Do Firms Want to Borrow More?
Testing Credit Constraints Using a Directed Lending Program*

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Abstract

We begin the paper by laying out a simple methodology that allows us to determine whether firms are credit constrained, based on how they react to changes in directed lending programs. The basic idea is that while both constrained and unconstrained firms may be willing to absorb all the directed credit that they can get (because it may be cheap than other sources of credit), constrained firms will use it to expand production, while unconstrained firms will primarily use it as a substitute for other borrowing. We the apply this methodology to firms in India that became eligible for directed credit as a result of a policy change in 1998, and lost eligibility as a result of the reversal of this reform in 2000. Using firms that were already getting this kind of credit before 1998, and retained eligibility in 2000 to control for time trends, we show that there is no evidence that directed credit is being used as a substitute for other forms of credit. Instead the credit was used to finance more production– there was significant acceleration in the rate of growth of sales and profits for these firms. We conclude that many of the firms must have been severely credit constrained. *Keywords*: Banking, Credit constraints, India *JEL*: O16, G2

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1 Introduction

That there are limits to access to credit is widely accepted today as an important part of an economist's description of the world. Credit constraints now figure prominently in economic analyzes of short-term fluctuations and long-term growth.[1] Yet one is hard-pressed to find tight evidence of the existence of credit constraints on firms, especially in a developing country setting.

This is in some ways what is to be expected: a firm is credit constrained when it cannot borrow as much as it would like to at the going market rate or, in other words, when the marginal product of capital in the firm is greater than the market interest rate. It is however not clear how one should go about estimating the marginal product of capital. The most obvious approach, which relies on using shocks to the market supply curve of capital to estimate the demand curve, is only valid under the assumption that the supply is always equal to demand, i.e., if the firm is never credit constrained.

The literature has therefore taken a less direct route: The idea is to study the effects of access to what are taken to be close substitutes for credit—current cash flow, parental wealth, community wealth—on investment. If there are no credit constraints, greater access to a substitute for credit would be irrelevant for the investment decision. While this literature has typically found that these credit substitutes do affect investment,[2] suggesting that firms are indeed credit constrained, the interpretation of this evidence is not uncontroversial. The problem is that access to these other resources is unlikely to be entirely uncorrelated with other characteristics of the firm (such as productivity) that may influence how much it wants to invest. To take an obvious example, a shock to cash-flow potentially contains information about the firm's future performance. Of course, if one has enough information about the shock, one can isolate shocks that are clearly uninformative. Lamont's (1997) use of oil-price shocks to look at non-oil investment of oil companies is an example of this strategy. However it is not an accident that the companies

[1] See Bernanke and Gertler (1989) and Kiyotaki and Moore (1997) on theories of business cycles based on credit constraints and Banerjee and Newman (1993) and Galor and Zeira (1993) on theories of growth and development based on limited credit access.

[2] The literature on the effects of cash-flow on investment is enormous. Fazzari, Hubbard and Petersen (1998) provide a useful introduction to this literature. The effects of family wealth on investment have also been extensively studied (see Blanchflower and Oswald (1998), for an interesting example). There is also a growing literature on the effects on community ties on investment (see, for example, Banerjee and Munshi (2001)).

for which Lamont is able to have precise enough information about the nature of shocks tend to be very large companies and, as emphasized by Lamont and others, [3] cash-flow shocks can have very different effects on big cash-rich firms than on small cash-poor firms.[4]

Here we take a different approach to this question. We make use of a policy change that affected the flow of directed credit to an identifiable subset of firms. Such policy changes are common in many developing and developed countries—even the U.S. has the Community Reinvestment Act, which obliges banks to lend more to specific communities.

The advantage of our approach is that it gives us a specific exogenous shock to the supply of credit to specific firms (as compared to a shift in the overall supply of credit). Its disadvantage is that directed credit need not be priced at its true market price, and therefore a shock to the supply of directed credit might lead to more investment even if the firm is not credit constrained.

In this paper we develop a simple methodology based on ideas from elementary price theory that allows to deal with this problem. The methodology is based on two observations: first, if a firm is not credit constrained then an increase in the supply of subsidized directed credit to the firm must lead it to substitute directed credit for credit from the market. Second, while investment and therefore total production may go up even if the firm is not credit constrained, it will only go up if the firm has already fully substituted market credit with directed credit.

We test these implications using firm-level data that we collected from a sample of small to medium size firms in India. We make use of a change in the so-called priority sector regulation, under which firms smaller than a certain limit are given priority access to bank lending.[5] The first experiment we exploit is a 1998 reform which increased the maximum size below which a firm is eligible to receive priority sector lending. Our basic empirical strategy is a difference-in-difference-in-difference approach: that is, we focus on the *changes in the rate of change* in various firm outcomes before and after the reform for firms that got included in the priority

[3] Kaplan and Zingales (2000) make the same point.

[4] The estimation of the effects of credit constraints on farmers is significantly more straightforward since variations in the weather provide a powerful source of exogeneous short-term variation in cash flow. Rosenzweig and Wolpin (1993) use this strategy to study the effect of credit constraints on investment in bullocks in rural India.

[5] Banks are penalized for failing to lend a certain fraction of the portfolio to firms that classified to be in the priority sector.

sector as a result of the new limit, using the corresponding changes for firms that were already in the priority sector as a control. We find that bank lending and firm revenues went up for the newly targeted firms in the year of the reform. We find no evidence that this was accompanied by substitution of bank credit for borrowing from the market and no evidence that revenue growth was confined to firms that had fully substituted bank credit for market borrowing. As already argued, the last two observations are inconsistent with the firms being unconstrained in their market borrowing. Our second experiment uses the fact that a subset of the firms that were included in the priority sector in 1998 were excluded again in 2000. We find that bank lending and firm revenues went down for these firms, both compared to the firms that has always been part of the priority sector and to firms that were included in 1998, and remained part of the priority sector in 2000. This second experiment makes it unlikely that the results we obtain are an artifact of differential firms for large, medium and small firms.

We also use this data to estimate parameters of the production function. We find no evidence of diminishing returns to additional investment, which reinforces the idea that the firms are not at the point where the marginal product is about to fall below the interest rate. Finally, we try to estimate the effect of the program-induced additional investment on profits. While the interpretation of this result relies on some additional assumptions, it suggests a very large gap between the marginal product and the market interest rate (the point estimate is that Rs. 1 more in loans increased profits *net of interest payment* by Rs. 1.36, which is much too large to be explained as just the effect of getting a subsidized loan).

The rest of the paper is organized as follows: the next section describes the institutional environment and our data sources, provides some descriptive evidence and informally argues that firms may be expected to be credit constrained in this environment. The next section develops our empirical strategy, starting with the theory and ending with the equation we estimate. The penultimate section reports the results. We conclude with some admittedly speculative discussion of what our results imply for credit policy in India.

2 Institutions, Data and Some Descriptive Evidence

2.1 The Banking Sector in India

Despite the emergence of a number of dynamic private sector banks and entry by a large number of foreign banks, the biggest banks in India are all in the public sector, i.e., they are corporatized banks with the government as the controlling share-holder. The 27 public sector banks collect over 77% of deposits and comprise over 90% of all branches.

The particular bank we study is a public sector bank. While we are bound by confidentiality requirements not to reveal the name of the bank, we note it was rated among the top five public sector banks in 1999 and 2000 by Business Today, a major business magazine.

While banks in India occasionally provide longer-term loans, financing fixed capital is primarily the responsibility of specialized long-term lending institutions such as the Industrial Finance Corporation of India. Banks typically provide short-term working capital to firms. These loans are given as a credit line with a pre-specified limit and an interest rate that is set a few percentage points above prime. The gap between the interest rate and the prime rate is fixed in advance based on the firm's credit rating and other characteristics, but cannot be more than 4%. Credit lines in India charge interest only on the part that is used and, given that the interest rate is pre-specified, many borrowers want as large a credit line as they can get.

2.2 Priority Sector Regulation

All banks are required to lend at least 40% of their net credit to the "priority sector", which includes agriculture, agricultural processing, transport industry, and small scale industry (SSI). If banks do not satisfy the priority sector target, they are required to lend money to specific government agencies at very low rates of interest.

In January 1998, there was a change in the definition of the small scale industry sector. Before this date only firms with total investment in plant and machinery below Rs. 6.5 million were included in the priority sector. The reform extended the definition of priority sector to include firms with investment in plants and machinery up to Rs. 30 million. In January 2000, the reform was partially undone by a new change: firms with investment in plants and machinery between Rs 10 million and Rs 30. million were excluded from the priority sector.

The "priority sector" targets seem to be binding for the bank we study (as well as for most banks): every year, the bank's share lent to the priority sector is very close to 40% (it was 42% in 2000-2001). It is plausible that the bank had to go some distance down the client quality ladder to achieve this target. Moreover, there is the issue of the physical cost of lending. In a previous study of this and three other banks (Banerjee and Duflo, 2000), we calculated that the labor and administrative costs associated to lending to the SSI sector was about 1.5 paisa per Rupee higher than that of lending in the unreserved sector. This is consistent with the common view that lending to smaller clients is more costly.

Two things changed when the priority sector limit was raised: first, the bank could draw from a larger pool and therefore could be more exacting in its standards for clients. Second, it could save on the cost of lending by focusing on slightly larger clients. For both these reasons the bank would like to switch its lending towards the newly inducted members of the priority sector. If these firms were constrained in their demand for credit before the policy change, one would expect to see an expansion of lending to these firms relative to firms that were already in the priority sector.[6] When the firms with investment in plant and machinery above 10 million were excluded again from the priority sector, loans to these firms were not counting towards the priority sector any more. The bank had to go back to the smaller clients to fulfill its priority sector obligation. One therefore expect that loans to those firms declined relative to the smaller firms, most probably by enough to offset the rapid increase of the two previous years.

2.3 Data Collection

The data for this study were obtained from one of the better-performing Indian public sector banks. This bank, like other public sector banks, routinely collects balance sheet and profit and loss account data from all firms that borrow from it and compiles the data in the firm's loan folder. Every year the firm also must apply for renewal/extension of its credit line, and the paper-work for this this is also stored in the folder, along with the firm's initial application even

[6]The increase in lending to larger firms may come entirely at the expense of smaller firms (without affecting total lending to the priority sector), or the reform could cause an increase in the amount lent to the priority sector. We will focus on the comparison between firms that were newly labelled as priority sector and smaller firms.

when there is no formal review of the file. The folder is typically stored in the branch until it is completely full.

With the help of employees from this bank, as well as a former bank officer, we first extracted data from the loan folders in the spring of 2000. We collected general information about the client (product description, investment in plant and machinery, date of incorporation of units, length or the relationship with the bank, current limits for term loans, working capital, and letter of credit). We also recorded a summary of the balance sheet and profit and loss information collected by the bank, as well as information about the bank's decision regarding the amount of credit to extend to the firm, and the interest rate charged.

