Essays on Banking and Corporate Finance

by

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B.S. Mechanical Engineering
Universidad Simón Bolívar, 1994

SUBMITTED TO THE DEPARTMENT OF ECONOMICS IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE OF

Ph. D. OF ECONOMICS
AT THE
MASSACHUSETTS INSTITUTE OF TECHNOLOGY

JUNE 2005

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Submitted to the Department of Economics on April 15, 2005
in Partial Fulfillment of the Requirements for the Degree of Ph. D. in Economics

ABSTRACT

The first essay provides evidence that banks are liquidity constrained and hold private information about borrowers that hinders substitution of financing sources. Using loan level data from a public credit bureau and exploiting an exogenous shock to bank liquidity, I show that adverse selection prevents full arbitrage of profitable opportunities by competing lenders and thus liquidity constraints propagate to bank-dependent borrowers.

The second essay evaluates a government program that targeted credit to small firms through existing financial intermediaries. Using the program eligibility rule to identify the effect on target firms, I find that target firms' total bank debt increased by 8 cents for every dollar of program financing provided to the banks. This effect is larger when the intermediary bank is more likely to lend to smaller firms according to observable bank characteristics.

The third essay evaluates empirically the effect of credit history disclosure on the financial position of a sample of manufacturing firms in Argentina. Results indicate that credit history disclosure has a negative impact in the ability of firms to raise external finance when firms are exposed to a high liquidity risk.

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ACKNOWLEDGEMENTS

I am in great debt to many people. First and foremost with my four advisors. With Esther Duflo, for always asking the right questions. With Antoinette Schoar, who taught me how to think about finance. To Abhijit Banerjee, for his incomparable economic insight. And to Sendhil Mullainathan, for having stirred me in the right direction. Much of what is good in this thesis is owed to them.

I benefited a great deal from interactions with my friends and fellow students at MIT. Too many to name them all here, I would like to thank specially Iván Fernández-Val, Andrew Hertzberg, Alexis León, and Verónica Rappoport. I learnt a lot from our discussions.

I am grateful for the financial support from the Fundación Gran Mariscal de Ayacucho, the MIT Economics Department, the World Economy Lab, the Corporación Andina de Fomento and the Schultz Fund. It made this venture possible.

I finally thank my family for their unconditional support.

CONTENTS

INTRODUCTION

Bank liquidity constraints affect investment only if bank credit cannot easily be substituted for other sources of finance. The first chapter of the dissertation provides evidence that banks are constrained and hold private information about borrowers that hinders substitution of financing sources. I test for liquidity constraints by showing that the amount of bank lending is sensitive to an exogenous change in the financial position of banks caused by a credit market intervention by the Argentine government. I estimate that lending increases by $0.7 for each dollar of additional liquidity. Furthermore, this expansion appears to be profitable: the additional loans are not more likely to default than other loans. Using loan level data from a public credit bureau, I track the effects of the liquidity shock on the composition and default risk of loans across borrowers for which the bank has an information advantage relative to other lenders. I find that when banks hold an information advantage they rely less on collateral to ration credit and are able to screen out high risk borrowers. Conversely, when banks are relatively uninformed they are reluctant to extend credit and attract high risk borrowers. The results suggest that adverse selection prevents full arbitrage by competing lenders and thus liquidity constraints propagate to bank-dependent borrowers.

The results of the first chapter suggest that policies that increase the availability of finance through banks may increase the availability of credit to bank-dependent firms. The second chapter addresses empirically the issue of whether this type of policy can be also used to target credit through financial intermediaries. I evaluate a targeted credit program to firms with less than 20 workers and $200,000 in annual sales in Argentina. I merge program data with loan level credit history data from a Public Credit Registry to assess the effect of the program on the evolution of bank debt of the target firms. I estimate the effect of the program on target firms by comparing their debt growth with that of ineligible firms close to the eligibility threshold. The results indicate that target firms' total bank debt increased by only 8 cents for every dollar of program financing provided to the banks. I also show that the impact of the program on target firms is larger when the intermediary bank is more likely to lend to smaller firms according to observable measures such as size and ownership.

Public disclosure of credit information has, in theory, ambiguous effects on the efficiency of credit allocation. The existing empirical evidence supporting the efficiency gain is mostly anecdotal or based on cross-country comparisons. The third chapter of the dissertation evaluates the effect of credit history disclosure on the financial position of a sample of manufacturing firms in Argentina. In 1998, the credit information of all firms and individuals with less than $200,000 in debt with any financial institution became publicly and freely available on the web page of the Central Bank of Argentina. The effect of disclosure is identified by comparing the evolution of firms below the eligibility threshold with firms above it. Credit history disclosure has a negative impact in the ability of firms to raise external finance when firms are exposed to a high liquidity risk. Firms with low ratios of liquid assets to short term liabilities experience a decline in bank and trade finance, and an increase in the average cost of external finance. This effect occurs regardless of whether the firm is actually affected by a liquidity shock, suggesting providers of credit anticipate the effects of disclosure and adjust credit supply accordingly. The results also show that firms with illiquid positions ex ante, hoard cash after disclosure to insure against potential liquidity shocks. The combined effect of costly external finance and the need to hoard cash appear to cause a disproportionate negative effect on illiquid firms' sales and returns. The results are consistent with the predictions of models that suggest that public disclosure of noisy signals about firm quality will increase the likelihood and costs of financial distress.

CHAPTER ONE
Constrained Banks, Constrained Borrowers: Effect of Bank Liquidity on the Availability of Credit

1. Introduction

The question of how bank liquidity constraints affect lending, economic activity and business cycles has long been of concern to the academic literature on financial institutions. Financially constrained banks are the proposed culprit behind recent accounts of the Great Depression (Bernanke 1983) and capital crunch based explanations of the US recession of the early 90's (Sharpe 1995). They are also key for the existence of a lending channel of transmission of monetary policy (Bernanke and Blinder 1988; Holmstrom and Tirole 1997; Stein 1998). Given the major role financial intermediaries play in the allocation of capital, frictions that affect the ability of intermediaries to lend can have a large effect on the overall efficiency of investment. However, constraints faced by individual banks will matter for financial intermediation only if other providers of finance (bank or non-bank) cannot cover the excess credit demand. The question of whether banks are financially constrained is therefore relevant to the extent that lender substitution is not frictionless.

This paper provides evidence that banks are liquidity constrained and hold private information about borrowers that hinders firms from freely substituting across financing sources. It does so by using loan level data to follow the effect of an exogenous liquidity shock on the amount, composition, and default risk of bank lending. As a source of exogenous variation in banks' financial position, I exploit a government program that made limited amounts of financing available to banks in Argentina. This setting disentangles changes in bank liquidity from variations in investment opportunities –i.e. the opportunity to make profitable loans– and thereby allows a clean test of liquidity constraints. Also, novel data from a Public Credit Registry allows constructing the individual credit histories of all the borrowers in the financial system at the time of the government program. I classify loan recipients by their credit history and argue that banks have an information advantage on lending to borrowers for whom they have an established a relationship, and a disadvantage if they have no pre-existing relationship but other banks do. Comparing credit allocation rules and repayment performance across types of loan recipients shows that private information is likely to affect the ability of borrowers to switch lenders.

I test for liquidity constraints using information from monthly bank balance sheets between 1998 and 2000 to show that lending reacts to changes in the financial position of the bank induced by the program. The magnitude of the response is substantial, on the order of $0.7 per dollar of liquidity expansion. The program financing considered in this study is not large enough to affect the bank's marginal cost of capital, and is also uncorrelated with investment opportunities of the bank. Therefore, the sensitivity of lending to exogenous changes in liquidity provides evidence that banks face binding liquidity constraints.[1]

In the past, testing empirically for the existence of bank liquidity constraints has proved a difficult task. A broad literature on the lending channel highlights the time series correlation between the financial position of banks and loan growth (Bernanke and Gertler 1995; Hubbard 1995; Peek and Rosengren 1997; Ostergaard 2001), and the observation that this correlation is stronger for smaller, less capitalized banks, which are more likely to be constrained (Jayaratne and Morgan 2000; Kashyap and Stein 2000; Kishan and Opiela 2000; Ashcraft 2003). The interpretation of the findings as evidence of liquidity constraints has been questioned on the grounds of reverse causality and omitted variables. The observed patterns could arise in the absence of liquidity constraints when the shocks to bank liquidity are associated to changes in bank investment opportunities.[2] Using the expansion in available finance provided by the government program as a source of variation in bank liquidity avoids this problem. Furthermore, the program rules induced non-linearities in participation that allow dealing with the potential endogeneity of program resource allocation.[3] The

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[1] The test for existence of liquidity constraints is based on rejecting the predictions of the Modigliani-Miller (MM) proposition for banks. The optimal response of an unconstrained bank to a cash windfall that doesn't affect the marginal cost of capital is to reduce market priced liabilities or to distribute it among investors as dividends. Expanding lending would yield a return below the opportunity cost of capital. The same underlying logic is behind the investment-cash flow literature in corporate finance and the empirical work on the lending channel. See Stein (2003) for a recent survey on both.

[2] See Kashyap and Stein (2000) for the critiques on the time series correlation and Kaplan and Zingales (1997; 2000) for the concerns with interpreting the cross sectional patterns of investment sensitivity to liquidity as evidence of financial constraints in the context of non-financial firms.

[3] The program was implemented in several waves of exogenously determined timing and size. Also, the allocation of program resources across banks was given by a strict administrative rule. Using the interaction of the amount and timing of the waves and the administrative formula I simulate available financing from the

exogenous shock to bank liquidity provides a test of the validity of the past work based on the comparison across banks sorted according to a proxy for liquidity constraints. I find that the cross sectional variation in the sensitivity of lending to liquidity is small relative to its absolute size, which suggests that cross sectional comparisons tend to underestimate the magnitude of this elasticity.

A positive sensitivity of lending to liquidity is, however, also consistent with free cash flows theories of investment (Jensen 1986). The free cash flow and the liquidity constraints interpretations of the observed sensitivity of lending to liquidity have contrasting predictions on the profitability of the marginal loan. Under the free cash flow view, an exogenous expansion in available financing leads to a deterioration of the quality of bank investment. The loan level data used in this paper allows testing this implication by looking at the performance of loans issued during liquidity expansions. The results indicate that loans financed during liquidity expansions are not more likely to default than loans issued in other periods. This finding is consistent with the liquidity constraints interpretation and, under plausible assumptions, suggests constraints prevent banks from undertaking profitable lending opportunities.

Having found banks are constrained, the rest of the paper is devoted to test whether borrowers can easily substitute bank credit for other sources of finance. The paper documents how lending behavior is consistent the predictions of well known models of private information in lending relationships (Sharpe 1990; Rajan 1992; Petersen and Rajan 1995; Von Thadden 2001). The common assumption in this literature is that banks elicit private information about borrower creditworthiness through the relationship. As a result, competing uniformed lenders face adverse selection and a potential winner's curse when attempting to bid a borrower away from an informed bank. Uninformed lenders are reluctant to extend credit to borrowers that are switching from informed sources of finance, and attract borrowers of a low average quality when they do so.[4]

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program using only exogenous sources of variation. Simulated financing is then used to instrument for bank liquidity.

[4] The winner's curse term is borrowed form the theory of competitive bidding under asymmetric information and common values. Broeker (1990), Rajan (1992), and Von Thadden (2001) all model lender competition for corporate borrowers as a bidding game with common values. The mixed strategy equilibria of these models

The paper proceeds by distinguishing borrowers for which a bank has an information advantage relative to other lenders. I make a distinction between loan recipients that had a previous relationship with the bank ('existing' borrowers) and those that did not ('new' borrowers). Within the category of new borrowers, those with a pre-existing relationship with other banks are considered as 'switching' borrowers. Banks presumably have an informational advantage over other lenders when issuing credit to existing borrowers, and a disadvantage when lending to switching ones. The exogenous change in loan supply induced by the program is then used to estimate how the average characteristics of borrowers and the probability of default change across these three types of borrowers during lending expansions. Changes in average observable borrower characteristics provide information about the margin along which banks ration credit. And variations in the average rate of default reflect the default risk of the marginal loan of the bank relative to the infra-marginal one.[5]

The results indicate, first, that when banks hold an informational advantage they rely less on collateral requirements to ration credit. Banks extend additional credit to existing borrowers during liquidity expansions with no decrease in the collateral to loan ratio. Since existing borrowers can increase the amount of collateral pledged in response to the lending expansion, this result implies that the access to credit of existing borrowers is not constrained by collateral. Instead, existing borrowers appear to be rationed using past performance: the proportion of existing borrowers that hold non-performing debt with the issuing bank increases during lending expansions. On the other hand, the evidence indicates new borrowers and switching borrowers are rationed using collateral, even though information on switching borrowers' past performance is publicly available through the Public Credit Registry. This suggests banks hold relationship-specific information about credit quality of existing borrowers and allocate credit accordingly.

predict that informed lenders capture informational rents, interest rates are above the market rate, some borrowers switch lenders and uninformed lenders make zero profits in expectation.

[5] If banks use some observable borrower characteristic to ration credit, e.g. collateral, then collateral requirements must drop when loan supply expands exogenously. Similarly, if the average probability of default increases during en exogenous loan expansion, then the marginal borrower of the bank must have a higher probability of default than the infra-marginal one.

Second, the findings suggest that banks can select low credit risk loan recipients among observationally equivalent existing borrowers. During credit expansions banks expand lending to borrowers with a worse past repayment performance. Even though past performance is a good predictor of future loan performance on average, I find that loans issued to existing borrowers during credit expansions are not more likely to default.

Third, the evidence is consistent with the adverse selection prediction. During lending expansions, the default rate on loans to switching borrowers increases four times as much as for new borrowers with no credit history at all. Banks are also reluctant to issue loans when relatively uninformed. Banks allocate 0.5% of the flow of lending to switching borrowers, compared to 12.5% to new ones with no credit history, and 87% to existing borrowers.

Overall, my results suggest that liquidity constrained banks have important implications for the aggregate availability of credit to firms that are dependent on bank finance. The effects of monetary policy or aggregate shocks on output and investment are amplified when the ability of financial institutions to raise finance is constrained.[6] The capacity of banks to produce private information on borrower quality plays an important role in this transmission mechanism. The findings suggest that bank lending is special and that "financial institutions do matter" (Allen 2001).

The paper proceeds as follows. Section 2 provides the institutional and analytical framework for the empirical analysis. Section 3 describes the data. Section 4 is devoted to the estimation of the lending sensitivity to liquidity. It discusses previous literature, presents the empirical specification and the identification strategy, and comments the results. Section 5 focuses on the relationship between bank liquidity, loan recipient characteristics and default risk. Section 6 concludes.

[6] A growing empirical literature that links bank liquidity to aggregate investment and output in the US often finds contradictory results. For example, Ashcraft (2003) finds that state output is not significantly affected by changes in loan supply, while Peek and Rosengren (2000) find there is a significant effect on state level real estate activity. The inconsistency in the evidence has often been attributed to a potentially high elasticity of substitution between bank and non-bank sources of financing. However, this evidence is based on cross sectional comparisons of banks, which I found tends to underestimate the effects of changes in bank liquidity.

2. Setting and Conceptual Background

2.1. The Argentine Banking System in the 90s

The Argentine banking sector and regulatory system were thoroughly overhauled twice during the 1990s. The period following the hyperinflation period that ended in 1990 was marked by the creation of an independent regulatory agency within the Central Bank, the abolition of deposit insurance, and an increase of capital requirements above Basel standards. The 1992-1994 period was characterized by fast economic growth, sharp rises in assets prices and fast development of the financial system.

The Tequila crisis in 1995 provoked widespread bank panics that put in evidence the weaknesses of the regulation. The regulatory system was amended again to introduce a combination of market discipline and supervision. Amendments included, among other things, the creation of a Public Credit Registry to ease the monitoring and disclosure of the risk composition of bank assets.[7] All the empirical results of this paper are estimated restricting the sample to the period that follows this second regulatory reform.

The banking system in this period is characterized by rapid deposit growth (Figure 1) and a large inflow of foreign capital.[8] Another feature of the post reform banking sector was its imperviousness to large emerging market shocks (1997 Asian crisis, 1998 Russian moratorium and 1999 Brazil devaluation). Finally, during the period of analysis the local currency was pegged to the dollar and thus the monetary authority had almost no control over the amount of money. This setting of liquidity growth, capital inflows and limited monetary intervention is ideal for addressing the empirical questions posed in the introduction.

2.2. Program Characteristics

The Credit Program to Small and Medium Sized Firms (referred to as MYPES for its Spanish acronym) was implemented in Argentina between 1993 and 1999 and provided financial intermediaries limited financing at a subsidized interest rate (average dollar deposit

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[7] Other reforms were the creation of a limited, fully funded deposit insurance; the replacement of reserve requirements with liquidity requirements, which decline with the residual maturity of each liability; the requirement of annual bank ratings provided by a rating agency registered with the Central Bank; mandatory bank subordinated liability of 2% of deposits each year and; the privatization of most government owned banks.

[8] By 1998 foreign-owned banks held 53% of the assets and 46% of the loans of the financial system.

rate). The program was funded by the Inter-American Development Bank (IDB) and had the objective of increasing formal intermediary lending to small businesses. The MYPES falls into the category of what is known in the development agency jargon as an *on-banking* or a *two-step lending* program. The common feature of on-banking programs is to make financing available to existing financial intermediaries, with the condition that a proportional amount must be lent in turn to a narrowly defined group of borrowers. This type of credit market intervention is widely used in developing countries. The IFC (World Bank) alone allocated during the last decade more resources to small firms through on-banking "than through any other individual program" (Barger 1998). The MYPES required banks to issue $1 of loans to eligible borrowers for every $0.75 of program financing received.[9] Firms with less than 20 workers and less than $200,000 in annual sales were eligible to receive program loans.

In previous research (Paravisini 2003) I show that *eligible borrowers'* debt increased by less that 10 cents for every dollar of program financing received by participating banks. I also show that banks circumvented the allocation rule by picking the best performing borrowers among their eligible clients and re-labeling existing debt as 'program loans'. The intuition of how re-labeling was put into practice is shown in Figure 2, which shows the monthly evolution of the average debt of firms that received program loans. Debt is plotted relative to the month firms received the program loan (labeled as month 0 on the horizontal axis). Each column represents the total bank debt of the firms and distinguishes three separate sources: program debt (black), other debt from bank that issued program debt (white), and other debt from other banks (grey). The graph shows that program debt substituted dollar for dollar debt previously held by loan recipients with the bank. The conclusion there was that banks were largely unconstrained in their use program financing, which makes it possible to treat as an exogenous source of liquidity to the bank.

Program financing was distributed in 12 waves between 1993 and 1999. The amount of resources allocated to each wave varied under the discretion of the IDB. The yearly flow of program financing is plotted in Figure 3. The plot displays two peaks: one during years 1995 and 1996 and another one in 1999. The first peak coincides with a period of massive deposit

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[9] To avoid confusion between loans from the government to the banks and the associated loans from the banks to eligible firms, I will call loans to banks "program financing" and loans to firms "program loans to firms".

drains triggered by the Tequila crisis (see Figure 1) and with the subsequent regulatory reforms. The second peak was driven by an 'administrative rush' to finish allocating the program resources before year 2000. According to MYPES managers, a second phase of the program (MYPES II) was planned to begin in 2000 and financing for this phase was conditional on the complete execution of the budget of the first one. I will take advantage of the fact that each wave of the program provides an independent shock to liquidity and perform estimations restricting the sample to the final waves. This avoids the potential bias that may arise from the program being purposely timed to provide liquidity to weaker banks when they most needed it.

A month prior to the beginning of each wave, the Central Bank of Argentina announced publicly the amount to be distributed and banks submitted an application to participate. Resources in the wave were allocated among all participant banks according to an administrative formula based on bank characteristics.[10] The formula assigned a higher fraction of the wave resources to banks with a smaller average size of loans and a higher proportion of loans in poor provinces.[11] Each participating bank was assigned a point score according to these characteristics[12] and the wave resources were allocated proportionally to each bank's score. I will use this formula later to predict the expected amount of available program financing to each bank in each wave. The resulting predicted available financing is uncorrelated with bank investment opportunities, which makes it suitable for use as an

[10] Formally, banks submitted in the application the amount of financing required. If the sum of the requested financing of all applicants exceeded the amount of resources in the wave, financing was distributed among applicants according to the formula. However, the financing demand surpassed available resources in every wave, and the formula was used to allocate resources in each of them.

[11] Originally, the banks had to submit also in their application the fraction of matching resources the bank would commit to the ensuing loans to eligible firms and the interest rate they would charge on these loans. Both of these variables were to be included in the distribution formula if the requested financing exceeded the amount of resources in the wave. However these variables were dropped from the formula after the first two waves because there was no cross sectional variation in the bids. The matching funds bid was exactly the minimum matching funds required by the program ($1 for every $3 of program financing) in 98% of the cases. And the interest rate bid matched a "suggested" rate provided by the government. The difference between the highest and the lowest interest rate bid in any wave was at most 0.06 percentage points and zero 81% of the time. This variation was negligible relative to the average interest rate of 13.7% during the period.

[12] The point score according to average loan size is shown in Table 1. The point score according to regional distribution allocated a weight of 30 to loans issued in the riches provinces (Capital Federal, La Pampa y Santa Cruz) and 100 to loans issued in the poorest (Formosa, Catamarca, Santiago del Estero, Chaco, Jujuy, Misiones, Corrientes, Salta, Chubut y Tucumán). Loans to other provinces received a weight of 70.

instrument for liquidity when estimating the sensitivity of lending to the financial position of the bank.

Of the 126 financial institutions in the sample between 1995 and 2001 (see the data description in the next subsection), 29 received program financing at least in one wave. The number of banks that participated in each wave varied between 5 and 15, and participation was positively correlated with the amount of resources to be distributed in each wave (see Figure 3). During interviews, executives of participating banks acknowledged that although the program provided a cheap source of finance, the amount at stake was sometimes too small to compensate the red tape costs involved in participating.[13] Since program participation was likely to be related to factors affecting the lending-liquidity sensitivity (e.g., negative deposit shocks, new investment opportunities) I will exploit changes in wave size as an exogenous source of variation in participation. The descriptive statistics of banks by their participation status show how the endogenous participation decision might produce biased estimates if unaccounted for (see Table 2). Participating banks are on average smaller and more likely to be constrained than non-participating ones. A comparison of participating versus non-participating banks could lead to an upward bias in the estimate of the sensitivity of lending to liquidity. All the specifications used in the empirical section of this paper will include bank fixed effects, which will account for the potential time invariant differences across banks.

Banks had three months to use the allocated resources or pay a penalty equal to twice the interest rate of the unused balance. The unused balance would be reassigned in the next wave of the program among the participating banks in that wave. Also, banks bore the credit risk of the loans to the eligible firms: repayment of program financing was not contingent on firm loan performance. However, the repayment schedule of the program financing matched exactly the schedule of the associated firm loan. The duration of loans to firms was limited to 36 months (plus 12 months of optional grace period). The descriptive statistics of the program loans to firms (Table 3) show that the median duration of program firm loans was 36 months and the median grace period was zero. This suggests banks also selected eligible

[13] Participating banks had to provide the program administrators with a database containing the characteristics of the recipients of the loans associated with the program. They also had to send monthly reports of the repayment performance of these loans.

firms in order to maximize the time they could hold program financing within the imposed constraint.

Finally, the MYPES program was small relative to the size of the financial system: it allocated around $90 million among participating banks, which represented 0.1% of total loans in 1995. This implies that the program had a small impact on aggregate liquidity and was unlikely to influence interest rates, which allows focusing on the partial equilibrium effects of the liquidity expansion. On the other hand, the amount of financing was sizeable relative to banks that participated in the program: financing represented about 1.8% of stock and 10.6% of the flow of loans during the months of implementation.

To summarize, the program provided banks with a limited amount of low cost, medium term financing. The cross sectional and time series variation of the *available* program financing and the probability of bank participation in the program can be predicted using wave size and timing and a cross sectional allocation rule which are independent of investment opportunities or deposit shocks. The predicted available financing can then be used as an exogenous shifter of bank liquidity to estimate the effects of liquidity shocks on the lending decision of banks. Next I discuss how financing frictions can affect the response of bank lending to the availability of new sources of subsidized finance and how this can be used to develop a test for financing frictions.

