number of manumission records.

master is bound to remunerate them, if he employs them." (Morris, 1954)

Manumission sometimes represented a complex legal contract, with owners placing conditions of future servitude, good behavior, or additional payments on freed slaves. Approximately 12 percent of manumission records included additional conditions; they are detailed in Table 3.4.[9] The most common condition freed a slave after death of the master, though freers also demanded additional payment and required additional years of service. It is not clear how well these conditions were enforced, but historians have documented instances of courts siding with slaves against their owners in disputes (Hanger, 1997.)

Some calculations presented in Fogel and Engerman (1974) are informative here. Using the best estimates of slave wages, the cost of raising slaves, and the opportunity cost of capital, they estimate that the break-even point of holding a slave in the South in 1850 was 26 years. Thus, slave-holders who chose to manumit slaves aged 26 and above easily earned accounting profits.

Economic theory does not provide a clear prediction about the relationship between the real price of slaves, and the incidence of manumission. The price of slaves increases with the value of their labor, but their earning potential (or the gains from reducing frictions in the principal-agent problem) should also increase. Findlay (1975) studies this problem in detail, and under strict assumptions derives comparative statics with respect to the interest rate, but does not examine the effect of price changes. Similarly, the total wealth of owners increases as slaves become more valuable, but so too does the cost of granting manumission.

I find that manumissions are positively correlated with prices. Figure 3.2 graphs the total number of manumissions each year, along with the annual real price of unskilled male field hands, aged 18-30. (Prices are deflated using data from McCusker, 1992.) A simple regression of number of manumissions on the real price of a 30-year old male field hand yields a positive and significant co-efficient; this relationship is robust to the inclusion of a time trend, and a dummy for U.S rule (1803-20). This holds both for total manumissions and for manumissions by purchase. These results should be taken as indicative rather than definitive, since it is not clear what drives the time-series variation in prices.

[9] Twelve percent is a lower bound, as surviving documents might not mention all of the conditions, particularly if they have already been fulfilled.

3.5 The Price of Freedom

Did slaves who purchased their own freedom, or whose freedom was purchased by others (such as relatives), pay a price higher than their replacement cost? A slave's willingness to pay exceeded the owner's valuation of the slave, since the slave would gain not only the economic value of her labor, but also freedom. Owners could purchase a replacement for a manumitted slave on the market. Owners may therefore have attempted to extract above-market prices from slaves seeking manumission, when legally allowed to do so.

The data show that institutions and attitudes governing manumission mattered. Figure 3.3 plots the number of manumissions and slave sales that took place from 1720-1820. A simple test, regressing the proportion of manumissions to slave sales on a constant and a dummy for U.S. rule confirms that there were significantly fewer manumissions per slave sale under the U.S. regime: the number of manumissions per slave record fell by approximately two-thirds.[10] This is driven by both a decrease in the number of manumissions, from an average of 114 per year in the ten years before the Louisiana Purchase, to 76 per year in the first decade of U.S. rule, and a substantial increase in the number of slave sales, from 1182 per year to 2512, as the free and slave populations expanded.[11] The elimination of coartación led to a severe drop in the level of manumissions, even as the number of slave sales (and slave population) increased dramatically.

To shed light on the economics of manumission, I use data from 400 manumissions and 5,512 slave sales from the period 1770 to 1820. To ensure as accurate data as possible, I limit attention to slaves who were sold individually, and to slaves aged 30 and above. (For much of the time period, there were restrictions on manumitting slaves younger than 30 years old. This also reduces the likelihood that the person freed was related to the slave-holder[12]).

[10]This regression gives a constant 0.088 (0.005), meaning about 9 percent of slave documents pre-U.S. rule relate to manumission, while the dummy (-0.062 (0.009)) indicates only 2.6 percent of documents per year were manumissions. I use manumissions per slave sale rather than per capita because there are no annual census records.

[11]Figure 3 also provides assurance that bias from non-surviving documents may be minimal. From 1807-13, the law required all manumissions be registered in parish court offices. Since the number of manumissions prior to and after this period is similar, it is reasonable to believe that the surviving manumission records in other periods are representative. Manumitted slaves would have every incentive to ensure their manumissions were well documented.

