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

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Abstract

This paper uses variation in access to a targeted lending program to estimate whether firms are credit constrained. 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 cheaper 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 apply these observations 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 a large 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, and that the marginal rate of return to capital was very high for these firms. *Keywords*: Banking, Credit constraints, India *JEL*: O16, G2

*We thank Tata Consulting Services for their help in understanding the Indian banking industry, Sankarnaranayan for his work collecting the data, Dean Yang and Niki Klonaris for excellent research assistance, and Robert Barro, Sugato Battacharya, Gary Becker, Shawn Cole, Ehanan Helpman, Sendhil Mullainathan, Kevin Murphy, Raghuram Rajan and Christopher Udry for very useful comments. We are particularly grateful to the administration and the employees of the bank we studied for their giving us access to the data we used in this paper.

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1 Introduction

That there are limits to credit access is widely accepted today as an important part of an economist's description of the world. Credit constraints figure prominently in economic analyzes of short-term fluctuations and long-term growth.[1] Yet there is still little tight evidence of the existence of credit constraints on larger firms in developing countries. While there is evidence of credit constraints in rural settings in developing countries,[2] except in the very poorest countries, the share of agriculture in the capital stock is not large enough for the credit constraints to have quantitatively large effects: in India, the share of agriculture in output is 24% and its share in the capital stock is even smaller. Moreover, in Banerjee and Duflo (2005), we argue that it is not enough to show that the very smallest firms are credit constrained, since while they are numerous, the share of capital in these firms is too small to have much power in terms of explaining the cross-country productivity differences. On the other hand, if it is the medium-sized firms that are constrained, the productivity loss due to the missallocation of capital caused by credit constraints may be potentially very large: indeed, we argue that it may be large enough to explain the entire productivity gap between India and the US.

Prima facie, the idea that such firms may be severely constrained is certainly consistent with the available evidence. Banerjee and Duflo (2005) survey evidence from many less developed countries, showing that borrowing interest rates are often of the order of 60% or above, even though deposit rates are less than half as much, and defaults are rare. This suggests that the marginal product of capital in the firms paying these rates might far exceed the opportunity cost of capital. However this evidence is only partly convincing: the problem is that we do not know whether the firms that pay these rates are somehow atypical (smaller, more desperate, irrational).[3]

[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 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.

[3] Although it is worth saying that the studies from which these interest rate numbers come concern normal

There is also some direct evidence on rate of returns on capital. McKenzie and Woodruff (2004) estimate parametric and non-parametric relationships between firm earnings and firm capital. Their estimates suggest huge returns to capital for these small firms: for firms with less than $200 invested, the rate of return reaches 15% per *month*, well above the informal interest rates available in pawn shops or through micro-credit programs (on the order of 3% per month). These regressions may suffer from of an "ability bias" caused by a correlation between investment level and rates of return to capital (Olley and Pakes (1996)). To address this issue, De Mel, Mckenzie and Woodruff (2006) randomly allocate small ($200 or so) capital grants to microenterprises in Sri Lanka, and find that the returns to capital are also very high for those firms: the average returns to capital is as high as 4% per month.

These firms are, however, very small and unconnected to any bank. Unfortunately, it difficult to imagine carrying out the same experiment for larger firms that are already connected to the banking sector. Thus, in this paper, we take advantage of a natural experiment to estimate the effect of an influx of credit on investment and productivity of medium-sized firms in India.[4] We make use of a policy change that affected the flow of directed credit to an identifiable subset of firms. Such policies and policy changes are common in many developing and developed countries. What makes this case particularly interesting is that the firms affected by the policy are officially registered firms (this means that they are not part of the informal economy, although none of these firms are listed on the stock market), fairly large by Indian standards, though not the largest corporate entities: the average capital stock of firms in the 95th percentile in the median industry in India was Rs. 36 million (the exchange rate was about 45 rupees to a dollar), which puts them at a size just above the category of firms that were affected by the policy change (which required a capital stock between Rs. 6.5 and Rs. 30 million).

The advantage of our approach is that it gives us a specific exogenous shock to the supply of credit to specific firms. Its disadvantage is that directed credit need not be priced at its true market price. This has two important implications: First, firms will want directed credit even if they are not credit constrained simply because it is a cheaper substitute for market credit.

production loans to stable firms, not emergency loans.

[4]The approach of looking directly at an identifiable shock to credit supply for a specific subgroup is similar in spirit to that in Peek and Rosengren (2000), who look at the impact of a reduction in credit at U.S. subsidiaries of Japanese banks during the Japanese banking crisis on real estate activity in the U.S.

Therefore the fact that a firm is willing to borrow more at the same price when more is offered to them, cannot be seen as evidence that they are credit constrained it is rationed with respect to this particular source of cheap credit, but not necessarily credit constrained. Second, and more troublingly, a shock to the supply of directed credit might lead not just to more borrowing but also to *more investment* even if a firm is not credit constrained.

In the theoretical section of this paper we develop a simple methodology based on ideas from price theory that allows us to deal with the inference problem suggested in the previous paragraph. 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 we collected from a sample of 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 (from Rs. 6.5 to Rs. 30 Million). 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 were included in the priority 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, relative to firms that were already included. 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 argued earlier, the last two observations are inconsistent with firms being unconstrained in their market borrowing. Our second experiment uses the fact that a large fraction of these firms (specifically those with investments higher than Rs. 10 million) that were included in the priority sector in 1998, got

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

excluded again in 2000. We find that bank lending and firm revenues went down for these firms, both compared to the firms that had always been a part of the priority sector and to firms that were included in 1998, and remained part of the priority sector in 2000. Moreover because we have two separate experiments that each allow us to estimate the effects on credit and revenue growth, we can implement an overidentification test which essentially ask whether the effect of credit on revenue is the same in the two cases. The results easily pass this test, making it much less likely that the effects are generated by differential time trends in the productivity of small, medium and large firms: for that to be the explanation for what we find, the productivity trends would have to have reversed in exactly the right way at exactly the right time.

We also use this data to estimate parameters of the production function. We estimate an elasticity of sales with respect to bank credit of 0.75. This ought to be a lower bound on the elasticity of sales with respect to working capital (since bank loans are only a part of working capital, we would expect working capital not to go up in the same proportion as bank loans when bank loans go up as a result of the policy shock). We will argue that if there is any substitution of market borrowing for bank borrowing, given what we know about the share of bank loans in total working capital, the elasticity of sales with respect to total working capital must be well above 1. This would imply that firms have increasing returns in the neighborhood of their current level of investment, and therefore must be credit constrained (otherwise they would keep borrowing).

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 interest rate paid on the marginal dollar (the point estimate is that Rs. 1 more in loans increased profits net of interest payments by Rs. 0.73, which is much too large to be explained as just the effect of getting a subsidized loan).

These very high estimates for the elasticities make it particularly important to understand how the extra credit that the program generated was allocated. In the second part of the theoretical section we therefore study the allocation problem faced by a loan officer who is rewarded for lending more but are also punished for defaults. We show that this creates an incentive for targeting credit (a) towards firms that are the least likely to default which may also be some of the more profitable firms, but also (b) towards firms that are on the brink of

default and therefore need to be bailed out. As a result, an OLS regression of revenue (or profitability) on credit growth may be biased downwards or upwards by selection. On the other hand, when the loan officer gets an unexpected inflow of additional loanable funds, he would want to give all of that to the firms that are least likely to default, since the set of firms that need to be bailed out remains unchanged and he was already taking care of those firms before the inflow of extra credit. Assuming that the firms that are the least likely to default are also among the most productive firms, we should therefore expect to see the instrumental variables estimate of the effect of credit growth on revenue growth to be much stronger than the OLS estimate, when the source of the variation is a program like the one we are studying.[6]

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 following section develops the theory that justifies our empirical strategy, and provides some useful insights for interpreting what we find. The next section develops the equations 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. In 2000 the 27 public sector banks collected over 77% of deposits and comprised over 90% of all branches.