2.3. Financing Constraints and Lending Behavior

2.1.3. Loan Sensitivity to Bank Liquidity

In a world without financing frictions, profit maximizing banks will be able to raise any amount of finance in the capital market at a constant cost, r_m, and lend until the marginal return on loans is equal to the marginal cost of finance. If banks face a declining schedule of marginal loan profitability, lending beyond this point yields a return lower than r_m. If a bank receives one dollar of subsidized financing (at a rate $r_s<r_m$), it will use it to repurchase a dollar debt and earn r_m-r_s. The alternative is to issue $1 in new loans, which would yield a return below r_m-r_s. Thus in a frictionless world, an extra dollar of available cheap financing will increase the infra-marginal profits of the bank, but will not affect either total loanable funds (total financing minus reserve requirements) or lending as long as banks hold some financing at the market rate.

In an alternative scenario with informational asymmetries and agency problems bank external financing is costly. Banks will be unable to raise unlimited amounts of financing at the market rate because issuing debt either could be a bad signal of the quality of banks assets or increases the incentives of self-interested managers to engage in opportunistic behavior (Stiglitz and Weiss 1981; Myers and Majluf 1984; Jensen 1986). These frictions imply that the marginal cost of external financing is not constant at r_m, but increasing in the amount of externally raised finance. Banks will lend until the marginal cost of finance is equal to the marginal return on loans, but now a $1 of subsidized financing will shift out the marginal cost of external finance. When banks face financing frictions, an increase in available cheap finance leads to an expansion total bank loanable funds and in lending.

This discussion suggests a simple test for financial constraints in the context of the program described in the last subsection. A positive relationship between bank loanable funds and the availability of program financing when banks hold liabilities priced at the market rate (e.g. uninsured CDs, subordinated debt or any bank liability other than deposits) can be taken as evidence of financing frictions. If this relationship exists, changes in the availability of program financing can be used as an exogenous source of variation of bank liquidity to estimate the magnitude of the sensitivity of lending to bank liquidity. The methodology parallels that of Banerjee and Duflo (2004), who use the expansion of a directed credit program in India to test whether non-financial firms are credit constrained.

To put these ideas in a conceptual framework, I consider an adaptation of Froot, Scharfstein and Stein (1993), Kaplan and Zingales (1997) and Stein (2003) reduced form two-period models to the case of financial firms. This framework is intended to convey the intuition behind the empirical strategy to test for liquidity constraints and not to explain optimal bank investment and financing decisions under asymmetric information. Banks choose the amount of lending, *L*, and external financing, *e*, to maximize expected profits:

$$\max_{L,e} \frac{1+\lambda}{1+r} f(L) - (1+r_s)s - [1+r_m+\theta C(e)]e \tag{2-1}$$

$$\text{s.t. } L = s + e$$

where $f(L)$ is the expected gross return on loans and s represents subsidized finance and r is the discount rate. Also, r_m and r_s are the market and the subsidized price of external finance respectively. The cost of external financing, $r_m+\theta C(e)$, is equal to the market rate when there are no financing frictions ($\theta=0$), and increasing in the amount external funds [C'>0, C''>0] otherwise ($\theta>0$). Expected return on loans, *f( )*, is an increasing and concave function of lending due to, for example, an increasing and convex profile in the probability of default of potential borrowers. Finally, λ represents the potential private benefits managers derive from investment.

The level of lending that maximizes expected profits when there are no financing frictions, L^*, equates the market cost of financing and the PDV of the marginal expected return on loans:

$$\frac{1+\lambda}{1+r} f'\left(L^*\right) = 1 + r_m \tag{2-2}$$

as long as the amount of subsidy does not exceed L^* ($s \leq L^*$). In a frictionless world, an increase in the amount of subsidized financing will lead to a one for one reduction in external financing ($de^*/ds=-1$), and bank total funding and lending will be unchanged. This result is depicted in Figure 4.

On the other hand, if θ>0, lending will be given by the first order condition of the bank's program (2-1):

$$\begin{aligned} &\frac{1+\lambda}{1+r} f'\left(\hat{L}\right) = 1 + r_m + \theta\, g\left(\hat{L}-s\right) \\ &\text{with } g\left(\hat{L}-s\right) = C\left(\hat{L}-s\right) + \left(\hat{L}-s\right)C'\left(\hat{L}-s\right) \end{aligned} \tag{2-3}$$

Since *g( )* is increasing and *f '( )* is decreasing, it follows that $d\hat{L}/ds > 0$. In words, total funding and lending are increasing in the amount of subsidized finance (see Figure 5). It is also easy to show that $d^2\hat{L}/dsd\theta > 0$, which implies that the sensitivity of lending to subsidized finance is increasing in the magnitude of the financing frictions. This result will be useful later to check whether the loan-liquidity sensitivity changes along observable proxies of financing constraints.

2.2.3. *Lending Profitability and Default Risk*

Liquidity constraints reduce the ability of banks to make loans. However, the fact that banks are constrained in the amount the can lend is not sufficient to conclude that banks are lending below the optimal level. Empire-building bank managers may have a tendency to 'over-lend' [$\lambda>0$ in (2-2)] and liquidity constraints may arise optimally to limit this behavior (Stulz 1990; Hart and Moore 1995).

If banks are inefficiently constrained, the marginal loan of the bank must yield positive expected profits. The empirical setting provided by the government program allows testing this implication under certain assumptions. The liquidity shock provides a shift in the marginal cost of financing that allows tracing the characteristics of the marginal loan of the bank. In the context of the previous analytical framework, the shift in the marginal cost curve in Figure 5 traces down the slope of the expected marginal return on lending.

Looking at actual loan profitability requires loan level data on interest rates and monitoring/screening costs which is unavailable. I will look instead at default risk, which will be inversely related to loan profitability as long as the interest rate on the marginal loan does not change substantially when lending expands. This assumption is reasonable if banks choose to ration borrowers instead of raising the interest rate to clear the market as in Stiglitz and Weiss (1981). The optimal interest rate is below the market clearing level because increasing the interest rate makes profile of borrowers riskier, both because it attracts riskier applicants and because it induces borrowers to take more risk. If borrowers are credit rationed, constrained banks need not reduce the interest rate in order to expand credit when facing a positive liquidity shock. Furthermore, constrained banks can use observable signals of borrower quality or collateral requirements to ration credit without facing a risk-return trade-off.[14] In the empirical section I will show evidence that is consistent with banks rationing credit using observable borrower characteristics.

The previous discussion can be expressed in terms of the analytical framework of the subsection 2.1.3 by writing the expected return on lending, $f(L)$ solely as a function of the optimal constant gross return on loans, R^*, and the resulting average probability of default of loans, p^*, as:

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[14] For example, banks can lend only to borrowers that are able (and willing) to pledge high collateral. A higher collateral (per dollar of lending) increases the payment to the bank in case of default and reduces the probability of default. Pledging collateral may serve as a signal of good borrower type, since it is costlier to pledge for bad entrepreneurs who fail more often. Also, collateral decreases the payment to the borrower in case of default, which reduces the incentives to misbehave.

$$f(L) = R^*\left[1 - p^*(L)\right] \tag{2-4}$$

where the probability of default is increasing and convex in the amount of lending.[15] If banks are severely constrained, such that they are operating in the flat portion of the marginal expected return curve in Figure 4, the default risk will not change when lending expands. A finding that the default risk of lending does not change when lending expands would indicate that not all borrowers in the same risk class are obtaining credit and implies banks can expand credit profitably when the liquidity constraints are relaxed.

3. Data Sources

Before heading into the empirical specifications and results I first describe the sources of data used in this paper. First, I use detailed information on balance sheets and monthly earnings reports for all the banks in the Argentine financial system between 1995 and 2001 from the Central Bank of Argentina. As I argued in the description of the program, the preferred estimates will be based on the 1998-2000 sub-sample, when the final waves of the program took place.

The second source of data is the Public Credit Registry database, or CDSF for its acronym in Spanish (Central de Deudores del Sistema Financiero). Each observation in this database represents a loan i held by firm j with bank k at month t. It containts monthly data on all loans held by firms or individuals with more than $50 of debt with a financial institution in Argentina. The CDSF is available for all borrowers after January 1998.[16] For every loan, the data available are: the name of the debtor, the name of the bank, the principal withstanding, the amount of collateral posted and a code describing the debt situation. This code ranges from 1 to 6, where 1 represents a good standing loan and 5 and6 represent unrecoverable

[15] As suggested by the discussion on Stiglitz and Weiss, the probability of default will in general depend on the gross return R. The asterisks emphasize that the amount of lending is chosen given an optimal interest rate. The underlying assumption is that the effect of changing the amount of lending on the optimal interest rate is of second order.

[16] The collection of this data started in early 1996. However, the accounts of what information was available before 1998 and when are contradictory. See for example Escudé et. al. (2001) and Fakenheim, M. and A. Powell (2003). However, all research conducted by the BCRA and others using the CDSF only includes post 1998 data.

loans. The categories are precisely defined in terms of the days behind in payment, debt refinancing and bankruptcy filings (Gutiérrez-Girault 2002).[17]

This data allows building the credit history of every borrower in the financial system. I construct measures of loan performance of loans issued at month t by looking at the debt situation code at month t+12 and t+24. I also construct two measures of ex-ante credit quality: the collateral to loan ratio and a dummy equal to one if the loan recipient has some non-performing debt at the time of receiving a loan. And finally, this dataset allows measuring these variables for different types of borrowers. I look in particular at loan recipients with a previous relationship with the issuing bank and those without one ('existing' and 'new'). Among the new borrowers, those that have a previous credit history with other financial institutions are also considered ('switching' borrowers). The loan level descriptive statistics by type of borrower are shown in Table 7.

It is important to note a feature of the CDSF related to the information reported about credit lines (including credit cards) and credit commitments. Since the bureau was created for regulatory reasons to measure bank asset risk, credit limits and not actual amounts of credit outstanding are reported. That is, if a firm opens a credit line for up to $100,000 with a bank, then the CDSF will show a loan of $100,000 for every month the line is available regardless of the actual amount borrowed. This feature is actually an advantage in our application since the outcome of interest is the availability of credit.

A third source of data is the program database, collected and managed by the Ministry of Economy in Argentina. This database has detailed characteristics about firms that received loans from the program, such as characteristics of the loan (date of initiation, principal, duration, grace period, amount of each payment, grace period, interest rate), characteristics of the firm (number of workers, annual sales), and name of the intermediary bank that made

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[17] Situation 1 (normal): all payments on time. Situation 2 (with potential risk): small and occasional delays in repayment. Situation 3 (with problems): delays in repayment between 90 and 180 days. Repays accrued interest but requires principal refinancing. Situation 4 (high insolvency risk): repayment delays between 180 and 360 days, bankruptcy filings for more than 5% of the firm's equity, has principal and interest refinancing requiring principal condoning, the bank received payments in kind. Situation 5 (unrecoverable): bankruptcy declared. Situation 6 (unrecoverable by technical disposition): late repayments of more than 180 days with intervened financial institutions.

the loan.[18] The program database and the CDSF could be linked using a unique tax identification code (CUIT).

4. Measuring the Lending-Liquidity Sensitivity

4.1. Empirical Specification and Previous Research

The usual specification used in the lending channel literature looks at the relationship between loan growth and a measure of changes in the bank liquidity, typically given by changes in monetary policy (Bernanke and Gertler 1995; Hubbard 1995; Kashyap and Stein 2000; Kishan and Opiela 2000), deposit growth (Jayaratne and Morgan 2000; Ashcraft 2003), internal cash (Ostergaard 2001) or stock price (Peek and Rosengren 1997):

$$L_{it} = \alpha_i + \alpha_t + \beta D_{it} + \gamma X_{it} + \varepsilon_{it} \tag{4-1}$$

where L_{it} is loan growth of bank i at month t, D_{it} represents a measure of the liquidity shifter, α_i and α_t are bank and month fixed effects, X_{it} is a set of controls and ε_{it} is the error term. The main caveat in this literature is that the sources of variation in bank liquidity are likely to be correlated with investment opportunities in loans, which would lead to a biased estimation of β in (4-1). For example, an increase in deposits, internal cash or stock prices may signal better future lending prospects of the bank and will be correlated with loans even in the absence of financing constraints.

This problem has been approached in several ways. First, by introducing a measure of investment opportunities among the controls (e.g. Tobin's q, level of economic activity). Second, by looking at the differences in the lending-liquidity sensitivity across banks that are more likely to face financing constraints according to observable characteristics (e.g. smaller, less capitalized banks). Both of these approaches are also used in the early literature on the investment-cash flow sensitivity and have been criticized from empirical and theoretical grounds. Poterba (1988) and Erickson and Whited (2000) suggest the observed correlation between investment and cash flow can be entirely driven by measurement errors in q. Furthermore, the cross sectional variations in the investment-cash flow sensitivity appear in the data even for firms that are not likely to be financially constrained (Kaplan and Zingales 1997; 2000), and can be induced by models without financing frictions (Alti 2003). The same

[18] The firm program loan descriptive statistics shown in Table 3 are calculated using the program database.

caveats are likely to apply when looking at the sensitivity of bank investment (lending) to liquidity, as long as bank deposits, cash flow or stock price are correlated with bank investment opportunities. The third and most convincing approach is to look at the lending-liquidity sensitivity in a 'natural experiment' setting, in which the shock to the financial position of the bank is independent of its investment opportunities (Stein 2003).

I use a natural experiment approach in this paper by exploiting the expansion of available financing provided by the government program as the source of variation in bank liquidity, as was described in section 2.1.3. The relationship between loan growth and liquidity is estimated as in (4-1), with liquidity measured as the growth of the bank loanable funds, F:

$$L_{it} = \alpha_i + \alpha_t + \beta F_{it} + \varepsilon_{it} \tag{4-2}$$

Loanable funds are the sum of equity, deposits and other liabilities, minus reserve requirements. Growth refers to the proportional growth rate, calculated as the change in the log of the variable.[19] Changes in the *predicted availability* of program finance, $\tilde{E}$, are used as an instrument for F and estimate β in (4-2) by 2SLS. Section 4.2 discusses in detail how the availability of program finance is predicted using the predicted probability of participation and argues that conditional on bank and month fixed effects, predicted program financing can be regarded as exogenous. The estimated β_{2SLS} can be interpreted as the elasticity of lending to changes in liquidity. The first stage of this estimation represents the effect of the expansion in the predicted available financing on bank liquidity:

$$F_{it} = \alpha_i + \alpha_t + \varphi \tilde{E}_{it} + \eta_{it} \tag{4-3}$$

The discussion in section 2.1.3 suggests that a positive estimation of the sensitivity of liquidity to available finance, φ, will occur only if banks face financing constraints.

The sensitivity obtained through 2SLS can be compared with the estimate that results when using deposits as an instrument for liquidity. Which estimate will be higher is a priori ambiguous. On the one hand, as previously argued, changes in deposits are likely to be caused by factors that also affect the demand for bank credit. This would lead to upward biased estimations of the sensitivity of lending to liquidity. On the other hand, banks are likely to endogenously choose to lend out a lower proportion of a liquidity expansion when

[19] That is, $\ln(X_t)-\ln(X_{t-1}) \approx (X_t - X_{t-1})/ X_{t-1}$ when $X_t - X_{t-1}$ is small.

it is less liquid. Since the program provided medium term financing (3 years on average) which is less liquid than deposits, it is reasonable to expect a higher sensitivity of lending to liquidity shocks induced by the program.

I will also verify whether the loan-liquidity sensitivity varies across observable measures of financing frictions by estimating the following specifications:

$$L_{it} = \alpha_i + \alpha_t + \beta_1 F_{it} + \beta_2 F_{it} \times DumSmall_i + \varepsilon_{it} \quad (4\text{-}4)$$

$$L_{it} = \alpha_i + \alpha_t + \beta_1 F_{it} + \beta_2 F_{it} \times DumLowCap_i + \varepsilon_{it} \quad (4\text{-}5)$$

Here the liquidity measure is interacted with a dummy equal to one if the bank is in the lowest 20% of the assets distribution (4-4), and equal to one when in the lowest 20% of the equity to capital ratio (4-5). The instruments in each of these specifications are the expansion in available finance as before, and also the interaction between the available finance and the DumSmall and DumLowCap dummies respectively. The coefficients on the interaction term, β_2, will be positive if small and less capitalized banks indeed have a higher lending-liquidity sensitivity than other banks.

4.2. Dealing with Endogenous Program Financing

The key assumption in the previous empirical strategy is that program financing affects bank liquidity in a way uncorrelated with investment opportunities, deposit shocks or other factors that affect either bank liquidity or the decision to lend. As the description of the program in Section 2.2 suggested, *actual* program participation and available financing were likely to be correlated with these factors since banks facing a greater need for liquidity were more likely to apply for program financing. This section describes how the potential endogeneity in participation and financing are dealt with using the time series variation of wave size and the cross sectional allocation rule of the program.

4.1.2. Predicted Probability of Participation

Bank executives commented that when the amount of resources in a program wave was small, the potential funding from the program was too low to justify the administrative costs of participating. Potential funding available to a bank in each wave was driven by two main factors: in the one hand, total available financing, decided in every wave by the IDB; in the other hand, the share of the wave resources that a given bank would receive if it were to participate, in turn determined by the number of participants in the wave as well as by predetermined characteristics of the banks. Bank participation can then be predicted based solely with variables that are uncorrelated with the lending decision after controlling for bank and month fixed effects.

First, the participation choice is modeled by assuming bank *i* participates in program wave *w* only if the potential financing that can be obtained from participation exceeds a bank and wave specific parameter η_{iw}.[20] Potential financing, h(.), is a function of wave size, A_w, and the point score bank i obtains according to its average loan size ($Zsize_{iw}$) and regional loan distribution ($Zregion_{iw}$). Potential financing bank i expects to receive from the program in a certain wave will be a non-linear function of these parameters for two reasons. First, the actual administrative function, discussed in detail in the next subsection, is also non-linear in wave size and bank scores. And second, because potential financing also depends on the expected number and characteristics of other participating banks.[21] I assume an arbitrary and flexible functional form for potential financing. In particular, I choose a second-degree polynomial on wave size and the point scores: [22]

$$h(A_w, Zsize_{iw}, Zregion_{iw}) = \sum_{s=0}^{2}\sum_{u=0}^{2}\sum_{v=0}^{2} \xi_{s,u,v} A_w^s Zsize_{iw}^u Zregion_{iw}^v \qquad (4\text{-}6)$$

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[20] This is a version of the standard linear latent index models commonly used in econometric program evaluations [for an early example, see Heckman and Hotz (1989)].

[21] The participation decision of banks can be formally modeled as a private value auction. Suppose N banks are deciding whether to participate in a program wave of size A. For simplicity assume all banks are equal except for their cost of participation, η, which is private information. Thus, the amount of resources in a wave is evenly distributed across the banks that decide to participate. The common knowledge p.d.f. of the cost among potential participants is $f(\eta)$. Optimal participation will be given by a cutoff rule: bank i participates if $\eta_i < \eta^*$. So the probability bank i participates is given by:

$$p = \int_0^{\eta^*} f(\eta)\,d\eta$$

The expected program financing net of participation cost that a bank will receive if it decides to join the program is:

$$A\sum_{j=0}^{N-1}\frac{1}{1+j}p^j(1-p)^{N-1-j}\binom{N-1}{j} - \eta_i$$

Finally, the cutoff η^* is defined implicitly by the value of η that equates the net expected program financing to zero.

[22] $h(A_w, Zsize_{iw}, Zregion_{iw}) = \xi_0 + \xi_1 A_w + \xi_2 Zsize_{iw} + \xi_3 Zregion_{iw} + \xi_4 A_w Zsize_{iw} + \xi_5 A_w Zregion_{iw} + \xi_6 Zsize_{iw} Zregion_{iw} + \xi_7 A_w^2 Zsize_{iw} + \xi_8 A_w^2 Zregion_{iw} + \xi_9 A_w Zsize_{iw}^2 + \xi_{10} A_w Zregion_{iw}^2 + \xi_{11} Zsize_{iw}^2 Zregion_{iw} + \xi_{12} Zsize_{iw} Zregion_{iw}^2 + \xi_{13} A_w Zsize_{iw} Zregion_{iw} + \xi_{14} A_w^2 Zsize_{iw} Zregion_{iw} + \xi_{15} A_w Zsize_{iw}^2 Zregion_{iw} + \xi_{16} A_w Zsize_{iw} Zregion_{iw}^2 + \xi_{17} A_w^2 Zsize_{iw}^2 Zregion_{iw} + \xi_{18} A_w^2 Zsize_{iw} Zregion_{iw}^2 + \xi_{19} A_w Zsize_{iw}^2 Zregion_{iw}^2 + \xi_{20} A_w^2 Zsize_{iw}^2 Zregion_{iw}^2$

Assuming η_{iw} is normally distributed, the probability that bank i participates in wave w, p_{iw}, is given by:

$$p_{iw} = Pr(\, h(A_w,\, Zsize_{iw},\, Zregion_{iw}) > \eta_{iw}) = \Phi(\, h(A_w,\, Zsize_{iw},\, Zregion_{iw})\,) \tag{4-7}$$

where Φ is the normal cumulative distribution function. The parameters of this participation model can be estimated using maximum likelihood (probit) and used to obtain a predicted probability of participation, $\hat{p}_{iw}$. It is possible that banks tried to game the resource allocation formula by manipulating the loan size and distribution to increase their share of program resources. To avoid introducing this source of bias in the estimation of the probability of participation, the region and size point scores do not vary by wave. Instead I use the scores corresponding to the first time the banks are observed in the sample. This is, the following probit specification is estimated:

$$p_{iw} = \Phi\,(\, h(A_w,\, Zsize_i,\, Zregion_i)\,) \tag{4-8}$$

where the variation in participation across waves and across banks are given by the interaction between wave size and initial bank characteristics. To see how close predicted participation fits actual participation, Figure 6 plots the actual and the predicted number of bank participations by year.[23] The plot shows that the predicted participation series tracks the actual one quite well.

4.2.2. *Predicted Availability of Program Financing*

When a bank participates in a wave, the amount of program finance it will receive depends on the amount of resources in the wave and the number and characteristics of all the participating banks in that wave. The administrative allocation formula stipulated that each bank would receive a fraction of the resources available in the wave that was proportional to the ratio of their score points relative to the sum of the scores of all participating banks, or:

$$E_{iw} = A_w \left[\frac{Zregion_{iw}}{2 \cdot \sum_{j}^{n_w} Zregion_{jw}} + \frac{Zsize_{iw}}{2 \cdot \sum_{j}^{n_w} Zsize_{jw}} \right] \tag{4-9}$$

[23] The predicted number of participants in a wave is just the sum of the predicted probabilities of participation across all banks. If the same bank participates in two waves during a year it counts as two participations for the graph.

E_{iw} is the *actual* amount of financing bank i receives from the program when it participates in wave w, and is a function of wave size, A_w, bank i's point scores, $Zsize_{iw}$ and $Zregion_{iw}$, the number of participants in the wave, n_w, and the sum of all participants' point scores.

I use the predicted probability of participation from the previous subsection to estimate the expected sum of the characteristics of program participants. This is done by summing the bank characteristics of all banks (participating and non-participating) weighted by the predicted probability of participation ($\hat{p}_{iw}$). Using this expected sum in (4-9) I predict the amount of program financing bank i would have received if it had participated in wave w:

$$\tilde{E}_{iw} = A_w \left[\frac{Zregion_i}{2 \cdot \sum_j^N \hat{p}_{jw} Zregion_j} + \frac{Zsize_i}{2 \cdot \sum_j^N \hat{p}_{jw} Zsize_j} \right] \qquad (4\text{-}10)$$

where the region and size point scores of each bank are taken when first observed in the sample to avoid the rule gaming bias discussed before. Equation (4-10) gives us an approximation of the expected increase in the availability of program financing of bank i at wave w. To calculate the changes in available financing by month ($\tilde{E}_{it}$) I assume, first, that banks drew the available finance in three equal parts during the months following the date a wave begins; and second, that the program financing was repaid in 36 equal monthly parts after being received. The first assumption follows since banks had three months to draw the resources from the credit line in the Central Bank without penalty. The second assumption attaches to program financing the same repayment schedule of the median firm loan as described in Table 3.

In the last two lines of Table 2 show the descriptive statistics of the resulting available financing variable (in levels and as a proportion of loans outstanding) by bank participation status. Available financing represents about 7.6% of loans during the sample period. Changes in this predicted available finance are used as an instrument for changes in bank liquidity in specification (4-2).

The use of a rule based non-linear allocation as an instrument for the actual one is applied in another context by Angrist and Lavy (1999) when evaluating the effects of class size on test scores. In that case the endogenous variable to be instrumented, class size, varies with enrollment in a non-linear fashion due to a maximum class size rule. This allows instrumenting class size while still controlling for enrolment and unobservable school characteristics. In this paper, the amount of program financing and the probability of participation in the program vary non-linearly with wave size and predetermined bank characteristics. As previously argued, wave size varies for exogenous reasons in the final waves of the program and all time invariant bank characteristics will be controlled for using bank fixed effects specifications.