[12]This law is somewhat at odds with the facts: children were commonly manumitted. However, if purchased by or manumitted with their parents, the intent of the Louisiana restriction would be satisfied, since they would

The richness of the data allows for a relatively detailed pricing model, which is presented in Table 3.5. The estimated equation in column 1 is

$$price_{ipt} = \alpha_1 X + \delta_S\,(Manum * Span) + \delta_{US}\,(Manum * US) + \theta_p + \gamma_t + \varepsilon_{ipt}$$

The dependent variable is log price, and the regressors of interest are δ_S, the coefficient on the manumission dummy in the Spanish period, and δ_{US}, the coefficient on the manumission dummy for the US period. These give estimates of the manumission premium before and after the Louisiana Purchase. Control variables X include a three-degree age polynomial, dummies for male, light color male, light color female, skilled (aged 30-40), skilled (aged 40+), African birth, and eleven month dummies, as well as fixed effects for parish, θ_p, and year, γ_t.

The general features of the model are similar to those found by Kotlikoff (1979). Column 1 presents the results of including the entire sample, 1770-1820. The results indicate that male slaves commanded a premium, while, not surprisingly, sick slaves, and those born in Africa fetched lower prices. Light-color skin was valued, but apparently more for males than for females. The age-profile is declining, with 30-year old slaves commanding the highest price, and falling almost linearly as the age of the slave increase, again similar to Kotlikoff.

The estimates do suggest that the system of coartación was effective: the point estimate of the coefficient on δ_S is very close to zero, and precisely estimated, indicating that manumission prices were equal slave sale prices in the Spanish regime. However, once coartación was abolished, slaves paid a substantial manumission premium. The coefficient on δ_{US} is large and statistically significant, and suggests that slaves seeking manumission paid 19 percent more than their market value. This result is statistically significant at the 1 percent level.[13]

To allay the concern that a change in the pricing model (for example, different age-price profiles) confounds the estimated value of the manumission dummy, I estimate the models separately for the Spanish and U.S. periods in columns 2 and 3. A Chow test (results not reported) suggests that the pricing model did not change substantially between the two periods: the manumission premium, the male dummy, and the age polynomial are the only parameters

not be wards of the state., The results presented do not change if all slaves are included, not just those over 30.

[13] Including a manumission dummy throughout the entire sample, rather than one for each time period, yields an insignificant point estimate of .05.

for which the hypothesis of equality across time periods can be rejected.

To ensure that this result is not driven by functional form assumptions, I also provide a non-parametric estimate of the manumission premium. The technique of matching on observables, introduced by Rubin (1974), compares "treatment" individuals (manumitted slaves) to control individuals (slave sales) who are observationally very similar, and requires no assumptions on the functional relationship between slave qualities and prices.

The observations are partitioned, for both manumissions and slave sales, into different cells, by the following criteria: (i) manumission or slave sale, (ii) sex, (iii) age (30-35, 35-45, 45-55, 55-65, 65+), (iv) origin (born in Africa, born in the New World and light skin, and born in the New World not light skin), (v) skilled or un-skilled, and (vi) time period (U.S. or Spanish Rule). Because there are not enough manumission observations to further divide the cells into years, prices are instead normalized, by dividing each slave's sale price by the average price of male field hands in the year they were sold.[14] The estimator then compares the difference in sale price between slave sales and manumissions for the various groups, and aggregate the difference. For example, unskilled male slaves aged 30-34, without "light skin," born in the New World, during the Spanish regime, are compared to a similar group of manumitted slaves. Formally, for each cell, the average sale price, is estimated, where m indicates the cell number, and the subscript k indicates whether the mean is for slave sales (k=0) or manumissive sales (k=1). Denoting N_{m_k} the number of manumission observations in each cell, I then calculate the value:

$$\hat{\alpha} = \sum_{m \in M} \delta_m N_{m1} \left(\overline{y}_{m1} - \overline{y}_{m0} \right) / \sum_{m \in M} \delta_m N_{m1}$$

where δ_m is an indicator variable taking the value of 1 if cell m contains both manumission and slave sale data. Since not all cells have observations for both manumissions and slave sales, the number of manumissions used to generate the estimate is lower in the matching exercise than the regression estimates (380, compared to 400). Note also that the weighting is calculated

[14] As a robustness check, I performed the same exercise, substituting Phillips (1940) slave sale price for the years 1800-20. The results were unchanged.

over the population distribution of the manumitted slaves.