As we discuss in more detail below, part of our empirical strategy called for a comparison between accounts that have always been a part of the priority sector, and accounts that became part of the priority sector in 1998. We first selected all the branches that handle business accounts in the 6 major regions of the bank's operation (including New Delhi and Mumbai). In each of these branches, we collected information on all the accounts that were included in the priority sector after January, 1998 (these are the accounts for which the investment in plant and machinery is between 6.5 and 30 million rupees). We collected data on a total of 253 firms, including 93 firms with investment in plants and machinery between 6.5 and 30 million rupees). We aimed to collect data for the years 1996-1999, but when a folder is full, older information is not always kept in the branch. We have 1996 data on lending for 120 accounts (of the 166 firms that had started their relationship with the banks by 1996), 1997 data for 175 accounts (of 191 possible accounts), 1998 data for 226 accounts (of 238) , and 1999 data for 239 accounts.

In the winter 2002-2003, we collected a new wave of data on the same firms, in order to study the impact of the priority sector contraction on loans, sales and profit.

Table 1 presents the summary statistics for all data that will be used in the analysis of credit constraint and credit rationing (in the full sample, and in the sample for which we have information on the change in lending between the previous period and that period, which will be the sample of interest for the analysis).

2.4 Descriptive Evidence on Lending Decisions

In this subsection, we provide some description of lending decisions in the banking sector. We use this evidence to argue that this is an environment where credit constraints arise quite naturally.

Tables 2 and 3 show descriptive statistics regarding the loans in the sample. The first row of table 2 shows that, in a majority of cases, the loan limit does not change from year to year: in 1999, the limit was not updated *even in nominal terms* for 65% of the loans. This is not because the limit is set so high that it is essentially non-binding: row 2 shows that in the six years in the sample, 63% to 80% of the accounts reached or exceeded the credit limit at least once in the year.

This lack of growth in the credit limit granted by the bank is particularly striking given that the Indian economy registered nominal growth rates of over 12% per year. This would suggest that the demand for bank credit should have increased from year to year over the period, unless the firms have increasing access to another source of finance. There is no evidence that they were using any other formal source of credit. On average 98% of the working capital loans provided to firms in our sample come from this one bank and in any case, the same kind of inertia shows up in the data on total bank loans to the firm.

That the demand for formal sector credit increased from year to year, is suggested by rows 3 to 5 in table 2. The bank's official guidelines for lending explicitly state that the bank should try to meet the legitimate needs of the borrower. For this reason, the maximum lending limits that can be authorized by the bank are explicitly linked to the projected sales of the borrower–the maximum limit is supposed to be one fifth of the predicted sales for the year. Every year, a bank officer must approve a sales projection for the firm and calculate a maximum lending limit on the basis of the turnover.[7] Projected sales therefore provide a measure of the credit needs of the firm. Row 3 shows that actual sales have increased from year to year for most firms. Rows 4 and 5 show that both projected sales and the maximum authorized lending also increased from year to year in a large majority of cases. Yet there was no corresponding change in lending from the bank. The change in the credit limit that was actually sanctioned systematically fell short of what the bank determined to be the firm's needs as determined by the bank. In 1999,

[7]The exact rule is that the limit on turnover basis should be the minimum of 20% of the projected sales and 25% of the projected sales minus the finances available to the firm from other sources.

80% of the actual limits granted were below 20% of the predicted sales, and 60% were below the maximum limit calculated by the banker. On average, the granted limit was 89% of the recommended limit, and 67% of what following the rule based on 20% of predicted sales would give. It is possible that some of the shortfall was covered by informal credit, including trade credit: according to the balance sheet, total current liabilities excluding bank credit increased by 3.8% every year on average. However, some expenses (such as wages) are typically not covered by trade credit and, moreover, trade credit could be rationed as well. The question that is at the heart of this paper is whether such substitution operates to the point where a firm is not credit constrained.

In table 3, we examine in more detail whether this tendency could be explained by other factors that might have affected a firm's need for credit. Column (3) shows that no variable we observe seems to explain why a firm's credit limit was changed: firms are not more likely to get an increase in limit if they reached the maximum limit in the previous year, if their projected sales (*according to the bank itself*) have increased, if their current sales have increased, if the ratio of profits to sales has increased, or if the current ratio (the ratio of current assets to current liabilities, a traditional indicator of how secure a working capital loan is, in India as well as in the US) has increased. Turning to the direction or the magnitude of changes, only an increase in projected sales or current sales predicts an increase in granted limit, and only an increase in projected sales predict the level of increase. This could well be due to reverse causality, however: The bank officer could be more likely to predict an increase in sales when he is willing to give a larger credit extension to the firm.

One reason the granted limit may not change is that the previous year's limit already incorporated all information relevant to the lending decision: the limit is not responsive to what is currently going on in the firm, because these are just short-run fluctuations which tell us little about the future of the firm. If this were the case, we should observe that granted limits are much more responsive to these factors for young firms than for old firms. Columns 5 and 6 in table 3 repeat the analysis, breaking the sample into recent and older clients. Changes in limits are more frequent for younger clients, but they do not seem to be more sensitive to either past utilization, increases in projected sales, or profits.

The fact that the probability of a limit's change is entirely uncorrelated with observable

firm characteristics is striking. One plausible theory relates this to the fact, noted above, that changes in the limit are surprisingly rare. If bank officials are reluctant to change the limit, a large fraction of the observed changes may reflect effective lobbying or something purely procedural ("it has been five years since the limit was raised") rather than economic rationality.

What explains the reluctance of loan officers to do what is, palpably, their job? A recent report on banking policies commissioned by the Reserve Bank of India suggests one potential explanation: "The [working group] observed that it has received representations from the management and the unions of the bank complaining about the diffidence in taking credit decisions with which the banks are beset at present. This is due to investigations by outside agencies on the accountability of staff in respect to Non Performing Assets." (Tannan (2001)). In other words, the problem is that changing the limit (in either direction) involves sticking one's neck out–if one cuts the limit the firm may complain, and if one raises it, there is a possibility one would be held responsible if the loan goes bad: the Central Vigilance Commission (a government body entrusted with monitoring the probity of public officials) is formally notified of every instance of a bad loan in a public sector bank, and investigates a fraction of them.[8] Simply renewing the loan without changing the amount is one easy way to avoid such responsibility, especially if the original decision was someone else's (loan officers are frequently transferred). The problem is likely exacerbated by the fact that the link between the profitability of the bank and the prospects of an individual loan officer, is, at best, rather weak.

Whatever the explanation, the fact that the bank does not seem to be responding to changes in the firm's credit needs, suggests that some firms would have an unmet demand for bank credit. Of course the firm could borrow from the market (e.g. use trade credit, or moneylenders) to supplement what it gets from the banking system. Nevertheless, it does make it more plausible that the firms will be credit constrained.

[8]There were 1380 investigations of bank officers in 2000 for credit related frauds, 55% of which resulted in major sanctions)

3 Establishing Credit Constraints

3.1 Theory

Consider a firm with the following fairly standard production technology: the firm must pay a fixed cost C before starting production (say the cost of setting up the factory and installing the machinery). The firm then invests in labor and other variable inputs. k rupees of working capital invested in variable inputs, yield $R = F(k)$ rupees of revenue after a suitable period. $F(k)$ has the usual shape—it is increasing and concave.

Denote the market rate of interest in this economy by r_m and the interest rate on bank lending by r_b. Since the interest rate that public sector banks were allowed to charge on priority sector lending was capped above, there is reason to believe that the bank lending rate was below the market rate: $r_b \leq r_m$. We will say that a firm is *credit rationed* if at either of the interest rates it faces, it would like to borrow more. We will say it is *credit constrained* if it wants to borrow more at the market interest rate. It is clear that being credit constrained implies being credit rationed, but not the other way around.

The policy change we analyze involved the firms in question being offered additional bank credit. We will show in the next section that there was no corresponding change in the interest rate. To the extent that firms accepted the additional credit being offered to them, *this is direct evidence of credit rationing.* However this in itself does not imply that they would have borrowed more at the market interest rate. A possible scenario is depicted in figure 1. The horizontal axis in the figure measures k while the vertical axis represents output. The downward sloping curve in the figure represents the marginal product of capital, $F'(k)$. The step function represents the supply of capital. In the case represented in the figure, we assume that the firm has access to k_{b0} units of capital at the bank rate r_b but was free to borrow as much as it wanted at the higher market rate r_m. As a result, it borrowed additional resources at the market rate until the point where the marginal product of capital is equal to r_m. Its total outlay in this equilibrium is k_0. Now consider what happens if the firm is now allowed to borrow a greater amount, k_{b1}, at the bank rate. Clearly since at k_{b1} the marginal product of capital is higher than r_b, the firm will borrow the entire additional amount offered to it. Moreover it will continue to borrow at the market interest rate, though the amount is now reduced. The total outlay however is unchanged

at k_0. This will remain the case as long as $k_{b1} < k_0$: The effect of the policy will be to substitute market borrowing by bank loans. The firms profits will go up because of the additional subsidies but its total outlay and output will remain unchanged.

The expansion of bank credit will have output effects in this setting if $k_{b1} > k_0$. In this case the firm will stop borrowing from the market and the marginal cost of credit it faces will r_b. It will borrow as much it can get from the bank but no more than k_{b2}, the point where the marginal product of capital is equal to r_b. We summarize these arguments in:

Result 1: If the firm is not credit constrained (i.e., it can borrow as much as it wants at the market rate), but is rationed for bank loans, an expansion of the availability of bank credit should always lead to a fall in its borrowing from the market as long as $r_b < r_m$. Profits will also go up as long as market borrowing falls. However the firm's total outlay and output will go up only if the priority sector credit fully substitutes for its market borrowing. If $r_b = r_m$, the expansion of the availability of bank credit will have no effect on outlay, output or profits.

We contrast this with the scenario in figure 2, where the assumption is that the firm is rationed in both markets and is therefore credit constrained. In the initial situation the firm borrows the maximum possible amount from the banks (k_{b0}) and supplements it with borrowing the maximum possible amount from the market, for a total investment of k_0. Available credit from the bank then goes up to k_{b1}. This has no effect on market borrowing (since the total outlay is still less than what the firm would like at the rate r_m) and therefore total outlay expands to k_1. There is a corresponding expansion of output and profits.[9]

Result 2: If the firm is credit constrained, an expansion of the availability of bank credit will lead to an increase in its total outlay, output and profits, without any change in market borrowing.

We have assumed a particularly simple form of the credit constraint. If instead of the strict rationing we have assumed here, there is an upward supply curve for credit market, there will be a decrease in market borrowing as a result of the increase in formal lending. The important point, however, is that the increase in sales and profit will take place even if the increase in bank borrowing does not fully substitute for the entire market borrowing. The results also goes

[9]Of course, if k_{p1} were so large that $F'(k_{p1}) < r_m$, then there would be substitution of market borrowing in this case as well.

through if the market supply curve of credit is itself a function of bank credit (for example because bank credit serves as collateral for market credit). In this case, there might be an *increase* in market borrowing as the result of the reform. The instrumental variable estimate we will present below will then be an estimate of the total effect of the increase in bank credit, inclusive of the induced effect on market credit.