4.3.2. *Identification Checks*

First, I check whether the predicted available financing variable is correlated with the actual program financing received by the banks. Figure 7 shows that the expected predicted stock of available finance tracks the actual stock quite well in the time series. [24] A regression version of this comparison, which also accounts for the cross sectional variations in financing and includes bank and month fixed effects, implies estimating the following regression of actual financing on predicted financing:

$$E_{it} = \alpha_i + \alpha_t + \varphi_1 \widetilde{E}_{it} + \eta'_{it} \tag{4-11}$$

The estimated φ_1 is close to one and statistically significant, which implies that the administrative formula was strictly applied.

Next I revisit one of the key identification assumptions mentioned at the beginning of this section: that the predicted financing expansion should not be correlated with other shocks to liquidity or to investment opportunities. To check this I estimate a regression of actual and predicted program financing on lagged deposit growth and lagged bank cash flow. The estimated parameters are shown in Table 4. Column 1 shows that actual financing was in fact negatively and significantly correlated with lagged shocks to deposits (2 and 4 lags). This was expected if banks decided to participate in the program when they received a negative shock to deposits. However, columns 2 and 3 show that predicted financing is not. There is also no correlation between past cash flows and predicted available financing growth. These results corroborate that the predicted financing variable rids of the potential endogenous correlation that might be present in the actual financing variable.

[24] The expected predicted stock is the predicted stock times the probability of participation summed across all banks.

4.3. First Stage: the Effect of a Credit Expansion on Loanable Funds

The discussion in Section 2.3 about the effects of financing frictions on lending behavior led to the conclusion that a change in the availability of cheap financing will affect bank liquidity only when banks are constrained. The relationship between available financing and liquidity is embodied in the first stage regression (4-3). A positive relationship between the available finance growth and liquidity (loanable fund growth), φ, would be consistent with financing constraints.

Table 5 shows the estimated parameters of the first stage. There is a positive and significant relationship between credit expansion growth and bank liquidity in the entire sample of banks (Column 1). As an additional specification check, the first stage regression is estimated again dividing the sample between banks with a high average probability of participation in the program and banks with a low probability of participation, which is a predetermined bank characteristic since it is based on pre-program data only. Banks are defined as having a high average probability of participation if they are in the top quartile of the probability distribution estimated using (4-8). The results are shown in columns 2 and 3 of Table 5. As expected, predicted available finance has a positive and significant effect on the liquidity of banks with a high probability of participation in the program, and an insignificant effect on low-probability banks.

This discussion suggests a graphical version of the first stage estimates. The loanable funds of banks with a high average probability of participation in the program should increase relative to the loanable funds of banks with a low average probability of participation, when the available financing increases. To check this is the case, the top panel of Figure 8 plots the predicted and the actual available program finance to banks in the top quartile of the distribution of the predicted probability estimated from (4-8). The bottom panel shows the ratio between the loanable funds of high-probability and low-probability of participation banks. The time series of both graphs follows a similar pattern, which indicates that the liquidity of banks that were likely to participate in the program increased when available program financing expanded.

The results suggest that banks increased their holdings of loanable funds as a result of the expansion of available cheap financing. This is opposite to the predicted response for an unconstrained profit maximizing bank in section 2.3. Such a bank would have reduced holdings of other more expensive liabilities and kept liquidity unchanged. The fact that the predicted financing expansion is driven only by exogenous sources of variation, assures that the expansion in liquidity is not driven by other confounding factors that affect either liquidity directly or the demand for loans. Thus, the evidence supports the hypothesis that banks are liquidity constrained. The next step is to explore the relationship between bank liquidity and lending of constrained banks.

4.4. 2SLS Estimation: The Sensitivity of Lending to Bank Liquidity

This section uses specification (4-2) to obtain the 2SLS estimate of the sensitivity of lending to bank liquidity, β, using the predicted expansion of available financing as a liquidity shifter. All the results that follow are estimated using the entire sample of banks. Table 6 shows the OLS and the 2SLS estimation results of β. The preferred estimate of the lending-liquidity sensitivity is 0.745 (column 3), obtained from restricting the sample to the final waves of the program. Considering that the average loans in the sample are \$536 million and average loanable funds \$616 million, the estimated elasticity implies that loans increase by \$0.66 for every dollar of liquidity expansion. Figure 9 shows this result graphically: the ratio of loans by high-probability to low-probability (average) of participation banks also increases when available program financing is expanding (as shown in Figure 8 for loanable funds).

The estimate of the sensitivity of lending to bank liquidity is lower (0.481) when all the waves of the program are used in the sample. Recall that the initial waves of the program coincided with massive deposit drains from the banking system. A negative bias in the loan-liquidity estimate during this period would result if the fall in deposits of program banks was relatively larger than for the rest of the banks. The rest of the results in the paper will be estimated using the restricted sample.

As an additional specification check, the estimation is repeated including in the sample only those banks that participated at least once in the program, but excluding all banks that participated in every wave. If the identification strategy is valid this estimate should be the same as using the entire sample of banks. The estimated β for an unreported estimation using the restricted sample of banks is 0.66 with a standard error of 0.29, which is statistically indistinguishable from the estimate using the entire sample. This result suggests that the use of predicted finance as an instrument for changes in bank liquidity deals successfully with the endogeneity of bank selection into the program.

In order to compare the results with the previous literature I estimate the sensitivity of lending to liquidity across banks of different size and capitalization using specifications (4-4) and (4-5). The coefficient of interest in this specification is the interaction term, β_2, which will be positive if banks in the lowest quintile of the asset or capitalization distributions have a higher sensitivity of lending to changes in bank liquidity. Both point estimates are positive, but neither of them is statistically significant (Columns 5 and 6 of Table 6). These results hint at the potential bias that may result when identifying financing constraints relying on cross sectional variations in the sensitivity of lending to liquidity. The magnitude of the cross sectional variation may be very small in proportion to the actual level of this sensitivity, and might lead to underestimate the importance of the effect of financing frictions on bank lending.

Summarizing the finding of this section, an increase in available financing produces an increase in bank liquidity that is consistent with the existence of financing frictions. The sensitivity of lending to changes in liquidity that results from these frictions can have a substantial magnitude and has potentially been underestimated by previous research. I now turn to analyze the effects of financing constraints on bank lending behavior, and in particular on lending risk.

5. Liquidity and Lending Risk

5.1. Specifications with Loan Level Data

The following version of (4-2) is used to estimate the change in bank loan default risk and average borrower characteristics due to a liquidity expansion:

$$Y_{ijt} = \alpha_i + \alpha_t + \alpha_s + \varphi DumExp_{it} + \omega_{it} \qquad (5\text{-}1)$$

Every observation represents a loan j given by bank i at month t. The left hand side variable is a measure of loan default or borrower characteristics. Loan default is measured as a dummy equal to one when a loan issued at time t has defaulted by time t+12. I also look at defaults at t+24 to check for potential changes in the timing of defaults. As mentioned previously, I use loan collateralization and loan recipient past performance as measures of observable borrower quality. Collateralization is the ratio of collateral to the amount of loan j. Past performance is measured as a dummy equal to one if the recipient of loan j has any non-performing debt between t-1 and t-12.

The variable of interest on the right hand side is $DumExp_{it}$, a dummy equal to one when bank i receives program financing at month t. Financing expansion months are defined as the three months following the date a program wave begins. I instrument this variable with the predicted probability of program participation, described in section 4.2. The estimated φ can be interpreted as the change in the average of the dependent variable (averaged over bank-month cells) that results from a liquidity expansion. For example, assume (5-1) is estimated using the default at t+12 dummy as the dependent variable and we obtain φ=0.05. This result indicates the fraction of loans issued by a bank that defaults after 12 months increases by 5 percentage points when the bank receives a liquidity expansion. The rest of the right hand side variables are α_i and α_t, bank and month dummies as in (4-2), and α_s, an industry dummy. The industry dummy allows controlling for potential changes in the industry composition of the loan portfolio of the banks. Finally, ω is the error term.

The descriptive statistics of the loans issued during the sample period are shown in Table 7. Of the 750,526 loans in the sample, 130,201 were issued to *new* borrowers, or borrowers without a previous relationship with the bank. On average, 12.2% of the value of the loan was covered by some type of collateral, 12.2% of the loans is non-performing after 12 months and 16.8% is non performing after 24 months. Loans to new borrowers are less collateralized and are more likely to default than loans to existing borrowers (borrowers with a pre-existing relationship with the bank). Existing borrowers have on average \$58,550 of debt outstanding when received the loan and 14.1% of loan recipients hold some non-performing debt at the moment of receiving the loan.

5.2. Default Rate Results

The effect of the liquidity expansion on loan default risk can be characterized by estimating specification (5-1) using the default dummies as the dependent variable and the probability of participation as an instrument for the liquidity expansión dummy. The estimates of the first stage, the regression of the liquidity expansion dummy on the probability of participation, are shown in the bottom panel of Table 5. The 2SLS results for the 12 month and the 24 month default dummy and for various samples are shown in Table 8. The coefficient of interest can be interpreted as the change in the proportion of loans that default due to a liquidity expansion. The point estimates for the entire sample of loans are negative but insignificant for both the 12 and the 24 month measures (columns 1 and 2 of panel 1). This implies that the liquidity expansion do not change in a statistically significant way the default risk of bank loans, which suggests banks are lending at a point where the schedule of loan risk is constant on average.

To check whether the 12 to 24 month window is appropriate to measure changes in default i estimate the hazard rate function of default. The kernel estimation of the monthly hazard rate, probability a loan is defaulted t months after it is issued given that it has not defaulted at month t-1, is plotted in Figure 10. The kernel is estimated for the sample of loans issued between January and August 1998, for which I can observe at least three years and up to four years of repayment history. The plot shows that the hazard rate is initially increasing and peaks before 12 months and then decreases monotonically. Looking at the cumulative hazard, 45% of the loans that default in the sample period will have defaulted by month 12, and 85% will have done so by month 24. Looking at default in the 12 and 24 windows is likely to capture most of the defaults.

As suggested in the discussion in 2.2.3, the fact that banks can expand lending without facing an increase in the default rate of loans is consistent with banks being inefficiently constrained. In terms of that discussion, given the plausible assumption that the interest rate on the marginal loan does not change substantially when lending expands, banks are operating at a point where the expected return of the marginal loan is flat, which is consistent with lending below the optimal level. From the borrowers' perspective, these results also suggest that financing constraints at the bank level may result in credit rationing of viable projects, a topic I explore further in the next subsections.

5.3. Liquidity Constraints and the Composition of Lending

Before going ahead, I show evidence corroborating the claim made in Section 2.2 that the program targeting rule did not affect the investment decision of the banks. To do this I estimate specification (5-1) using a dummy equal to one if the loan is issued to an eligible borrower (less than $200,000 in sales and 20 workers).[25] If the targeting rule was binding, the proportion of lending to eligible firms should increase when available program financing increases ($\beta>0$). The estimates in columns 1 of Table 9 show this is not the case. The 2SLS estimate of β is not significantly different from zero.

The fact that financial constraints hinder the ability of banks to channel resources to potentially profitable projects brings up the question of which margins of lending are affected. In particular, constraints may affect the extensive margin of lending (access to bank financing) or the intensive one (amount of lending once access has been gained). I investigate this issue by looking at how the allocation of the flow of loans between borrowers with and without a previous debt with the bank ('existing' and 'new' borrowers) changes during liquidity expansion periods. I estimate specification (4-2) using the fraction of the amount of lending to new borrowers as the dependent variable and specification (5-1) using a dummy equal to one if the loan is issued to a new borrower. The results (shown in columns 1 and 2 of Table 9 respectively) indicate that neither the composition of the number of loans or the amount of lending change significantly during liquidity expansions. This implies that the marginal loan is allocated across new and existing borrowers as the infra marginal one is: according to Table 7, 87% of the lending expansion goes to know borrowers. When a bank receives a liquidity shock, it directs a large fraction of resources to increase the debt of firms that are already borrowing from the bank. This evidence is consistent with bank borrowers being credit constrained.

The results indicate that liquidity constraints affect both the extensive and the intensive margins of lending and that the loan allocation choice is not altered in the margin by the liquidity shocks. This cannot be extrapolated to imply that the portfolio composition of the bank will be unaffected by any shock, but it means that the source of shocks considered in this paper is small enough not to change the composition of the marginal investment of the bank. This is convenient for the purposes of this paper because the results can be interpreted

[25] Loan recipient sales and workers are imputed using data from a sample of manufacturing firms collected by Unión Industrial Argentina. This database was used instead of the program database for imputation in order to avoid potential misreporting biases. The worker and sales data in the program database were self-reported by program banks and were not subject to verification by the program administrators. See a further discussion in Paravisini (2003).

as if the risk profile of the banks is not changing and allows focusing on the characteristics of the marginal loan, to which I turn next.

5.4. Marginal Loan Default Risk and Relationship Lending

Even if liquidity constrained banks are prevented from taking all profitable loan opportunities, unconstrained banks may step up and fill in the gap. The previous results suggest that there are frictions preventing borrowers from obtaining finance from other lenders when the bank they borrow from is constrained. These frictions may arise if banks obtain private information from their borrowers and have an advantage over uninformed banks (Sharpe 1990; Rajan 1992; Von Thadden 2001). In this case only the worse borrowers will substitute lenders and uninformed banks will face a 'winners curse'.

This subsection presents evidence consistent with banks holding private information about their borrowers and some of its consequences. I will show that the result obtained in section 5.2, that banks can expand lending without facing an increase in the default rate, is mostly driven by the fact that the default risk profile of the marginal loan to existing borrowers is flat. On the contrary, expanding lending to new borrowers involves an increase in default risk, in particular when lending to new borrowers that are switching from another bank.

The second panel of Table 8 shows the estimated parameters of specification (5-1) in subsection 5.2 but for the sample of existing borrowers. The results for this sub-sample follow are similar to those of the entire sample: the default rate on loans to existing borrowers remains unchanged during lending expansions. On the contrary, the results for the sample of new borrowers, shown in panel 3 of Table 8, indicate that the 12-month default rate for new borrowers increases by 3.8 percentage points when lending expands due to the liquidity shock. The 24-month default rate is also positive but statistically insignificant. Expanding lending to new borrowers does imply an important increase in loan risk. And the time profile of the default rate suggests that loans to new borrowers given during liquidity expansion also tend to default earlier.

The finding that the default rate of loans to new borrowers increases during lending expansions does not necessarily imply that expanding lending to new borrowers is inefficient. If banks have poor information about the quality of new borrowers but can obtain signals of this quality through a lending relationship, then lending to new borrowers can be interpreted as an investment in information. As in Petersen and Rajan (1995), banks will be willing to face a higher default rate in the short run in exchange of higher returns in the future.

Note also that the credit history of all borrowers is public information in Argentina. The entire history of past performance of all borrowers is available to all lenders through the Public Credit Registry in the Central bank. Private information of borrower quality revealed through the lending relationship is less likely to be important if observable past repayment performance is a good enough signal of quality. I examine whether this is the case by repeating the estimation of the change in the default rate during liquidity expansions for new borrowers, but restricting the sample to borrowers that have previous history with another bank. Expanding lending to these 'switching' borrowers should be less risky than lending to new borrowers if public Credit Registry information is a good signal for borrower quality.

The estimate for the sub-sample of switching borrowers is shown in panel 4 of Table 8. First note that the fraction of lending to switching borrowers is very low: 4% of the loans issued to new borrowers and 0.7% of total loans. Second, the estimated change in the default rate during liquidity expansions suggest that the default risk of the marginal loan to a switching borrower is four times that of the marginal loan to new borrowers. Both the reluctance of banks to lend to switching borrowers and the high default risk involved in doing so are consistent with the winners curse story. These results suggest that the private information banks have about their borrowers is still important in this environment of full credit history disclosure. This raises the question of whether the public signals of borrower quality available through the Credit Registry are relevant in the decision of banks to lend, and moreover, whether they are related at all with the probability of default of a firm. I address these issues next.

5.5. Observable Borrower Quality and Rationing

The interpretation of the results so far has been based on the presumption than banks ration borrowers according to default risk. However, borrower risk is unobservable and rationing must be based on observable borrower characteristics that are related to default risk. If borrowers are rationed according to observable borrower quality, then the average observable quality should drop during lending expansions. I test for this prediction by evaluating how two borrower characteristics available in the Public Credit Registry change during lending expansions. First, I look at the collateral to loan ratio of the loan. Higher collateral increases the contingency of the loan contract and should elicit a higher effort from the borrower, leading to a lower probability of default. Second, I use whether a loan recipient has any a non-performing debt outstanding at the moment of receiving the loan as a measure of past repayment performance. Better past performance is a positive signal of the ability of the entrepreneur and the quality of the project and should also predict a lower probability of default.

Specification (5-1) is estimated using the collateral to loan ratio as the dependent variable and the results are shown in column 1 of Table 10. The estimate using the entire sample of loans (panel 1) indicates that the collateral to loan ratio fell by one percentage point due to the liquidity expansion and this change is significant. The fact that the marginal borrower of the bank has a lower collateral than the average borrower is consistent with banks rationing borrowers according to collateral. The result supports the hypothesis that banks lend to lower observable quality borrowers when liquidity expands.

The estimates for the new and existing borrower samples (panels 2 and 3 respectively) suggest that the drop in collateral requirements comes entirely from lending to new borrowers. The collateral to loan ratio of loans to new borrowers drops by 3.4 percentage points during liquidity expansions. Thus, the result regarding collateral could potentially explain the observed patterns in the sensitivity of the default rate to liquidity. If loan collateralization is a good predictor of default and banks are able to expand lending to existing borrowers without relaxing collateral requirements, then it is to be expected that the default rate of existing borrowers does not react to liquidity expansions.

I turn next to past repayment performance and estimate specification (5-1) again using as the dependent variable a dummy equal to one if the loan recipient has some non-performing debt outstanding at the moment it receives the loan. The result in column 2 of Table 10, which uses the sub-sample of existing borrowers, shows that the fraction of loans issued to borrowers with non-performing debt increased by 4.7 percentage points during liquidity expansions. This result is again consistent with rationing according to observable characteristics and indicates banks relaxed their rationing criteria of existing borrowers as well.

Recalling the results from the previous subsection, the default rate of existing borrowers did not increase during lending expansions even though these borrowers are of a lower observable quality. This suggests banks are able to pick the best performing borrowers among a pool of existing borrowers of equivalent observable quality. This result supports the hypothesis that banks have private information about borrower quality and use it effectively to screen borrower risk.

An alternative interpretation of the previous results is that past performance is not a good predictor of borrower quality and only collateral matters. To test whether this is the case I estimate the following linear probability model of default using past performance and collateral as dependent variables:

$$DumDef_{ijt} = \alpha_i + \alpha_t + \alpha_s + \zeta_1 DumPast_{it} + \zeta_2 Collat_{it} + v_{it} \qquad (5\text{-}2)$$

As before, each observation represents a loan j issued by bank i at month t. The dependent variable is a dummy equal to one if the loan defaults within 12 (24) months. On the right hand side are the past default dummy and the collateral to loan amount ratio. Also a full set of bank, month and sector dummies are included. The estimated parameters are shown in Table 11. The results show a significant relationship between both collateral and past performance with the probability of default of a loan. A 10 percentage point decrease in the collateral to loan ratio can be associated with 0.5 percentage point increase in the probability of default in the entire sample, a 1 percentage point increase in the new borrower sample and a 0.35 percentage point increase in the existing borrower sample. Also, a loan recipient that holds non-performing debt is 54% more likely to default than one with a clean slate.

These results corroborate the initial interpretation of the results: bank-borrower interactions elicit information about borrower quality that is observable only by the lender and is used effectively to screen default risk.

6. Conclusions

This paper provides evidence that banks are liquidity constrained and that these constraints lead to a high sensitivity of lending to changes in the financial position of the bank. The results also indicate that liquidity constraints hinder bank investment in potentially profitable loans. The paper addresses the difficulty of distinguishing changes in bank liquidity from changes in loan investment opportunities encountered in the previous literature by exploiting the exogenous variation in available finance produced by a government program in Argentina. The results in this paper vindicate previous findings by showing that the empirical strategy based on the cross sectional comparison of banks is biased against finding a substantial sensitivity to liquidity.

Bank liquidity constraints will affect investment only if borrowers cannot easily substitute bank credit with other sources of financing. The results of this paper suggest that specialized lenders like banks can produce information that outperforms observable hard information as a signal of borrower creditworthiness. Banks are able to distinguish the best investment prospects among existing borrowers that are observationally equivalent to outsiders. The findings also suggest collateral becomes less important in determining access to credit in the margin when this private information is available. I also provide evidence consistent with adverse selection in the bank credit market. Banks are reluctant to issue credit to borrowers that are switching from another lender and the default risk of the marginal loan to these borrowers is steeply increasing.

The relevance of soft data collected by sophisticated lenders raises a concern regarding the availability of noisy public data on borrower quality. The results indicated that past mistakes by some borrowers may have too much weight when estimating probabilities of default based on raw credit history data. Unsophisticated lenders that rely on this data to assess credit risk may punish past mistakes too harshly and precipitate the foreclosure of perfectly viable borrowers. The recent literature on the effects of credit information disclosure has generally emphasized the advantages due to increased availability of credit (see Miller 2003 for a recent survey). The results of this paper suggest there is a potential downside of disclosure, a point that has been raised in theoretical work (Morris and Shin 2001), although not without controversy (Angeletos and Pavan 2004).

Finally, the results of this paper shed some light on the nature of bank liquidity constraint and the potential welfare gains in loosening them. On the one hand, liquidity constraints may arise optimally as an incentive device to mitigate the propensity of bank owners to engage in risk shifting, or to keep in check the propensity empire-building managers to over-invest (Jensen 1986; Hart and Moore 1990; Stulz 1990; Aghion and Bolton 1992). In this scenario, relaxing liquidity constraints is likely to result in an inefficient increase in the risk profile of the bank portfolio and in lending. On the other hand, liquidity constraints may lead to under-investment when they arise from adverse selection in the financing market (Stiglitz and Weiss 1981; Myers 1984; Myers and Majluf 1984; Stein 1998) or are a consequence of tight capital requirements (Besanko and Kanatas 1996; Thakor 1996). The evidence presented in this paper is consistent with the second view of bank liquidity constraints.

7. References

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8. Tables

Table 1

Allocation Formula Score Point According to the Average Size of Loan

Average size of loan		
From ($)	To ($)	Points
0	3000	100
3000	6000	97
6000	9000	94
9000	12000	91
12000	15000	88
15000	18000	85
18000	21000	82
21000	24000	79
24000	27000	76
27000	30000	73
30000	33000	70
33000	36000	67
36000	39000	64
39000	42000	61
42000	45000	58
45000	48000	55
48000	50000	52
50000	100000	30
100000	200000	20
200000	∞	10

Table 2

Bank Descriptive Statistics, by Program Participation (Thousands of $)

	All banks	Program banks	Non-program banks
Assets	1,095,287	543,985	1,244,598
	[2,396,431]	[599,719]	[2,667,256]
Loans	536,344	283,790	604,744
	[1,241,008]	[332,044]	[1,382,174]
Liabilities	979,350	488,200	1,112,370
	[2,168,293]	[539,852]	[2,414,047]
Deposits	569,590	361,719	625,888
	[1,331,549]	[407,961]	[1,483,052]
Loanable funds	616,099	382,853	680,614
	[1,348,276]	[418,375]	[1,503,195]
Loans/Assets	0.500	0.500	0.485
	[0.146]	[0.109]	[0.199]
Deposits/Assets	0.515	0.626	0.485
	[0.194]	[0.124]	[0.199]
Equity/Assets	0.133	0.133	0.133
	[0.135]	[0.130]	[0.137]
ROA	0.31%	0.14%	0.35%
	[1.22]	[1.12]	[1.24]
Financial Rev./Loans (%)	13.6%	12.8%	13.9%
	[7.2]	[2.4]	[8.0]
Predicted financing	1,547.5	1,598.7	1,532.9
	[494.9]	[429.2]	[513.1]
Exp. financing/Loans	0.068	0.076	0.065
	[0.166]	[0.196]	[0.159]

Means and standard deviations (in brackets) are reported. The statistics are calculated for a universe of 122 banks (26 program, 96 non-program) between 1998 and 2000. Loanable funds: the sum of equity, deposits and other liabilities minus the reserve requirements for each type of liability (for example 20% for checking accounts and 5% for 90 day deposits, 0% for one year deposits and so on). Program banks hold on average 10.2% of total assets, 11.2% of total loans and 12.1% of total deposits of the banking system.