This estimate is consistent under weak conditions: in particular, the true model need not be linear. The results are presented in Table 3.6. The estimates are quite similar to the results obtained by linear regression: there is no premium in the Spanish regime, while slaves manumitting pay 15 percent above their observationally equivalent counterparts once the U.S. took over. The premium is precisely estimated, and statistically distinguishable from zero at the 1 percent level.

These results are therefore robust to a variety of estimations techniques. Yet even the estimate of 19 percent may be an underestimate: after all, a significant number of manumission records indicate that manumission was granted at least in part as compensation for "good service." Recall also that approximately 10 percent of the manumissive records place some condition on manumission, such as additional service. Though these records were not included in the above analysis, it is certainly possible that some of the manumissions included in the analysis had additional, unrecorded, service requirements.

A non-statistical concern is the potential presence of other omitted variables that could bias the estimate of the manumission premium. However, several reasons suggest this is not a serious concern. Most importantly, there is no evidence, or reason to believe, that the recording of skills or characteristics changed significantly before and after 1803. Thus, whatever bias there exists would be present in both periods. Yet, during the Spanish period, the system of coartación would ensure the prices should be the true prices, and that is precisely what I find.

As a second test, we compare the manumission premium of those who purchased their own freedom to the manumission premium of those whose freedom was paid for by another. In both cases the owner could potentially exploit monopoly power, but individuals whose freedom was purchased by a third party are plausibly less exceptional than individuals who purchase their own freedom. In the fourth column of Table 3.5, I present results from a regression that includes dummies for four types of manumission: self-purchase (Spanish), purchase by other (Spanish), self-purchase (US), and purchase by other (US). The point estimate for the manumission premium for self-purchase and purchase by other are both indistinguishable from zero in the Spanish period; in the US period, the point estimates are both .18, though the estimate for purchase by other is imprecise.

I conduct a similar comparison for urban and rural manumissions, under the premise that rural slaves may exhibit less unobserved heterogeneity.[15] The point estimates for the urban and rural manumission premium during the US regime, reported in column 5 of Table 3.5, are identical, though the rural premium estimate is imprecise. Nevertheless, neither approach provides evidence that slaves purchasing their freedom were different in unobservable ways.

To sum up, it seems unlikely that the manumission premium is driven by omitted factors. Any explanation of systematic differences of manumitted slaves would have to explain why those differences appeared only after 1803. And even if the level of the premium were wrong, we still find the striking fact that the quantity of manumissions falls as the price paid by manumitting slaves rises, following the abolition of the right of coartación.

3.6 Discussion

The evidence shows that a non-trivial faction of slaves, including rural slaves, were able to purchase their own freedom, often at prices above their replacement price. Why then, was manumission, so common in other slave systems, so rare in the United States? The most compelling explanation advanced is that of Fenoaltea, who argues in great detail that the threat of pain was such a powerful incentive to work in effort-intensive enterprises, that free labor could never be as productive as slave labor. Thus, a freed slave would never be able to compensate her master for the net present value of her labor.

However, this theory is not sufficient to explain the patterns demonstrated in the above data. The frequency of manumission dropped precipitously after the Louisiana Purchase, as U.S. rule erected additional barriers to manumission. Moreover, while Fenoaltea's theory posits that the nature of work in which slaves are engaged determines whether manumission is a feasible incentive, these arguments were not advanced by those who advocated limiting manumission.

Opponents of manumission did express substantial concerns about potential externalities of freeing slaves. Some thought the presence of some free blacks would give slaves unreasonable expectations of freedom, rendering them less productive workers. (Holland, 1822, quoted in Matison, 1948) A Louisiana court shared this view, ruling that negative externalities posed

[15] I designate a manumission as rural manumissions if it occurs outside the parish of New Orleans.

by free blacks justified state limitations of manumission.[16] These beliefs contrast sharply with Spanish rule, under which the prospect of manumission was held out as an incentive for work, and significant numbers of slaves gained freedom.[17]

Manumission also carried important political implications. In a Union strongly divided over the morality of slavery, the presence of freed blacks in the South undermined the claim that "slavery was justifiable and necessary because Negroes were inferior beings and hence incapable of maintaining themselves as freemen" (Matison, 1948.) Finally, many southerners feared slave revolts. Ulrich Phillips argues that many Southerners feared that free slaves were likely to incite rebellion (Phillips, 1940.)