3.2 Empirical Strategy: Reduced Form Estimates

The empirical work follows directly from the previous subsection, and seeks to establish the facts that will allow us to determine whether firms are credit rationed, and to distinguish credit rationing from credit constraint.

Our empirical strategy takes advantage of the extension of the priority sector definition in 1998 and its subsequent contraction in 2000 . As we described above, the reform extended the definition of the priority sector to firms with investment in plants and machinery between Rs. 6.5 and 30 million. In 2000, firms with investment in plant and machinery above 10 million were excluded from the priority sector. As we noted, since the priority sector target (40% of the lending portfolio) was binding for our bank before and after this reform, there is good reason to believe that the reform reduced the shadow cost of lending for the bigger firms newly included in the priority sector, and thus resulted in an increase in their credit. Conversely, the 2000 reform increased the shadow cost of lending for firms with investment in plant and machinery between 10 and 30 million, and should have resulted in a decrease in credit to these firms. The reform did not seem to have large effects on the composition of clients of the banks: In the sample, 25% of the small firm, and 28% of the big firms, have entered their relationship with the bank in 1998 or 1999. Thus, our sample is not obviously biased by the reform.

Since the granted limit, as well as all the outcomes we will consider, are very strongly auto-correlated, we focus on the proportional change in this limit, i.e. log(limit granted in year t) − log(limit granted in year t-1). Table 4 shows the average change in the credit limit faced by the firm in the three periods of interest (loans granted before the change in January 1998, between January 1998 and January 2000, after January 2000). separately for the largest firms (investment in plant and machinery above Rs 10 million), the medium-sized firms (investment in plant and machinery between Rs 6.5 and Rs 10 million) and the smaller firms (investment in

plant and machinery below Rs 6.5 million).

The average enhancement was 7% larger for the small firms than for medium firms between in 1997, and 2% larger than for the big firms, whereas in 1998 it was 2% larger for medium firms, and 7% larger for the big firms. In fact the size of the average enhancement grew for medium and large firms and shrunk for the small ones. After 2000, enhancements declined for all firms, but the biggest declined happened for the larger firms, whose enhancement declined from an average of 14% in 1998 and 1999 to 0% in 2000.

Panel B in table 4 shows that the average increase in enhancement was not due to an increase in the probability that the working capital limit was changed : big firms did not become more likely to experience a change in 1998 or 1999 than in 1997. This may appear surprising, but it is entirely consistent with the previous evidence showing that it is not possible to explain why certain firms experienced a change in their credit limit. It is plausible that the same kind of bureaucratic inertia is at work here as well. While loan officers do need to respond to pressure from the bank to expand lending to the newly eligible big firms, they seem to prefer giving larger increases to those which would have received an increase in any case (for one reason or another), rather than increasing the number of firms that receive enhancement.

In Panel C, we show the average increase in limit, conditional on the limit changing. The average percentage enhancement was larger for the small firms than the medium and large firms in 1997, smaller for the small firms than for the large firms in 1998 and 1999 (and about the same for the medium firms), and larger after 2000. The average enhancement conditional on a change in limit declined dramatically for the largest firm after 2000 (it when from an average of 0.44 to an average of slightly less than 0).

Our strategy will be to use this change in policy as a source of shock to the availability of bank credit to these firms, using firms outside this category to control for possible trends. The first step however is to formally establish that there was indeed such a shock. To do this we use the data from 1997 to 1999 and we estimate equation of the form:

$$\log k_{bit} - \log k_{bit-1} = \alpha_{1kb} BIG_i + \beta_{1kb} POST + \gamma_{1kb} BIG_i * POST_t + \epsilon_{1kbit}, \qquad (1)$$

where k_{bit} is a measure of bank credit to firm i in year t[10] , BIG is a dummy indicating whether

[10]The credit available in year t is granted in year $t-1$

the firm has investment in plant and machinery between Rs. 6.5 millions and Rs. 30 millions, and $POST$ is a dummy equal to one in the years 1998 and 1999. We focus on working capital loans from this bank as well as total working capital loans from the banking sector. We estimate this equation in the entire sample and in the sample of accounts for which there was no revision whatsoever. We will also consider whether or not the firms is more likely to see a change, and increase or a decrease in their credit limit from this bank. For working capital loans, we expect a positive γ_b. We do not expect a change in term loans in the years immediately following the reform, since it takes some time for a fixed capital investment project to be planned and processed.

To study the impact of the contraction on bank loans, we use the 1998-2002 data and estimate the following equation:

$$\log k_{bit} - \log k_{bit-1} = \alpha_{2kb} BIG2_i + \beta_{2kb} POST2 + \gamma_{2kb} BIG2_i * POST2_t + \epsilon_{2kbit}, \qquad (2)$$

where $BIG2$ is a dummy indicating whether the firm has investment in plant and machinery between Rs. 10 millions and Rs. 30 millions, and $POST2$ is a dummy equal to one in the years 2000, 2001, and 2002.

Finally, we pool the data and estimate the equation:

$$\begin{aligned} \log k_{bit} - \log k_{bit-1} \quad = \quad & \alpha_{3kb} MED_i + \alpha_{4kb} BIG2_i + \beta_{3kb} POST + \beta_{4kb} POST2 \\ & \gamma_{3kb} MED_i * POST_t + \gamma_{4kb} BIG2_i * POST_t + \\ & \gamma_{5kb} MED_i * POST2_t + \gamma_{6kb} BIG2_i * POST2_t + \epsilon_{3kbit}, \qquad (3) \end{aligned}$$

As pointed out in the previous subsection, the impact of the shock on the firm depends crucially on whether the firm was credit constrained, credit rationed or entirely unconstrained. In order to distinguish between these cases we need to look at a number of other credit variables for the firm. We therefore run a number of other regressions that exactly parallel equation 1 to 3. First, we use the sample 1997-1999 to estimate:

$$y_{it} - y_{it-1} = \alpha_y BIG_i + \beta_y POST_t + \gamma_y BIG_i * POST_t + \epsilon_{yit}, \qquad (4)$$

where y_{it} is a credit variable for firm i in year t.[11] Second we estimate:

$$\log y_{it} - \log y_{it-1} = \alpha_{2y} BIG2_i + \beta_{2y} POST2 + \gamma_{2y} BIG2_i * POST2_t + \epsilon_{2yit}, \tag{5}$$

in the sample 1998-2002 , and finally we estimate:

$$\begin{aligned} \log y_{it} - \log y_{it-1} \quad = \quad & \alpha_{3y} MED_i + \alpha_{4y} BIG2_i + \beta_{3y} POST + \beta_{4y} POST2 \\ & \gamma_{3y} MED_i * POST_t + \gamma_{4y} BIG2_i * POST_t \\ & + \gamma_{5y} MED_i * POST2_t + \gamma_{6y} BIG2_i * POST2_t + \epsilon_{3yit}, \end{aligned} \tag{6}$$

in the pooled sample.

• Credit rationing

Our Result 1 above suggests that to establish credit rationing, we need two more pieces of evidence.

First, since the working capital loans take the form of a line of credit (and firms are charged only for what they use), we need to examine what happened to the rate at which firms draw upon their granted limit. We thus use as our measure of credit utilization, the logarithm of the ratio of turnover on accounts (the sum of all debts over the past year) to the credit limit.

Second, this would not be evidence of credit rationing if the interest rate charged on this loan decreased at the same time. Priority sectors loans are *not* supposed to have lower interest rates (the interest rate charged on a loan is the prime lending rate plus a premium depending on the credit rating of the firm–without regard for its status), so there is no prima facie reason the rate should fall. However, we directly check whether there is evidence of this using three specifications: using $y_{it} = r_{bit}$in equation (1) , for r_{bit} equal to the interest rate in logarithm and in level, and replacing $y_{it} - y_{it-1}$ in equation (4) by a dummy indicating whether the interest rate fell.

• Credit constraints

Credit rationing does not necessarily imply credit constraint. To establish that the firms were indeed credit constrained, we look at a number of other pieces of evidence.

[11] Standard errors are corrected for clustering at the firm level.

First, if a firm were credit constrained, our theory tells us that sales revenue would definitely go up, while if it were not, sales should only go up for firms that have already fully substituted bank credit for their market borrowing. To look at the effect of credit expansion on sales, we posit a simple parametric relation between credit and sales revenue: $R_{it} = A_{it}k_{it}^{\theta}$. Note that this is a specific parametrization of the production function introduced in the previous sub-section.[12]

$$\log R_{it} = \log A_{it} + \theta \log k_{it}. \tag{7}$$

Differencing this equation gives:

$$\log R_{it} - \log R_{it-1} = \log A_{it} - \log A_{it-1} + \theta[\log k_{it} - \log k_{it-1}]. \tag{8}$$

Focusing on the first experiment, we have already posited that the growth of bank credit is given by:

$$\log k_{bit} - \log k_{bit-1} = \alpha_{kb}BIG_i + \beta_{kb}POST_t + \gamma_{kb}BIG_i * POST_t + \epsilon_{kbit}, \tag{9}$$

In the absence of complete substitution between bank credit and market credit, this implies a relationship of the same shape for capital stock:

$$\log k_{it} - \log k_{it-1} = \alpha_{k}BIG_i + \beta_{k}POST_t + \gamma_{kb}BIG_i * POST_t + \epsilon_{kit}, \tag{10}$$

which when substituted in equation (8) yields

$$\log R_{it} - \log R_{it-1} = \log A_{it} - \log A_{it-1} + \theta[\alpha_{k}BIG_i + \beta_{k}POST_t + \gamma_{k}BIG_i * POST_t + \epsilon_{kit}]. \tag{11}$$

Since we do not observe $\log A_{it} - \log A_{it-1}$ directly, we end up estimating an equation that exactly mimics equation 4 above:

$$\log R_{it} - \log R_{it-1} = \alpha_{R}BIG_i + \beta_{R}POST_t + \gamma_{R}BIG_i * POST_t + \epsilon_{kit}]. \tag{12}$$

Our identification hypothesis is that

$$\log A_{it} - \log A_{it-1} = \alpha_{A}BIG_i + \beta_{A}POST_t. \tag{13}$$

[12]This is best thought of as a reduced form, derived from a more primitive technology which makes output a Cobb-Douglas function of the amount of n inputs $x_1, x_2...x_n$. As long as the inputs have to purchased using the working capital and all inputs are purchased in competitive markets, it can be shown that the resulting indirect production function has the form given above.

This amounts to assuming that the rate of change of A (which is a shift parameter in the production function) *did not* change differentially for big and small firms in the year of the policy reform. Under this assumption γ_R gives the reduced form effect of the program on sales revenue.