The particular bank we study is a public sector bank, generally considered to be a good bank.[7]

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

[6]Note that we are still consistently estimating a causal effect of the extra credit with this experiment, but this is the effect on the extra credit on good firms, which are the ones affected by the reform.

[7]It is consistently rated among the top five public sector banks by Business Today, a major business magazine.

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 borrower draws from the limit when needed, and reimburses on a quarterly basis. This paper therefore estimates the impact of short term capital loans, not that of long term investment credit; moreover, it focuses on the working capital limit (which is the amount of working capital financing available to the firm at any point).

The spread 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 (public and private) 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. The reform extended the definition 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 seems to have been 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 administrative cost of lending. Banerjee and Duflo (2000), calculated that, for four Indian public banks, the labor and administrative costs associated with lending to the priority sector were about 1.5 higher per rupees than that of lending in the unreserved sector. This is consistent with the common view that lending to smaller clients is more costly.

With the reform, we thus expect an increase in lending to the larger firms newly included in the priority sector, possibly at the expense of the smaller firms. When firms with investment in plant and machinery above 10 million were excluded again from the priority sector, loans to these firms no longer counted towards the priority sector target. The bank had to go back to the smaller clients to fulfill its priority sector obligation. One therefore expects that loans to those firms declined relative to the smaller firms. We focus on the comparison between larger firms and smaller firms, and evaluate whether any relative change in loans between these groups was matched by a corresponding change in sales and revenue.

2.3 Data Collection

The bank we study, like other public sector banks, routinely collects balance sheets 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 is also stored in the folder, along with the firm's initial application even when there is no formal review of the file. The folder is typically stored in the branch until it is physically impossible to put more documents in it.

With the help of employees from this bank and a former bank officer, we first extracted data from the loan folders from the clients of the bank 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 it charges.

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, and the sample was selected with this in mind. We first selected all the branches that primarily handle business accounts in the six major regions of the bank's operation (including New Delhi and Mumbai). In each of these branches, we collected information on all the accounts of the clients of the bank of firms which, as of 1998, had investment in plant and machinery below 30 million Rupees. This gave us a total of 249 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, so that old data gets "lost". Moreover, in some years, data is not collected for some firms. We have data on lending from 1996 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 217 accounts (of 238) , and 1999 data for 213 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. We have 2000 data for 175 accounts, 2001 data for 163 accounts, and 2002 data for 124 accounts.

There are two reasons why we have less data in 2000, 2001 and 2002 than in 1999. First, that some firms had not had their 2002 review when we re-surveyed them in late 2002. Second, 43 accounts were closed between 2000 and 2002. The proportion of accounts closed is balanced: it is 15% among firms with investment in plant and machinery above 10 million, 20% among firms with investment in plant and machinery between 6.5 and 10 million, and 20% among firms with investment in plant and machinery below 6.5 million. Thus, it does not appear that there sample selection bias would emerge from the closing of those accounts.[8]

Table 1 presents the summary statistics for all data 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 is 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 working capital limit that the bank makes

[8]The reason why we do not observe attrition in the 1998-1999 period is because our data for that period was collected retrospectively in 2000: to be in our data set, an account had to still be in existence in 1999. This implies that our sample only represents the survivor as of 1999. However, given that attrition is not differential in response to second reform in 2000, there is again little concern that this sample selection biases the results.

available to the firm 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 means that the borrower had drawn more from the limit in the course of a quarter than was available in the credit limit.

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. Indeed, sales have increased from year to year for most firms (row 2), as did the maximum authorized lending (a function of projected sales). Yet there was no corresponding change in lending from the bank. In fact 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 itself. On average, the granted limit was 89% of the recommended limit.

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 were *not* more likely to get an increase in limit if they had hit the limit in the previous year, if their projected sales (*according to the bank itself*) or their current sales had gone up, if their ratio of profits to sales had risen, or if their current ratio (the ratio of current assets to current liabilities, a

standard indicator, in India as well as in the US, of how secure a working capital loan is) had 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 last result could well be due to reverse causality, however: the bank officers appear to be more likely to predict an increase in sales when he is preparing to give a larger credit extension to the firm.

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 past utilization, increases in projected sales, or profits. This suggest that the lack of information to new signal may not reflect that all the relevant information about the firm was already incorporated in the lending decisions.

3 Theory: the demand and supply of bank credit

Motivated by this evidence, the goal of this section is to develop some intuition about the demand and supply of subsidized bank credit. The sub-section on demand sketches the choice problem faced by a firm that has (limited) access to cheap bank credit but can also borrow from the market at a higher rate. We are interested in how increased access to cheap bank credit affects the market borrowing of the firm as well as its revenues and profits. We contrast its reaction in the case where it has unlimited access to market credit at a fixed rate with the case where it is constrained in its access to market credit.

The sub-section on supply then tries to understand the allocation of subsidized bank credit among users who all want it. In particular we analyze the incentives facing a loan officer and the choices he will make. This will provide a framework to interpret our findings.

3.1 The demand side: the key to identifying credit constraints

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 a factory and installing 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.

As mentioned above, the interesting case is where the firm has access to both low cost bank credit and more expensive credit from other sources. We will say that a firm is *credit rationed* with respect to a particular lender if there is no interest rate r such that the amount the firm wants to borrow at that rate is strictly positive and equal to an amount that the lender is willing to lend at that rate.[9] Essentially this says that the supply curve of loans from that lender to the firm is not horizontal at some fixed interest rate.

We will say the firm is *credit constrained* if there is no interest rate r such that the amount that the firm wants to borrow at that rate is equal to an amount that all the lenders taken together are willing to lend at that rate. This says that the aggregate supply curve of capital to the firm is not horizontal at some fixed interest rate.

Note that a firm could be credit rationed with respect to every lender without being credit constrained in our sense. This can be the case, for example, when there is an infinite supply of lenders, each willing to lend to no more than \$10 at an interest rate of 10%.

We can get all the relevant intuition from the simple case where there are only two lenders, which we will call the "market" and the "bank". Denote the market rate of interest by r_m and the interest rate that the bank charges by r_b. Given that the bank is statutorily required to lend a certain amount to the priority sector, there is reason to believe that the bank would have to set a rate that is below the market rate: $r_b \leq r_m$.

The policy change we analyze involves the firms in question being offered additional bank credit, for the purpose of working capital investment. 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 with respect to the bank*. 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

[9]The amount the firm wants to borrow at a given rate is assumed to be an amount that would maximize the firm's profit if it could borrow as much (or as little) as it wants at that rate.

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. 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 firm's 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 be 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.[10]

Result 2: If the firm is credit constrained, an expansion of the availability of bank credit

[10]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.

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. However, both results hold if instead of the strict rationing we have assumed here the firms face an upward supply curve for bank credit. The result also holds if there are more than two lenders, as long we interpret it to be telling us what happens to the more expensive sources of credit when the supply of cheap credit is expanded.

The fact that the supply curve of market credit is drawn as a horizontal line in figure 2 is also not important—what is important is that the supply curve of market credit in this figure eventually becomes vertical. More generally, the key distinction between figure 1 and figure 2 is that in figure 1, the supply curve of market credit is always horizontal (which is why the firm is unconstrained) while in figure 2, the supply curve slopes up (which is why the firm is constrained).

The results also go 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 but this should be counted as a part of the effect of the reform.

One case where these results fail is when the firm can borrow as much as it wants from the market but not as little as it wants (because it wants to keep an ongoing credit relationship with this source). If the minimum market borrowing constraint takes the form of a minimum *share* of total borrowing that has to be from the market and this constraint binds, a firm will respond to the availability of extra bank credit by also borrowing more from the market, in order to maintain the required minimum share of market borrowing. In this case, our result 1 will fail. However as long as there are some firms that are not at this constraint, there will be some substitution of bank credit for market credit. Therefore the direct test of substitution, proposed below, would apply even in this case, as long as the minimum market borrowing constraint does not bind for all the firms.