3.7 Conclusion

My study confirms the consensus view that manumission as an incentive scheme would probably not have brought an end to the American system of slavery: manumission was not common, and laws prohibiting it were effective. My results, however, suggest that it is incorrect to simply dismiss manumission as a very rare, infeasible, or exclusively urban phenomenon. In Spanish Louisiana, a system of coartación provided a system of incentives to slaves to work diligently, which made both the slaves and the masters better off. Owners manumitting their slaves were indeed compensated the fair market value of the slaves. When the United States took over, the right of coartación was repealed, and the Spanish paternalist organization was replaced by one less sympathetic to the motives, character, and aspiration of slaves. The loss of the right to sue for freedom was an important factor in the subsequent sharp drop in the number of manumissions. However, concern about potential externalities (real or imaginary), racism, and the desire to preserve slavery as an institution ensured the decline. Still, hundreds of slaves were manumitted gratuitously as a reward for "good service," and some managed to purchase their own (or relatives) freedom. Owners in this new regime appeared to exploit their monopoly position, and charged prices about 20 percent higher than market prices.

[16] Henriette v. Barnes, La. An. 453, 454 (1856), cited in Wahl, "Legal Constraints."

[17] Goveia (1970) reports that Cuba in 1774 had 96,440 whites, 30,847 free people of color, and 44,333 slaves, while the ratio of free people to color to slaves in Puerto Rico in 1827 was almost four to one (p. 17.)

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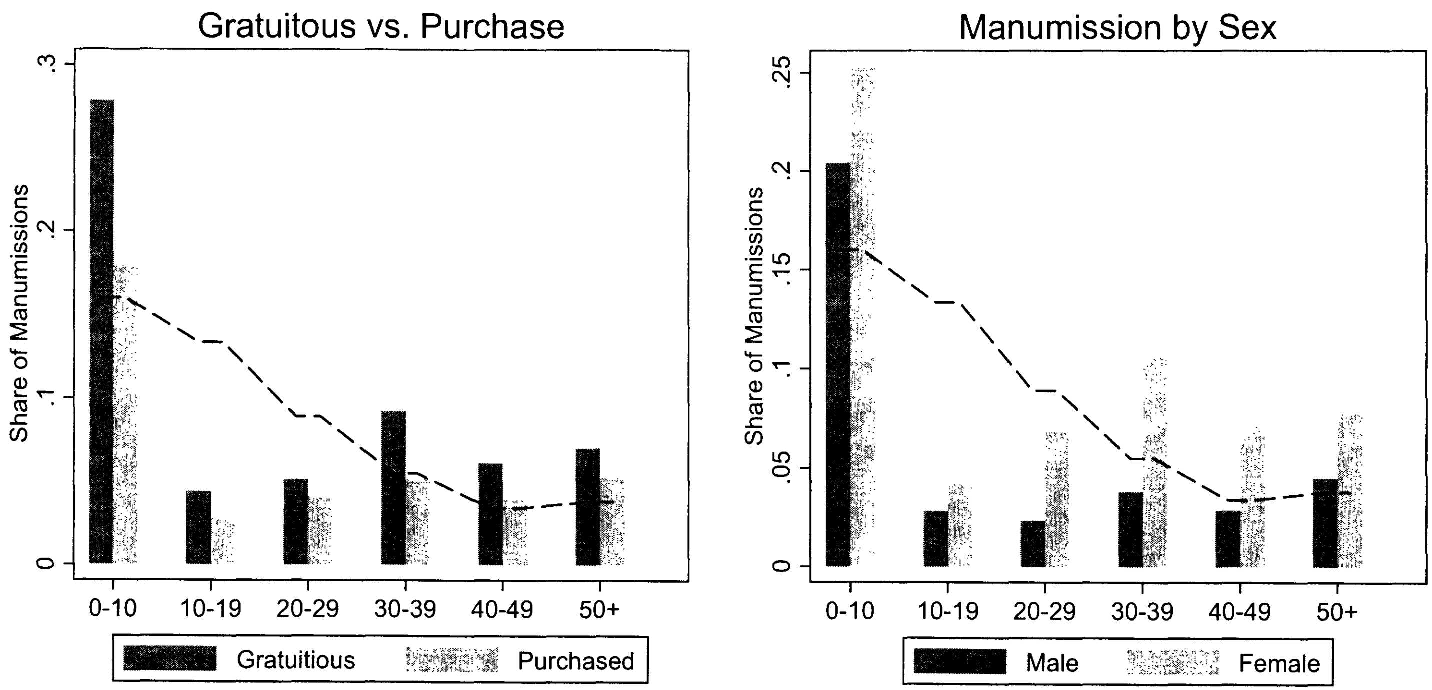
Figure 3.1: Age Distribution of Manumisses