Table 3

Program Firm Loans' Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max	Median
Amount of loan ($)	9,438.4	4,322.2	500	26,666	10,000
Value of collateral posted	10,527.5	9,751.9	0	350,000	10,000
Interest rate (%)	13.74	1.302	11.5	16	13.5
Grace period (months)	2.15	4.32	0	47	0
Frequency of payments (months)	1.30	1.10	1	6	1
Number of payments	33.19	13.38	0	48	36
Duration (months)	35.60	11.72	1	48	36

* Source: Program database, Secretaría de la Pequeña y Mediana Industria, Ministry of Economy, Government of Argentina. The table is based on 12,192 observations where each observation corresponds to a program loan. Duration is the number of months that results when multiplying the frequency of payment times the number of payments.

Table 4

Regression of Actual and Predicted Financing Expansion Growth on Past Deposit Growth, Past Cash Flow and Bank/Month Fixed Effects

	Actual Financing (growth)	Predicted Financing (growth) Program banks	Predicted Financing (growth) All banks
	(1)	(2)	(3)
$DepositGrowth_{t-1}$	-0.028	0.02	0.002
	[0.049]	[0.018]	[0.002]
$DepositGrowth_{t-2}$	-0.162***	-0.015	-0.001
	[0.058]	[0.011]	[0.003]
$DepositGrowth_{t-3}$	-0.094	-0.021	0
	[0.086]	[0.016]	[0.004]
$DepositGrowth_{t-4}$	-0.111*	-0.027	-0.002
	[0.055]	[0.019]	[0.003]
$CashFlow_{t-1}$	0.00078	0.00074	0.00076
	[0.0035]	[0.00071]	[0.00073]
$CashFlow_{t-2}$	0.0033	0.00070	0.00055
	[0.0035]	[0.00074]	[0.00040]
$CashFlow_{t-3}$	0.0058	0.00096	0.00027
	[0.0056]	[0.00098]	[0.00044]
$CashFlow_{t-4}$	0.0049	0.00071	0.00019
	[0.0071]	[0.00074]	[0.00044]
Observations	1001	1003	5818
R-squared	0.31	0.85	0.88

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in brackets, clustered at the bank level. All specifications include a full set of bank and month dummies.

Table 5

First Stage: Regression of Liquidity (Liquidity Expansion Dummy) on Predicted Credit Expansion Growth (Predicted Probability of Participation) and Bank/Month Fixed Effects

	All Waves			Final Waves
	All banks	High-probability of participation banks	Low-probability of participation banks	All banks
Liquidity	(1)	(2)	(3)	(4)
1. Dependent Variable: Liquidity				
Predicted Financing	0.033**	0.074***	0.011	0.068***
Expansion Growth	[0.012]	[0.021]	[0.040]	[0.021]
Observations	6,746	2,015	4,731	4,654
R-squared	0.08	0.15	0.10	0.07
2. Dependent Variable: Liquidity Expansion Dummy				
Predicted Probability				0.057***
of Participation				[0.011]
Bank/Month/IndustryFE				Yes
Observations				750,533
R-squared				0.59

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in brackets, clustered at the bank level. Banks with a high probability of participation are banks in the top quartile of the distribution of predicted probability of participation. All specifications include a full set of bank and month dummies. Liquidity is defined as the loanable fund growth, where loanable funds are equity plus deposits plus other liabilities minus reserve requirements. The liquidity expansion dummy is equal to one during the three following month after a bank participated in a program wave. The predicted probability of participation is estimated using (4-8).

Table 6

OLS/2SLS Estimates of the Sensitivity of Lending to Liquidity by Bank Size and Capitalization (Bank/Month Fixed Effects)

	OLS	2SLS		2SLS		
Sample	All waves	All waves		Final waves		
Loan growth	(1)	(2)	(3)	(4)	(5)	(6)
Instrument: Predicted Financing Expansion						
Liquidity	0.307***	0.481***	0.745***	0.745***	0.692***	0.627***
	[0.074]	[0.170]	[0.139]	[0.139]	[0.145]	[0.147]
Liquidity x Small					0.012	
					[0.221]	
Liquidity x LowCap						0.063
						[0.190]
# Banks	117	117	113	113	113	113
Observations	6,671	6,436	4,654	4,654	4,654	4,654
R-squared	0.15					

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in brackets, clustered at the bank level. All specifications include full set of bank/month dummies. Liquidity is loanable fund growth, where loanable funds are equity plus deposits plus other liabilities minus reserve requirements. Of the program banks in the reduced sample, 40.7% are classified as small and 25.9% as low capitalized. Of the non-program banks, 51.0% are classified as small and 14.9% as low capitalized.

Table 7

Loan and Loan Recipient Summary Statistics, by Borrower Type

	All	Existing Borrowers	New Borrowers - All	New Borrowers - Switching
Number	750,526	620,325	130,201	5,488
Loan Characteristics				
Loan amount ($)	16,691	17,722	11,776	15,602
	[226,660]	[218,790]	[260,858]	[326,867]
Collateral/Loan	0.123	0.124	0.118	0.129
	[0.301]	[0.299]	[0.312]	[0.324]
Loan Performance				
Default after 12 months (yes=1)	0.122	0.104	0.191	0.206
	[0.328]	[0.306]	[0.393]	[0.404]
Default after 24 months (yes=1)	0.168	0.153	0.228	0.254
	[0.374]	[0.359]	[0.420]	[0.435]
Borrower History				
Total bank debt		58,551		6,444
		[601,468]		[258,200]
Past non-performing loan (yes=1)		0.141		0.057
		[0.348]		[0.231]

Means and standard deviations (in brackets) are reported. Statistics are estimated from the post-1998 sub-sample. Each observation corresponds to a new loan issued during the sample period. Default after 12 (24) months is a dummy equal to one if the loan is non performing 12 (24) months after the loan is issued. Past non-performing loan is a dummy equal to one in the loan recipient has any non-performing debt during the 12 months previous to the loan issuance. A loan recipient is classified as *new* if it has no previous credit with the issuing bank, and *existing* otherwise.

Table 8

Bank Liquidity and Loan Risk – IV Estimates of Loan Default Rate on Liquidity Expansion Dummy including Bank/Month/Industry Fixed Effects

	Loan with problems after:	
	12 months	24 months
	(1)	(2)
1. All Loans		
Liquidity expansion bank-month	-0.004	-0.009
	[0.028]	[0.020]
Observations	750,563	750,563
2. Existing Borrowers		
Liquidity expansion bank-month	-0.002	-0.012
	[0.022]	[0.018]
Observations	620,325	620,325
3. New Borrowers		
Liquidity expansion bank-month	0.038**	0.017
	[0.018]	[0.016]
Observations	130,201	130,201
4. New Borrowers w/history		
Liquidity expansion bank-month	0.176**	0.161**
	[0.062]	[0.067]
Observations	5,488	5,488

Robust standard errors in brackets, clustered at the bank level. All specifications include bank and month fixed effects. * significant at 10%; ** significant at 5%; *** significant at 1%. Each observation corresponds to a loan made by bank i to firm j at month t. The dependent variable in columns 1 and 2 (3 and 4) is a dummy equal to one if the loan repayment is at least six months late, the loan is defaulted or the loan recipient has filed for bankruptcy 12 (24) months after issued. All specifications include bank, industry and month dummies. The liquidity expansion dummy is instrumented with the predicted probability of participation of bank i in a wave that begins at month t.

Table 9

Bank Liquidity and Composition of Lending: 2SLS Estimation of the Effect of Liquidity Expansion on Proportion of Number of Loans to Small and New Borrowers

Fraction of Loans to	Small Borrowers (1)	New Borrowers (3)
Liquidity	-0.011	
	[.034]	
Liquidity Expansion		-0.010
Bank-Month		[.050]
Bank/Month FE	Yes	Yes
Observations	4,654	4,654

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in brackets, clustered at the bank level. Each observation in the specification in column 1 is a loanj given by bank i at month t. Each observation in the specification of column 2 is lending by bank i at month t. Liquidity is defined as the loanable fund growth, where loanable funds are equity plus deposits plus other liabilities minus reserve requirements. Liquidity is instrumented with the predicted available finance. The liquidity expansion dummy is equal to one during the three following month after a bank participated in a program wave. The predicted probability of participation is estimated using (4-8). New borrowers are loan recipients with no previous relationship with the bank. Small borrowers are borrowers with less than $200,000 in sales and 20 workers.

Table 10

Bank Liquidity, Collateral and Borrower Past Performance: IV Estimates of Loan Collateral to Debt Ratio and Default History on Liquidity Expansion Dummy including Bank/Month/Industry Fixed Effects

	Collateral/Loan (1)	Bad Past Performance (2)
1. All Loans		
Liquidity expansion bank-month	-0.010**	
	[0.004]	
Observations	750,526	
R-squared	0.09	
2. New Borrowers		
Liquidity expansion bank-month	-0.034*	
	[0.019]	
Observations	130,201	
R-squared	0.05	
3. Existing Borrowers		
Liquidity expansion bank-month	0.002	0.047*
	[0.008]	[0.025]
Observations	620,325	620,325
R-squared	0.14	0.07
4. New Borrowers w/history		
Liquidity expansion bank-month	-0.068*	0.039***
	[0.037]	[0.013]
Observations	5,488	5,488
R-squared	0.20	0.02

Robust standard errors in brackets, clustered at the bank level. All specifications include bank, industry and month dummies. * significant at 10%; ** significant at 5%; *** significant at 1%. Each observation corresponds to a loan made by bank i to firm j at month t. The dependent variable in columns 1 and 2 is the proportion of the value of the loan covered with collateral. The dependent variable in column 3 is a dummy equal to one if the loan recipient has some non-performing debt outstanding (non-performing is defined as at least six months late in repayment). The liquidity expansion dummy is instrumented with the predicted probability of participation of bank i in a wave that begins at month t. The small loans dummy is equal to one if the amount of the loan is in the lowest quintile of the loan amount distribution.

Table 11

Collateral and Past Performance as Predictors of Default: Probit Estimation of the Probability of Default as a Function of the Collateral to Debt Ratio and Past Defaults including Bank/Month/Industry Fixed Effects

	Probability of Default	
	OLS	Probit (a)
	(1)	(2)
1. All Loans		
Collateral/Debt	-0.052***	-0.052***
	[0.004]	[0.004]
Observations	750,526	750,526
R-squared (pseudo)	0.04	0.04
2. New Borrowers		
Collateral/Debt	-0.104***	-0.109***
	[0.005]	[0.005]
Observations	130,238	129,804
R-squared (pseudo)	0.08	0.08
3. Existing Borrowers		
Collateral/Debt	-0.035***	-0.038***
	[0.004]	[0.005]
Past Default Dummy	0.544***	0.554***
	[0.002]	[0.003]
Observations	620,325	617,863
R-squared (pseudo)	0.21	0.17
4. New Borrowers w/history		
Collateral/Debt	-0.096***	-0.104***
	[0.019]	[0.022]
Past Default Dummy	0.339***	0.367***
	[0.065]	[0.069]
Observations	5,488	5,488
R-squared (pseudo)	0.08	0.07

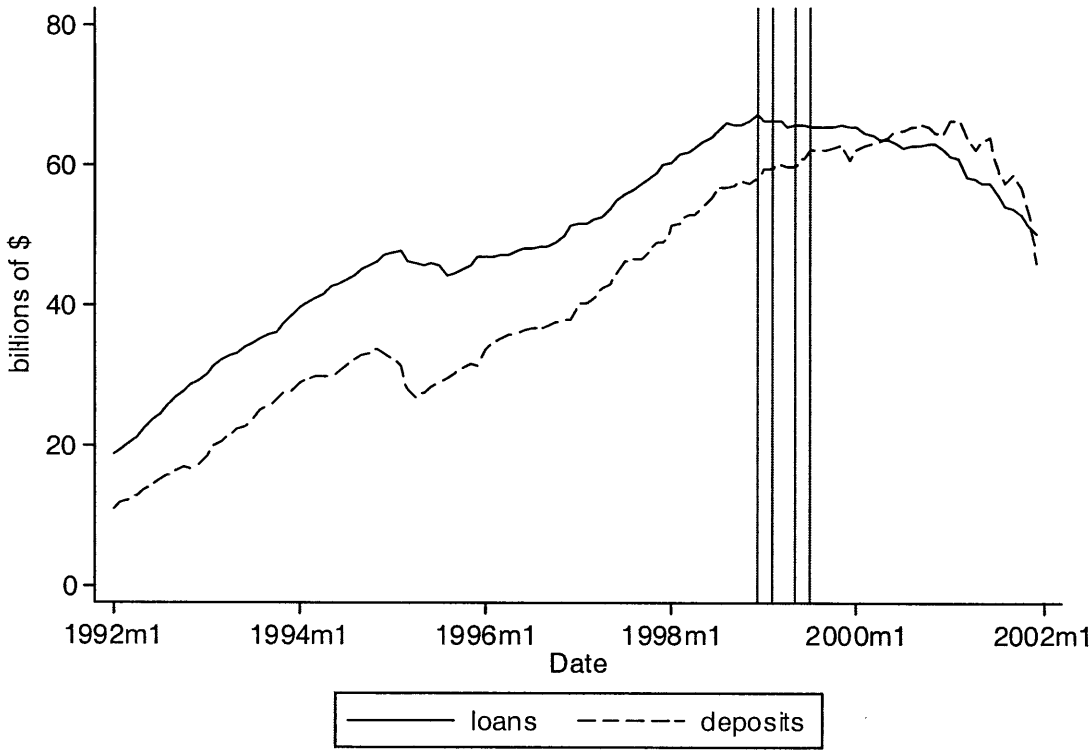
Robust standard errors in brackets, clustered at the firm level (222,146 clusters in the entire sample). All specifications include bank, industry and month dummies. * significant at 10%; ** significant at 5%; *** significant at 1%. Each observation corresponds to a loan made by bank i to firm j at month t. The small loans dummy is equal to one if the amount of the loan is in the lowest quintile of the loan amount distribution.

(a) Marginal effects evaluated at the sample mean are reported.

9. Figures

Figure 1

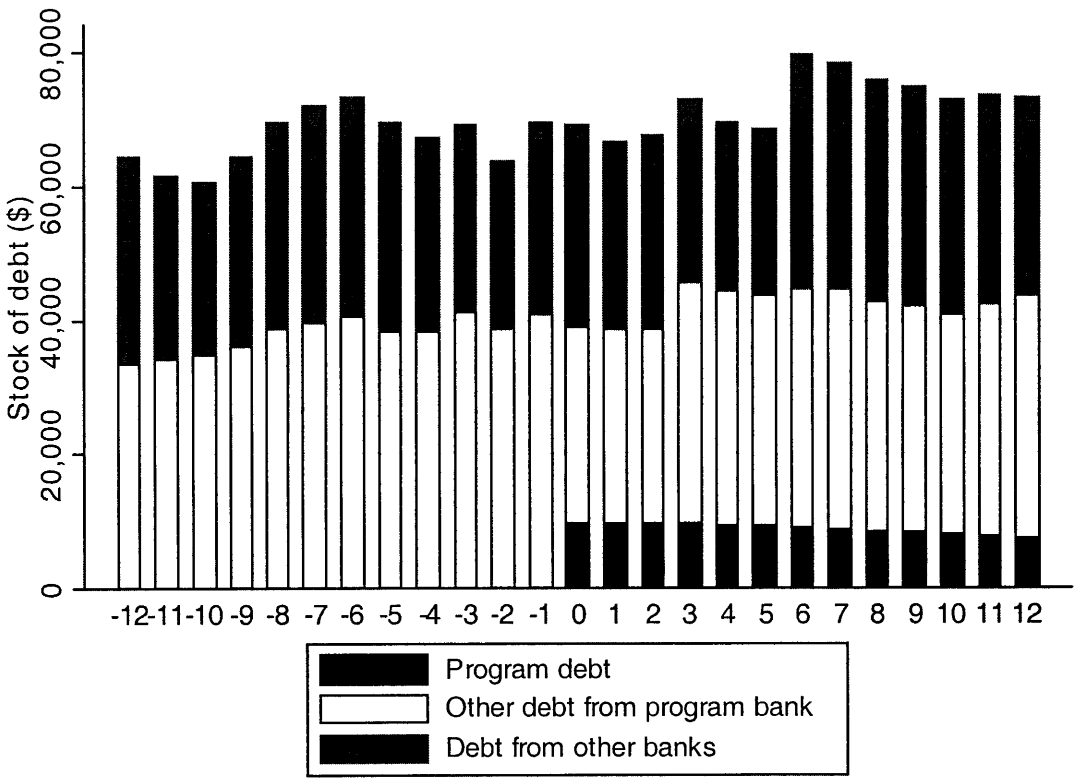
Time Series Evolution of Loans and Deposits in the Banking System, monthly data from 1992 to 2001



Source: Central Bank of Argentina. The vertical lines represent the start date of the final four waves of the program.

Figure 2

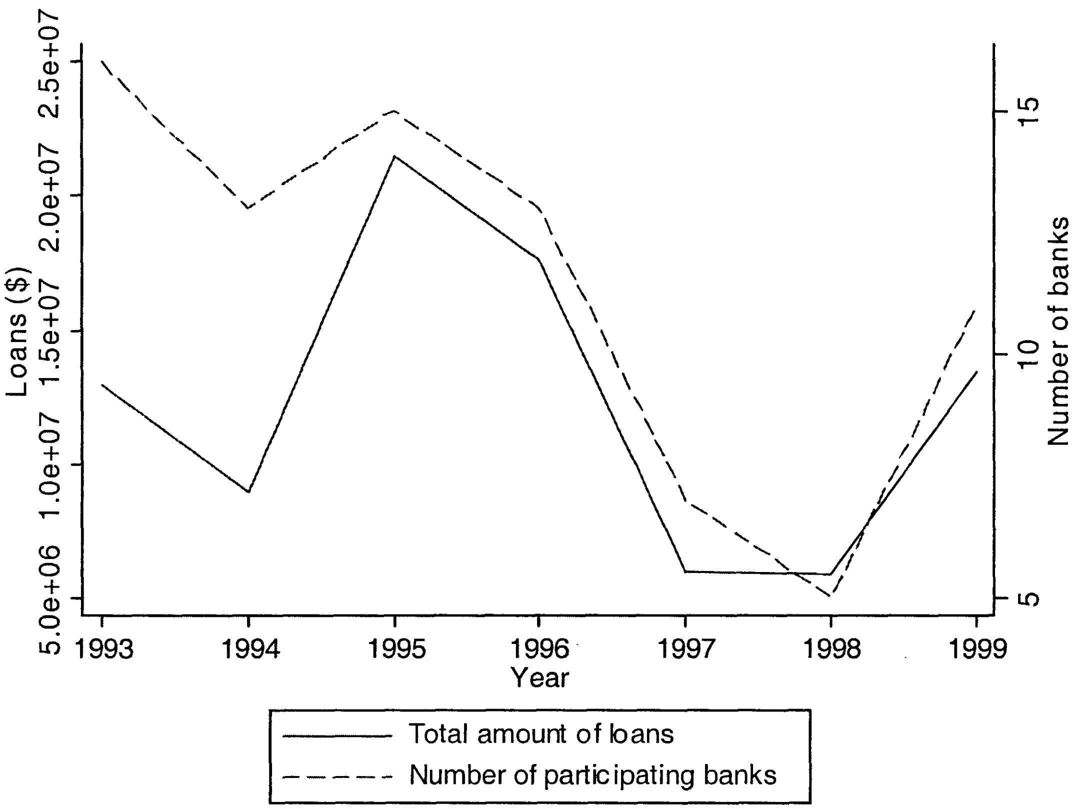
Evidence on Loan Re-Labeling: Monthly Debt Evolution of the Firms that Received Program Loans, by Source



Source: own calculations using MYPES program database and CDSF credit bureau data. Based on a sample of 2,596 firms that received program loans after January 1996. The horizontal axis measures time in months relative to the moment of reception of the program loan (0 is the month the program loan was received by the firm).

Figure 3

Flow of Program Financing and Number of Participating Banks, by Year



Source: own calculations using MYPES program data. The flow of financing during a year is the sum of the amount of resources allocated to all the waves that began during that year. The number of banks counts a bank only once even if it participated in two waves during a year.

Figure 4

No Financing Frictions: Profit Maximizing Choice of Loans when Subsidized Financing Increases

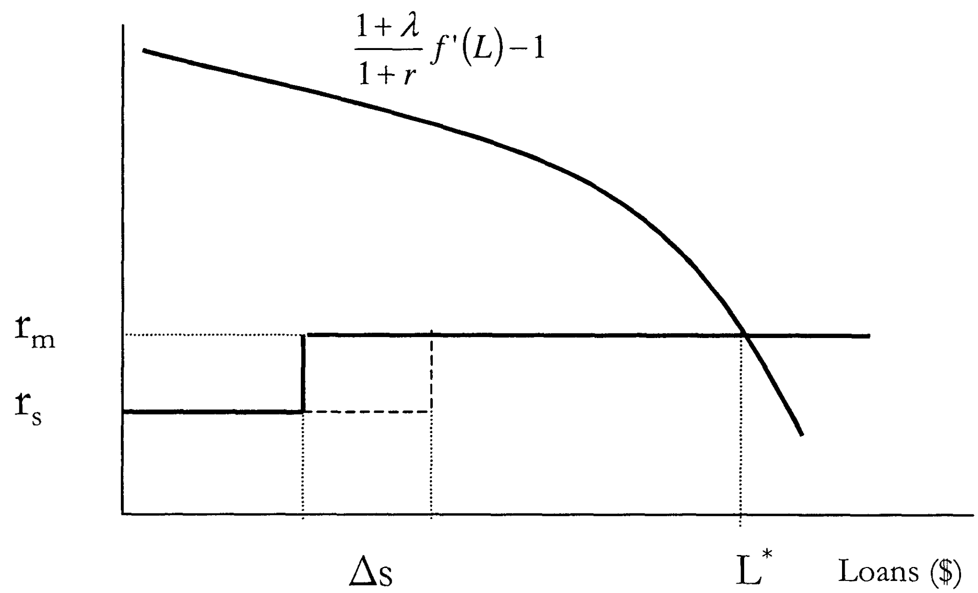


Figure 5

Financing Frictions: Profit Maximizing Choice of Loans when Subsidized Financing Increases

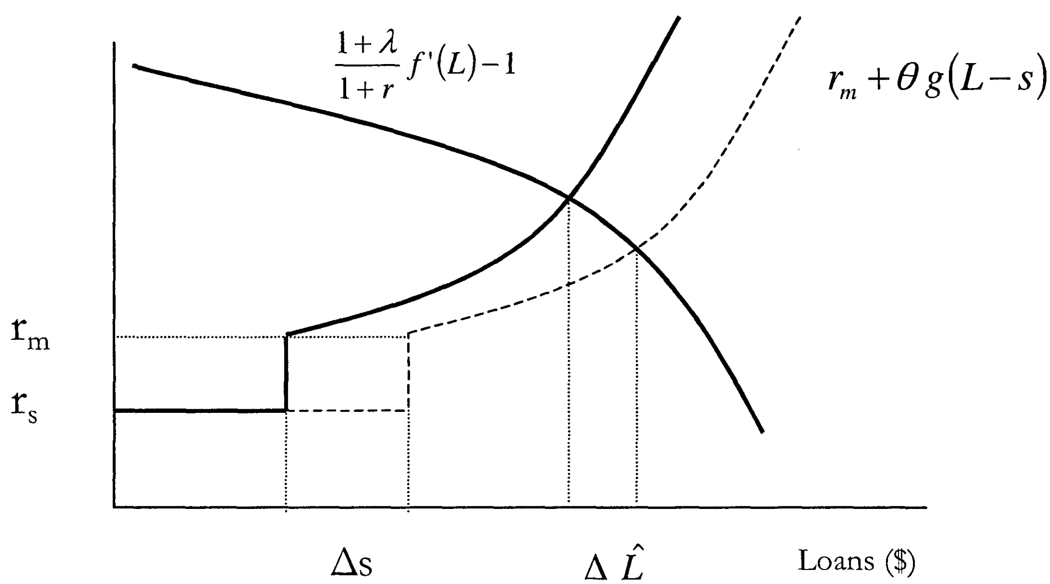
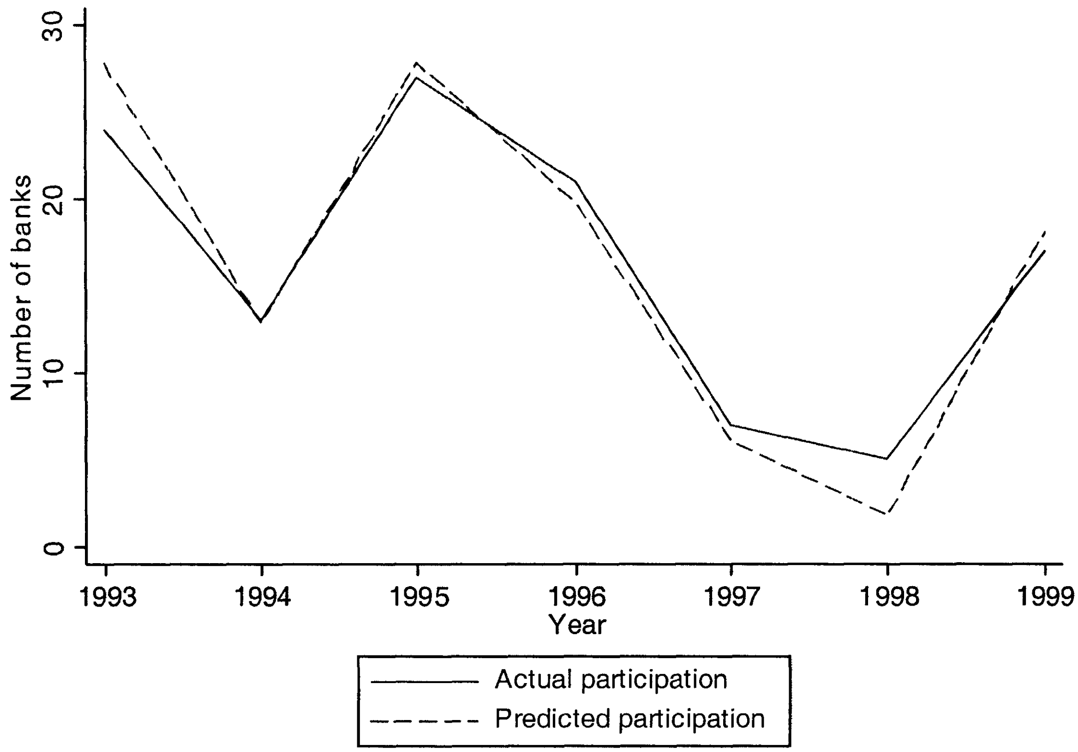