Another case where the results would fail is if the firm were not making a marginal choice: If the firm was choosing whether to shut down or not, and there was a fixed cost of operating the business, the availability of additional subsidized credit might be decisive and in this case, the

effect of subsidized credit on sales would be positive even if the firm were unconstrained in the credit market and had not fully substituted its market borrowing. Similarly a certain number of unconstrained firms would shut down when deprived of their access to subsidized credit.

This can be addressed by looking at what happened to the firms that were in our sample in 2000, when the subsidy they were getting were removed. We observe in the sub-section on data collection, there is no systematic difference in exit rates between large and small firms in the 2000-2002 period. Indeed, rather surprisingly, attrition is actually slightly lower for bigger firms in this period. This gives us some confidence that the results we show below are not driven by exit resulting from the withdrawal of the subsidy.

3.2 The supply side: understanding lending behavior in Indian Banks

The analysis of the supply side will help us build some intuition about how to interpret the empirical results. In particular we want to understand how subsidized bank loans will be allocated to firms before and after the reform. Which types of firms tend to get more credit before the reform? How is the new credit allocated to firms after the reform? Are some firms getting more credit or are more firms getting credit? Are the better firms or the worst firms getting the marginal credit? Portfolio allocation by credit officers in a bureaucratic settings is potentially a complicated problem which we are studying in some parallel research (Banerjee, Cole and Duflo, 2008). Here we focus on an extremely simplified illustrative example, which provides some hints to what we might learn from a more general analysis of this problem.

The model is intended to capture a very simple intuition: The two performance measures for loan officers that are most easily observed are the volume of his lending and whether the loans got repaid. In a large bank, and especially in the highly bureaucratic Indian public sector banks, this is probably all that the bank can use to give the loan officer incentives. In other words, the only features of firm performance that the loan officer cares about is their willingness to borrow and their likelihood of default. At some level this is also what the bank cares about: The problem is that it does not observe the ex ante likelihood of default but only the ex post fact that there has been default. This introduces a wedge between the incentives of the loan officer and the incentives that the bank would have liked him to have had, which leads the loan officer to bail out failing firms, whereas the bank would have preferred them to fail.

3.2.1 A simple model of loan allocation

We start from the model in the previous section. However in order to focus on the issue at hand we make a couple of additional simplifying assumptions. First, we set r_b, the subsidized interest rate charged by the bank, equal to zero. This simply makes the expressions less ugly. Second, since we find that the firms are indeed credit constrained, we ignore market lending in everything we do. If every firm started with a fixed amount of market credit (instead of zero) but were still credit constrained, all our main conclusions would continue to hold.

Where we complicate the model is by introducing the idea that firms come in two types, H and L, in fractions p_0 and $1 - p_0$. The production function $f(k)$ of the previous section should now be interpreted as an expected production function (given that firms are risk neutral, this change does not affect the analysis in the previous sub-section). For a firm of type H,the probability of success is 1, and correspondingly, for type L is $p_L < 1$. When a firm (of either type) succeeds it gets $\widetilde{f}(k)$. Otherwise it gets 0. Assume as before that $\widetilde{f}(k)$ is strictly concave.

Each firm lives for 2 periods and there is no discounting between periods. At the end of the second period the firm shuts down. We assume that the firm's probability of success is independent across the periods. Firms do not deliberately default, but if they get 0 they cannot pay (they start with zero and do not retain earnings).

Lending on behalf of the bank is by decided upon by loan officers. Each loan officer's tenure is also 2 periods and once again, there is no discounting between periods. Loan officers are given incentives to lend out money, and to avoid default. Specifically, each loan officer starts his job with a population of size 1 of new firms assigned to him and is supposed to lend 1 unit to each new borrower. In the second period he is given a portfolio of size $1 + g$ and is free to chose how to allocate it (since at this point, he has more information than the bank). Each unit that is unlent costs the banker an amount C.

The loan officer is penalized for any loan where there is a default. This punishment is F per unit of default.[11] This assumption is a part of the reason why there are bailouts–it says

[11]When a loan in an Indian public sector bank (like bank we study) becomes non performing, it triggers the possibility of an investigation by the Central Vigilance Commission (CVC), the government body entrusted with monitoring the probity of public officials. The CVC is formally notified of every instance of a bad loan in a public sector bank, and investigates a fraction of them. There were 1380 investigations of bank officers in 2000 for credit

that the punishment is linear in the size of the default. Since bailouts are a way to substitute a probability of bigger future default for the certainty of a smaller current default, making the penalty convex enough in the size of the default would discourage bailouts. We justify this assumption with the usual convenience argument for linear incentives schemes. In real world settings, the size of the first period loan presumably depends on a range of factors that have to do with the industry that the firm is in, the interest rate in the market, the firm's access to other sources of credit etc. For each such firm type, the optimal incentives for the loan officer would require the penalty for default to be convex over a different range. Since the penalty is ultimately bounded, it cannot be globally convex—it must therefore also be concave over other ranges. Linear incentive schemes avoid the need to get these specific details exactly right for a large number of firms types, which makes them attractive in large bureaucracies.

In the first period neither the loan officer nor the borrower knows the borrower's type; i.e. each borrower is a random draw from the population. At the end of the first period, if the borrower has failed it is common knowledge between the borrower and the lender that he is a type L. If he is successful then with probability π both the lender and the borrower get a signal that the borrower is a type H. With probability $1-\pi$, all that they know is that he did not fail, which makes him a type H with probability $p_1 = \frac{p_0}{p_0+p_L(1-p_0)} > p_0$.We call the firms on which the loan officer gets no signal the type U firms.

In analyzing this model we will focus on the case where firms in both periods are willing to take the loans that they get offered (the exact condition for this is given below). Therefore the loan officer is the one who has to decide how to allocate the available capital. In the first period the loan officer has no discretion–he has to give 1 unit to each borrower. We are studying the allocation problem the loan officer faces in period 2, when he has information that the bank does not have and has the discretion to use it.

3.2.2 Analysis of lending decisions

Given that there is a large population of borrowers we know that at the end of the first period the loan officer will have a fraction $p_0\pi$ of borrowers who are known to be type H and have been

related frauds, 55% of which resulted in major sanctions. F is naturally thought of as the expected punishment resulting from being investigated (there is clearly a cost of being investigated even if you are innocent).

successful, a fraction $(1-p_0)(1-p_L)$ of known L types who have all failed, and the rest, who have also been successful, of an unknown type (type U). The first decision he has to take is what to do about the firms that have failed. He can either report a default and take his punishment of F or bail out the firm by giving it a fresh loan.[12] How big does the bailout loan need to be? *The loan officer will only bailout if, when the firm succeeds in the 2nd period, it can pay back the new loan.* If the bailout loan is l, the firm only gets to invest $l-1$, because it has to give 1 unit back to the bank so that it does not default right away. Therefore for a successful bailout we require that $\widetilde{f}(l-1) > l$. The minimum loan size that will allow a successful bailout is l^* such that $\widetilde{f}(l^*-1) = l^*$. Obviously $l^* > 1$. Since these firms are of type L, they are more likely to default in the second period than either of the other types of firms. Hence, as long as the other types of firms are willing to borrow more (which is what we will assume below), there is never any reason to lend more than l^* to a firm that is being bailed out. The choice is therefore between giving the firm l^*, which generates a possibility of a larger default in the future and giving it nothing, which leads to a smaller but certain default now. Bailing out dominates if

$$F > (1-p_L)l^*F$$

which clearly holds, if (and only if)

$$1-p_L < \frac{1}{l^*} \tag{1}$$

Assume that this condition holds so that the loan officer always bails out those who fail in the first period. There is no scope for bailing out in the second period because there is no future in the relationship.