Note: The left graph gives age distribution of gratuitous and paid manumissions; the right graph gives the age distribution of manumission by sex. In each graph the sum of all the bars is equal to one. For reference the age distribution of the 1850 Slave Population is given by the dashed line. (The area under the dotted lines is also equal to 1). Manumission data are from Hall, "Databases", while slave population data are from Tadman, "Speculators."

Figure 3.2: Number of Manumissions and Real Slave Price

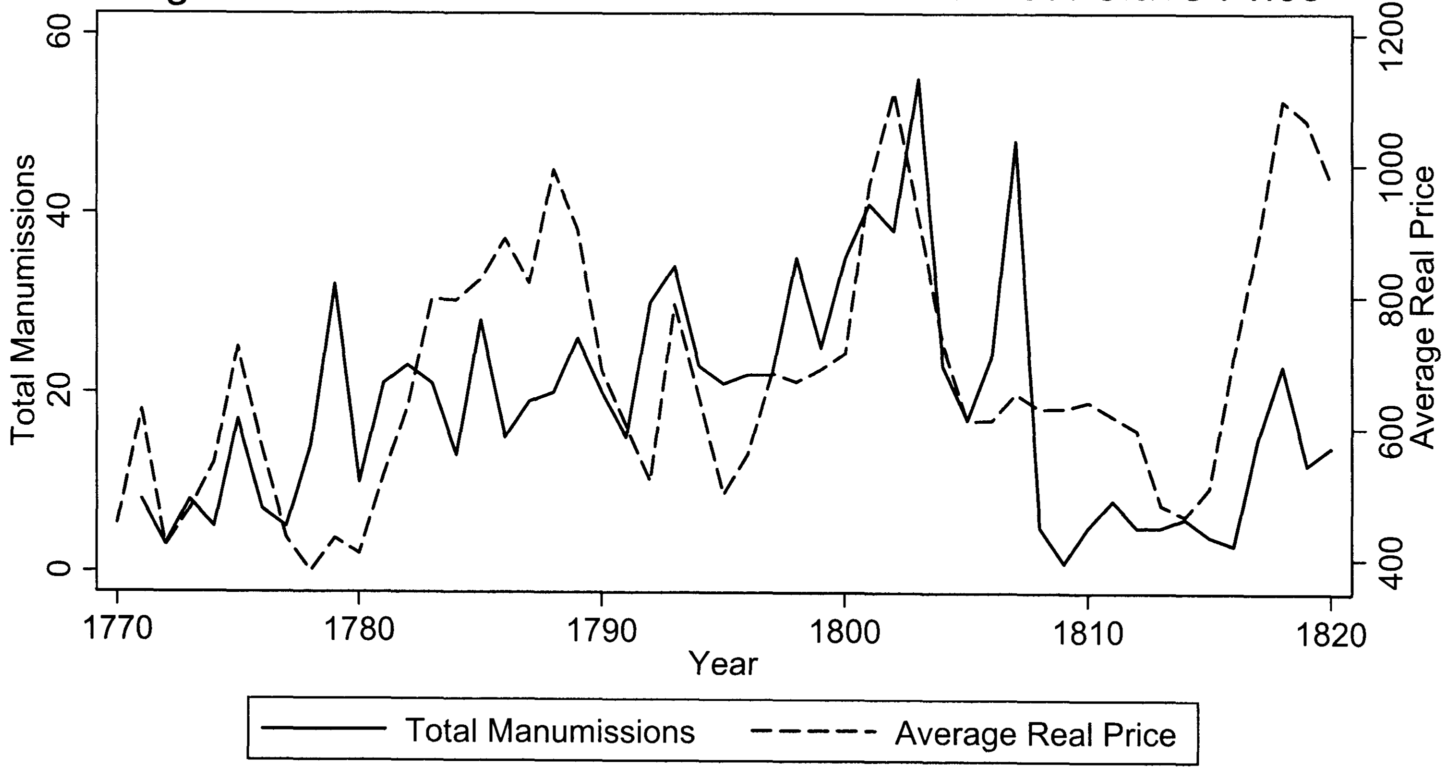
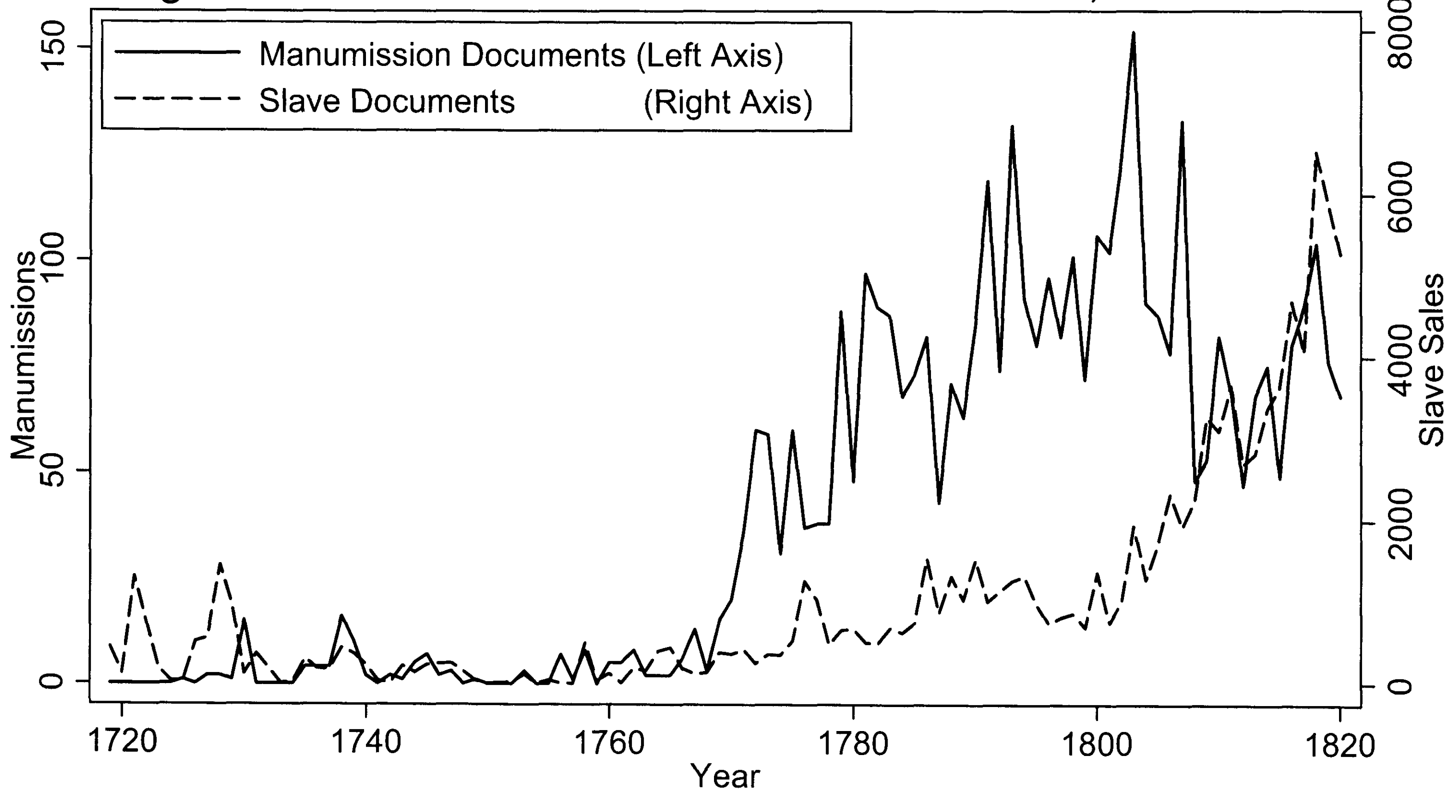


Figure 3.2 plots average real price of male field hand and number of manumissions by year, 1770-1820
Using data from the Hall, "Databases." Prices were deflated using data from McKusker, "How Much."

Figure 3.3: Manumission and Slave Documents, 1720-1820



Note: Measure of the number of manumission and slave records in Hall, "Databases" by year, from 1720 to 1820.