Figure 6

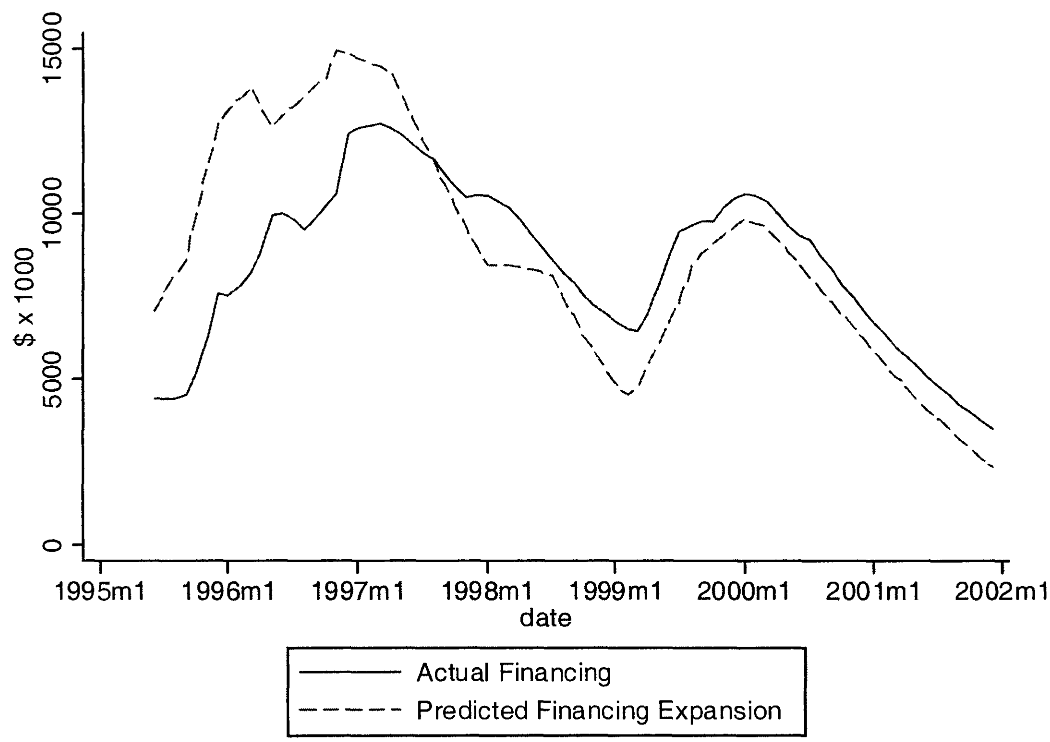
Actual and Predicted Number of Bank Participations in a Wave, by Year



Source: Own calculations using MYPES program data and bank balance sheet data and monthly income reports. The predicted participation is the result of the estimation of a probit model of the probability of participation of bank i in wave w, on a third degree polynomial of wave size, and the point scores of each bank. Participations in a wave are higher that participating banks per year (last graph) when the same bank participated in more than one wave during a year

Figure 7

Stock of Actual Program Financing and Predicted Program Disbursement, by Month

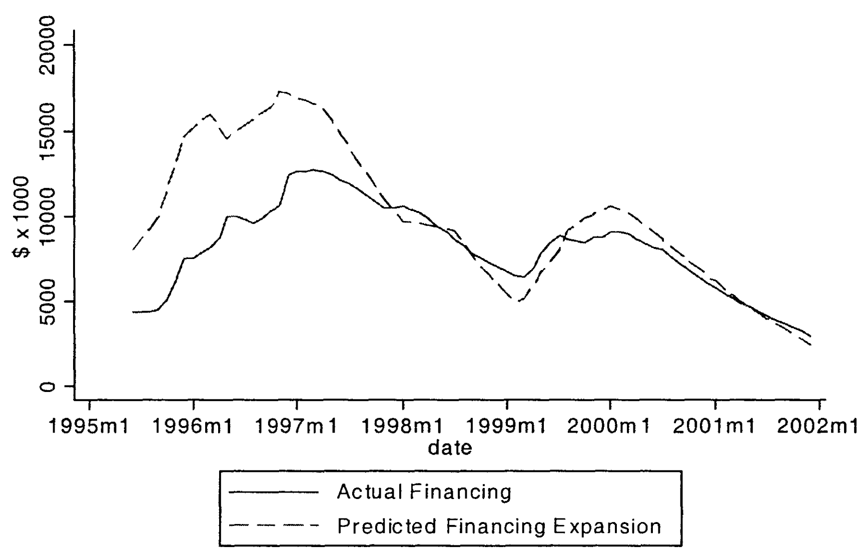


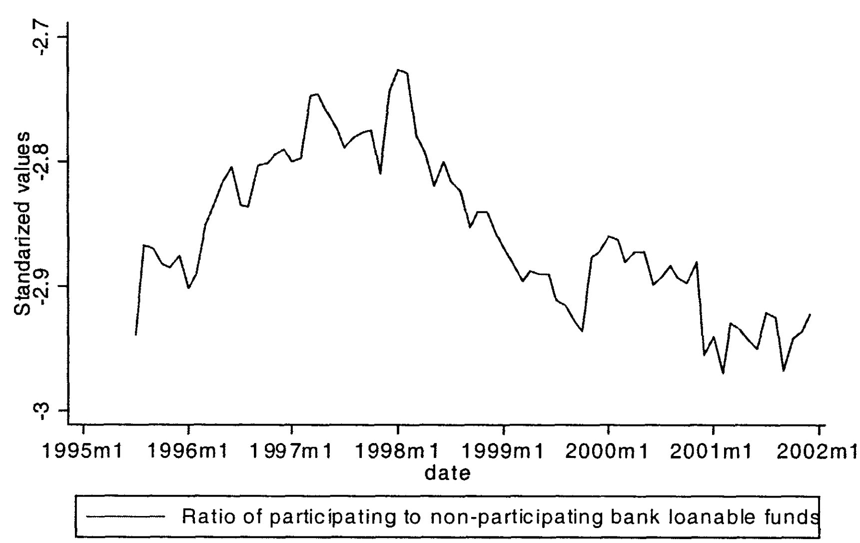
Source: Own calculations using MYPES program data and bank balance sheet data and monthly income reports. Actual program financing is the stock of program debt outstanding. The predicted program disbursement is the predicted financing expansion using (4-10) times the probability of participation of each bank, summed across all banks.

Figure 8

Top Panel: Stock of Actual Program Financing and Predicted Disbursement to Banks with High Probability of Participation, by month

Bottom Panel: Ratio of High Probability to Low-Probability of Participation Bank Loanable Funds, by month (normalized)

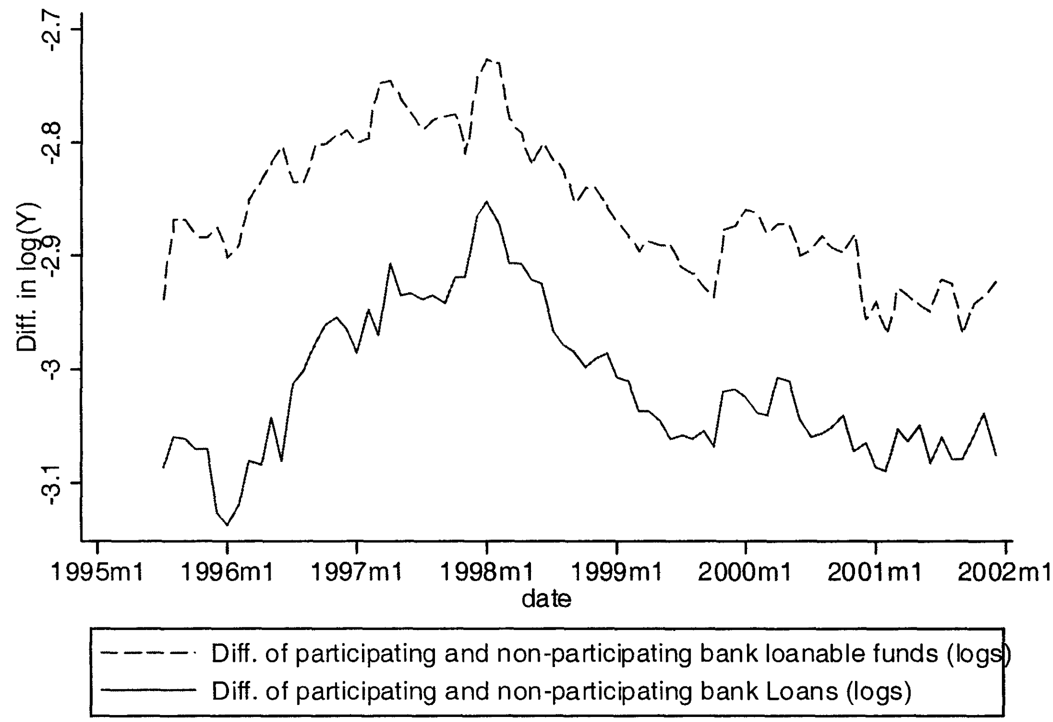




Source: Own calculations using MYPES program data and bank balance sheet data and monthly income reports. Loanable Funds are the sum of Equity, Deposits, and other liabilities, minus reserve requirements. Actual program financing is the stock of program debt outstanding. The predicted program disbursement is the predicted financing expansion using (4-10) times the probability of participation of each bank, summed across all banks. Banks with a high probability of participation are banks in the top quartile of the distribution of predicted probability of participation.

Figure 9

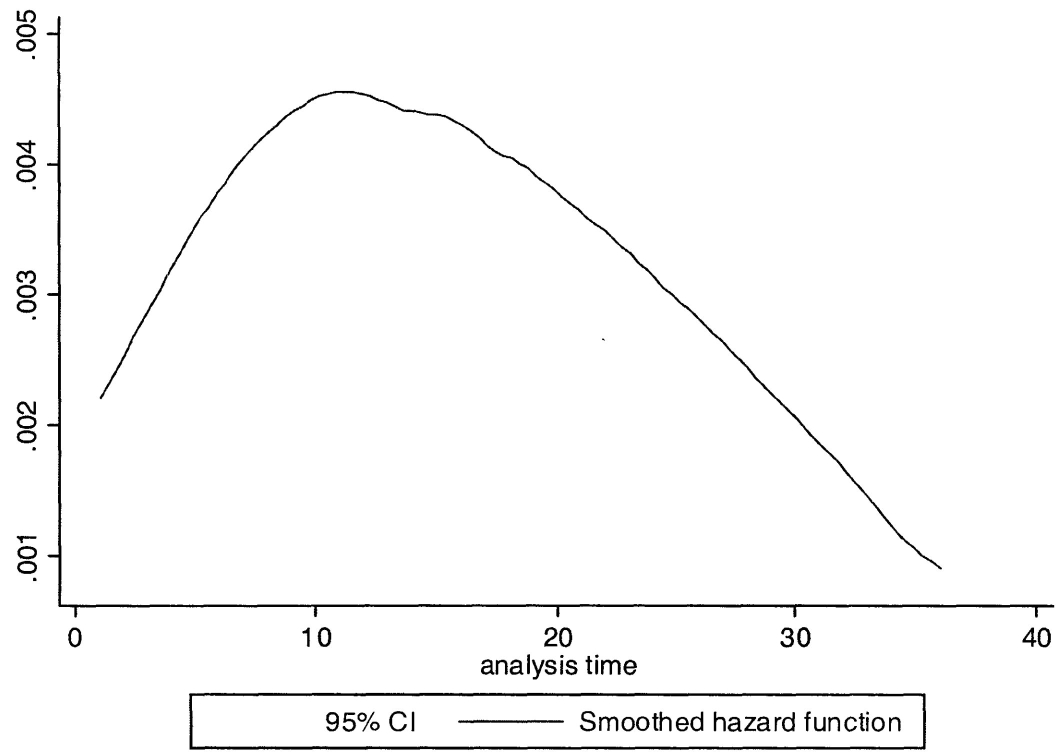
Difference in the log Average Loanable Funds and Loans of High-Probability vs. Low-Probability of Participation Banks



Source: Own calculations using MYPES program data and bank balance sheet data and monthly income reports. Loanable Funds are the sum of Equity, Deposits, and other liabilities, minus reserve requirements. Banks with a high probability of participation are banks in the top quartile of the distribution of predicted probability of participation.

Figure 10

Kernel Estimation of the Monthly Hazard Rate of Default



Source: Own calculations using Public Credit Bureau database from the sample of loans issued between January and August 1998. The vertical axis plots the weighted kernel density estimate utilizing the estimated monthly default hazard. The horizontal axis measures months elapsed after the loan was issued. Loan performance is observed until August 2001 where the sample is truncated to avoid the period of high default rates that preceded the December 2001 crisis in Argentina.

CHAPTER TWO
Targeting Credit to Small Firms Through the Banking System: Does it Work?

1. Introduction

The belief that it is possible to overcome the frictions that prevent the access of small firms to formal credit markets through policy interventions is nowadays highly controversial. Interventions in the formal credit market that were widely popular in the post WWII period (Besley 1995) have given way more recently to policies supporting alternative providers of credit (e.g. micro-finance institutions). The dismissal of traditional interventions, however, seems to have arisen more from overly optimistic expectations of their potential effects, than from systematic evidence of their failure. Only recently has research formally addressed the issue of evaluating the effects of interventions in bank credit markets and the conclusions are all but negative (see Aportela (1999), Burgess and Pande (2003), Zinman (2002)).

This paper evaluates a very common type of credit market intervention to enhance access to credit of small firms in developing countries: "on-banking". On-banking, or two-step lending, involves financing credit to targeted sectors of the economy using existing financial institutions as intermediaries. The government, or a development agency, makes subsidized financing available to existing financial intermediaries, earmarked to be lent in turn to the target sector. The hope is that this design will not only increase the immediate availability of credit, but also increase the long-term availability by enhancing the institutional capabilities of financial intermediaries to channel resources to the target group.

The use of on-banking is widespread around the developing world. The World Bank alone, through the IFC, allocated more resources to small firms through on-banking than through any other individual program (Barger 1998). The existing evidence on the success of this type of intervention is mostly based on budget execution and profitability. The main conclusions regarding the IFC on-banking experience are that programs have had on average poor budget execution performances, but that the default rate on the program loans has been practically insignificant. Conventional wisdom suggests that if the execution performance issues are addressed, these programs will unambiguously improve the credit access conditions of the target firms. This paper puts this convention to the test by evaluating an on-lending program in Argentina and addressing the previously unanswered question: do on-banking programs increase the availability of bank financing to target firms?

The main issue to address, as in any policy evaluation, is the lack of an appropriate counterfactual: how would a `treated' firm's debt have evolved in the absence of the program. In this paper I exploit the data available in a Public Credit Registry in Argentina to match treated firms with their entire bank debt history. Thus, the treated firms' bank credit before and after treatment can be observed. In addition, the credit history of all non-treated firms is also observed, from were a suitable comparison group can be drawn. In the application at hand, the firm eligibility rule of the program provides an appropriate comparison group. Only firms with less than 21 workers and less than $200,000 in annual sales were eligible to receive program loans. Ineligible firms slightly above the eligibility threshold were similar to the treated firms, but excluded from treatment due to exogenous reasons. The effect of the program on treated firms is identified by comparing the debt evolution of firms above and below the eligibility threshold.

I show first that banks selected the safest and fastest growing firms among their existing eligible borrowers to receive program loans. This firm selection explains most of the observed post-program performance of the target firms. The results also indicate that the debt of the target firms increases on average by 8 cents for every dollar of subsidized financing received by the banks. There is evidence that banks circumvented the targeting rule by re-labeling the debt of existing borrowers as "program loans" and were largely unconstrained in their use program financing. There is also some evidence that banks used the redirected funds to expand credit to non-eligible firms. Finally, the results indicate that bank identity matters. The impact of the program on target firms is considerably larger (20 cents per dollar of financing) when the intermediary bank is more likely to lend to smaller firms according to observable measures such as size and ownership.

The rest of the paper proceeds as follows. Section 2 discusses under which circumstances it should be expected that an on-lending program expands the availability of credit to target sector. Section 3 describes the particular features of the on-lending program that is evaluated, and section 4 describes in detail the identification strategy. Section 5 describes the data and the criteria for the sample selection and section 6 presents and discusses the results. Section 7 concludes.

2. On-Banking, Access to Bank Credit and Targeting

On-banking will have no effect on credit in the standard neoclassical model of credit. In such a model, the expected return on loans is equal to the marginal cost of financing of banks. When banks obtain subsidized finance, it will be profit maximizing to use it to substitute for more expensive finance at the market rate and not to expand credit.[26] Earmarking will also be ineffective because resources are fungible and program loans can substitute one for one loans that banks were going to issue anyway.

When the neoclassical assumptions fail, though, subsidized financing to banks may affect the availability of credit to firms. For example, banks may be constrained in their access to external financing as a consequence of information asymmetries between bank management and the providers of finance (Holmstrom and Tirole 1997; Stein 1998).[27] Also banks may have an explicit objective to maximize lending subject to a break-even constraint, as cooperative banks in Argentina do. Subsidized financing relaxes both credit and break-even constraints of the bank and may result in additional lending.

The resulting additional lending will be directed to target sector borrowers if these borrowers are rationed in the margin. In principle we expect banks to rank borrowers according to their risk and profitability profiles and ration them accordingly. In general, it will be hard to predict ex ante whether the next borrower in line to receive bank credit belongs to the objective sector. This will be more likely the case when the bank that receives the subsidized financing is specialized in lending to the objective sector. For example, a bank located a rural area that has a significant portion of its loan portfolio allocated to agricultural firms is more likely to lend the marginal dollar to a farm than a commercial bank located in an urban area.

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[26] As long as the amount of subsidized financing is infra-marginal (that is, smaller than the total financing at the market price).

[27] Empirically, there are extensive accounts on how loan supply is sensitive to shocks to deposits (Bernanke and Gertler 1995; Kashiap and Stein 2000; Angeloni, Kashiap, Mojon et al. 2003; Ashcraft 2003) and other sources of financing (Peek and Rosengren 1997; 2000; Paravisini 2005).

Or as argued in Stein (2002), smaller banks are more likely to allocate the marginal loan to small firms than larger ones.

The program rules can also provide incentives to banks to lend the marginal dollar to the target sector. On-banking programs usually require banks to issue loans to firms that meet a set of eligibility criteria. The effectiveness of earmarking will depend on how costly it is for the bank to exploit resource fungibility. If a bank is already lending to firms in the target sector, it will be very easy to substitute existing debt with program debt without expanding lending to this sector at all. On the other hand, a bank that lends nothing to the objective sector to begin with will have to employ costlier methods to circumvent the targeting rule (misreporting, lending to firm subsidiaries that meet the criteria, etc.).

The previous discussion suggests a potential trade-off in focusing on specialized banks when implementing on-lending programs. Banks that specialize in loans to the objective sector are more likely to voluntarily allocate the marginal loan to this sector, but can also avoid expanding credit altogether more easily using loan substitution. Whether earmarked and subsidized financing to financial intermediaries will increase the availability of credit to target sector, and through which types of financial institutions earmarking is more likely to be effective has to be addressed empirically.

Preliminary evidence on the on-lending program evaluated in this paper suggests the program may have had little effect on the availability of credit of target firms. Figure 2 shows the bank debt held on average by a sample of 2,596 firms that received program loans. The horizontal axis measures time relative to the month the firm received the program loan. Three components of total debt are shown in the graph. Program debt, non-program debt with a program bank, and debt with non-program banks.

There are several salient features of this graph. First, firms that received program loans had more than $60,000 of bank debt on average, and more than half of this debt was debt with program banks. Second, debt held with program banks was increasing even before these firms received program loans. And third, there is no observable increase in the amount of debt held by these firms when they receive the program loan. All these features are consistent with program loans crowding out bank debt that the target firms would have received in the absence of the program.

3. Description of the Program

The Credit Program to Small and Medium Sized Firms (MYPES for its acronym in Spanish) was a credit market intervention targeted to firms with less than 21 workers and less than $200,000 in annual sales. MYPES was financed by the Inter-American Development Bank (IDB) and allocated around $90 million in credit to target firms through existing financial intermediaries between 1993 and 1999.

Banks would receive subsidized financing from the government, and in exchange they had to issue loans to firms that matched the eligibility criteria. Banks were free to set the interest rate, collateral requirements and other characteristics of the loans to firms. Only the duration (48 months maximum, including an optional 12 month grace period) and the size ($20,000 maximum) were limited by the program rules.[28]

Bank participation in the program was voluntary, and all banks were eligible to participate. Of the 207 financial institutions that existed in 1995, 31 participated in the program. Participating banks were smaller and less profitable than the average bank in the Argentine financial system (see Table 12). No foreign owned bank chose to participate in the program.

The program was implemented in 12 waves during seven years. The amount of resources distributed and the number of participating banks varied by year (Table 13). The amount of resources in each wave was decided by the IDB, and resources in each wave were distributed among participating banks according to a formula based on bank characteristics and the banks' bids on matching funds and interest rates. The formula would allocate more resources to banks with: a) smaller average size of loans, b) higher proportion of loans in poor provinces, c) a higher proportion of own resources allocated to program loans, and d) lower interest rate charged to recipient firms.

In practice, there was little or no within wave cross sectional variation in the interest rate or the matching fund bids. In 96% of the bids banks offered to put $1 for every $3 of program resources, which was the minimum possible according to the program rules. And the difference between the highest and lowest interest rate bids on a wave was at most 0.06 percentage points (and zero 81% of the time), which represents a negligible variation relative

[28] The only additional restriction imposed by the program was that the amount of the median loan of every wave should be $10,000.

to the average interest rate of 13.7%. Thus, the resulting cross sectional allocation between participating banks responded mostly to the average size of loans and the regional loan distribution.

The program resources allocated to a bank were made available through a credit line in the Central Bank. The price of financing was the average interest rate of dollar denominated deposits in the financial system. Banks had three months to draw the allocated resources or pay a penalty equal to twice the interest rate of the unused balance. The unused balance would be reassigned in the next wave of the program.

Finally, in order to draw resources from the credit line, banks had to extend a new loan to an eligible firm. The repayment schedule to the Central Bank matched exactly the schedule of the associated loan to the target firm. Repayment was not contingent to firm repayment, which implied the credit risk of the loans was borne by banks.