Given this the loan officer will have $1+g-(1-p_0)(1-p_L)l^*$ units of capital left to allocate among the rest of the firms. Assume that he divides this equally among the known type H firms. This gives them each $\frac{1+g-(1-p_0)(1-p_L)l^*}{p_0\pi}$ units. Assume that

$$f'(\frac{1+g-(1-p_0)(1-p_L)l^*}{p_0\pi}) > 1. \tag{2}$$

In other words, if the remaining capital is divided equally among the known H type firms, they will be happy to take it. Since this also minimizes risk of default, this is what the loan officer

[12]This process of "evergreening" of loans by loan officers who prefer not to have a default in their hands, has been widely noted in the Indian context (see, for example, Topalova (2004)), as well as elsewhere in the world.

should do.[13]

Notice as long as this condition 2 continues to hold, this result does not depend on the size of g. Hence if as a result of a policy shift, the amount of subsidized credit available for lending is larger, the essential pattern of lending does not change: the loans go to the type L and to known type H firms, but now the type H firms get a bigger increment in their loan. This would also be true if g went down as long as it is still the case that $1 + g - (1 - p_0)(1 - p_L)l^* > 0$.

Result 3: Under assumptions 1 and 2 the loan officer's optimal allocation of second period credit is to give known type H firms an amount $\frac{1+g-(1-p_0)(1-p_L)l^*}{p_0\pi}$, type L firms an amount l^* and the rest (i.e. type U firms) nothing. Variation in the size of g, within limits, does not change the set of firms that get loans in the second period.

The logic of this result is straightforward. The loan officer wants to avoid default. Hence he will bailout the existing firms that are in trouble but otherwise would like to focus entirely on the firms that are proven to be safe. Given that subsidized credit is scarce, these firms will also be happy to take what he is offering them.

The prediction that the firms of type U actually get a cut in their loan seems counterfactual at least in the world of Indian firms. In our data many firms show no loan growth, but few see an actual decline. This may be because if the firm anticipates a large cut in its loan, it will prefer to default, and as a result loan officers want to commit to not cut loans between the first and second period as long as the first period loan was repaid. If we make the auxiliary assumption that loan size never goes down as long as the first period loan is repaid, and assume that g is always large enough to allow this to happen, Result 3 would be restated as:

Result 4: Under the assumption that loan size never goes down as long as the first period loan is repaid, as well as assumptions 1 and 2, the loan officer's optimal allocation of second period credit is to give type H firms an loan increment of $\frac{g-(1-p_0)(1-p_L)(l^*-1)}{p_0\pi}$, the type L firms an increment of $l^* - 1$ and the rest (i.e. type U firms) no increment. Variation in the size of g, within limits, does not change the set of firms that get increments in the second period.

[13] We are cheating a bit here. The loan officer is actually indifferent between dividing the capital equally among the H types and a range of other allocations where some H types get more than others. However the equal division outcome is socially efficient (because f is strictly concave) and also the one that would obtain if the firms lobby the loan officer for each extra dollar and those who have more to gain lobby more (once again, because f is concave).

3.2.3 Implications of results

Under the conditions 1 and 2, this very simple model therefore has several interesting implications.

1. The relation between loan growth and ex ante measures of firm performance (such as first period revenue, first period profits) in the cross-section of firms, can be positive or negative, or zero. The firms that have the highest loan growth from the first period to the second may be either the best performing H type firms or worst performing L type firms (depends on how l^* compares with $\frac{1+g-(1-p_0)(1-p_L)l^*}{p_0\pi}$). The intermediate U type firms get no increments.

Note that this is quite consistent with the descriptive evidence reported in section 2, where we showed no systematic relationship between measures of firm performance and probability of a loan increment or amount of the increment.

2. A substantial part of loan growth under normal circumstances goes to firms that get bailed out because they have failed (and are thus known to be bad). These firms are more likely to fail again than the average firm. Therefore, the OLS estimate of loan growth on profit growth will be biased downwards, since it confounds this (negative) selection effect and the causal effect of loans. In contrast, the immediate impact of an unexpected policy change that increases g is an increase in credit flows to firms that are expected to do well (type H firms in our model). Therefore, an instrumental variable estimate of the impact of loans on profit using the policy change as an instrument for change in lending will give us the causal impact of extra lending on *successful firms*. This is because the IV estimate gives us the "local average treatment effect" (LATE), i.e. the effect of additional unit on credit on the type of firms for which credit actually changes

   The IV will therefore typically be larger than the OLS for two reasons: While it does represent a causal effect, it is a causal effect within a selected group (in other words, the "compliers" in this experiment will tend to have higher treatment effect than a random firm chosen from the population).

3. The set of firms that have credit growth is unchanged by the policy change–only the magnitude of the credit inflow changes. This is because every firm wants more subsidized credit and the loan officer always wants to give it to the safest firms (and to give more to them if more if available) and therefore has no reason to try to spread it around. All the effect of the reform should therefore be on the intensive margin.

4 Empirical Strategy

4.1 Reduced Form Estimates

The empirical work follows directly from the previous section and seeks to establish the facts that will allow us to determine whether firms are credit rationed and/or credit constrained.

Our empirical strategy takes advantage of the extension of the priority sector definition in 1998 and its subsequent contraction in 2000. The reform did not seem to have large effects on the composition of clients of the banks: in the sample, 25% of the small firms, and 28% of the big firms have entered their relationship with the bank in 1998 or 1999. This suggests that the banks was no more likely to take on big firms after the reform and that our results will not be affected by sample selection.

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).[14] As motivation, 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 between Rs. 10 million and Rs. 30 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).

For limits granted in 1997 the average increment in the limit over the previous years's limit was 7% larger for the small firms compared to medium firms and 2% larger for small firms compared to the biggest firms. For limit granted in 1998 and 1999, it was 2% larger for medium

[14]Since the source of variation in this paper is closely related to the size of the firm, we express all the variables in log to avoid spurious scale effects.

firms, and 7% larger for the biggest firms, compared, once again to the smallest firms. After 2000, limit increases were smaller 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.[15]

Panel B in table 4 shows that the average increase in the limit was not due to an increase in the probability that the working capital limit got changed: big firms were no more likely to experience a change in 1998 or 1999 than in 1997. This is consistent with implication 3 from the model in the previous section, which tells us that when loan officers need to respond to pressure from the bank to expand lending to the newly eligible big firms, they prefer giving larger increases to those which would have received an increase in any case and are known to be safe, rather than increasing the number of firms whose limits are increased.

In Panel C, we show the average increase in the limit, conditional on the limit having changed. 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 went from an average of 0.44 to an average of slightly less than 0).

Our strategy will be to use these two changes in policy as a source of shock to the availability of bank credit to the medium and larger firms, using firms outside this category to control for possible trends.

We start by running the regression equivalent of the simple difference-in-differences above. First use the data from 1997 to 2000 and estimate and equation of the form:[16]

$$\log k_{bit} - \log k_{bit-1} = \alpha_{1kb} BIG_i + \beta_{1kb} POST + \gamma_{1kb} BIG_i * POST_t + \epsilon_{1kbit}, \tag{3}$$

where we adopt the following convention for the notation: k_{bit} is a measure of the bank credit limit to firm i in year t (and therefore *granted* (i.e., decided upon) some time during the year $t-1$[17]), BIG is a dummy indicating whether 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

[15]Note that there is no reason to expect a decline in the loan limit for the larger firms in 2000, we just expect a *bigger drop* in the increase in limit between 1998-1999 and 2000.

[16]All the standard errors are clustered at the sector level.

[17]70% of the credit reviews happen during the last 6 months of the year, including 15% in December alone.

1999 and 2000 (The reform was passed in 1998. It therefore affected the credit decisions for the revision conducted during the year 1998 and 1999, affecting the credit available in 1999 and 2000). We focus on working capital loans from this bank.[18] We estimate this equation in the entire sample and in the sample of accounts for which there was no revision in the amount of the loan. We expect a positive γ_{1kb}.

We will also run a regression of the same form using a dummy for whether the firms got any increment as the dependent variable. The model predicts in this case that the coefficient of the variable $BIG * POST$ should be zero. Finally, equation (3) will be estimated in the sample with an increment greater than zero.

To study the impact of the contraction of the priority sector on bank loans, we use the 1999-2002 data and estimate the following equation:

$$\log k_{bit} - \log k_{bit-1} = \alpha_{2kb} BIG2_i + \beta_{2kb} POST2 + \gamma_{2kb} BIG2_i * POST2_t + \epsilon_{2kbit}, \qquad (4)$$

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 2001 and 2002.[19]. Once again, this equation will be estimated in the whole sample and for the firms that got a positive increment we will also estimate a similar equation for an indicator for whether the firms had any change in the limit.