Table 3.1: Summary Statistics in the Slave and Free Database

	Documents in Manumission Database (N=4,060)		Documents in Slave Database (N=100,666)	
	% Coded	Summary	Percent	Summary
Age is coded	98.6%		96.5%	
Mean		20.0		17.8
Sex is coded	99.0%		90.0%	
%Women	62.5%		43.6%	
Race is coded	94.0%		90.0%	
% Black		56.5%		93.0%
% "Mulatto"		38.8%		5.8%
% other		4.6%		1.2%
Sale price is coded	36.0%		59.0%	
Mean[a]		619.3		635.5
Document type			100.0%	
Estate inventory				27.5%
Estate sale				3.7%
Non-probate sale				49.8%
Other				19.0%
Skill indicated	1.7%		8.7%	
If skilled, artisan skill		23.9%		17.1%
Number of Documents:				
1770-1803		2,606		29,823
1804-1820		1,296		58,610

a Average individual sale price in real 1800 dollars. (Prices were deflated using the series in McCusker, *How Much is That?*)

Notes : Summary statistics from the Hall, "Databases." Because values are often missing, summary statistics, as well as the percentage of records for which values were non-missing, are indicated.

Table 3.2: Means of Manumission

Means of Manumission	New Orleans **1725-1803**	**1804-1820**	Rural Louisiana **1725-1803**	**1804-1820**
	2,268 Manumissions	707 Manumissions	496 Manumissions	589 Manumissions
	(Slave sales: 26,909)	(Slave sales 32,035)	(Slave sales: 15,147)	(Slave sales: 26,575)
	Percent	Percent	Percent	Percent
Living Master/Mistress	33.7%	51.6%	47.6%	43.8%
Self-purchase	19.5%	10.3%	14.9%	8.3%
Purchase by Other	22.2%	11.2%	16.5%	25.0%
Under will	15.0%	13.3%	9.7%	11.5%
Other	8.5%	13.2%	9.3%	8.7%
Missing / Unclear	1.1%	0.4%	1.8%	2.7%

Notes : This table gives the means by which each of the slaves in the Free database were manumitted. There are a total of 4,060 manumission records. Descriptions were manually coded into the above categories. For comparison purposes, the number of slave sales in each region-period cell is also given. Data are from Hall, "Databases."

Table 3.3: Reasons for Gratuitous Manumission

	Manumission by living master (N=1624)		Manumission by Testament (N=554)		Example of Comments
	Pre-US	US	Pre-US	US	
Number of Manumissions	990	621	387	160	
Proportion of manumissions for which following reason was given:					
No Reason Given	0.28	0.64	0.71	0.79	
Service	0.58	0.31	0.24	0.18	Gratuitous for services given.
Affection	0.26	0.04	0.00	0.00	good services and love and affection
Moral	0.03	0.00	0.06	0.00	"For various motives moving my soul"
Related (declared)[a]	0.12	0.05	0.09	0.04	his natural children baptised as free
Related (investigator)	0.20	0.08	0.16	0.06	Carlos Noel is probably the father.

[a]"Related (declared)" indicates that the person manumitting admitted to a blood relationship with the freed slave, while "(investigator)" indicates the investigator coding the document determined that it was likely that the manumiss was related to the slaveholder. Data are from Hall, "Databases"

Notes : Many jurisdictions required slave owners to give "just cause" for freeing a slave. The proportions do not sum to one because in many cases more than one reason was given.

Table 3.4: Conditions on Manumission

Condition	Frequency	Examples
No Conditions, or Condition not listed	3591	
After death of slaveholder	201	"After death & burial of master" "After death of both masteres"
After N years more service	69	"Freed at the end of 3 years" "Must serve 2.5 years more"
Upon Payment, or More Money Required	62	"Must pay 100 per year" "Pay p200 to estate"
Until Legal Age for Manumission	32	"Until 30 years of age" "treated as free till legal emancipation'
Other	100	"If master does not return" "kid must remain with master"

Notes : The author manually coded into categories information about conditions placed on manumissions from the records in Hall's "Databases.