The descriptive statistics of the program loans to firms are shown in Table 14. Banks issued 12,192 program loans, with an average size of $9,440 and average duration around 35 months. The median loan amount was $10,000, as required by the program rules, and less than 1% of the loans exceeded the maximum allowed amount of $20,000.[29]

The average collateral posted by firms that received program loans was higher than the average size of loans, and the median loan was fully collateralized. Program loans were allocated to firms with relatively abundant collateral, since the average exposure (1-collateral/loan) of loans of eligible firms in the sample is around 0.76 and the median loan poses no collateral.

This evidence is suggestive that firms selected to receive program loans were different from the average eligible firm. Firm selection will induce biases in program evaluations based on post treatment firm characteristics. Section 4 below discusses how the empirical strategy of this paper addresses this identification issue.

[29] Note that banks were able to allocate more than $20,000 in program loans to a single firm without breaking the maximum size rule. The program imposed no limitations on the maximum number of loans that could be allocated to a single firm. Out of the 10,828 firms that participated in the program, 1,130 (10.4%) received more than one program loan, and 336 firms (3.1%) received more that $20,000 in program credit.

4. Identification Strategy

The goal is to evaluate empirically how the on-lending program changed the availability of credit of target firms. The difficulty of assessing the effect of the program is that the debt treated firms (firms that received program financing) would have had in the absence of the program is unobservable. As a proxy for this counterfactual I look at firms with similar characteristics to the treated firms, but that were not selected into the program for exogenous reasons (or for reasons that depend on observable firm characteristics). The firm eligibility rule provides such an exogenous exclusion from treatment: firms with more than 20 workers or more than $200,000 in sales were not eligible for program loans.

The main identification assumption is that the debt of firms above and below the eligibility cutoff would have evolved in a similar fashion in the absence of the program. This is a plausible assumption as long as firms are close enough to the eligibility threshold. Another requirement for this identification strategy to be valid is that eligible firms were more likely to receive program loans than non-eligible ones.

If both of these assumptions hold, then comparison between the pre-post change (before and after the beginning of each program wave) of the debt of eligible firms and that of not-eligible ones will represent the average program effect on eligible firms (differences-in-differences estimator). This average effect on eligible firms can be adjusted by the fraction of eligible firms that receive program loans to obtain the program effect on treated firms (Wald estimator).

The regression version of the differences in differences estimator is the reduced form of debt growth on firm eligibility. The regression version of the Wald estimator is the instrumental variable estimation of debt growth on program participation, using eligibility as an instrument for program participation. Thus, the identification strategy will be implemented using the following panel IV specification:

$$\begin{aligned}\Delta \ln(debt)_{jt} = {} & \beta.TREAT_{jt} + \gamma.ELIG_j + \varphi_1.POSTw1_{jt} + \varphi_2.POSTw2_{jt} + \\ & + \varphi_3.POSTw3_{jt} + \varphi_4.POSTw4_{jt} + \theta X_{jt} + \nu_{jt}\end{aligned} \qquad \text{4-1}$$

using $EPwk_{jt} = ELIG_j.POSTwk_{jt}$, as instruments for $TREAT_{jt}$, and where j represents a firm and t a month. I will implement the IV with the two-step method suggested by Wooldridge

(2002). First, a probit model for treatment is estimated by maximum likelihood. Then the fitted probabilities are used as an instrument for treatment in 4-1. This estimator is convenient because it is asymptotically efficient under certain assumptions and robust to misspecification of the first stage. Also, as I will show later, because the empirical probability of treatment conditional on eligibility is sufficiently close to zero to render a linear probability model inadequate.

The dependent variable is the proportional growth of the bank debt of the firm. Specifications with both the total bank debt of the firm and debt with the program bank are estimated. The first will allow inferring whether the program expanded the total amount of bank credit of the firm, and the second whether program banks expanded credit to target firms. Comparing the two estimates allows assessing the degree to which firms used program loans to substitute for debt from other sources (other banks). Since credit constrained firms are less likely to substitute, this should provide an indirect test for whether program loan recipients were credit constrained.

Regarding the dependent variables in 4-1, $TREAT_{jt}$ is a dummy equal to one every period after firm j receives a program loan and zero otherwise. The coefficient of interest, β, is the effect of the program on the average treated firm debt growth, or the local average treatment effect as defined in Imbens and Angrist (1994). $ELIG_j$ is a dummy variable equal to one if firm j is eligible for the program. Eligibility is time invariant and imputed to a firm when it is first observed. This avoids debt growth to be correlated with eligibility, which would render this last variable invalid as an instrument. $POSTwk_{jt}$ is a dummy variable equal to one every month after wave w of the program started. X_{jt} represents a set of control variables that includes the square root of collateral, lagged number of loans, lagged number of bank relationships, lagged loan performance index, log sales, a time trend, a set of industry dummies and a constant. Finally, ν_{jt} is the error term.

5. Data and Sample Description

5.1. Data Sources

This paper uses data from three sources. The Public Credit Registry database (Central de Deudores del Sistema Financiero or CDSF) contains information on all debt above \$50 held by any individual or firm with a financial institution in Argentina after 1998. For each credit and each month, the database reports the name of the debtor, the name of the bank, the principal outstanding, the collateral offered as guarantee of the loan and a code describing the debt situation.[30] The detail of the database allows determining the amount of debt each firm holds with any commercial bank or credit institution at any month.

The second source of data is the program database, collected and managed by the Secretaría de la Pequeña y Mediana Empresa (SEPyME), of the Ministry of Economy in Argentina. This database has detailed characteristics about firms that received loans from the program, such as characteristics of the loan (date, amount, interest rate, duration, grace period, interest rate, etc.), characteristics of the firm (number of workers, annual sales, etc.), and name of the intermediary bank. The program database and the CDSF were linked using the tax identification code (CUIT). In order to maintain the anonymity of program participants, technical staff of the Ministry of Economy merged the two databases. The merged databases were then made available for this research without the identifying codes.

Finally, the database of small manufacturing firms collected by Unión Industrial Argentina was used to impute sales and number of workers to the CDSF database. This third database was used instead of the program database for imputation in order to avoid potential misreporting biases: the worker and sales data in the program database were self-reported by program banks and were not subject to verification by the program administrators. Figure 12 shows the distribution of the annual sales reported by program firms in the sample (Panel 1) and the imputed sales (Panel 2). The reported sale distribution has a high density right before the $200,000 eligibility threshold that is not present in the predicted sale distribution. This is consistent with misreporting.

Another fact worth highlighting from the predicted sales distribution is that even though some treated firms have predicted sales above the eligibility threshold, the density of the predicted sale distribution does drop sharply after $200,000. This means that the identification assumption that non-eligible firms have a low probability of treatment is likely to hold.

[30] This code has six categories with the first one corresponding to the best credit situation, 1: normal situation, 2: potential risk, 3: with problems, 4: with high insolvency, 5 and 6: default.

5.2. Sample Selection

The loan level data is available after 1998, so the estimation of the treatment effect with 4-1 will be restricted to the final four waves of the program. This way pre-treatment data is available for each firm in the sample. The sample of firms is further restricted to those that borrow from banks that participated in at least one wave of the program. Banks that participated in the program have different characteristics and are likely to have a different portfolio of loans than non-participating ones (see characteristics of participating and non-participating banks in Table 12).

All non-firm borrowers are excluded from the sample. The CDSF database groups all loans made to individuals in a single category, regardless of whether they are consumption loans or mortgages. A potential caveat of this sample restriction is that many small firm loans in Argentina are issued as personal loans to business owners. Nevertheless, unreported estimations excluding only individuals that held less than $2,500 of debt on average during the sample period obtain results that do not differ significantly form the ones shown in the next section.

Finally, the sample is restricted to include only firms that are around the eligibility threshold. In particular, only firms with predicted sales between 180,000 and $220,000 are left in the sample. The sample is selected according to sales because it was eligibility along the sales dimension what appears to have been binding for firm participation. The choice of a $20,000 interval around the $200,000 eligibility threshold is arbitrary, but there is a tradeoff when making it tighter. On the one hand, making it tighter is better in terms of satisfying the identification assumption that eligible and non-eligible firms are comparable. On the other hand, tightening the interval reduces the number of treated firms that remain in the sample, which lowers the precision of the IV estimates. Unreported sensitivity analysis indicated that the point estimates shown in the next section do not vary significantly when the sales interval is varied from $10,000 to $50,000 around the eligibility threshold or when firms with predicted workers between 15 and 25 are added to the sample.

The resulting sample constitutes an unbalanced panel of 65,366 firms observed monthly between September 1999 and May 2000. The panel is unbalanced because firms that obtain credit for the first time after September 1999 are observed fewer times in the sample. The summary statistics of the firms in the sample are presented in column 1 of Table 15. The statistics are also presented for the sub samples of eligible, eligible-treated and non-eligible firms (columns 2 through 4).

As expected, eligible firms are smaller than the average firm in the sample but they are no different in several important dimensions. Their credit quality, measured as their debt repayment performance, and the level of indebtedness, measured as the debt to sales ratio, are not significantly different from the rest of the firms in the sample. The dimensions in which they are different, the variation is very small. For example, 71.1% of the debt of eligible firms is not backed by collateral, compared to 68.5% of the average firm in the sample. Also, eligible firms have relationships with 1.71 banks on average, compared with 1.86 for the entire sample.

The eligible firms that received program loans (eligible treated firms), on the other hand, are noticeable different from the rest of the eligible firms. On average, treated firms have relationships with one more bank, have a better credit quality, are almost 5 times more indebted and have less than half of the fraction of their debts uncollateralized than the average eligible firm. This evidence is consistent with banks selecting firms that are safer and less constrained in their access to credit to allocate program loans.

Finally, the fraction of treated firms in the sub-sample of eligible firms is close to 2%, while the fraction of non-eligible firms that were treated is 0.1%. Thus, the probability of treatment conditional on eligibility appears to satisfy the requirements for identification using the proposed empirical strategy. This fact is confirmed next when the results of the estimation of the first stage of the empirical specification are reported.

6. Results

6.1. First Stage: Is Eligibility a good Predictor of Treatment?

Identification hinges on non-eligible firms being excluded from treatment due to observable firm characteristics. In each wave, the probability that eligible firm receives a program loan should be higher than the probability a non-eligible firm receives one. To check this I estimate a conditional probability that firm j receives a program loan at month t (e.g. is treated) using the following probit specification:[31]

$$\begin{aligned} \Pr(TREAT_{jt}=1) = \Phi(&\xi_1.EPw1_{jt} + \xi_2.EPw2_{jt} + \xi_3.EPw3_{jt} + \xi_4.EPw4_{jt} + \\ &+ \chi.ELIG_j + \psi_1.POSTw1_{jt} + \psi_2.POSTw2_{jt} + \\ &+ \psi_3.POSTw3_{jt} + \psi_4.POSTw4_{jt} + \varkappa X_{jt}) \end{aligned} \qquad \text{6-1}$$

The marginal effect of $EPwk_{jt}$ represents the difference in the probability of treatment between eligible and ineligible firms. The estimated marginal effects, which are expected to be positive, are reported in Table 16. All the estimated marginal effects are positive and significant, except for the one corresponding to the last wave of the program. This suggests that eligible firms were more likely to be treated in all waves except for the last one, where banks appear to have relaxed the eligibility criteria.

Figure 13 gives a graphical account of the first stage results. The dashed line represents the change in the average estimated probability of treatment of eligible firms by month. The solid line represents the same estimated change for non-eligible firms. The vertical lines represent the dates when the last four waves of the program begin. There is a surge in the probability of treatment of eligible firms every time a wave begins. There is also a small surge in the probability of treatment of non-eligible firms, reflecting the fact that some non-eligible firms were treated. Nevertheless, the increase in probability of treatment is larger for treated firms, which is necessary for the identification strategy to work. The figure also shows an abnormally large increase in the probability of treatment of non-eligible firms in the last wave of the program. This is consistent with the eligibility rule being relaxed for this final wave, as was suggested by the estimated marginal effects estimated from the probit.

The bottom line of the first stage results is that firm eligibility is a good predictor of the probability of treatment. The next section uses eligibility as an instrument for treatment to estimate the effect of the program on firm debt.

[31] The choice of a probit model is justified by the low ratio of treated firms to eligible firms (about 1 in 90). The small empirical probabilities of treatment conditional on eligibility render the linear probability model inadequate for this application.

6.2. The Effect of the Program on Firm Debt

Table 17 shows the OLS and 2SLS estimates of the parameters of 4-1. The probabilities of treatment estimated using 6-1, $\hat{p}_{jt}$, are used as an instrument for the treatment dummy in the 2SLS specification. The OLS estimate is around five times the IV one. This upward bias of the OLS estimate is consistent with firm selection: treated firms' debt would have grown faster than non-treated firms' even in the absence of the program. The IV estimation suggests that treatment increased debt growth after controlling for the selection bias, although the point estimate is not significantly different from zero in all specifications. In particular, it is different from zero when additional controls are not included.

The point estimate of the treatment effect is around 0.01, which implies a $600 increase in debt of a treated firm with $60,000 of total bank debt. This is a very small increase in total debt considering that the average program loan was $9,440. Furthermore, taking into account that the program funded three fourths of each loan, this result implies that firm debt increased by 8 cents for every $1 of financing provided by the program.

This result suggests that program financing crowded out bank lending. An alternative interpretation is that program banks in fact expanded lending to target firms, but that treated firms were not credit constrained and they used program loans to substitute for other bank debt. In this scenario there would be no effect on the firms' total debt even when lending by program banks to target firms increased. To investigate this, 4-1 is estimated using the debt with program banks (instead of total bank debt) as the dependent variable, and the estimated coefficients are shown in column 4 of Table 17. The estimated treatment effect on program bank debt is statistically indistinguishable from the treatment effect on total bank debt, although it is significantly different from zero at a 1% level of confidence in all specifications. This suggests that the small treatment effect on total firm debt is not due to firms reducing borrowing from other banks, and corroborates the crowding out hypothesis.

6.3. Do bank characteristics matter?

This section investigates whether the effectiveness of the targeting program varies across different types of financial institutions. Section 0 argued that even if the bank is unconstrained by the targeting rule, it might decide optimally to allocate the marginal dollar of investment to the target sector. Small banks, for example, might be more likely to allocate the marginal loans to smaller firms. On the other hand, crowding out is more likely to happen if the bank is already allocating a significant fraction of its loan portfolio to the target sector. It is a priori ambiguous whether choosing institutions that specialize on the target sector to channel funds is going to improve the effectiveness of targeting.

To shed light on these issues I explore how the average treatment effect varies across banks of different ownership and size. Only crude divisions among types of banks can be used due to the small number of banks that participated in the program. Regarding ownership, out of the 28 non-government banks that participated in the program, 17 are privately owned and 11 are cooperative. Cooperative banks are non-for-profit institutions whose control rights are exercised by a large group of member firms. Member firms vote regularly to appoint a board of directors and most use the "one member, one vote" rule. Cooperative banks' objectives are usually expressed in terms of insuring the availability of credit to its members and lend to smaller firms that private, for-profit banks.

Regarding bank size, a bank is considered small if it has assets below the mean assets of participating banks. As a result 18 of the 28 banks considered in the sample are classified as small. Using these classifications I find that 35.7% of the program loans was given by cooperative banks and 32.7% by small banks.

Specification 4-1 is estimated again adding as explanatory variables the post treatment dummy interacted with a dummy equal to one if the firm received a program loan from a small bank or a cooperative bank, $Bsmall_{jt}$ and $Bcoop_{jt}$, the new explanatory variables are endogenous, so the estimated probability of treatment interacted with the bank type dummies are added as instruments when implementing 2SLS. The bank type dummies, and these dummies interacted with the eligibility dummy are also included as additional controls. This is to take into account the possibility that different types of banks lend to different types of firms. The resulting specification is estimated:

$$\begin{aligned}\Delta \ln(debt)_{jt} = {} & \beta.TREAT_{jt} + \beta_{small}.TREAT_{jt}.Bsmall_{jt} + \beta_{coop}.TREAT_{jt}.Bcoop_{jt} + \\ & + \gamma.ELIG_{j} + \varphi_1.POSTw1_{jt} + \varphi_2.POSTw2_{jt} + \varphi_3.POSTw3_{jt} + \\ & + \varphi_4.POSTw4_{jt} + \varphi_{small}.Bsmall_{jt} + \varphi_{coop}.Bcoop_{jt} + \theta X_{jt} + \nu_{jt}\end{aligned} \qquad 6\text{-}2$$

Including the interaction between the fitted probability of treatment and the bank dummies, $\hat{p}_{jt}$.Bsmall$_{jt}$ and $\hat{p}_{jt}$.Bcoop$_{jt}$, as instruments for the interacted treatment dummies.

The estimated parameters are reported in Table 18. Panels 1 and 2 show the estimations including the treatment by cooperative and treatment by small banks separately and in panel 3 both are included. A common feature to all specifications is that the not-interacted treatment dummy is not significantly different from zero. This means that the average treatment effect estimated in the previous subsection is entirely driven by program loans made by cooperative and small banks.

The treatment effect of the program on total firm debt when small and cooperative banks intermediated the program financing is positive and significant at the 1% level in all specifications. The magnitude of the estimated treatment effects still implies an important amount of crowding out. Back of the envelope calculations using the preferred treatment effect estimate (using specification in column 3 of Table 18) indicate that treatment by a cooperative or a small bank increased firm debt by $1,200.

6.4. Discussion

The results so far suggest that banks were able to circumvent the targeting rule by exploiting resource fungibility. Although banks made program loans to eligible firms, these loans do not appear to increase firm debt by more than a few cents per every dollar of loan extended. Going back to the plot of the debt evolution of treated firms of Figure 2, banks achieved this by substituting loans they were already giving to the treated firms for program loans. This loan re-labeling does not mean, however, that banks did not expand lending as a consequence of the additional financing. It is possible that banks expanded lending to firms other than the "program firms".

It is possible that banks used the additional financing to expand lending to other eligible firms apart from the program ones. For example, a bank that receives $100 in financing could lend $1 to each of 100 different eligible firms, but only call only one of these loans a program loan. In fact, the estimated treatment effect using the empirical strategy of this paper would give the same result as if the bank gave $100 to a single eligible firm. In both cases the average debt of eligible firms increases by $1, and the probability of treatment is 1/100, so the estimated treatment effect is $100. The IV estimate picks up the average effect of the program on the eligible group and then scales it up by the conditional probability of treatment. Thus, if banks were expanding lending to eligible firms other than the treated ones would be picked up by the estimates.

The empirical strategy of this paper would not pick up the effect if banks used the additional financing to expand lending to non-eligible firms. In fact, if this was the case the treatment effect could be biased downwards. To illustrate this point, suppose that a bank that receives $100 in additional financing extends $55 of extra credit to an eligible firm and $45 to an ineligible one. Comparing the debt evolution of eligible versus non-eligible firms within a bank would lead to estimate a treatment effect of $10.

This hypothesis can be explored by comparing the debt evolution of eligible firms that borrow from a bank that received program financing (financed bank), with that of eligible firms from a bank that did not. Specification 4-1 is estimated again restricting the sample to eligible firms, and using as an instrument for firm treatment, the interaction between the fitted probability of treatment and a dummy equal to one if the firm borrows from a financed bank ($\hat{p}_{jt}$.FINBANK$_j$). Note that the sample was already restricted to firms that borrow from banks that participated in at least one wave of the program. Table 13 shows that 11 banks in the sample received financing in the last waves of the program. The new estimate is based on the comparison between eligible firms that borrow from these banks, and eligible firms that borrow from banks that participated only in the initial waves of the program.

The estimated treatment effect is reported in panel A of Table 19, for both the total firm debt and the program firm debt (columns 1 and 2). The treatment effect appears to be three times the treatment effect found using eligibility as an instrument for treatment and implies that the program expanded treated firm debt by $1,800. This finding is consistent with banks expanding lending also to non-eligible firms, but still suggests that there is significant crowding out. Again, the estimate using program bank debt instead of total debt indicates that this crowding out is not due to debt substitution by the firm. These estimates, however, are based on the identification assumption that bank participation in the final waves of the program was exogenous. This is unlikely to be the case because bank participation into the program was voluntary. It is possible that banks decided to participate in the program when they identified profitable lending opportunities to eligible firms, which would lead to an upward bias in the estimated treatment effect.

To verify whether this identification hypothesis is valid, the treatment effect can be estimated using both the variation in eligibility and the variation in bank financing as instruments. That is, the difference in debt evolution between eligible and ineligible firms can be compared across banks that received and didn't receive program financing. The reduced form of this specification would represent a differences-in-differences-in-differences (triple differences) estimation. If bank participation is exogenous, the estimation using triple-differences will be the same as the estimation using eligibility as an instrument. The estimated treatment effect using the interaction of the fitted probability of treatment and financed bank dummies ($\hat{p}_{jt}$.FINBANK$_j$) is shown in panel B of Table 19. The treatment effect is 0.018 and significantly different from the estimate using eligibility alone, which is consistent with an upward bias caused by endogenous bank participation.[32]

The bottom line of this discussion is that even in the most optimistic scenario the program expanded the available credit of target firms by 15 cents per dollar of subsidized financing. Banks were able to circumvent the targeting rule and program financing crowded out lending to the target firms. The results suggest that this crowding out did not come from firms substituting debt from other banks, but from banks reallocating financing to other uses. There is some evidence that banks used subsidized program financing to expand credit to non-eligible firms.

7. Conclusions

This paper evaluates the treatment effect of an on-lending program in Argentina targeted to small firms. It deals with endogenous firm selection into the program by exploiting the exogenous source of variation in treatment provided by the eligibility rule of firms into the program. The findings suggest that the financing provided to banks significantly crowded out lending to the target group of firms. One dollar of subsidized financing to a bank resulted in an increase of target firm total debt of 8 cents.

[32] The specification estimated in section 6.2 does not require this assumption because it does not distinguish between banks that received and did not receive financing in each wave.

The crowding out is not the result of firms using program debt to substitute for other financing, in particular for financing from other banks. Total bank debt increases by a dollar for every additional dollar of financing a firm receives from the program. Thus, despite the crowding out, the program appears to have expanded the supply of bank credit to target firms. Furthermore, since program financing to firms was not subsidized, the evidence is consistent with program recipient firms being credit constrained to begin with.

The results also indicate that the characteristics of the intermediary bank matter. Crowding out is lower when the program is intermediated by banks that specialize in the target sector. In particular, small banks and banks with an explicit objective to maximize lending to borrowers (cooperative banks) expand lending to the target sector by 20 cents per dollar of financing.

The evidence of this paper suggests that on-lending programs can be subject to a substantial "leaky-bucket" effect. The targeting efficiency of these types of programs may be improved by selecting banks that specialize in lending to the target sector. But even though careful policy design may reduce the reallocation of resources away from the target sector, banks are likely to have many more instruments at their disposition to circumvent the rules than policymakers have instruments to give the right incentives. Trial and error will be a natural way to improve, but improvement will be achieved only if policy evaluation is done and disclosed, even when policies fail (Kremer 2003).

There are also reasons for optimism regarding on-lending programs. The paper provided some evidence that banks used the additional available financing to expand credit to firms that were not eligible to the program. This is a good outcome considering the alternative that, for example, banks used the subsidized program resources to pay dividends. Furthermore, if banks chose optimally to expand credit to previously constrained non-eligible firms, on-lending programs may still improve credit access. In fact, the targeting requirements could play an important role of bank self-selection into the program. Banks that already lend to the target sector will be more likely to participate in on-lending programs since it will be easier for them to circumvent the targeting rule. The marginal borrower of these banks is likely to be smaller and more constrained than the average firm, and thus a worthwhile target for policy intervention.

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9. Tables

Table 12

Bank Descriptive Statistics, by Program Participation (Thousands of $)

	All banks	Program banks	Non-program banks
Assets	1,095,287	543,985	1,244,598
	[2,396,431]	[599,719]	[2,667,256]
Loans/Assets	0.500	0.500	0.485
	[0.146]	[0.109]	[0.199]
Deposits/Assets	0.515	0.626	0.485
	[0.194]	[0.124]	[0.199]
Equity/Assets	0.133	0.133	0.133
	[0.135]	[0.130]	[0.137]
ROA	0.31%	0.14%	0.35%
	[1.22]	[1.12]	[1.24]

Means and standard deviations (in brackets) are reported. The statistics are calculated for a universe of 122 banks (26 program, 96 non-program) between 1998 and 2000. Program banks hold on average 10.2% of total assets, 11.2% of total loans and 12.1% of total deposits of the banking system.