Finally, we pool the data and estimate the equation:

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

[18] Using total working capital loans from the banking sector instead leads to almost identical results, since most firms borrow only from this bank.

[19] Once again, we adopt the convention that we look at credit available in year t, and therefore granted in year $t-1$. The reform was passed in 2000 and therefore affected credit decisions taken during the year 2000 and credit available in the year 2001.

where MED is a dummy indicating that the firm's investment in plant and machinery is between Rs. 6.5 million and Rs. 10 million.

After having demonstrated that the reform did cause relatively larger increases in bank loans for the affected firm, we run a number of other regressions that exactly parallel equations (3) to (5). First, we use the sample 1997-2000 to estimate:

$$y_{it} - y_{it-1} = \alpha_{1y} BIG_i + \beta_{1y} POST_t + \gamma_{1y} BIG_i * POST_t + \epsilon_{1yit}, \tag{6}$$

where y_{it} is an outcome variable (such as credit, sales, or cost) for firm i in year t. Second, we estimate:

$$\log y_{it} - \log y_{it-1} = \alpha_{2y} BIG2_i + \beta_{2y} POST2 + \gamma_{2y} BIG2_i * POST2_t + \epsilon_{2yit}, \tag{7}$$

in the sample 1999-2002 , and finally we estimate:

$$\begin{aligned} \log y_{it} - \log y_{it-1} \quad = \quad & \alpha_{3y} BIG2_i + \alpha_{4y} MED_i + \beta_{3y} POST + \beta_{4y} POST2 + \\ & \gamma_{3y} BIG2_i * POST_t + \gamma_{4y} MED_i * POST_t + \\ & + \gamma_{5y} BIG2_i * POST2_t + \gamma_{6y} MED_i * POST2_t + \epsilon_{3yit} \end{aligned} \tag{8}$$

in the 1997-2003 sample.

Our model predicts that only impact of the reform is on the intensive margin: firms pre-identified as good will now get a larger increase in their loan. Some firms which had previously failed will also get an increase in loan to be bailed out, but that probability will not be affected by the reform. The firms which did not fail but on which the credit officer has no information will not receive an increment. Under the assumption in the model, it is thus appropriate to estimate equation y_1 to y_3 separately in two sub-samples: the sample with an increment in limit, and the sample without increment. Sample selection will not bias the results, because it is uncorrelated with the regressors of interest (the variable $BIG * POST$) (Heckman (1979), Heckman and Robb (1986), Angrist (1995)).

The prediction that selection of firms getting positive increment is uncorrelated to the reform is consistent with what we observe in table 3 and 4. In particular, there isn't any evidence that

the probability of a change in the limit is affected by the policy change. It could of course still be the case that the *number* of firms that get a change in the limit is unaffected by the reform, but the *type* of the firms that get chosen is affected by the reform. This could then bias the results in the selected sample. However, this is not what the model predicts. Both before and after the reforms, failing firms and firms that have been identified as efficient should be selected. Empirically, when we regress pre-determined characteristics of firms with positive increment on the variables $POST$, BIG and $BIG * POST$ before and after the reforms, we see no impact (results omitted).

If the assumptions in the model are right, we should then expect the coefficients of $BIG * POST$ and $BIG2 * POST2$ to be zero in all the equations in the sample without change in limit, which provides a test of the identification assumptions. Restricting the sample to firms with a positive increment in limit will also increase the precision of the estimates of the reform on sales, costs and profits for firms which were actually affected by the reform.

Below, we describe the variables we use and their justification.

• Credit rationing

Following result 1, we provide two additional pieces of evidence to establish credit rationing: First, we show that the firm *used* the extra funds they got, using a standard measure of utilization for a credit limit: the logarithm of the ratio of total borrowing under the line of credit during the year (in banker's parlance, the turnover on the account) to the credit limit. Second, we check that the interest rate did not change (which is what would be expected)

• Credit constraints

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. Given that we are looking at increases in the availability of working capital, we expect the increase in sales to take place the year the capital becomes available (the data shows that the working capital limits is turned over several times during the year), i.e. the year after the limits was decided upon. To interpret the effect of credit expansion on sales, we posit a simple parametric relation between credit and sales revenue in the case of success: $R_{it} = A_{it} k_{it}^{\theta}$. Note that this is a specific parametrization of the production function

introduced in the previous sub-section.[20] Taking logs:

$$\log R_{it} = \log A_{it} + \theta \log k_{it}. \tag{9}$$

Differencing this equation gives:

$$\log R_{it} - \log R_{it-1} = \log A_{it} - \log A_{it-1} + \theta[\log k_{it} - \log k_{it-1}], \tag{10}$$

for firms that succeeded in both periods. Assume that when firms fail, they get a small amount of sales such that $\log R_{it} = \upsilon_{it}$ (failing likely does not involve zero sales, but rather zero profit).[21]

Focusing on the first experiment (credit expansion), the growth of bank credit for the firms that were successful in period 1, and for which the bank received a signal is given an equation of the form of equation (3).

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

$$\log k_{it} - \log k_{it-1} = \alpha_{1Sk} BIG_i + \beta_{1Sk} POST_t + \gamma_{1Sk} BIG_i * POST_t + \epsilon_{1kit}, \tag{11}$$

which when substituted in equation (10) yields

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

If we restrict the sample to firms with a positive increment in limits, there are two kinds of such firms in period t: the firms that failed in period t-1 (which needs to be bailed out), and the firms that succeeded in period t-1 and about which the bank received a positive signal.

The latter firms are successful in both period, so their sales is governed by equation 12. In the case of the failed firms the loan increment they get is fixed (it is the minimum they need to be bailed out). In period $t-1$, they get the failure outcome ($log(R_{it-1}) = u_{it-1}$). In period

[20] 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.

[21] It would simplify the exposition to say that the failing outcome is simply a low A_{it} so the sales remain proportional to the amount lent. But we show below that this is not a necessary assumption in this set up, so we avoid making it.

t, they get the success outcome with probability p (and this equal to $A_{it}\overline{k_{it}}^{\theta}$, where $\overline{k_{it}}$ is the minimum capital a failed firm needs to be bailed out, and the failure outcome with probability $1-p$). Thus, for firms that failed in period t-1, neither the increase in loans nor the increase in revenues is correlated with the reform, though they might get more money in period t than in period t-1, and big firms may be getting more money because they need more to survive. In other words, for these firms, the increase in loans and revenues in both periods take respectively the form:

$$\log k_{it} - \log k_{it-1} = \alpha_{1Fk} BIG_i + \beta_{1Fk} POST_t + \omega_{1kit}, \tag{13}$$

and:

$$\log R_{it} - \log R_{it-1} = \alpha_{1FR} BIG_i + \beta_{1FR} POST_t + \eta_{1kit}, \tag{14}$$

Combining failed firms and successful firms in the loan equation (which is what we do when we estimate equation 3 in the sample of firms with positive increment) thus implies that $\gamma_{1kb} = \gamma_{1skb}\phi$, where ϕ is the share of successful firms in the sample of firms with positive increment, and estimating equation (6) above in the sample sample (with sales as the dependent variable, and denoting γ_{1R} the coefficient of $BIG * POST$ in this equation) implies that $\gamma_{1R} = \theta\gamma_{1Sk}\phi$. Thus, the coefficient in both equations are the causal impact of the reform for the successful firms, multiplied by the fraction of successful firms. In section 5.4 below, we discuss that the ratio of the two can be interpreted as an IV estimate of the impact of bank loan on revenues.

Our identification hypothesis is that for successful firms

$$\log A_{it} - \log A_{it-1} = \alpha_{1A} BIG_i + \beta_{1A} POST_t + \xi_{it}, \tag{15}$$

and that a similar conditions is true for failed firms (neither the failed payoff nor the successfull payoff is correlated with the interaction $BIG * POST$).