If firms are credit constrained γ_R should be positive, while if no firms are credit constrained γ_R will only be positive for those firms that have *fully* substituted market credit. We therefore also estimate a version of equation 12 in the sample of firms whose total current liabilities exceed their bank credit. If the firms were not credit constrained, the value of γ_R in this sample should be zero.

A second strategy is to look at substitution directly. Unfortunately we do not have reliable data on market borrowing. We therefore adopt the following strategy: equation (8) above can be rewritten in the form:

$$\begin{aligned} \log R_{it}/k_{bit} - \log R_{it-1}/k_{bit-1} &= \log A_{it} - \log A_{it-1} \\ &\quad +\theta[\log k_{it} - \log k_{it-1}] - [\log k_{bit} - \log k_{bit-1}]. \end{aligned} \tag{14}$$

Differencing one more time gives us

$$\begin{aligned} & [\log R_{it}/k_{bit} - \log R_{it-1}/k_{bit-1}] - [\log R_{it-1}/k_{bit-1} - \log R_{it-2}/k_{bit-2}] \\ = \; & [\log A_{it} - \log A_{it-1}] - [\log A_{it-1} - \log A_{it-2}] \\ & +\theta([\log k_{it}/k_{bit} - \log k_{it-1}/k_{bit-1}] - [\log k_{it-1}/k_{bit-1} - \log k_{it-2}/k_{bit-2}]) \\ & -(1-\theta)([\log k_{bit} - \log k_{bit-1}] - [\log k_{bit-1} - \log k_{bit-2}]). \end{aligned} \tag{15}$$

We now take the difference of this expression for big firms and that for small firms. Denoting by the operator Δ the operation of difference across firm categories and using (13) we get

$$\begin{aligned} & \Delta\{[\log R_t/k_{bt} - \log R_{t-1}/k_{bt-1}] - [\log R_{t-1}/k_{bt-1} - \log R_{t-2}/k_{bt-2}]\} \\ = \; & \theta\Delta\{([\log k_t/k_{bt} - \log k_{t-1}/k_{bt-1}] - [\log k_{t-1}/k_{bt-1} - \log k_{t-2}/k_{bt-2}])\} \\ & -(1-\theta)\Delta\{[\log k_{bt} - \log k_{bt-1}] - [\log k_{bt-1} - \log k_{bt-2}]\}. \end{aligned} \tag{16}$$

We have suggested, and will show formally below that $\Delta\{[\log k_{bt} - \log k_{bt-1}] - [\log k_{bt-1} - \log k_{bt-2}]\}$ is positive when we compare pre-reform and post-reform periods. If a firm is not

credit constrained it should substitute bank loans for market loans, which implies that bank capital should grow faster than total capital stock for the big firms after the reform, relative to the small firms. $\Delta\{([\log k_t/k_{bt} - \log k_{t-1}/k_{bt-1}] - [\log k_{t-1}/k_{bt-1} - \log k_{t-2}/k_{bt-2}])\}$ is therefore negative. As long as $\theta \leq 1$, these two observations together imply that the expression on the right should be negative. If $\theta > 1$, this need not be necessarily be case, but with increasing return to scale there cannot be an equilibrium in which the firms are not credit constrained.

We implement this by estimating equation 4 with $y_{it} = \frac{Ri_t}{k_{bt}}$. If the firm is credit constrained, γ_{R/k_b}should be negative. If not, we presume that there is no substitution.

A final piece of evidence comes from looking at profits. Assuming that the firm buys all its inputs using its working capital, we can write

$$\Pi_{it} = A_{it}(k_{bit} + k_{mit})^{\theta} - (1 + r_{bit})k_{bit} - (1 + r_{mit})k_{mit} - C.$$

It follows that

$$\begin{aligned}\frac{d\log\Pi_{it}}{dt} &= \frac{A_{it}(k_{bit}+k_{mit})^{\theta}}{\Pi}\frac{d\log A_{it}}{dt} + \frac{\theta A_{it}(k_{bit}+k_{mit})^{\theta-1}k_{bit} - (1+r_{bit})k_{bit}}{\Pi}\frac{d\log k_{bit}}{dt} \\ &+ \frac{\theta A_{it}(k_{bit}+k_{mit})^{\theta-1}k_{mit} - (1+r_{mit})k_{mit}}{\Pi}\frac{d\log k_{mit}}{dt} - \frac{r_{mit}k_{mit}}{\Pi}\frac{d\log r_{mit}}{dt},\end{aligned}$$

ignoring the effect of changes in the bank interest rate, which, given evidence to be shown later, does not seem to be much of an issue. If the firm was not credit constrained, k_{mit} would be chosen optimally and therefore we can drop the third term in this expression. Taking time derivatives again and dropping all terms that involve a product of two rates of change (assuming that rates of change are always small and therefore their products are going to be negligible), we get

$$\begin{aligned}\frac{d^2\log\Pi_{it}}{dt^2} &= \frac{A_{it}(k_{bit}+k_{mit})^{\theta}}{\Pi}\frac{d^2\log A_{it}}{dt^2} + \frac{\theta A_{it}(k_{bit}+k_{mit})^{\theta-1}k_{bit} - (1+r_{bit})k_{bit}}{\Pi}\frac{d^2\log k_{bit}}{dt^2} \\ &- \frac{r_{mit}k_{mit}}{\Pi}\frac{d^2\log r_{mit}}{dt^2}.\end{aligned}$$

Comparing big and small firms, and invoking the Δ operator again, we have:

$$\begin{aligned}\Delta\{\frac{d^2\log\Pi_t}{dt^2}\} &= \Delta\{\frac{A_t(k_{bt}+k_{mt})^{\theta}}{\Pi}\}\frac{d^2\log A_t}{dt^2} + \frac{A_t(k_{bt}+k_{mt})^{\theta}}{\Pi}\Delta\{\frac{d^2\log A_t}{dt^2}\} \\ &- \Delta\{\frac{r_{mt}k_{mt}}{\Pi}\}\frac{d^2\log r_{mt}}{dt^2} - \frac{r_{mt}k_{mt}}{\Pi}\Delta\{\frac{d^2\log r_{mt}}{dt^2}\} \\ &+ \Delta\{\frac{\theta A_t(k_{bt}+k_{mt})^{\theta-1}k_{bt} - (1+r_{bt})k_{bt}}{\Pi}\frac{d^2\log k_{bt}}{dt^2}\}\end{aligned}$$

.

Now $\frac{d^2 \log r_{mt}}{dt^2}$ should be the same for both large and small firms, since it is the market interest rate. Therefore $\Delta\{\frac{d^2 \log r_{mt}}{dt^2}\} = 0$. By equation 13 above, $\Delta\{\frac{d^2 \log A_t}{dt^2}\} = 0$. This leaves us with:

$$\Delta\{\frac{d^2 \log \Pi_t}{dt^2}\} = \Delta\{\frac{A_t(k_{bt}+k_{mt})^{\theta}}{\Pi}\}\frac{d^2 \log A_t}{dt^2} \tag{17}$$
$$-\Delta\{\frac{r_{mt}k_{mt}}{\Pi}\}\frac{d^2 \log r_{mt}}{dt^2} \qquad +\Delta\{\frac{\theta A_t(k_{bt}+k_{mt})^{\theta-1}k_{bt} - (1+r_{bt})k_{bt}}{\Pi}\frac{d^2 \log k_{bt}}{dt^2}\}.$$

The last term here is the effect of the reform. To separate it from other time varying effects, the first two terms must be small enough to be ignored. The second term, $\Delta\{\frac{r_{mt}k_{mt}}{\Pi}\}\frac{d^2 \log r_{mt}}{dt^2}$ can safely be assumed to be small. It has been shown that, in India, the average interest rate on the internal market is linked to the bank rate. $\frac{d^2 \log r_{mt}}{dt^2}$ is thus closely linked to $\frac{d^2 \log r_{bt}}{dt^2}$, which is given by the $POST$ dummy when we estimate equation (4) with r_{bt} as the dependent variable. Below, we estimate this coefficient to be -0.21 percentage point (the average interest rate is 14%).

One scenario where $\Delta\{\frac{A_t(k_{bt}+k_{mt})^{\theta}}{\Pi}\}\frac{d^2 \log A_t}{dt^2}$ is small is if $\frac{d^2 \log A_t}{dt^2}$ is small. That would be true if either there was not much change in A_t or if there was a directional trend but not much variation around the trend. Assuming that $\Delta\{\frac{r_{mt}k_{mt}}{\Pi}\}\frac{d^2 \log r_{mt}}{dt^2}$ is indeed small, this hypothesis can be directly tested by looking at the coefficient of the $POST$ dummy when we estimate equation (4) with profits as the dependent variable: the coefficient of the $POST$ dummy will be equal to $\Delta\{\frac{A_t(k_{bt}+k_{mt})^{\theta}}{\Pi}\}\frac{d^2 \log A_t}{dt^2}$ for the small firms. Since $\Delta\{\frac{A_t(k_{bt}+k_{mt})^{\theta}}{\Pi}\}$ is not equal to zero, if the product is zero, $\frac{d^2 \log A_t}{dt^2}$ must be zero.

Note that the effect of the reform on profit is due to the gap between the marginal product of capital and the *bank* interest rate: in other words, it combines the subsidy effect and the credit constraint effect: even if firms were not credit constrained, their profit would still increase after the reform if more subsidized credit is made available to them, because they substitute cheaper capital for expansive capital.

3.3 Empirical Strategy: Testing the Identification assumptions

The interpretation of the central result on sales growth crucially depends on the assumption made in equation (13). Likewise, the interpretation of the other results depends on the assumption that the error term is not correlated with the regressors, most notably $BIG * POST$. However, there are many reasons why this assumption may not hold. For example, big and small firms may be

differently affected by other measures of economic policy (they could tend to belong to different sectors, with different policies during these years). The fact that we have two experiments affecting a different set of firms helps us provide evidence that differential trends not linked to the priority sector are probably not explaining the result we obtain. The two reforms went in different directions, and did not affect all the firms identically. For our coefficients in the sales equation, for example, to be explained by credit constraints, rather than differential trends, we expect γ_y in equation (4) to be positive, γ_{2y} in equation (5) to be negative, γ_{3y} and γ_{4y} in equation (6) to both be positive, and γ_{5y} and γ_{6y} in the same equation to be respectively zero and negative. Furthermore, equation (11) suggests a more stringent overidentification test: our model suggests that the ratios $\frac{\gamma_{3y}}{\gamma_{3kb}}$, $\frac{\gamma_{4y}}{\gamma_{4kb}}$ and $\frac{\gamma_{6y}}{\gamma_{6kb}}$ should be equal. Moreover, they should also be equal to the ratios $\frac{\gamma_y}{\gamma_{kb}}$ and $\frac{\gamma_{2y}}{\gamma_{2kb}}$. If all coefficients have the expected signs and magnitude, and if these equality are satisfied, it will be very unlikely that the patterns of results we obtain is due to differential trends affecting firms of different sizes that have nothing to do with the priority sector.