Table 13

Wave, Resource and Bank Participation Distribution by Year

Year	Wave number	Amount of resources (in $1000s)	Number of participating banks
1993	1-2	12,908	16
1994	3	8,947	13
1995	4-5	21,471	15
1996	6-7	17,646	13
1997	8	5,972	7
1998	9	5,855	5
1999	10-11-12	13,405	11
Total		86,205	

Table 14

Descriptive Statistics of Program Loans to Firms

Variable	Mean	Std. Dev.	Min	Max	Median
Amount of loan ($)	9,438.4	4,322.2	500	26,666	10,000
Value of collateral posted	10,527.5	9,751.9	0	350,000	10,000
Interest rate (%)	13.74	1.302	11.5	16	13.5
Grace period (months)	2.15	4.32	0	47	0
Frequency of payments (months)	1.30	1.10	1	6	1
Number of payments	33.19	13.38	0	48	36
Duration (months)	35.60	11.72	1	48	36

* Source: Program database, Secretaría de la Pequeña y Mediana Industria, Ministry of Economy, Government of Argentina. The table is based on 12,192 observations where each observation corresponds to a program loan. Duration is the number of months that results when multiplying the frequency of payment times the number of payments.

Table 15

Firm Summary Statistics by Eligibility in the Final Sample

Sample of Firms	All	Eligible		Non-Eligible
		All	Treated	
	(1)	(2)	(3)	(4)
Number of Firms	65,366	37,712	717	27,654
Fraction Treated	0.011	0.019	1.000	0.001
Bank Debt	14,933	13,588	60,916	16,768
	[39,473]	[36,586]	[95,056]	[43,032]
Collateral	7,991	6,532	43,344	9,980
	[20,696]	[18,707]	[53,852]	[22,986]
Workers	5.635	5.507	6.126	5.809
	[1.369]	[1.278]	[2.632]	[1.466]
Sales	196,250	186,039	189,687	210,174
	[19,369]	[19,455]	[5,667]	[5,875]
Loan Size	8,796	8,404	22,398	9,331
	[23,138]	[23,510]	[65,686]	[22,611]
Number of Banks	1.863	1.708	2.907	2.076
	[0.860]	[0.811]	[1.416]	[0.880]
Performance Index	3.150	3.130	2.846	3.178
	[1.426]	[1.419]	[1.422]	[1.435]
Uncollateralized Debt (fraction)	0.6851	0.7171	0.4319	0.6414
	[0.375]	[0.358]	[0.3771]	[0.391]
Debt/Sales	0.0776	0.0762	0.3574	0.0796
	[0.203]	[0.203]	[0.5713]	[0.203]
Sales/Workers	36,228	35,041	33,797	37,847
	[8,220]	[7,880]	[13,301]	[8,396]

Averages and standard deviations (in brackets) are reported for a sample of firms with sales between $180,000 and $220,000. Firm data are averaged over the period between September 1999 and May 2000, for which monthly data is available. Eligible (non-eligible) firms are firms with 20 workers or less (more than 20 workers) and less (more) than $200,000 in sales. Treated firms are firms that received program loans.
The performance index is a credit rating index calculated by banks. It goes from 1 to 6, 1 meaning that all interest rate and capital payments have been paid and 6 means unrecoverable loan

Table 16

First Stage: Probit of Treatment Dummy on the Interaction Between the Eligibility Dummy and the Post Wave Dummy and Controls

	Probability of Treatment
	(1)
Eligible x Post wave 9	0.0225***
	[0.00632]
Eligible x Post wave 10	0.0169***
	[0.00596]
Eligible x Post wave 11	0.0165***
	[0.00655]
Eligible x Post wave 12	-0.0046
	[0.00607]
Eligible	0.4477***
	[0.3121]
Controls	Yes
Observations	1,325,957
Pseudo R-squared	0.588

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in brackets, clustered at the firm level. Marginal effects are reported. The eligible x Post marginal effects are reported times a thousand. Control variable marginal effects are omitted. The included controls are post wave dummies, a time trend for eligible and non-eligible firms, the square root of collateral, lagged number of loans, lagged number of bank relationships, the log of sales, lagged loan performance, and industry dummies.

Table 17

Treatment Effect on Debt Growth – OLS and 2SLS Estimation of Regression of Debt Growth on Treatment Dummy (Total Debt and Program Bank Debt)

Dependent Variable:	Δln(total bank debt)			Δln(prog. bank debt)
	OLS	IV	IV	IV
	(1)	(2)	(3)	(4)
Post Treatment	0.050***	0.009	0.011*	0.008***
	[0.004]	[0.006]	[0.006]	[0.002]
Eligible	-0.006***	-0.006***	-0.029***	-0.008***
	[0.001]	[0.001]	[0.001]	[0.001]
Additional controls	Yes	Yes	No	Yes
Observations	1,328,037	1,325,048	1,325,048	782,522
Firms	69,541	69,541	69,541	52,317
R-squared	0.05			

Robust standard errors in brackets, clustered at the firm level. * significant at 10%; ** significant at 5%; *** significant at 1%. The treatment dummy is one every month after a firm receives a program loan. The instrument is the fitted probability of treatment using 6-1. Additional controls are: trends for eligible and non-eligible firms, collateral posted by the firm (square root), lagged number of loans, loan performance, number of bank relationships, log sales and industry dummies.

Table 18

2SLS Estimation of the Treatment Effect on Debt Growth, by Type of Bank

Δln(total bank debt)	(1)	(2)	(3)
Post Treatment	0.002	0.002	0.001
	[0.007]	[0.007]	[0.007]
Post Treatment x	0.028***		0.021***
Cooperative bank	[0.006]		[0.006]
Post Treatment x		0.025***	0.019***
Small bank		[0.005]	[0.006]
Eligible	-0.006***	-0.006***	-0.006***
	[0.002]	[0.002]	[0.002]
Additional controls	Yes	Yes	Yes
Observations	1,325,048	1,325,048	1,325,048

Robust standard errors in brackets, clustered at the firm level. * significant at 10%; ** significant at 5%; *** significant at 1%. The treatment dummy is one every month after a firm receives a program loan. The instrument is the fitted probability of treatment using 6-1. Additional controls are: trends for eligible and non-eligible firms, collateral posted by the firm (square root), lagged number of loans, loan performance, number of bank relationships, log sales and industry dummies.

Table 19

2SLS Estimation of the Treatment Effect on Debt Growth – Using Both Cross-Bank Comparison of Eligible Firms and Eligibility as Instruments

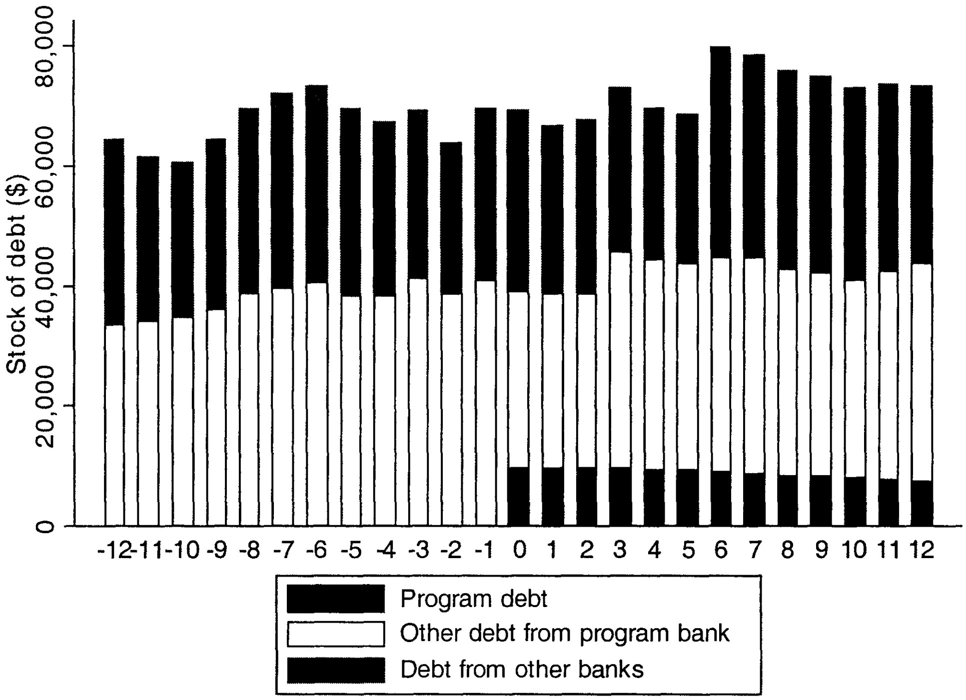
Dependent Variable:	Δln(total bank debt)	Δln(prog. bank debt)
	(1)	(3)
A. Eligible Firm Sample, Across Bank Comparison		
Post Treatment	0.030***	0.024***
	[0.011]	[0.007]
Financed Bank	0.073***	0.132***
	[0.002]	[0.002]
Additional controls	Yes	Yes
Observations	711,269	415,682
Firms	37,796	28,057
B. Triple Differences, using Across Bank Comparison		
Post Treatment	0.0178**	0.0212***
	[0.007]	[0.005]
Eligible	-0.015***	-0.001
	[0.001]	[0.001]
Financed Bank	0.053***	0.177***
	[0.002]	[0.002]
Eligible x Financed Bank	0.005***	-0.001
	[0.001]	[0.001]
Additional controls	Yes	Yes
Observations	1,325,048	782,522
Firms	69,541	52,317

Robust standard errors in brackets, clustered at the firm level. * significant at 10%; ** significant at 5%; *** significant at 1%. The treatment dummy is one every month after a firm receives a program loan. The instrument is the fitted probability of treatment using 6-1. Additional controls are: trends for eligible and non-eligible firms, collateral posted by the firm (square root), lagged number of loans, loan performance, number of bank relationships, log sales and industry dummies. A financed bank is a program bank that participated in at least one of the four waves f the program. Thus, the Financed Bank dummy is equal to one if a firm received a loan from a financed bank during the sample period.

10. Figures

Figure 11

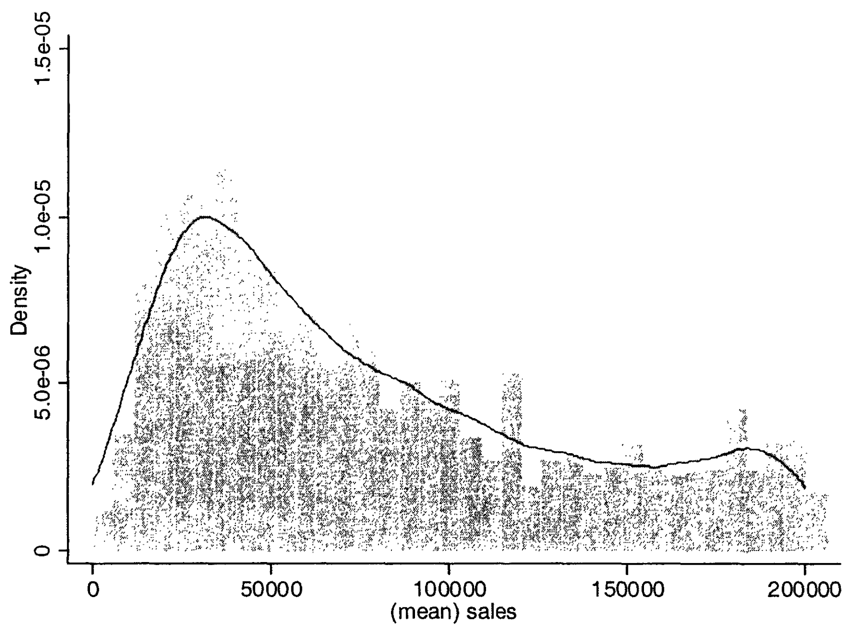
Evidence on Loan Re-Labeling: Monthly Debt Evolution of the Firms that Received Program Loans, by Source



Source: own calculations using MYPES program database and CDSF credit bureau data. Based on a sample of 2,596 firms that received program loans after January 1996. The horizontal axis measures time in months relative to the moment of reception of the program loan (0 is the month the program loan was received by the firm).

Figure 12

Panel 1: Histogram and kernel distribution of the reported sales of treated firms



Panel 2: Histogram and kernel distribution of the predicted sales of treated firms

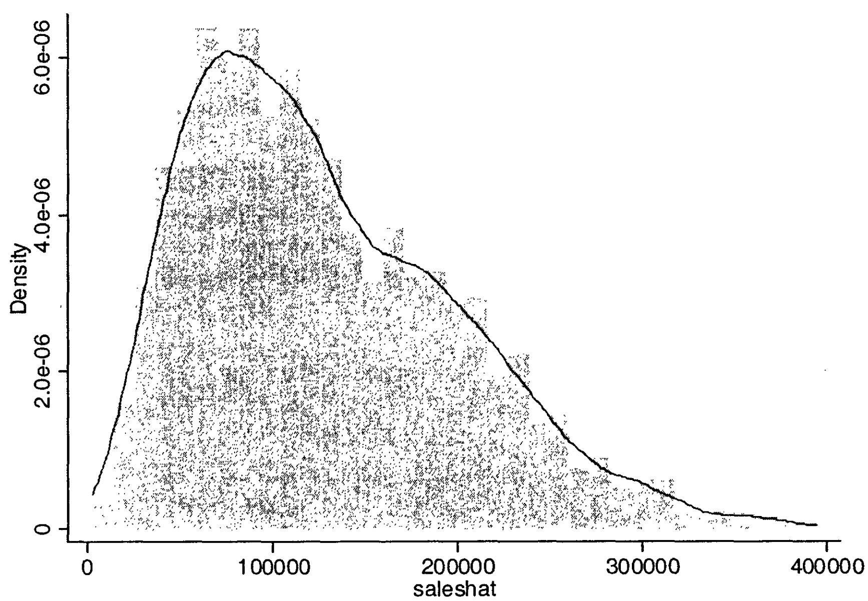
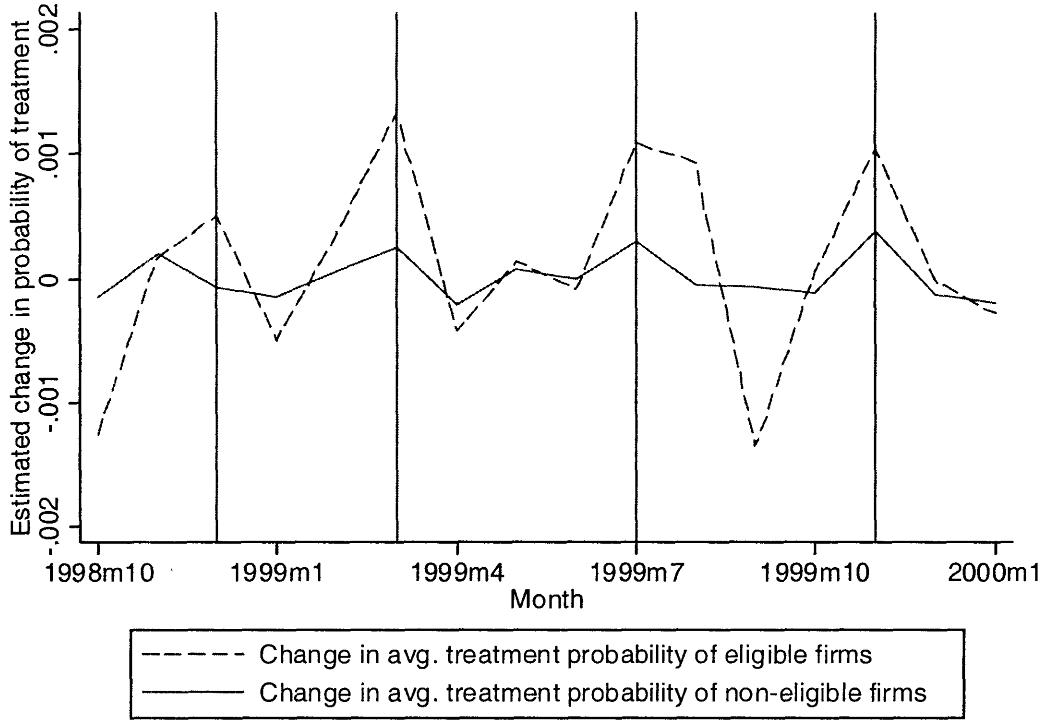


Figure 13

Treatment Effect on Target Firms (First Stage) – Change in the Predicted Probability of Treatment, by Firm Eligibility to the Program



* The vertical lines mark the beginning of the four waves considered in the sample

CHAPTER THREE
Credit Information Disclosure and Financial Distress

1. Introduction

There has been a longstanding interest among academics and practitioners alike on whether markets will lead to the disclosure of the optimal amount and quality of information. Originally focused on disclosure by publicly traded firms (Jovanovic 1982; Diamond 1985; Fishman and Hagerty 1990; Diamond and Verrecchia 1991; Admati and Pfleiderer 2000; Boot and Thakor 2001), the literature has more recently also pondered whether private information about firm creditworthiness collected by financial institutions will be optimally shared and disclosed.

The theoretical predictions on the effect of information disclosure depend heavily, however, on the underlying assumptions that are made regarding the functioning of credit markets. One view is that information disclosure will reduce informational asymmetries between lenders and borrowers, and reduce the monopoly power of financial institutions over borrower quality information. Under this view disclosure unambiguously enhances credit market efficiency (Pagano and Jappelli 1993; Padilla and Pagano 1997). Another view is that disclosure may reduce efficiency in the allocation of credit by exacerbating coordination problems among multiple lenders (Morris and Shin 2001). In this view, short term liquidity problems may trigger inefficient credit foreclosure and financial distress when noisy public signals about firm creditworthiness are made available. More public information may be bad in this context because lenders are trying to guess what other lenders are going to do in order to make credit decisions. More precise public signals might lead to increased coordination of rational lenders, even on the wrong course of action from the social welfare point of view.

Despite the ambiguous theoretical predictions and the deep involvement of the government in the collection, processing and disclosure of credit market information around the world (Miller 2003) there is little systematic evidence on how disclosure affects firm finance. This paper explores empirically the link between information disclosure and firm liquidity management. It does so by exploiting a peculiar policy experiment in Argentina, where the

Central Bank (CBA) disclosed monthly information on the credit situation of all debtors of the domestic financial system. Credit history data had been collected by the CBA since 1995 in a Public Credit Registry to monitor the quality of bank loan portfolios. Access to credit history data contained in the Registry was originally limited to financial institutions, and information about small debtors (debt below $200,000) was only available in a high level of aggregation. But starting January 1998, information about the amount and repayment status of every credit transaction of every individual or firm became available in the CBA web page at no cost.

The effect of information disclosure is identified by comparing the evolution of firm financial statements of firms above and below the $200,000 of debt threshold. The results indicate that firms with low ratios of liquid assets to short term debt were significantly affected by disclosure. Firms that have low liquidity ratios before disclosure experience a decline in both trade and bank debt, and an increase in the average cost of external financing relative to firms with high liquidity ratios. This evidence suggests disclosure affects external financing of firms that are more likely to face liquidity shortages.

The more important question is whether liquidity shortages will lead to more costly external finance independently of the true underlying profitability of firms. I explore this question by looking at whether firms that received an unexpected and permanent shock to their profitability were disproportionately affected by disclosure. The devaluation of the Brazilian Real in 1998 produced and unexpected decline in sales and profitability of firms in the Argentine food industry, a major exporter to Brazil. The results indicate that the exogenous profitability shock did not significantly affect the external finance of firms whose information was disclosed, once initial firm liquidity position is controlled for. Disclosure appears to have affected firms with weak liquidity positions regardless of their profitability. These results are consistent with Morris and Shin's inefficient foreclosure prediction.

I also find some evidence that the cost of capital and firm liquidity management policies changed in a way that is consistent with the lender coordination story. The results suggest that the average cost of external financing of liquidity poor firms increased relative to other firms after credit histories were disclosed. This is the expected reaction to an increase in the probability of default and foreclosure due to the information regime change. Also, liquidity poor firms increase their cash holdings after the change in disclosure policy. Hoarding cash provides insurance against liquidity shortages (Almeida, Campello and Weisbach 2004), so increasing cash holdings may be the optimal response to a simultaneous increase in the probability of financial distress and the cost of external financing.

Finally, the results suggest that the policy disclosure might have had a disproportionate effect on firm sales and profits. Since the need to insure against liquidity shortages comes at a time when external finance becomes scarcer, cash hoarding appears to happen at the expense of inventories, trade credit and other liquid assets related to sales. This may explain why the average effect of disclosure on external financing (averaged over all firms, high and low liquidity) is insignificant, but disclosure has a significant negative impact on average firm sales in the sample. Cash hoarding amplified the impact of disclosure on firm sales beyond the direct effect due to the decline in external financing.

The results of this paper suggest that the disclosure of information about small firm creditworthiness is intrinsically linked to liquidity management. Disclosure may have exacerbated the likelihood of financial distress and the costs firms must face to avoid it. This evidence highlights the potential efficiency cost of information disclosure that has spurred the recent debate on policy design on transparency (Morris and Shin 2002; Economist 2004; Svensson 2005).

The rest of the paper proceeds as follows. Section 2 describes the details of the credit information disclosure policy. Section 3 discusses the potential theoretical effects of information disclosure on firm finance and discusses the existing evidence. Section 4 presents the identification strategy, and section 5 describes the data sources. Section 6 presents the results and section 7 concludes.

2. The Disclosure Policy

The banking sector regulation in Argentina requires financial institutions to provide detailed information on every individual credit transaction. The Central Bank of Argentina (CBA) centralized this information in a Public Credit Registry, which it then used assess the quality of the loan portfolios of banks. The information on all but the smallest borrowers (less than $200,000) was also made available to all financial institutions. In 1998 the Central Bank introduced a new disclosure policy that the data in the Credit Registry publicly and freely available through the CBA web page. By introducing the name or tax identification number of an individual or firm, the web page would provide the amount of debt outstanding by the individual/firm with every financial institution and the repayment status.

As in most countries, private credit bureaus in Argentina provided credit information about businesses and firms even before the disclosure policy was implemented. There was one dominant firm in the market for credit reporting, who had struck deals with the major banks in the financial system to share information about their creditors in exchange for risk analysis services. According to executives of this firm, only large foreign owned banks formed part of the information sharing agreement. Information about credit with local owned, regional, cooperative and government owned banks was not publicly available before 1998. In fact, the private reporting firm started incorporating the information in the Public Credit Bureau in their credit analysis, when the CBA began to make it public in 1998. The change in disclosure policy appears to have changed both the price and scope of credit history information that was publicly available.

I interviewed credit executives of financial institutions in Argentina regarding the perceived effects of the disclosure policy. Neither reduced moral hazard/adverse selection, nor increased business stealing and poaching by competing banks appeared as relevant concerns after disclosure, even for smaller banks whose information was made public in the Public Credit Registry after 1998. The consensus response was that disclosure affected the expected return of small firm and individual loan portfolios. After disclosure, borrower short-term liquidity problems became more likely to turn into solvency problems and default.

3. Background: Information Disclosure and Firm Finance

The public disclosure of firm credit histories may affect firm finance and credit markets in several distinct ways. First, information disclosure allows better prediction of default risks by lenders. It allows lenders to price loans better according to relevant borrower characteristics, like past defaults or actual amount of borrowing form different sources (Bizer and DeMarzo 1992; Padilla and Pagano 1997). Disclosure, thus, reduces adverse selection and rationing. The effect of disclosure on each individual firm due to the reduction of adverse selection will depend on the characteristics of each firm. The interest rate will go up for borrowers whose bad quality is revealed by disclosure, and the opposite will occur with good quality firms. Also, some borrowers that were previously rationed from credit might gain access to it. The average effect of disclosure of the amount of credit and interest rates will depend on the composition of borrowers in the market after disclosure.

Second, information disclosure may affect borrower behavior ex post by increasing the cost of default. Borrowers might choose to default strategically when there is a positive probability that the default will go unnoticed by other lenders. In the extreme where all lenders become fully aware of the entire credit history of the borrower, this probability is zero and the incentives for strategic default decrease. This should cause the average interest rate lenders require to break even to drop, and the drop in the price of financing should be larger for those firms whose incentives to engage in strategic default are larger (more leveraged, younger/smaller, less profitable firms, with less collateralized loans). The reduction in moral hazard should unambiguously reduce the average interest rates and expand the supply of credit.

Finally, disclosure of credit histories makes public a noisy signal of the private information held by financial institutions. On the one hand, this may reduce the informational rents that lenders may extract from their borrowers. The increased competition for borrowers ex post lowers the interest rates, but also reduces the incentives of banks to invest in getting to know new borrowers (Petersen and Rajan 1995). On average this will both decrease the cost of credit and increase the average quality of borrowers, both due to the drop in price and the exclusion of lower quality new borrowers. On the other hand, public noisy signals of borrower quality increase lender coordination. The downside of increased coordination is that a bad public signal about a borrower quality might lead to inefficient foreclosures and increased liquidity risk when the soundness of a firm depends on multiple independent lenders providing credit (Morris and Shin 2001; 2002). This reduces the incentives of lenders to renegotiate loans even if they know borrowers confront transitory shocks, which may lead to higher interest rates, and less access to credit of firms that are more likely to confront short term liquidity problems.