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 priority sector expansion. Under this assumption γ_R gives the reduced form effect of the expansion of the priority sector on sales revenue.

If we consider the entire sample instead if looking only at the firms that have received a positive increment, the reasoning would be exactly the same, except that in that case, the coefficients of $BIG * POST$ in the sales equation and in the loan equations are both multiplied by the share of successful firms on which the loan officer has received a signal in the entire sample.

Similar calculations lead to an equation of the same form, similar to equation (8) for the priority sector contraction (2000-2002), where the identification hypothesis is that

$$\log A_{it} - \log A_{it-1} = \alpha_{2A} BIG2_i + \beta_{2A} POST2_t. \tag{16}$$

If firms are credit constrained, γ_{1R} should be positive and γ_{2R} should be negative, while if no firms are credit constrained γ_{1R} will only be positive for those firms that have *fully* substituted market credit, and γ_{2R} will be negative only for those firms that had no market credit initially. We therefore also estimate a version of equation (6) 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 and γ_{2R} in this sample should be zero.

A final piece of evidence comes from looking at profit. Profits are expected to increase regardless of whether the firm is credit constrained or not (since the interest payments go down), but the extent of the increase in profit is nevertheless interesting, since it can give us some indication of the firm's marginal return to capital.[22]

4.2 Empirical Strategy: Testing the Identification assumptions

The interpretation of the central result on sales growth crucially depends on the assumptions made in equation (15) and (16). Likewise, the interpretation of the other results depends -on the assumption that the error term is not correlated with the regressors, most importantly $BIG * POST$ in equation (6) and $BIG2 * POST2$ in equation (7). 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 belong to different sectors, and these sectors may be affected by different policies during this period).

[22] In the working paper version of the paper, we derive and discuss interpretation of the results on profits under various assumptions.

The fact that we have two experiments affecting different sets of firms help distinguishing the effect of the priority sector regulation from trends affecting different groups of firms differentially. The two reforms went in different directions and did not affect all the firms identically. Credit constraints would predict γ_{1R} in equation (6) to be positive and γ_{2R} in equation (7) to be negative. Moreover, the ratio $\frac{\gamma_{1R}}{\gamma_{1kb}}$ and $\frac{\gamma_{2y}}{\gamma_{2kb}}$ should be equal.

The same reasoning of course applies to equations (5) and (8) (which combine the two experiments), as well, so that the ratios $\frac{\gamma_{3y}}{\gamma_{3kb}}$, $\frac{\gamma_{4y}}{\gamma_{4kb}}$, $\frac{\gamma_{5y}}{\gamma_{5kb}}$ should also all be equal. This is a natural over-identification test: if all these equalities are satisfied, it would be extremely implausible that the observed patterns come from the fact that the time trends are different for small and large firms.

Even if all these tests work, we would still need to worry about the possibility that, being labeled as a priority sector firm affects the sales and profitability of a firm over and above its effects on credit access. 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 in all specifications. The second 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. Excluding firms that manufactured SSI reserved products in 1998 does not change the results. However it remains possible that some of the big firms moved into reserved product after 1998 and this increased their sales and profits.

A way to resolve this issue is to focus on a different test of the identification assumption, which is to estimate equations (6) to (8) for all the different outcomes variables separately in two subsamples: one subsample made of the firm-year observations where there was no change in the granted limit from the previous year to the current year, and one subsample made of firms where there was a change. If there is an effect of just becoming entitled to produce the products on the SSI list even the big firms that had no change in the granted limit should show a change after the reform. We therefore test whether the coefficient of $BIG*POST$ is statistically indistinguishable from zero in the sample of firms that did not get a change. As we discussed above this is consistent under the assumptions of the mode.

5 Results

5.1 Credit

• Credit Expansion

Panel A in table 5 presents the results of estimating equation (3) for several credit variables.[23] We start with a variable indicating whether there 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 model and the evidence we discussed above, there seem to be absolutely no correlation between the probability of getting a 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 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 of getting an increase or a decrease in the limit.

In the columns (4), to (7) we look at limit granted by the bank.[24] 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. Both of these coefficient are statistically significant, and indicate a large change in the availability of credit for the sample of firms that were reviewed. 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 smaller 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 the difference is small).

In columns (6) and (7) , we restrict the sample to observations where we have data on future sales (which will be the first stage for the IV estimation of the impact of bank loans on sales). The coefficient is almost the same (0.26) and still significant.

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

[24] If, instead, we use 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.

• **Credit contraction**

In panel B, we present the result of estimating equation (4). Here again, we find no effect of the contraction on the probability that the limit is changed (column (1)), which reinforces the claim that the decision to change the limit has nothing to do with the priority sector regulation. However, the probability that the limit is cut goes up significantly for the largest firms after the reversal of the reform in 2000 (the coefficient is 0.119, with a standard error of 0.033). Turning to the magnitude of the change in limit, the coefficient of the interaction $BIG2 * POST$ is negative both in the entire sample (in 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 after 2000 is larger than the average yearly increase in limit in 1998 and 1999. The results are very similar in the sample where we have data on sales (columns (6) and (7)).[25]

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). The coefficient of $MED * POST2$ is positive and significant in column (1): Relative to other firms, medium firms became less likely to experience a change in limit after 2000. It may be because they have experienced relatively large changes in the two years before.

The effect on the magnitude in the change in the limit granted by the banks are presented in column (4) (whole sample) and (5) (the sample where the limit was changed). During the expansion of the priority sector, the limits of both medium and large firms increased significantly more than that of small firms. The impact of the reform was similar for medium and large firms, both of which became eligible. During the contraction, large firms, who lost eligibility, experienced a significant reduction in their credit limit relative to small firms. Medium firms (who did not lose eligibility) also suffered a decline but the coefficient is much smaller than that for large firms. (In column (5) for example, the coefficient of $MED * POST2$ is -0.18, while that of $BIG * POST$ is -0.48. Only the later is significant).[26]

[25]The sample size drops in this column since we are not using the data from the last year when we have data on loans but not on sales.

[26]The effect on medium firms may come from the fact that we classified firms as medium firms based on the earliest data we have on them (1997). Some of them have almost certainly grown since and are now being treated by the bank as large firms, even though we are treating them as medium firms.

5.2 Evidence of Credit Rationing

Table 6 presents evidence on credit rationing. 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 shows levels, the second column logarithms, and the third column replaces the difference $r_t - r_{t-1}$ 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 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 0.073, with a standard error of 0.17.[27] In logs the coefficient of the interaction is 0.002, with a standard error of 0.011. In panel B, the coefficient of $BIG2 * POST2$ is likewise insignificant in all the specifications.

This shows that the fact that big firms are borrowing more from the banks after the expansions and less after the contraction, is not explained by a fall in the interest rate on bank lending. To complete the argument we also need to show that firms actually use the additional credit they get when there is an expansion.[28] To look at this, we compute limit utilization. When we use this variable as the dependent variable, the coefficient of $BIG * POST$ is negative and insignificant both during the expansion and during the contraction.

This results are far from definitive, due to the limited number of observations for which the data on turnover is available.[29] However, the evidence available suggests that firms did make use of the extension in credit without a change in interest rate. This suggests that firms are willing to absorb the additional credit at the rate at which it is offered by the bank. We now turn to sales and profit data to assess whether firm's activity is constrained by their limited access to credit.

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

[28] This is not automatic, since under the Indian system the bank gives the firms an extension of their credit line, but firms only pay for the amount they actually draw.

[29] For example, we do not present the results for loan utilization for firms whose limit changed, because we have very to few observations on turnover in each cell in this restricted sample.

5.3 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 sales. In order to keep the table manageable, we present only the coefficient of the interactions, which are the coefficients of interest (the coefficients of the main effects are available upon request). Of note among unreported coefficients is the coefficient of the "POST" variable, which is small in absolute value and insignificant in all specifications and for all dependent variables.

The coefficient of the interaction $BIG * POST$ is 0.194 in the sample with a change in limit, with a standard error of 0.106. In the sample where there is no change in limits, 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.