Table 3.5: The Price of Freedom

	Baseline[a]	Spanish Period Only	US Period Only	Manumission By Self or Other	Rural or Urban Manumission
Spanish * manumission	0.01	-0.04			
	(0.04)	(0.04)			
US * manumission	0.18 **		0.19 **		
	(0.06)		0.06		
Spanish* manumission * self				-0.01	
				(0.04)	
Spanish * manumission * other				0.05	
				(0.06)	
US * manumission * self				0.18 **	
				(0.07)	
US * manumission * other				0.18	
				(0.13)	
Spanish* manumission * urban					0.01
					0.04
Spanish * manumission * rural					-0.03
					0.12
US * manumission * urban					0.18 *
					0.07
US * manumission * rural					0.19
					0.12
Male	0.18 **	0.13 **	0.21 **	0.18 **	0.18 **
	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)
Light color male	0.28 **	0.29 **	0.26 **	0.28 **	0.28 **
	(0.05)	(0.11)	(0.06)	(0.05)	(0.05)
Light color female	0.06	0.05	0.08	0.06	0.06
	(0.04)	(0.09)	(0.05)	(0.04)	(0.04)
Born in Africa	-0.12 **	-0.12 **	-0.10 **	-0.12 **	-0.12 **
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
Disease	-0.46 **	-0.37 **	-0.51 **	-0.46 **	-0.46 **
	(0.05)	(0.08)	(0.06)	(0.05)	(0.05)
Artisan, aged 30-39	0.35 **	0.45 **	0.31 **	0.35 **	0.35 **
	(0.04)	(0.07)	(0.05)	(0.04)	(0.04)
Artisan, aged 40 and above	0.41 **	0.39 **	0.41 **	0.41 **	0.41 **
	(0.06)	(0.11)	(0.06)	(0.06)	(0.06)
Age[b]	0.13 **	0.12 *	0.16 *	0.13 **	0.13 **
	(0.03)	(0.05)	(0.07)	(0.03)	(0.03)
Age^2	-2.80E-03 **	-2.55E-03 **	-3.49E-03 *	0.00 **	0.00 **
	(6.79E-04)	(9.51E-04)	(1.51E-03)	(0.00)	(0.00)
Age^3	1.44E-05 **	1.27E-05 *	1.94E-05	1.44E-05 **	1.44E-05 **
	(4.69E-06)	(6.29E-06)	(1.10E-05)	(4.69E-06)	(4.69E-06)
N	5912	1985	3927	5912	5912
R-squared	0.39	0.43	0.37	0.39	0.39

*=coefficient significantly different from zero at the 5-percent level.
**=coefficient significantly different from zero at the 1-percent level.
[a]The estimated model in columns 1-5 includes year, parish, and month fixed effects, in addition to the listed controls.
[b]A third-degree age polynomial minimizes the Akaike Information Criterion.
[c]Columns 2 and 3 estimate the same regression on data from only the Spanish, and only the US periods, respectively.

Notes: Robust standard errors are given in parentheses. The model is estimated on a sample of slave sales and manumisses aged 30 and above, because the legal age at which a slave could be manumitted was typically 30 years of age. Data are from Hall, "Databases."

Table 3.6: Matching

	Manumission Premium	N, Slaves	N, Manumissions
Entire Sample (1770-1803)	0.034	4599	380
	(0.024)		
Spanish Regime (1770-1803)	0.000	1373	293
	(0.03)		
US Regime (1804-1820)	0.149	3226	87
	(0.052)		

Table 6 gives the results from the matching estimator of Angrist, "Estimating the Labor Market Impact." In particular, we partition the observations for both slave and manumission sales into different cells, by (i) manumission or slave sale, (ii) sex, (iii) age (30-34, 35-44, 45-54, 55-64, 65+), (iv) origin (born in the "Old World," born in the New World and light skin, and born in the New World not light skin), (v) skilled or un-skilled, and (vi) time period (US or Spanish Rule). The estimated manumission premium is then the average price difference between manumission sales and slave sales in each of the 60 resulting cells. Formally, the estimate is:

$$\hat{\alpha} \equiv \sum_{m \in M} \delta_m N_{m1} (\overline{y}_{m1} - \overline{y}_{m0}) / \sum_{m e M} \delta_m N_{m1}$$

where α is the estimated premium is, δ_m is an indicator variable taking the value 1 if the cell contains both manumission and slave sales, N_{m1} is the number of manumissions in cell m, $\overline{y}_{m1}$ is the average manumission price for cell m, and $\overline{y}_{m0}$ is the average slave sale price for cell m. Prices are measured as the log of the real price. Data are from Hall, "Databases."