It is still possible that, being labeled as a priority sector firm may have consequences for big firms over and above its effects on credit access. Other than borrowing, there are two other ways in which being included in the priority sector may affect firms. First, SSI firms are exempt from some types of excise taxation. Second, the right to manufacture certain products is reserved for the SSI sector. We will address the first concern by using profit before tax, and the second by showing that in the time span covered by our data set, reform did not significantly affect term borrowing. The third concern could be a problem: among the small firms, 44% manufacture a product that is reserved for SSI. Among the big firms, 24% do. One control strategy would be to leave out all firms that manufacture products that are reserved for SSI. Unfortunately, we only know what products the firm manufactured in 1998. It remains possible that some of the big firms moved into reserved product after 1998 and this increased their profits.

As a way to test our identification assumption and to improve the precision of the estimates, we will estimate equation (4) to (6) for the different outcomes variables separately using two samples: the sample where there was no change in the granted limit from one year to the next, and the sample where there was a change (either an increase or decrease). In doing so, we make use of the fact, noted above, and shown more formally below, that the probability of a change

in the limit appears to be unaffected by the policy change (the variable $BIG * POST$). Given this fact, and a simple monotonicity assumption, estimating an equation of the form of equation (4) separately in the sample where there was a change in limit and in the sample where there was no change in limit will lead to consistent estimates of the parameter of interest γ in both subsamples (Heckman (1979), Heckman and Robb (1986)).

Specifically, denote c_i a variable equal to 1 if there is a change in limit and 0 otherwise, Z_i the interaction $BIG * POST$, and X_i the vector $(BIG_i, POST_i)$. Define c_{i1} as the potential selection status when $Z_i = 1$ and c_{i0} as the potential selection status when $Z_i = 0$.[13]. Assume that (i) $(\epsilon_i, (c_{i1}, c_{i0}))$ are jointly independent of Z_i conditional on X_i (ii) one of the following is true: conditional on X_i, $c_{i1} >= c_{i10}$ for all i or $c_{i1} >= c_{i10}$ for all i. The first assumption guarantees the validity of Z_i as a regressor in the complete sample (but not in the selected sample), and that the marginal distribution of c_{i0} and c_{i1} (but not the joint distribution) are identified in a sample with data on Z_i and c_i. The second assumption restricts the relationship between the instrument and selection to be monotonic: for all firms, the instrument makes it either more likely or less likely to experience a change in limit.[14]

Given these two assumptions, $P(c_i = 1|Z_i = 1) = P(c_i = 1|Z_i = 0)$ implies that (c_i, ϵ_i) is jointly independent of Z_i conditional on X_i (for a proof using this notation, see Angrist (1995)). Therefore, $E[\epsilon_i|c_i = 1, Z_i = 1, X_i] = E[\epsilon_i|c_i = 1, Z_i = 0, X_i]$, and $E[\epsilon_i|c_i = 0, Z_i = 1, X_i] = E[\epsilon_i|c_i = 0, Z_i = 0, X_i]$: the OLS assumptions are satisfied in both subsamples.

This implies that the independence assumption (which is equivalent to the assumption in equation (13)) can be tested as long as $P(c_i = 1|Z_i = 1) = P(c_i = 1|Z_i = 0)$. This later assumption can be shown to hold by regressing the probability of a change in limit on the interaction $BIG * POST$ (and finding a coefficient of zero on this variable).

The test of the identification assumption is to run regressions of the form (4) in the sample with no increase in credit limit: since the coefficient of the variable $BIG * POST$ is zero in the

[13]For each observation, we will observe only either c_{i1} or c_{i0}

[14]The latent index formulation with a linear model linking the latent variable to the instrument satisfies these two assumptions. However, the assumptions are not necessarily true: in particular, one could imagine that the limit from a big firm is more likely to be left at zero than to be decreased, but more likely to be increased than to be left at 0. This would violate the monotonicity assumption. We will present evidence below that this did not seem to have happened.

credit equation, it should also be zero in the regression of the other outcomes. Of particular interest, obviously, are the coefficients in the equation of sales and profit. If the firms are credit constrained, we expect the coefficients of Z_i to be positive in the sample with credit limit increases, and equal to zero in the sample with no increases in loans.

3.4 Empirical Strategy: Structural Estimates

The fact that the firms are credit constrained tells us that the marginal product of capital is higher than the market interest rate. The question is, by how much?

We begin by observing that an alternative to estimating equation (8) is to estimate the structural relationship (8) using $BIG_i * POST_t$ as an instrument.[15] This would allow us to estimate θ, the elasticity of revenue with respect to working capital investment. It is worth observing that for an equilibrium without credit rationing to exist it must be the case that $\theta < 1$ in the neighborhood of the equilibrium: otherwise the marginal product of capital is not declining at the equilibrium, which rules out it being an optimum for an unconstrained firm. Conversely, finding that $\theta \geq 1$, makes it likely that there are credit constraints in equilibrium.

In practice, as we have already mentioned, we do not have a measure of k_{it}, but only a measure of k_{bit}. Rewriting structural equation (8), we obtain:

$$\log R_{it} = \log A_{it} + \theta \log k_{bit} - \theta \log \frac{k_{bit}}{kit}. \tag{18}$$

Differencing over time:

$$\log R_{it} - \log R_{it-1} = \log A_{it} - \log A_{it-1} + \theta(\log k_{bit} - \log k_{bit-1}] - \theta[\log \frac{k_{bit}}{kit} - \log \frac{k_{bit-1}}{kit-1}]. \tag{19}$$

The term $\theta[\log \frac{k_{bit}}{kit} - \log \frac{k_{bit-1}}{kit-1}]$, which is omitted in the regression, is positively affected by the reform in the presence of any substitution of bank credit for market credit. Thus, when we use $BIG_i * POST_t$ as an instrument for $\log k_{bit}$, we obtain a lower bound for θ.

Similarly, equations 5) and (2) form the reduced form and the first stage, respectively, of an instrumental strategy that will produce a lower bound for θ in the sample 1998-2002. As we

[15] Following the discussion in the previous subsection, we will run this IV regression in the sample where we observe an increase in loans.

mentioned earlier, we expect these two estimates to be equal. Finally, equation 6) and (3) form the reduced form and the first stage of an estimate of a lower bound for θ in the entire sample, which, once again, is expected to be equal to the previous ones under the assumption in the model.

The expression we derived for the profit rate was directly expressed as a function of difference-in-difference in the rate of changes of bank credit. Thus, one way to see what happens to profit is to estimate the equation

$$\log \Pi_{it} - \log \Pi_{it-1} = \alpha POST + \beta BIG + \lambda[\log k_{bit} - \log k_{bit-1}], \tag{20}$$

using the interaction $POST * BIG$ as an instrument for $[\log k_{bit} - \log k_{bit-1}]$.[16] Under the assumptions in the previous subsection, $\lambda = \frac{\theta A_t (k_{bt}+k_{mt})^{\theta-1} k_{bt} - (1+r_{bt})k_{bt}}{\Pi}$.

The problem with this approach is that $log(\Pi_{it}$ is not defined when the firm has negative profits, which introduce sample selection. To deal with this problem, we instead estimate the same equation using $log(C_{it})$, with $C_{it} = R_{it} - \Pi_{it}$ as the dependent variable. The estimate of the effect on sales and costs allows us to compute an estimated impact on profits for any firms, that is not affected by sample selection.

4 Results

4.1 Credit

• Credit Expansion

Panel A in table 5 presents the results of estimating equation (1) for several credit variables.[17] We start with a variable indicating whether there is any was any change in the granted limit (columns (1)), and two dummies indicating whether there was an increase or a decrease in the granted limit. Consistent with the evidence we discussed above, there seem to be absolutely no correlation between the probability to get any change in limit and the interaction $BIG * POST$. Moreover, even the main effects of BIG and $POST$ are very small: none of the variables in

[16] And, as before, corresponding estimates can be obtained using the second experiments, or both together.

[17] The standard errors in all regressions are adjusted for heteroskedaticity and clustering at the firm and sector levels.

this regression seem to affect whether the file was granted a change in limit or not. There is also no effect of the interaction on the probability to get an increase or a decraese In the following columns, we look at limit grated by the bank (columns (4), to (7)).[18] As the descriptive evidence in table 4 suggested, relative to small firms, loans from this bank to big firms increased significantly faster after 1998 than before: the coefficient of the interaction $POST*BIG$ is 0.095, in the complete sample, and 0.27 in the sample for which there is any change in limit. There was a decline in the average enhancement for small firms (the dummy for $POST$ is negative). Before the expansion of the priority sector, medium and large firms were granted larger proportional enhancement than small firms (the coefficient of the variable BIG is -0.22, with a standard error of 0.088). The gap completely closed after the reform (the coefficient of the interaction is actually larger in absolute value than the coefficient of the variable BIG, although this difference is small). In column (6) and (7) , we restrict the sample to observations where we have data on future sales (which is the first stage for the IV strategy for the impact of bank loan on sales) : the coefficient is almost the same (0.26), and still significant.

Credit contraction

In panel B, we present the result of estimating equation 2. Here again, we find not effect of the contraction on the probability that the limit is changed (column (1)), which reinforces the claim that the process of decision for whether a file is reviewed or not has nothing to do with the priority sector regulation. However, limits became significantly more likely to be decreased for the largest firms after the reversal in the 1998 reform (the coefficient is 0.119, with a standard error of 0.033). Turning to the magnitude in the change in limit, the coefficient of the interaction $BIG2*POST$ is negative both in the entire sample (column (4), the coefficient is -0.12) and the sample with a change in limit (column (5), the coefficient is -0.44). The average yearly decline in the limit for big firms, rather than large firms after 2000 is larger than the average yearly increase in limit in 1998 and 1999. The effect is even larger in the sample where we have data on sales (columns (6) and (7)).[19]

In panel C, we present the interaction coefficients γ_{3kb} to γ_{6kb} (the corresponding main effects are not presented in the tables, but were included in the regression). Medium firms appeared

[18] If we use instead the sum of the limits from the entire banking sector, we obtain virtually identical estimates: this simply reflects the fact that most firms borrow only from one bank.

[19] The sample size drops in this column since we are not using the last year of data on loan.

to have more likely to not see a change in limit after 2000 and all other types of firm. It may be because they have experienced relatively large changes in the two years before. The effect on the sized of the change in limit granted by the banks are presented in column (4) (whole sample) and (5) (sample were the limits was changed). In 1998 and 1999, both medium and big firms experience significantly larger changes in limit enhancement than small firms (the magnitude of the change is similar). After 2000, medium firm experience a reduction in limit relative to small firms (the coefficient is -0.18 in column (5)) , but the reduction is insignificant, and smaller than that for large firms (which have a coefficient of -0.48 in column (6)).