The 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 that, according to this measure, have not stopped using trade credit (i.e., this measure has not become 0 or smaller). The coefficient of $BIG * POST$ is similar as what it is in the full sample (0.168): the increase in sales is not due to firms that had first completely substituted away from trade credit. Moreover, note that very few firms drop from the sample where we focus on firms that have positive non-bank liability (i.e, we drop firms without any market borrowing), which in itself suggests that substitution cannot be easy. The results in column (1) and (2) together with the previous results establishing credit rationing, suggest that firms are credit constrained: sales increased for firms that still had non-bank credit, and very few firms substituted entirely. Below, we use the magnitude of the estimates to argue that there seems to have been little substitution of bank credit for market credit.

Although finding an effect on profit would not be sufficient to establish the presence of credit constraints (since part of the effect on profit comes directly from the subsidy), establishing the magnitude of the effect on profit is a useful complement to the results on sales. Using the

logarithm of profit as the dependent variable 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, which introduces sample selection and makes the profit regressions difficult to interpret.

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, without sample selection bias. The increase in sales is accompanied by an increase in cost of comparable magnitude: the coefficient on the $BIG * POST$ interaction is 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.

• Credit Contraction

Panel B presents the estimate of the effect of the credit contraction on the sales and costs of firms with investment in plant and machinery between Rs. 10 million (using all the other firms as a control) and Rs. 30 million. In the sample where there was a 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 of substitution. The result is similar if we restrict the analysis to the sample of firms that have some market borrowing. The coefficient of the interaction $BIG2 * POST2$ in the cost equation is negative and similar to the effect on sales (-0.374).

In the sample where there was no change in limit, in contrast there is no significant effect either on sales or on costs.

• Full sample and overidentification tests

Table 8 present the results of estimating equation (8) for sales and costs. We use the entire period, and we estimate separately the coefficients of the interactions $BIG * POST$, $MED * POST$, $BIG2 * POST2$ and $MED * POST2$ (where MED is a dummy indicating that the firm's investment in plant and machinery is between Rs. 6.5 million and Rs. 10 million). 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 equations, the coefficients have the expected pattern: both the coefficients of the $MED * POST$ and $BIG2 * POST$ interactions are positive (though when introduced separately, they lose significance). The coefficient of the interaction $BIG2 * POST2$ is negative and significant and, while negative, the coefficient of the interaction $MED * POST2$ is only 20% of the $BIG2 * POST2$ coefficient and insignificant. The coefficients are similar in the full sample and the sample without substitution.

Formally, the overidentification test does not reject the hypothesis that the implied effect of credit on the sales and cost variables is the same for all the sources of variation. For example, if we look at the sales equation in column (1), the ratio between the coefficients in the sales equation and the corresponding coefficients in the loan equation are similar (they range between 0.73 and 0.83), and the test does not reject the hypothesis that they are equal. This result makes it very implausible that the estimated coefficient reflect differential trends arising from other, unobserved, factors.

Taken together, these results present a consistent picture which suggests that firms face 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 led to a contraction. A subset of firms that was affected by the expansion, but not the contraction, behaved like the affected firms in the expansion, but like an unaffected firms in the contraction. These results taken together suggest that it is unlikely that the effects are driven by time trends affecting different firms differentially. Furthermore, these results are concentrated in the firms that experienced a change in loans, which makes it unlikely that the effect is driven by differential trends.

A last piece of important evidence is whether a credit expansion is associated with an increase in the probability of default : the increase in profits (and sales) may otherwise reflect more risky strategies pursued by the large firms. In order to answer this question, we use data on Non Performing Assets (NPAs). Since it takes at least a year for a loan that has gone bad to be officially qualified as an NPA, we treat the years 1998 and 1999 as the "pre" period, the year 2000 and 2001 as the period following the expansion, and 2002 as the period following the contraction. In 1998 and 1999, 1% of the loans to medium and large firms, and 4% of the loans to small firms, became NPA. 5.5% of the medium and large firms, and 5% of the small firms

that were not NPAs in 1999 became NPAs in 2000 or 2001. While the growth in NPA is faster for the loans to big firms, the difference is very small. Conversely, 3% of the loans to the largest firms (with investment in plant and machinery above 10 million) and 2% of those to small and medium firms that were not NPAs by 2001 became NPAs in 2002. Additional credit does not seem to have led an unusually large number of firms to default.

5.4 Instrumental Variables Estimates: the impact of bank credit on sales and profit

The discussion in section 4 suggests that equation (3) to (5) and (6) to (8) respectively form the first stage and the reduced form of an instrumental strategy of estimating the impact of bank loan on sales (or any other outcome variable y): in the sample of firms with an increment, the coefficient of the interaction $BIG * POST$ in equation (3) is the causal impact of the priority sector expansion on the volume of loans to good firms, multiplied by the fraction of good firms in this sample. The coefficient of the interaction $BIG * POST$ is the causal effect of the reform on sales, multiplied by the fraction of good firms in the sample. Assuming that the only impact of the reform on sales is due to its impact of credit (which we will verify later), this indicates that, controlling for BIG and $POST$, $BIG * POST$ is thus a valid instrument for the impact of bank loans on the sales of the firms that are known to be good (the ratio of the coefficients in the reduced from and and first stage equation is equal to the impact of credit on sales for good firms).

In this last sub-section, we present (in table 9) 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 1997-2000 period. The coefficient is 0.75, with a standard error of 0.37. Column (2) uses the "contraction" experiment (the instrument $BIG2 * POST2$ in the 1999-2002 period). This estimate (0.73) is very close to the previous one, which is just a way to restate the result of the overidentification test that we already saw. Finally, column (3) uses the entire period and three instruments ($MED * POST$, $BIG * POST$ and $BIG2 * POST2$). The coefficient is, once again, very close to what it was in columns (1) and (2) (0.76).

If firms do not increase market credit in proportion to the increase in bank credit, these estimates of $\widetilde{\theta}$ (the elasticity of sales with respect to bank credit) provide a lower bound on θ (the elasticity of sales with respect to overall credit) for these firms: To see why, rewrite equation (10), to obtain:

$$\log R_{it} = \log A_{it} + \theta \log k_{bit} - \theta \log \frac{k_{bit}}{kit}. \tag{17}$$

Differencing over time:

$$\log R_{it} - \log R_{it-1} = \log A_{it} - \log A_{it-1} + \theta[\log k_{bit} - \log k_{bit-1}] - \theta[\log \frac{k_{bit}}{kit} - \log \frac{k_{bit-1}}{kit-1}]. \tag{18}$$

We do not observe $\log \frac{k_{bit}}{kit} - \log \frac{k_{bit-1}}{kit-1}$ and therefore estimate an equation of the form:

$$\log R_{it} - \log R_{it-1} = \widetilde{\theta}[\log k_{bit} - \log k_{bit-1}] + \upsilon_{it} \tag{19}$$

The term $\theta[\log \frac{k_{bit}}{kit} - \log \frac{k_{bit-1}}{kit-1}]$, which is omitted when estimating equation (19), should typically be positively affected by the reform. The one exception is the case where the firm is credit constrained and access to market capital increases so fast as a function of access to bank capital that total capital stock goes up faster than bank capital–which seems rather implausible. This suggests that $\widetilde{\theta}$ will be a lower bound for θ.

The elasticity of sales with respect to bank loans, $\widetilde{\theta}$, gives some additional information about the plausibility of the firms having substituted the bank loans for the market credit. To see this, note that

$$\theta \approx \frac{\Delta R}{\Delta k}\frac{k}{R} = \frac{\Delta R}{\Delta k_b + \Delta k_m}\frac{k_b + k_m}{R} = \widetilde{\theta}\frac{1}{1 + \Delta k_m/\Delta k_b}[1 + \frac{k_m}{k_b}]$$

If there is substitution of bank credit for market credit, Δk_m is negative while Δk_b is positive In this (possibly hypothetical) scenario

$$\theta \geq \widetilde{\theta}[1 + \frac{k_m}{k_b}]. \tag{20}$$

$\widetilde{\theta}$ is equal to about 0.75. Using the Prowess data set, Topalova (2004) estimates that the ratio of short run bank debt over total liability in Indian manufacturing is about 0.5 from 1996 onwards, implying a ratio of $\frac{k_b}{k_m}$ of about 1. Therefore in this scenario our estimate would suggest an θ

of above 1.5 in the neighborhood of the current capital stock, implying that the firm must be credit constrained (a $\theta > 1$ suggests local increasing returns and hence the firm must want to borrow more) and therefore unwilling to cut back on any form of credit.