These inefficiency consequences of public disclosure are not robust to changes in the assumptions about the production technology or the parameters on the model, as noted by Angeletos and Pavan (2004) and Svensson (2005) among others. Assessing the effects of increased coordination on efficiency remains an empirical issue.

The existing evidence of the effects of credit information disclosure comes from cross-country comparisons. Jappelli and Pagano (2000) find a positive correlation between the bank lending to GDP ratio and credit information sharing in a country.[33] Galindo and Miller (2001) find that in countries with a higher quality credit registries, firms have a higher debt/capital ratios and a lower sensitivity of investment to internal cash. Love and Mylenko (2003) find that the existence of private credit registry in a country, and not a public one, is related to lower financing constraints as perceived by managers.

As with most cross-country studies, this evidence is hard to interpret. It is impossible to distinguish whether the observed correlations are due to a causal effect of credit registries and credit information disclosure on credit markets; or if instead credit registries are setup in countries with higher financial development, growth and investor protection, all of which are related to better functioning credit markets.

4. Identification Strategy

This paper exploits a change in disclosure policy in Argentina to identify the effect of disclosure. Looking at how firm finance changes after new information is disclosed allows controlling for firm specific characteristics that may bias the cross sectional analysis. I also exploit the fact that disclosure affected a limited and well defined group of firms and not others for the identification strategy. The issue with the time series analysis is that it is hard to tell what would have been firm behavior in the absence of the change in disclosure policy. By using firms that were not affected by the disclosure policy directly as a control group, this unobserved counterfactual can be accounted for. The estimation of the effect of disclosure on firm outcome, Y, will be based on the comparison of the change in Y for firms affected

[33] Their measure of information sharing is based on classifying countries according to whether financial institutions share information through private credit bureaus and public credit registries or not.

by disclosure relative to the change in Y for forms not affected by it (differences-in-differences estimator).

I use the following baseline specification to estimate the effect of disclosure on different firm outcomes:

$$Y_{it} = \beta DDisclose_i \,.\, D1998_t + \gamma DDisclose_i + \Sigma_k \zeta_k Dindustk_i + \alpha_t + \varepsilon_{it} \qquad (4\text{-}1)$$

where Y represents the outcome (e.g. log of debt, assets or sales, ROA, etc.) of firm i at year t. $DDisclose_i$ is a dummy equal to one if firm i's credit history was private information before 1998 and disclosed by the Public Credit Registry afterwards. That is, a dummy equal to one if firm i had a total bank debt of less than $200,000 before 1998. D1998 is a dummy equal to one for year 1998. A full set of year dummies is included. Some specifications will also include a full set of a six-digit industry sector dummy.

The coefficient of interest, β, represents the average effect of disclosure on firms affected by disclosure in the sample. The advantage if this approach is that it allows exploring how the effect of disclosure varies in the cross section of firms, in particular regarding the liquidity position of firms before disclosure.

5. Data

I use detailed balance sheet and earnings report information of a sample of small manufacturing firms in Argentina. The information was collected by the Unión Industrial Argentina (UIA), a non-for-profit association of manufacturing firms founded in 1887 to promote and monitor this sector's economic activity. The UIA collects regularly information on a representative sample of 1,000 manufacturing firms containing data on employment, productivity, exports, financing conditions, investment, and corporate governance. During years 1996, 1997 and 1998, the UIA also collected balance sheet and earning report information from a random sub-sample of 500 firms.

For the purposes of this paper I need to exploit the panel structure of the balance sheet data, so I keep in the sample firms for which there is information of at least two years. The final sample used to for the estimations is an unbalanced panel of 157 firms and 333 observations over three years. Table 20 shows the mean assets of all firms in the original and the restricted sample, by year. The restricted sample has about 50% less observations per year in all years, and the average assets of the firms in the original and the restricted sample are not statistically different for any of the years.

The descriptive statistics of the firms in the final sample are shown in Table 21, by disclosure status. Of the 157 firms in the final sample, 90 had bank debt before 1998 below $200,000 and thus had their credit information disclosed in 1998. These firms tend to be smaller, hold more cash, and are less leveraged that the comparison group of firms. They also have lower average financing costs and higher return on assets than firms in the control group. The differences in size, cash holdings, financing cost and ROA between the two groups are not significant once the firm industry is controlled for.[34]

6. Results

6.1. Average Effect of Disclosure

The estimated parameters of the baseline specification (4-1) are shown in Table 22 (odd columns). The results indicate that there is no statistically significant effect of disclosure on trade debt or bank debt, on the average external financing cost or firm sales. As suggested by the discussion in section 3, the average effect of disclosure will depend on which mechanism is prevalent and the composition of firms in the sample. Thus, there is no clear prediction of what will happen with firm finance and outcome on average.

This baseline specification does not fully control for all the observable firm characteristics that may affect the lenders pricing and decision to lend ex post. To deal with this issue I include a full set of industry dummies in specification (4-1). The 157 firms in the sample can be classified into 84 six-digit sector codes. Short of including firm fixed effects, this controls for the broadest set of time invariant firm characteristics without imposing more structure in the specification. The even numbered columns of Table 22 show the estimated parameters using the full set of industry dummies. All the effects become stronger and more precisely estimated when the industry dummies are included. Even though the results suggest that there is a decline in trade credit and bank debt, the results are not significant at the 10% level

[34] I run a cross sectional regression of assets, cash, average financing cost and ROA on a dummy equal to one if the firm belongs to the disclosure group and a full set of dummies for the six-digit sector code. The coefficient on the disclosure dummy is not significant in all specifications.

of confidence. There is a statistically significant negative effect of disclosure on firm sales and trade credit issued by the firms.

This prima facie evidence on the average effect of disclosure appear contradictory if we take the point estimates seriously. They show a significant decline in firm sales with no apparent change in the average amount or cost of external financing. I turn next to explore how the effect of disclosure varies in the cross section of firms.

6.2. Disclosure and Distress

The discussion in section 3 suggested that credit history disclosure would affect the probability of financial distress due to lender coordination. To explore this issue I classify firms according to how likely they are to face a short term liquidity problem. A firm is likely to have a short term liquidity problem when it has a low ratio of liquid assets to short term liabilities. A firm is defined to have low liquid assets (low cash) when the firm has a liquid asset (cash) to short term liability ratio below the median before 1998. I include the interaction of the disclosure times 1998 dummy with a dummy equal to one if a firm has low liquidity (cash) to specification (4-1):

$$\begin{aligned} Y_{it} = {} & \beta_1 \, DDisclose_i \, . \, D1998_t + \beta_2 \, DDisclose_i \, . \, D1998_t \, . \, LowLiq_i + \\ & + \gamma_1 DDisclose_i + \gamma_2 LowLiq_i + \gamma_3 D1998_t \, . \, LowLiq_i + \textstyle\sum_k \zeta_k Dindustk_i + \alpha_t + \varepsilon_{it} \end{aligned} \tag{6-1}$$

The β_2 coefficient will represent how disclosure affected firms with low liquid positions relative to firms with high liquid positions. The low liquidity dummy and the low liquidity dummy interacted with the 1998 dummy are also included in the specification. This takes into account both time invariant characteristics of low liquidity firms and shocks in 1998 that are specific to low liquidity firms. I also include the full sets of 6-digit sector code and year dummies.

The results using firm liabilities as the dependent variable in specification (6-1) are shown in Table 23. The point estimate of the effect on high liquidity firms is positive and not significant in all specifications. The estimated effect of disclosure on low liquidity firms is negative and significant. This result indicates that firms with low liquid positions face a decline in their external financing relative to other firms when their credit histories are disclosed.

These results are consistent with the hypothesis that firms that are more likely to face short term liquidity problems are negatively affected by disclosure. They also suggest that disclosure affects credit from financial institutions as well as credit from less sophisticated lenders like trade creditors.

These results do not allow to distinguish, however, whether the decline in credit comes as a consequence of a short term liquidity problem, or occurs because creditors anticipate problems and either reduce their willingness to extend credit or increase the price of financing. Both effects are will be observationally equivalent given the low frequency of balance sheet data.

I address this issue by comparing firms in the cross section that were more likely to receive and exogenous shock to sales. The devaluation of the Brazil real in February of 1998 had a negative impact on the Argentine export industry. Argentina is a major exporter of foodstuff to Brazil, so it is to be expected that sales of the food industry were negatively affected during 1998. Figure 14 shows the yearly evolution of average sales of the food industry firms in the sample and the rest of the firms. The figure shows how sales of food industry firms in the sample dropped in 1998, while sales of the other industry increased during the same year. I repeat the estimation of specification (6-1) adding an interaction between the disclosure and 1998 dummies with a dummy equal to one if the firm belongs to the food industry. The interaction between the 1998 dummy and the food industry dummy is also added as a control. The estimated parameters, shown in Table 24, indicate the there is still a negative and significant effect of disclosure on low liquidity firms. Disclosure appears to have a negative effect on the external financing of firms with a low liquidity position, regardless of whether they receive a negative shock to sales or not. Disclosure also seems to affect negatively firms that receive negative sales shocks, but these effects are not statistically significant once firm initial liquidity position is considered.

6.3. Cost of External Financing, Returns and Assets

The results suggest then that firm external financing drops when firms are more likely to face liquidity problems. I next explore what drives this drop in external financing, how it affects firm sales and returns, and how it modifies the asset composition of firms.

I estimate specification (4-1) using as the dependent variable the financial expenditure to total liability ratio of the firm. This provides a measure of the average cost of external financing. The estimated parameters, shown in column 1 of Table 24, indicate that disclosure increases the cost of external financing of firms that are more likely to face short-term liquidity problems. This result is consistent with disclosure increasing the probability that a short term liquidity shortage will lead to default.

Columns 3 through 6 of Table 24 show the estimated parameters using the log of sales and return on assets as the dependent variable. The results suggest that disclosure has a negative and significant effect on the returns and sales of firms with less liquid positions. This is to be expected given the decline in external financing ad increase in financing costs induced by disclosure.

Table 26 shows the effects of disclosure on firm assets. The results indicate that total firm assets of low liquidity firms drop relative to other firms. The results also indicate that all the effect on assets is due to a decrease in firm liquid assets, and that fixed assets are not affected. Looking at different categories of liquid assets shows that trade credit and inventories (not shown) decrease, while cash holdings increase.

The expansion in cash holdings of firms with low liquid positions is also consistent with disclosure increasing the likelihood that a short-term liquidity shortage will lead to financial distress. Firms appear to be hoarding cash to insure against the risk of a liquidity shortage. Moreover, this finding may explain the apparent contradiction in the results regarding the average effects of disclosure. The findings there indicated that disclosure had on average no significant effect on the amount or cost of external financing, but a significant effect on sales. Low liquidity firms increased their cash holdings in a period where credit supply shrank, which may have further affected the ability of firms to maintain inventories and issue trade credit, and as a result amplified the negative effect of disclosure on sales.

7. Conclusions

This paper evaluates empirically the implications of credit history disclosure on the corporate finance of small firms. It looks at a peculiar policy intervention in Argentina, where the Central Bank made freely and publicly available the credit records of all firms and individuals with local financial institutions. Credit information for firms with less than $200,000 of debt with financial institutions was made available through the web page of the Central Bank. Balance sheet and earnings report information of a sample of manufacturing firms I assess the effect of disclosure on different firm outcomes. The effect of disclosure is identified by comparing the change in firm behavior after disclosure of firms above and below the disclosure threshold.

There is weak evidence that disclosure reduced firm external financing on average, although firm sales did drop significantly after disclosure. These results are significant only when a wide range of firm characteristics are accounted for by introducing a six-digit sector code dummies.

The effect of disclosure appears to be large and significant on firms that are more likely to face liquidity shortages. These firms face a significant drop in external financing from both banks and trade creditors, and this drop occurs regardless of whether firms have a liquidity shortage or not. Credit markets seem to anticipate liquidity problems due to disclosure and appear to price this accordingly, since the average cost of external financing of firms in tight liquid positions increases after disclosure.

Disclosure has a negative effect on firm assets, sales and returns of firms with low liquid positions. This is in part because of the decrease in availability/increase in cost of external financing. But firms also increase their cash holdings after disclosure. Since disclosure both increases the cost of financial distress and increases the cost of external financing, hoarding cash may be the optimal way to insure against distress.

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9. Tables

Table 20

Comparison of the Average Firm Assets, Leverage and ROA in the Original Sample and the Restricted Sample, by Year

	Original Sample			Restricted Sample		
Year	1996	1997	1998	1996	1997	1998
Observations	207	273	185	101	136	96
Assets	2,870,905	4,053,354	4,831,447	2,964,147	3,842,656	4,778,890
	[3,676,388]	[5,254,539]	[6,713,453]	[4,131,420]	[5,608,273]	[6,948,118]
Leverage	0.501	0.476	0.499	0.478	0.473	0.447
	[0.347]	[0.289]	[0.354]	[0.222]	[0.232]	[0.293]
ROA	0.064	0.084	0.070	0.093	0.084	0.071
	[0.180]	[0.138]	[0.183]	[0.148]	[0.146]	[0.212]

Standard deviation in brackets. There are 500 firms in the original sample and 157 in the restricted one. The restricted sample keeps firms in the original sample that are observed at least two years. The difference in the average assets and leverage (liabilities/sales) of firms on both samples is not statistically different from zero in any year. The ROA of firms in the restricted sample is higher that in the original one in 1996.

Table 21

Descriptive Statistics of Firm Financial Information

	All			Disclose			Control		
Variable	Average	Std. Dev.	Median	Average	Std. Dev.	Median	Average	Std. Dev.	Median
# Firms	157			90			67		
Assets	3,862,141	5,547,575	2,086,970	2,984,316	5,969,457	1,166,694	5,041,309	4,714,608	3,582,014
Sales	4,270,922	5,206,592	2,492,961	3,313,976	5,457,692	1,513,404	5,556,372	4,580,635	3,852,033
Cash/Assets	0.093	0.094	0.061	0.118	0.110	0.076	0.060	0.051	0.043
Liquid Assets/Assets	0.638	0.204	0.656	0.680	0.213	0.728	0.581	0.178	0.587
Debt/Sales	0.414	0.312	0.324	0.349	0.268	0.265	0.502	0.345	0.396
Trade Debt/Sales	0.155	0.123	0.133	0.131	0.104	0.109	0.186	0.140	0.160
Bank Debt/Sales	0.104	0.145	0.055	0.042	0.066	0.024	0.188	0.178	0.148
Financing Costs/Liabilities	7.2%	6.0%	5.8%	6.1%	6.1%	4.1%	8.6%	5.7%	7.5%
ROA	13.2%	11.8%	12.0%	14.1%	13.4%	14.5%	12.0%	9.1%	11.6%

There are 157 firms in the sample, 90 of which had their credit history disclosed in 1998 (bank debt above $0 and below $200,000 before 1998) and the rest are in the control group.

Table 22

The Average Effect of Disclosure on Firm Debt (Trade and Bank), Financing Cost, Sales and Trade Credit

	Log Trade Debt		Log Bank Debt		Average Financing Cost		Log Sales		Log Trade Credit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
DDisclose x D1998	-0.102	-0.242	-0.131	-0.205	-0.015	-0.023	-0.045	-0.273*	-0.230	-0.418*
	[0.270]	[0.249]	[0.340]	[0.331]	[0.030]	[0.034]	[0.169]	[0.164]	[0.225]	[0.232]
DDisclose	-1.253***	-1.153***	-2.317***	-2.317***	-0.005	0.018	-0.909***	-0.735***	-1.137***	-0.875***
	[0.224]	[0.359]	[0.187]	[0.264]	[0.019]	[0.031]	[0.147]	[0.242]	[0.197]	[0.311]
D1997	0.271*	0.124	0.074	0.053	-0.019	-0.023	0.194**	0.08	0.200*	0.074
	[0.151]	[0.171]	[0.139]	[0.172]	[0.016]	[0.019]	[0.086]	[0.084]	[0.117]	[0.104]
D1998	0.442**	0.361	0.430**	0.33	0.009	-0.009	0.365***	0.336**	0.381**	0.281
	[0.216]	[0.233]	[0.197]	[0.226]	[0.022]	[0.023]	[0.133]	[0.150]	[0.156]	[0.202]
Industry Dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	330	329	260	259	318	317	332	331	332	331
R-squared (adjusted)	0.17	0.53	0.47	0.72	0.01	0.35	0.19	0.59	0.16	0.49

Table 23

The Effect of Disclosure on External Financing (Total, Bank Debt, Trade Debt) by Firm Liquidity

	Log Total External Financing		Log Trade Debt		Log Bank Debt	
	(1)	(2)	(3)	(4)	(5)	(6)
DDisclose x D1998	0.256	0.104	0.305	0.118	0.055	0.427
	[0.295]	[0.250]	[0.344]	[0.301]	[0.506]	[0.496]
DDisclose x D1998 x LowCash	-0.381		-0.631		-0.340	
	[0.268]		[0.388]		[0.601]	
DDisclose x D1998 x LowLiqAs		-0.550**		-0.860**		-0.789*
		[0.273]		[0.356]		[0.421]
DDisclose	-1.015***	-1.006***	-1.042***	-0.960***	-2.340***	-2.219***
	[0.269]	[0.290]	[0.353]	[0.359]	[0.262]	[0.283]
LowCash	-0.796***		-0.793**		-0.133	
	[0.259]		[0.352]		[0.234]	
LowLiqAs		0.467**		0.749**		0.397
		[0.231]		[0.337]		[0.363]
LowCash x D1998	0.373*		0.491**		0.148	
	[0.215]		[0.235]		[0.368]	
LowLiqAs x D1998		0.057		0.089		-0.435
		[0.207]		[0.269]		[0.358]
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry Dummies (84)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	332	332	329	329	259	227
R-squared (adjusted)	0.65	0.62	0.57	0.56	0.72	0.70

Robust standard errors in brackets, clustered at the firm level. * significant at 10%; ** significant at 5%; *** significant at 1%. Each observation corresponds to a firm year observation. DDisclose is a dummy equal to one if the firm had less than $200,000 in bank debt before 1998. D1998 is a dummy equal to one for year 1998. LowCash (LowLiqAs) is a dummy equal to one if firm i had a cash (liquid assets) to short term liabilities ratio below the median before 1998.

Table 24

The Effect of Disclosure on External Financing (Total, Bank Debt, Trade Debt), by Firm Liquidity and Unexpected Sales Shock (Food Industry Dummy)

	Log Total External Financing	Log Trade Debt	Log Bank Debt
	(1)	(2)	(3)
DDisclose x D1998	0.189	0.046	0.564
	[0.272]	[0.336]	[0.506]
DDisclose x D1998 x LowLiqAs	-0.562**	-0.848**	-0.758*
	[0.273]	[0.359]	[0.408]
DDisclose x D1998 x DFoodInd	-0.618	0.489	-1.452
	[0.733]	[0.927]	[1.315]
DDisclose	-1.006***	-0.959***	-2.221***
	[0.291]	[0.361]	[0.281]
LowLiqAs	0.471**	0.746**	0.384
	[0.232]	[0.339]	[0.368]
D1998 x LowLiqAs	0.072	0.077	-0.357
	[0.209]	[0.270]	[0.391]
D1998 x DFoodInd	0.536	-0.399	1.161
	[0.570]	[0.759]	[0.864]
Year Dummies	Yes	Yes	Yes
Industry Dummies (84)	Yes	Yes	Yes
Observations	332	329	227
R-squared	0.62	0.56	0.70

Robust standard errors in brackets, clustered at the firm level. * significant at 10%; ** significant at 5%; *** significant at 1%. Each observation corresponds to a firm year observation. DDisclose is a dummy equal to one if the firm had less than $200,000 in bank debt before 1998. D1998 is a dummy equal to one for year 1998. LowLiqAs is a dummy equal to one if firm i had a liquid assets to short term liabilities ratio below the median before 1998. DfoodInd is a dummy equal to one if the firm is in the food industry, which received a negative shock in sales due to the Brazilian devaluation in 1998.

Table 25

The Effect of Disclosure on Firm Outcomes (Sales, Return on Assets, Average Financing Cost) by Firm Liquidity

	Average Financing Cost		Log Sales		ROA	
	(1)	(2)	(3)	(4)	(5)	(6)
DDisclose x D1998	-0.038*	-0.013	0.041	-0.113	-0.001	0.037
	[0.020]	[0.020]	[0.263]	[0.215]	[0.051]	[0.051]
DDisclose x D1998 x LowCash	0.039**		-0.547**		-0.038	
	[0.020]		[0.277]		[0.074]	
DDisclose x D1998 x LowLiqAs		0.014		-0.357		-0.099*
		[0.020]		[0.243]		[0.057]
DDisclose	-0.030**	-0.037**	-0.736***	-0.726***	-0.006	-0.005
	[0.013]	[0.015]	[0.243]	[0.242]	[0.035]	[0.036]
LowCash	0.001		-0.03		-0.054	
	[0.012]		[0.211]		[0.037]	
LowLiqAs		-0.031*		0.019		-0.032
		[0.017]		[0.212]		[0.032]
D1998 x LowCash	-0.008		0.001		0.013	
	[0.017]		[0.211]		[0.047]	
D1998 x LowLiqAs		0.043***		0.082		-0.036
		[0.014]		[0.180]		[0.048]
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry Dummies (84)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	325	325	331	331	325	325
R-squared (adjusted)	0.38	0.40	0.60	0.60	0.48	0.49

Robust standard errors in brackets, clustered at the firm level. * significant at 10%; ** significant at 5%; *** significant at 1%. Each observation corresponds to a firm year observation. ROA is the ratio of pre-tax profits plus depreciation to total assets. The dependent variable in specifications (5) and (6) is the ration of financial expenditures divided by total firm liabilities. DDisclose is a dummy equal to one if the firm had less than $200,000 in bank debt before 1998. D1998 is a dummy equal to one for year 1998. LowCash (LowLiqAs) is a dummy equal to one if firm i had a cash (liquid assets) to short term liabilities ratio below the median before 1998.

Table 26

The Effect of Disclosure on Assets (Total, Fixed, Liquid, Cash and Trade Credit) by Firm Liquidity

	Log Assets		Log Fixed Assets		Log Liquid Assets		Log Cash		Log Trade Credit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
DDisclose x D1998	0.238	0.107	0.098	0.048	0.311	0.132	-0.429	-0.073	0.061	-0.092
	[0.204]	[0.196]	[0.271]	[0.272]	[0.208]	[0.205]	[0.437]	[0.457]	[0.262]	[0.253]
DDisclose x D1998 x	-0.461**		-0.148		-0.530**		0.873*		-0.680**	
LowCash	[0.224]		[0.509]		[0.220]		[0.468]		[0.296]	
DDisclose x D1998 x		-0.32		-0.068		-0.338*		0.343		-0.447*
LowLiqAs		[0.196]		[0.526]		[0.192]		[0.423]		[0.254]
DDisclose	-0.778***	-0.510**	-1.120***	-0.883***	-0.728***	-0.428**	-0.672**	-0.093	-0.762**	-0.434
	[0.238]	[0.216]	[0.278]	[0.264]	[0.228]	[0.196]	[0.339]	[0.296]	[0.300]	[0.266]
LowCash	-0.813***		-0.751**		-0.987***		-1.330***		-0.813***	
	[0.218]		[0.293]		[0.211]		[0.318]		[0.276]	
LowLiqAs		-1.286***		-1.150***		-1.478***		-1.580***		-1.523***
		[0.179]		[0.257]		[0.147]		[0.249]		[0.207]
D1998 x LowCash	0.225		0.094		0.236		-0.923**		-0.012	
	[0.174]		[0.345]		[0.162]		[0.411]		[0.223]	
D1998 x LowLiqAs		0.003		-0.009		-0.02		-0.475		-0.153
		[0.162]		[0.371]		[0.156]		[0.330]		[0.218]
Observations	332	332	332	332	332	332	332	332	331	331
R-squared	0.69	0.77	0.64	0.68	0.71	0.81	0.60	0.63	0.67	0.76

Robust standard errors in brackets, clustered at the firm level. * significant at 10%; ** significant at 5%; *** significant at 1%. Each observation corresponds to a firm year observation. DDisclose is a dummy equal to one if the firm had less than $200,000 in bank debt before 1998. D1998 is a dummy equal to one for year 1998. LowCash (LowLiqAs) is a dummy equal to one if firm i had a cash (liquid assets) to short term liabilities ratio below the median before 1998.

10. Figures

Figure 14

Yearly Evolution of Average Firm Sales, by Industry