4.2 Evidence of Credit Rationing

Table 6 presents evidence on credit rationing. We present the results on limit utilization and interest rate changes in the complete sample and the sample with a change in limit. As before, panel A focuses on the expansion experiment, and panel B focuses on the contraction experiment.

Columns (1) to (3) present the results for the interest rate. The first column considers levels, the second column logarithms, and the third column replaces the difference $y_t - y_{t-1}$ by a dummy indicating whether the interest rate fell in between the two years. There seems to be strong evidence that the interest rate did not decline for big firms (relative to small firms) as they entered the priority sector. In all three samples and for all three measures we consider, the interaction $BIG * POST$ is insignificant in panel A, and in most cases the point estimate would suggest a relative *increase* of the interest rate, rather than a decrease. In the complete sample, in levels, the point estimate is 0.045, with a standard error of 0.174.[20]. In the sample with an increase in limit it is 0.16, with a standard error of 0.33. In logs the interaction is 0.009, with a standard error of 0.02. In panel B, the coefficient of $BIG2 * POST2$ is likewise insignificant in all the specifications.

As the firms are actually granted an extension of a credit line, we still need to assess whether they actually make use of the additional credit offered to them. To do that, we compute limit utilization, as the ratio of yearly turnover in the account over the granted limit. When we use this variable as the dependent variable, the coefficient of $BIG * POST$ is negative insignificant in the sample with increases in limit (column (8)), and is actually negative as well (and still

[20]The average change in interest rate in sample period was 0.34, with a standard deviation of 0.86

insignificant) in the contraction sample (whereas, if firms cut from credit had been using their limits more intensively one may have expected a positive number). The coefficients we obtain in the entire sample are more troubling: there, the coefficient of the interaction is negative and significant in panel A, and positive and significant in panel B, which would indicate substitution. This, however, does not seem to be due by the expansion in limits, since these coefficients are driven by the firms whose limits is not changed.

Thus, firms make use of the extension in credit without a change in interest rate: this suggests that they must be credit rationed.

4.3 Evidence of Credit Constraints

Table 7 present evidence on credit constraints.

• **Credit Expansion**

In panel A, column (1), we start by looking at the impact of the credit expansion on sale. In order to keep the table manageable, we present only the coefficient of the interactions. The coefficient of the interaction is 0.194 in the sample with a change in limit, with a standard error of 0.106. In the sample without review, sales did not increase disproportionately for large firms: the coefficient of the interaction is 0.007, with a standard error of 0.074. This supports our identification assumption that the difference in the annual rate of growth of A_{it} was not differentially affected in the year 1999.

This increase in sales suggests that firms were not only credit rationed, but also credit constrained, unless we are in the case where bank credit completely substituted for market credit. We do not have reliable data on market credit, but we have a proxy for trade credit, the difference between total current liabilities and the bank limit. In column (2) we restrict the sample to firms where, according to this measure, firms have not stopped using trade credit (i.e., this measure has not become 0 or smaller). The coefficient is almost identical (0.168) in this sample: the increase in sales is not due to firms that had first completely substituted away from trade credit. note that very few firms drop from the sample where we focus on firms that have positive non-bank liability. Most firms seem to be using a combination of bank credit and trade credit.

Finally, we look at substitution directly. As we discussed earlier, we use the year-to-year

growth in the log of the ratio between sales and bank loans as the dependent variable. In the absence of credit constraint, as firms substitute bank credit for market credit, the coefficient of the interaction $POST * BIG$ would be negative. It could also be negative, even in the presence of credit constraint, if θ were smaller than 1. This regression is presented in column (3). The coefficient of the interaction is equal to -0.065, and is not significant. For the most part, the new loans did not seem to have been used to substitute other, more expansive loans.

These three results, together with the previous results establishing credit rationing, suggest that firms are credit constrained: there was little substitution between bank credit and market credit, and sales increased.

Although finding an effect on profit would not be sufficient to establish the presence of credit constraint (since part of the effect of profit follows from the subsidy), establishing the magnitude of the effect on profit is a useful complement to the results on sales. Our derivation suggests that we should use the logarithm of profit as the dependent variable. This presents the difficulty that this variable is not defined whenever profit is negative. We can thus only estimate the effect on profit for firms that have a positive profit in both periods.

To avoid this problem, we look at the direct impact of the reform on the logarithm of cost (defined as sales-profits), which is always defined. The effect on profit for any particular firm or for the average firm can then be recovered from the estimate of the reform on sales and costs. The increase in sales is, not surprisingly, accompanied by an increase in cost of comparable magnitude: the coefficient on the $BIG * POST$ interaction ins 0.187 in the sample with change in limit, and only 0.005 in the sample without change in limit.

For comparison, we also present the results on directly estimating the profit equation in column (4). The effect on profit is very large. The coefficient of the interaction $BIG * POST$ in the sample with change in limit is 0.54, with a standard error of 0.28.

A last piece of important evidence is whether big firms become more likely to default than small firms after the reform : the increase in profits (and sales) may otherwise reflect more risky strategies pursued by the large firms. In order to answer this question, we collected additional data on the firms based in the Mumbai region (138 firms, a bit over half the sample). In particular, we collected data on whether any of these firm's loan had become non-performing assets (NPA) in 1999, 2000 or 2001, or were NPA before 1999. The number of NPAs is disturbingly

large (consistent with the high rate of NPAs in Indian banks), but large and small firms are equally likely to have a non-performing loan: 7.7% of the big firms and 7.29% of the small firms (who were not already NPA), default on their loan in 2000 or 2001. Among the firms in Mumbai, 2.5% of the large firms, and 5.96% of the small firms had defaulted between 1996 and 1998. The fraction of firms that had defaulted thus increased a little bit more for large firms, but the difference is small, and not significant. The increase in credit did not cause an unusually large number of big firms to default.

• Credit Contraction

Panel B presents the estimate of the effect of the credit contraction on the sales and costs of the firms with investment in plant and machinery larger than Rs 10 million (using all the other firms as control). In the sample with change in limit, the coefficient of the interaction $BIG2 * POST2$ is negative and large (-0.403, with a standard error of 0.207). Here again, there is little evidence for substitution: the result is similar in the sample without substitution, and the coefficient of the interaction $BIG2 * POST2$ when the ratio of sales over loan is used as the dependent variable, while positive (0.14) is small and insignificant. In the sample without change in limit, there is a small and insignificant decrease in sales (-0.092, with a standard error of 0.11). The coefficient of the interaction $BIG2 * POST2$ in the cost equation is negative and similar to the effect on sales (-0.374). Once again, there is no impact on cost in the sample where there is no change in limit. Estimating the effect of the contraction on log(profit) directly yields an estimate of -0.923, fairly imprecise (the standard error is 0.639).

• Full sample and overidentification tests

Table 8 present the results of estimating equation (6) for sales and costs. We use the entire period, and we estimate separately the coefficients of the interactions $MED * POST$, $BIG2 * POST$, $MED * POST2$ and $BIG2 * POST2$. We also present in the table the ratios of the interaction coefficient in the outcome equation and to the corresponding coefficient in the loan equation (from table 5, panel B, column (7)). In the sales and cost equation, the coefficients have the expected pattern: both the coefficients of the $MED * POST$ and $BIG2 * POST$ interactions are positive (though introduced separately, they loose some significance). The coefficient of the interaction $BIG2 * POST2$ is negative and significant and, while negative, the coefficient of the

interaction $MED * POST2$ is almost 5 times smaller and insignificant. The ratio between these coefficients and the corresponding coefficients in the loan equation are similar (in column (1), they range between 0.73 and 0.83 for the first three ratios, the three variables for which their is a significant first stage), and their equality cannot be rejected by the overidentification tests.

Taken together, these results present a consistent picture suggesting that firms are credit constraints. The sales of the firms affected by the reform increased when the reform resulted in an expansion in credit, and decreased when the reform was a contraction in credit. A subset of firms that was affected by the expansion, but not the contraction, behaved in the same way as the affected firms in the expansion, but as the unaffected firms in the contraction. The ratios between the changes in sales and the change in loans is similar for the two groups of firms, and during the expansion as well as the contraction. This makes it unlikely that the results are driven by differential trend. Furthermore, all these results are concentrated on the firms that experienced a change in loans, which makes it unlikely that the effect is driven either by differential trends or by a direct effect of being unconstrained.

4.4 Instrumental Variables Estimates

Finally, we present in table 9 the instrumental variable estimates of the effect of bank loans on sales, costs and profit. For comparison, we also present the weighted least squares estimate. Column (1) presents the IV estimate of the effect of bank loans on sales, using the instrument $BIG * POST$ in the sample with a change in loan in the 1997-1999 period. The coefficient is 0.75. Column (2) uses the "contraction" experiment (the instrument $BIG2 * POST2$ in the 1998-2002 period). This estimate is very close to the previous one (0.73). This is a other form of overidentification test: using either experiment leads to to estimate very similar impact of working capital loans on sales. Finally, column (3) uses the entire period, and uses two instuments ($BIG * POST$, $BIG2 * POST2$). The coefficient is, once again, very close. As we indicated, if firms do not increase market credit in proportion to the increase in bank credit, this is a lower bound on θ. The point estimate thus suggests that in the neighborhood of the equilibrium, θ is probably close to 1, which reinforces the conclusion that there must be credit constraint in equilibrium. Columns (4) restrict the sample to firms that do not produce SSI

products. The coefficient is somewhat smaller and less precise, though it cannot be directly distinguished (the coefficient is 0.50, with a standard error of 0.35). In column (5) we go back to all the firms, and we include firms with no change in limit. The estimate is a little higher (0.93) but very imprecise. Finally the last column present the OLS estimate, which is smaller than the IV estimate.

Panel B present the estimate of the effect of bank loans on costs. The estimate we obtain here are, again, very close to each other, and just a little smaller than the effect of the loans on sales.

We can use this estimate to get a sense of the average increase in profit caused by every rupee in loan.

We can use this estimate to get a sense of the average increase in profit caused by every rupee in loan. The average loan is 86,800 Rupees. Therefore, using the coefficients in column (3), an increase of Rs. 1,000 in the loan corresponds to an increase in Rs 6,100 in sales, and Rs 5,370 increase in costs. This implies an Rs 730 increase in profit for the average firm, after repaying interest.

In panel C, we present for comparison the direct IV estimate of loans on log(profit), despite the fact that these regression suffer from the sample selection induced by the negative profit. The estimates vary between 1.79 and 2.00. Taking 2 as the estimate of the effect of the log increase in loan on log increase in profit, an increase of Rs. 1,000 in lending is estimated to cause a increase in 2.2% increase in profit. At the mean profit (which is Rs. 36665), this would correspond to an increase in profit of Rs 844, an estimate which is close to what we obtained by using the sales and costs data separately.

The impact on profit of a Rs. 1,000 increase in loans seems thus much larger than the cost of capital, which confirms that firms are credit constrained. In particular, it is too big to be explained as the subsidy impact of the loans.

The OLS estimates of the effect of loan increases on sales or profit increases are both smaller than the IV estimates. For sales, the OLS estimate is significant, but equal to only 0.28. The OLS estimates of the effect of loans on profit is even smaller, and insignificant. Although we cannot reject the equality of the OLS and the IV estimates, the difference suggests that the bank tends to target larger expansion toward firms whose sales and profits would otherwise increase

more slowly.

5 Conclusion: Policy Issues

It might be tempting, faced with the evident failure of credit markets in India, to put the entire blame on the public ownership of banks in India. It is important to note, however, that our data is from the recent, post-liberalization, period: by 1996, both private Indian and multinational banks had made significant inroads into the market for banking services. Our firms, all based in relatively urban areas, certainly had the option of approaching a non-public sector bank for additional credit and perhaps did. The interesting question is why, nevertheless, they had not managed to invest much more, especially given the enormous profitability of additional investment.

One possible answer is that the local private banks were still in their infancy and did not yet have enough resources to lend to these firms—this puts the blame on the pre-liberalization policy of public ownership, albeit indirectly. It is, however, belied by the fact that these banks were investing heavily in government bonds throughout this period. It also seems less plausible in the case of the multinational banks. A more plausible version of this argument points to the fact that lending to the small-scale sector requires a certain rather specific expertise that is only acquired over time and most non-public sector do not yet have it. This would suggest that the existing public sector banks, once privatized, may be much more effective than the present crop of private banks, precisely because they have the requisite experience.[21]

There are however good reasons not to be quite so optimistic. Jeremy Stein, for one, has argued that the inability to lend effectively to small borrowers is in the very nature of being a bank:[22] banks have a natural tendency to be large, in order to spread out idiosyncratic risk. On the other hand, being larger necessarily increases the distance between the owners and the many loan officers who deal with small borrowers. Since loan officers need to take decisions about relatively large amounts of money that do not belong to them, and defaults are costly for

[21] This also suggests that while the public sector banks are probably over-staffed, the extent of over-staffing may be over-estimated if we directly compare private and public banks, because private and public sector currently play very different roles.

[22] See Stein (2001).

the bank,[23] it is very important that the loan officers have the right incentives. This obviously gets harder as the distance between the owner and the loan officer grows. Banks deal with this problem in part by restricting the domain of the loan officer's authority: in particular, by making rules, based on easily measured characteristics of the borrower, about how much they can borrow. This obviously limits the discretion the loan officer enjoys and makes his lending less effective, but it covers the bank.[24] An obvious social cost is that small firms have a hard time borrowing.[25]

It is therefore important not to lose track of the importance of policy changes that would make it easier to lend to small firms by focussing entirely on the privatization issue. In particular, it may help to set up special courts for the speedy dispositioon of default cases (some states in India are experimenting with this model). It is also important to improve the system of recording titles to, and liens on, property, to avoid the possibility that the same asset may be used to secure multiple loans. Severe punishments for those involved in asset-stripping and other types of fraud will also make lenders more forthcoming.

[23]Defaults are also quite common, at least in India. Working capital loans in India are not nearly as safe as they are supposed to be (and actually are, at least in the US).This is because the borrower can easily sell off the inventories that are supposed to be securing the loan before he defaults, and hide the proceeds. While this is potentially actionable, inefficiency of the legal system discourages going after borrowers. The result is that most commercial banks have a lot of non-performing assets (estimated to be as much as 10% of total assets) despite the fact that most of their lending is in the form of working capital loans.

[24]It is therefore not surprising that the existing rules in India leave little room for independent decision-making. In particular, projections of future profits (an area where judgement tends to be important) have no place in the decision. Maximum permissible bank finance is calculated as a percentage of projected sales. In turn, the guideline is that projected sales should not exceed current sales plus 15%.

[25]Berger et al. show that in the US, the increasing concentration in banking after deregulation, has significantly reduced access to credit for small firms.

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Table 1: Descriptive statistics

	levels		Change(t)-(t-1)	
	entire sample	change in loans not missing	entire sample	change in loans not missing
	(1)	(2)	(3)	(4)
PANEL A: LOANS AND INTEREST RATES				
working capital	87.66	96.29	10.29	7.46
loan (this bank)	(237.04)	(258.2)	(59.92)	(55.32)
	1226	928	966	928
log(working capital	3.39	3.44	0.07	0.07
loan) (this bank)	(1.47)	(1.5)	(.24)	(.24)
	1208	928	928	928
working capital	87	97	10	7
loans (all banks)	(246)	(273)	(69)	(67)
	1102	807	842	807
log(working capital loans)	3.36	3.41	0.06	0.06
(all banks)	(1.48)	(1.51)	(.26)	(.26)
	1085	807	807	807
other bank loans	0.0120	0.004	0.0000	-0.007
positive	(.11)	(.06)	(.14)	(.1)
	1748	807	1748	807
other bank loans	1.65	2.23	0.00	-0.62
(level)	(25.86)	(36.54)	(22.54)	(30.9)
	1748	807	1748	807
interest rate	15.75	15.58	-0.32	-0.32
	(1.63)	(1.59)	(.94)	(.94)
	1142	896	876	856
log(interest rate)	2.75	2.74	-0.02	-0.02
	(.18)	(.19)	(.16)	(.17)
	1142	896	878	858

Notes:
1-Columns 1 and 2 present the mean level of each variable, with the standard error in parentheses and the number of observations on the third line
2-Columns 3 and 4 present the mean change in each variable, with the standard error in parentheses and the number of observations on the third line
3. Value in current Rs.1000

Table 1 (continued) Descriptive statistics

	levels		Change t-t-1	
	entire sample	change in loans not missing	entire sample	change in loans not missing
	(1)	(2)	(3)	(4)
PANEL B: CREDIT UTILIZATION AND FIRM PERFORMANCE				
account reaches the	0.72	0.69	-0.01	-0.01
limit	(.45)	(.46)	(.44)	(.44)
	522	380	247	233
log(turnover/limit)	2.15	2.15	0.09	0.11
	(.95)	(.96)	(.72)	(.71)
	384	308	170	167
Sales	709.33	820.70	108.64	86.66
	(2487.24)	(2714.88)	(653.62)	(598.64)
	1259	746	1041	739
log(sales)	5.49	5.64	0.17	0.09
	(1.44)	(1.46)	(.53)	(.45)
	1248	740	1029	732
log(sales/loan ratio)	2.19	2.18	-0.01	0.02
	(.89)	(.87)	(.53)	(.49)
	1004	740	751	732
net profit	36.51	42.49	6.08	4.04
	(214.11)	(237.16)	(61.32)	(58.3)
	1259	747	1043	741
log(costs)	5.45	5.61	5.45	5.61
	(1.45)	(1.45)	(1.45)	(1.45)
	1245	739	1245	739
negative net profit	0.10	0.10	0.06	0.04
	(.3)	(.3)	(.23)	(.21)
	1259	747	1259	747

Notes:
1-Columns 1 and 2 present the mean level of each variable, with the standard error in parentheses and the number of observations on the third line
2-Columns 3 and 4 present the mean change in each variable, with the standard error in parentheses and the number of observations on the third line
3. Value in current Rs.1000

Table 2: Characteristics of Loans

	1997	1998	1999	2000	2001	2002
	(1)	(2)	(3)	(4)	(5)	(6)
proportions of cases in which						
Granted limit remained the same	0.66	0.64	0.65	0.76	0.73	0.73
Limit was attained by the borrower	0.81	0.67	0.77	0.76	0.68	0.57
Granted limit from banking system remained the same	0.66	0.63	0.63	0.76	0.73	n/a
Maximum authorized limit has increased	0.63	0.74	0.73	0.58	0.77	0.74
Predicted sales have increased	0.72	0.67	0.73	0.71	0.70	0.71
Granted limit <maximum authorized limit	0.60	0.63	0.60	0.50	0.47	0.22
Granted limit <0.20*predicted sales	0.85	0.85	0.79	0.82	0.82	0.81
Means:						
Ratio granted limit/maximum authorized	0.88	0.81	0.90	0.83	0.99	1.00
	(.061)	(.05)	(.054)	(.056)	(.126)	(.07)
Ratio granted limit/(0.20*predicted sales)	0.62	0.63	0.68	0.63	0.68	0.71
	(.041)	(.037)	(.034)	(.055)	(.064)	(.062)
number of loans	175	208	205	175	163	124

Table 3: Changes in working capital limits, by firm characteristics

	Proportion	Propoportion of cases where limit was increased	Propoportion of cases where limit was not changed	Mean of: log(current limit) -log(past limit)	Proportion of cases where limit was changed: Client<=5 years	Proportion of cases where limit was changed: Client>5 years
	(1)	(2)	(3)	(4)	(5)	(6)
A- PAST UTILIZATION HAS REACHED MAXIMUM (N=300)						
Yes	0.72	0.34	0.60	0.16	0.55	0.67
No	0.28	0.30	0.66	0.12	0.61	0.71
Difference		0.05	-0.05	0.03	-0.05	-0.04
(standard er	380	(.054)	(.056)	(.04)	(.081)	(.072)
B-PROJECTED SALES HAS INCREASED (n=163)						
Yes	0.71	0.43	0.52	0.19	0.54	0.54
No	0.29	0.25	0.61	0.06	0.50	0.67
Difference		0.18	-0.09	0.13	0.04	-0.13
(standard er	194	(.076)	(.079)	(.053)	(.114)	(.101)
C-CURRENT SALES HAS INCREASED (n=561)						
Yes	0.71	0.33	0.62	0.13	0.61	0.68
No	0.29	0.25	0.69	0.12	0.70	0.72
Difference		0.08	-0.06	0.02	-0.09	-0.04
(standard er	688	(.041)	(.043)	(.029)	(.059)	(.05)
D-PROFIT OVER SALE HAS INCREASED (N=474)						
Yes	0.56	0.29	0.67	0.11	0.64	0.69
No	0.44	0.35	0.61	0.16	0.61	0.69
Difference		-0.05	0.06	-0.05	0.03	0.00
(standard er	574	(.042)	(.044)	(.028)	(.059)	(.053)
E- CURRENT RATIO HAS INCREASED (n=557)						
Yes	0.53	0.32	0.62	0.12	0.61	0.70
No	0.47	0.29	0.67	0.14	0.67	0.68
Difference		0.03	-0.05	-0.02	-0.06	0.02
(standard er	683	(.038)	(.04)	(.027)	(.052)	(.049)

Table 4: Average change in limit

	Years		
	1997	1998-1999	2000-2002
Firm's category			
A. Average change in limit			
small	0.110	0.075	0.070
	(.021)	(.013)	(.014)
medium	0.040	0.093	0.011
	(.032)	(.03)	(.025)
biggest	0.093	0.147	0.000
	(.064)	(.04)	(.031)
B. Proportion of cases where limit was not changed			
small	0.701	0.701	0.724
	(.043)	(.031)	(.027)
medium	0.667	0.608	0.798
	(.088)	(.055)	(.04)
biggest	0.625	0.692	0.769
	(.183)	(.075)	(.053)
C. Average change in limit, conditional on change			
small	0.366	0.252	0.253
	(.045)	(.035)	(.045)
medium	0.119	0.237	0.053
	(.093)	(.068)	(.124)
biggest	0.248	0.479	-0.002
	(.137)	(.062)	(.138)

Notes:
1-The first row of each panel presentsthe average of log(working capital limit granted at date t)-log(working capital limit granted at date t-1)
2-Standard errors in parentheses below the average
3-Number of observations in the third row of each panel
4-"small firms" are firms with investment in plant and machinery below 6.5 million Rs.
"medium firms" are firms with investment in plant and machinery above Rs 6.5 million and below Rs 10 million
"big firms" are firms with investment in plant and machinery above Rs 10 million Rs

Table 5: Effect of the priority sector reform on credit (OLS regressions)

	Dummy equal to 1 if			Log(working capital limit granted at t)-log(working capital limit granted at t-1)			
	limit was changed	limit increased	limit decreased	Whole sample	Sample with change in limit	Whole sample	Sample with Change in limit
						Sales information not missing	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PANEL A							
post	0.000	-0.026	0.026	-0.034	-0.115	-0.025	-0.102
	(.05)	(.052)	(.024)	(.026)	(.074)	(.028)	(.071)
big	-0.043	0.016	0.027	-0.059	-0.218	-0.055	-0.206
	(.052)	(.051)	(.041)	(.028)	(.088)	(.028)	(.082)
post*big	-0.022	0.050	-0.028	0.095	0.271	0.087	0.259
	(.087)	(.079)	(.044)	(.033)	(.102)	(.032)	(.099)
	487	487	487	487	155	453	152
PANEL B							
post2	0.069	-0.073	0.004	-0.027	-0.038	-0.028	0.001
	(.032)	(.037)	(.024)	(.024)	(.075)	(.026)	(.077)
very big	0.017	0.041	-0.058	0.067	0.232	0.057	0.251
	(.129)	(.131)	(.017)	(.059)	(.063)	(.058)	(.057)
post2*biggest	0.008	-0.127	0.119	-0.121	-0.442	-0.128	-0.549
	(.179)	(.172)	(.033)	(.082)	(.191)	(.08)	(.171)
	769	769	769	769	217	569	168
PANEL C							
post*biggest	0.067	-0.041	-0.026	0.089	0.346	0.076	0.352
	(.15)	(.15)	(.024)	(.059)	(.146)	(.059)	(.145)
post*medium	-0.059	0.076	-0.016	0.088	0.233	0.083	0.221
	(.098)	(.09)	(.051)	(.041)	(.122)	(.042)	(.119)
post2*biggest	0.054	-0.176	0.122	-0.142	-0.482	-0.150	-0.581
	(.175)	(.17)	(.033)	(.077)	(.181)	(.076)	(.157)
post2*medium	0.168	-0.177	0.010	-0.077	-0.185	-0.078	-0.170
	(.034)	(.052)	(.04)	(.044)	(.167)	(.04)	(.159)
	924	924	924	924	265	718	215

Table 6: Credit rationing: Effect of the reform on interest rate and limit utilization (OLS regressions)

	Complete sample				Sample where limit was changed			
	granted interest rate$_t$ -granted interest rate$_{t-1}$	log(interest rate)$_t$ -log(interest rate)$_{t-1}$	dummy for interest rate decline	log(turnover/limit)$_{t+1}$ log(turnover/limit)$_t$	granted interest rate$_t$ -granted interest rate$_{t-1}$	log(interest rate)$_t$ -log(interest rate)$_{t-1}$	dummy for interest rate decline	log(turnover/limit)$_{t+1}$ log(turnover/limit)$_t$
	(1)	(2)	(3)	(8)	(5)	(6)	(7)	(8)
A. 1996-1999								
post	-0.165	-0.010	0.280	0.134	-0.127	-0.007	0.279	-0.030
	(.128)	(.008)	(.074)	(.189)	(.249)	(.015)	(.151)	(.336)
big	-0.002	0.000	0.098	0.324	-0.036	-0.002	0.052	0.257
	(.132)	(.008)	(.106)	(.191)	(.241)	(.014)	(.153)	(.362)
post*big	0.073	0.002	-0.135	-0.483	0.163	0.009	-0.144	-0.128
	(.169)	(.011)	(.125)	(.242)	(.337)	(.02)	(.225)	(.458)
	430	430	430	109	141	141	141	44
B. 1998-2001								
post2	0.035	-0.009	-0.029	0.062	-0.146	-0.008	0.225	-0.051
	(.072)	(.013)	(.038)	(.131)	(.167)	(.013)	(.068)	(.272)
big2	-0.062	-0.007	-0.010	-0.307	-0.077	-0.004	0.039	0.339
	(.11)	(.008)	(.063)	(.176)	(.188)	(.011)	(.14)	(.174)
pos2*big2	0.099	0.020	0.001	0.816	0.206	0.013	-0.036	-0.134
	(.147)	(.017)	(.098)	(.359)	(.385)	(.026)	(.184)	(.232)
	719	721	721	144	203	203	203	52

Note

1-Standard errors (corrected for heteroskedasticy and clustering at the sector level) in parentheses below the coefficients

Table 7: Credit constraints: Effect of the reform on sales, sales to loan ratios, and profits (OLS regressions)

	Dependent variables				
	$Log(sales)_t$-$log(sales)_{t-1}$		$log(sales/loans)_t$	$Log(costs)_t$	$Log(profit)_t$
	Complete Sample	Sample without substitution	$log(sales/loans)_{t-1}$	$-log(cost)_{t-1}$	$-log(profit)_{t-1}$
	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
A. 1996-1999					
1. Sample with Changes in limit					
post*big	0.194	0.168	-0.065	0.187	0.538
	(.106)	(.118)	(.104)	(.097)	(.281)
	152	136	152	151	141
2. Sample without Change in limit					
post*big	0.007	0.022	0.007	0.005	0.280
	(.074)	(.081)	(.074)	(.064)	(.473)
	301	285	301	301	250
3. Whole sample					
post*big	0.071	0.071	-0.016	0.068	0.316
	(.068)	(.069)	(.075)	(.055)	(.368)
	453	421	453	452	391
B. 1998-2001					
1 Sample with Changes in limit					
post*big	-0.403	-0.387	0.143	-0.374	-0.923
	(.207)	(.196)	(.206)	(.279)	(.639)
	168	150	169	168	151
2. Sample without Change in limit					
post*big	-0.092	-0.045	-0.092	-0.048	0.170
	(.108)	(.128)	(.108)	(.086)	(.56)
	401	380	401	399	321
3. Whole sample					
post*big	-0.143	-0.113	-0.016	-0.101	-0.253
	(.111)	(.134)	(.162)	(.094)	(.496)
	569	530	570	567	472

Table 8: Credit constraints: Effect of the reform on sales, sales to loan ratios, and profits (OLS regressions)

	Dependent variables		
	$Log(sales)_t$-$log(sales)_{t-1}$		$Log(costs)_t$
	Complete Sample	Sample without substitution	-$log(cost)_{t-1}$
	OLS	OLS	OLS
	(1)	(2)	(3)
A. Sample with Changes in limit			
post*big	0.238	0.235	0.205
	(.153)	(.162)	(.151)
post*med	0.182	0.146	0.183
	(.121)	(.134)	(.109)
post2*big	-0.421	-0.400	-0.384
	(.197)	(.186)	(.279)
post2*med	-0.091	-0.095	-0.072
	(.113)	(.115)	(.112)
	215	193	215
ratio 1: gamma_3y/gamma_3kb	0.676	0.666	0.583
ratio 2: gamma_4y/gamma_4kb	0.825	0.662	0.829
ratio 3: gamma_3y/gamma_3kb	0.725	0.689	0.660
ratio 4: gamma_3y/gamma_3kb	0.535	0.561	0.424
test ratio 1=ratio2	0.44	0.48	0.44
(p value)	(0.51)	(0.49)	(0.51)
test ratio 1=ratio2=ratio3	0.30	0.36	0.23
(p value)	(0.74)	(0.70)	(0.79)
test ratio 1=ratio2=ratio3=ratio4	0.20	0.25	0.19
(p value)	(0.90)	(0.86)	(0.91)

1-Standard errors (corrected for clustering at the sector level and heterosckedasticity) in parentheses below the coefficients.

Table 9: Effect of working capital loans on sales and profit, IV and OLS estimates

	Dependent variables					
	2SLS	2SLS	2SLS	2SLS	2SLS	WLS
	Sample with change	Sample with change	Sample with change	Sample with change	Complete sample	Complete sample
	1996-1999	1998-2001	1996-2001	1996-2001	1996-2001	1996-2001
				no ssi products		
Regressor:	(1)	(2)	(3)	(4)	(5)	(6)
A. log(sales$_{t+1}$)-log(sales$_t$)						
log(working capital limit_t)	0.75	0.73	0.76	0.50	0.93	0.21
-log(working capital limit_t-1)	(.37)	(.35)	(.32)	(.35)	(1.12)	(.07)
observations	152	168	215	190	718	718
B. log(cost$_{t+1}$)-log(cost$_t$)						
log(working capital limit_t)	0.72	0.68	0.70	0.44	0.67	0.24
-log(working capital limit_t-1)	(.36)	(.44)	(.4)	(.5)	(.82)	(.07)
observations	151	168	215	189	716	716
C. log(profit$_{t+1}$)-log(profit$_t$)						
log(working capital limit_t)	1.79	1.89	2.00	2.02	2.08	0.15
-log(working capital limit_t-1)	(.94)	(1.49)	(.996)	(.99)	(3.26)	(.2)
observations	141	151	192	166	598	598

Notes:
1-Standard errors (corrected for clustering at the sector level and heterosckedasticity) in parentheses below the coefficients.
2-The regression in column 1 and 6 control for the dummy for whether the firm has investment in plant and machinery above Rs 6.5 Million, and the dummy for whether data is for 1998 and later, or before. The interaction between the two variables is used as the instrument.
3-The regression in column 2 and 7 control for the dummy for whether the firm has investment in plant and machinery above Rs 100 million, and the dummy for whether data is for 1998 and later, or before. The interaction between the two variables is used as the instrument.
3-The regression in column 3,4 and 8 and 9 control for the dummy for whether the firm has investment in plant and machinery above Rs 100 million, the dummy for whether the firm has investment in plant and machinery above Rs 6.5 Million, a dummy for whether the data is for 1998 or later and a dummy for whether the data is for 2000 or later. The two interactions used in the other columns are used as instruments.