Column (4) restricts the sample to firms that do not produce SSI products, since, as we mention before, one advantage of SSI status is that it gives an exclusive right to produce some goods. The coefficient is somewhat smaller and less precise, though it is not statistically different from the result in the whole sample (the coefficient on sales 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, consistent with our model predicts.

Panel B present the estimate of the effect of bank loans on costs. The estimates 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 these estimate to get a sense of the average increase in profit caused by every rupee in loan. The average loan (averaging across years and firms) is Rs. 8,680,000 (about 45 days of sales) . Therefore, using the coefficients in column (3), an increase of Rs. 100,000 in the loan corresponds to an increase in Rs. (610,000 in sales, and Rs. 537,000 increase in costs. This implies an Rs. 73,000 increase in profit for the average firm, after repaying interest.

In panel C, we present, for the sake of comparison, the direct IV estimate of loans on log(profit), despite the fact that these regression suffer from the sample selection induced by the omission of the firms with negative profits. The estimates vary between 1.79 and 2.00. Taking 1.79 as the estimate of the effect of the log increase in loan on log increase in profit, an increase of Rs. 100,000 in lending causes a 2% increase in profit. At the mean profit (which is Rs. 3,670,000), this would correspond to an increase in profit of Rs. 72,000 after repaying interest, which is very similar to what we found using cost and sales as the dependent variables.

Can a net return of 72% or 73% be explained by the subsidy implicit in the program? After correcting for default risk and administrative costs and using a cost of capital of 12%, we estimate the cost of lending to the priority sector for Indian sector public banks to be 22%, which is higher than the 16% the firms actually pay. But a subsidy of 6% makes very little dent on a net return of 72%. Indeed the excess return would still be sizeable if we were to decide

that the public sector banks are pricing their credit wrong and that they should charge 3% per month (42% per year) which seems to be the going rate on trade credit for these relatively large firms.

The private return on an extra rupee of loans to firms in this sample is close to 90% (72% +16%). The social return is about 83% (the social cost of capital is 6% higher than the private cost). Both these returns are correctly read as answers to the question: what happens if the bank lends an extra rupee to the firms that have had a history of success. The private return is therefore the right number to use for calculating the short-run impact of a shock to the bank's balance sheet, while the social return is what the social planner should use in deciding how credit should be allocated. The magnitude of these numbers tells us that known highly profitable opportunities remain unexploited in the Indian economy.

However since this is the return to captial in case of success, both of these numbers probably over-state the impact we would expect if the bank's lending was permanently raised since in the long run not all "good" firms will remain that way (markets change, managers retire) and the bank will have to lend to new firms of unknown quality in order to identify future winners. Moreover these returns cannot be read as the average return on a dollar of bank lending. In our model it is clear that the average bank loan is much more likely to go to a "bad" firm than the marginal loan that results from the expansion of a directed lending program. Consistent with this we find that in the data 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 significantly positive, but only 0.28. The OLS estimates of the effect of loans on profit is even smaller and insignificant. Although due to large standard errors, we cannot reject the equality of the OLS and the IV estimates, the difference clearly goes in the right direction that OLS is smaller than IV. Our estimate therefore should be seen as the causal estimate of the marginal value of an additional dollar lent, as long as there are enough credit constrained good firms in the bank's portfolio.

It is, however, not obvious that we should think of these returns as the return to any specific factor (capital, in the form of machines, say). The most common use of this money is probably to pay wages (because paying labor is the one thing one cannot use trade credit for), but it is possible that getting access to this extra money will also impact the borrower's ability to get

more trade credit and hence expand the firm's use of other inputs as well. The observed effect of the extra rupee will then be some combination of the effect of extra labor and the effect of the extra units of the other factors.

6 Conclusion: Policy Issues

The evidence presented in this paper suggests that many relatively large firms in India were severely credit constrained during 1998-2002. It might be tempting to see this as a cautionary tale about what happens when banks, as in India, are largely publicly owned.

As shown in section 2, it is true that the particular public sector bank we study is quite rigid in the way it allocates credit, and one could imagine this leading to substantial deviations from optimality in the allocation. Indeed this is what the model in section 3 predicts.

However this cannot be the whole story: During the period of our study, and especially during the period covered by the later experiment (2000-2002), private banks were quite active in the Indian banking sector—almost a quarter of the total credit to firms in the economy came from private banks, including a number of multinational banks. If the entire underlending was a product of the irrationality of the public bank, any of these private banks could have stepped in – the firms in our sample are but a drop in the ocean compared to the total lending of any one of the private or multinational banks operating in India. 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 did not 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 specific expertise that is only acquired over time and most non-public sector banks 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.[30]

There are however good reasons not to be quite so optimistic. Stein (2001) has argued that the inability to lend effectively to small borrowers is in the very nature of being a bank: 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 the bank,[31] 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 and by penalizing the loan officer for defaults. As in our model, this discourages the loan officer from lending, unless the firm is a very sure bet. This obviously limits the discretion the loan officer enjoys and makes his lending less effective, but it covers the bank.[32] An obvious social cost is that small firms have a hard time borrowing.[33]

This is not to say that some characteristics of the India economy such as the cost of enforcing a loan contract are not important in understanding why no one wants to lend to these firms.

[30]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. Banerjee et al. (2004) contains an overall assessment of the performance of the Indian public sector.

[31]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.

[32]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%.

[33]Berger et al. (2001) show that in the US, the increasing concentration in banking after deregulation, has significantly reduced access to credit for small firms.

But there are many other countries with similar dysfunctionalities where we would expect the same kinds of results to apply.

It is therefore important not to lose track of policy changes that would make it easier to lend to small firms in developing countries by focussing entirely on the privatization issue. In particular, it may help to set up special courts for the speedy disposition of default cases (some states in India are experimenting with this model, and Visaria (2006) finds that this debt recovery tribunals do reduce default and interest rates charged on loans). 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.

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Figure 1

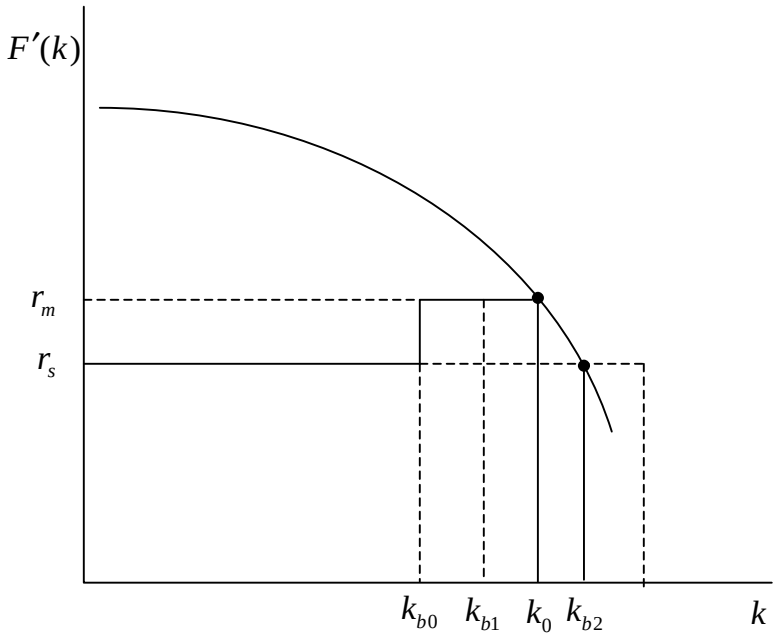

Figure 2

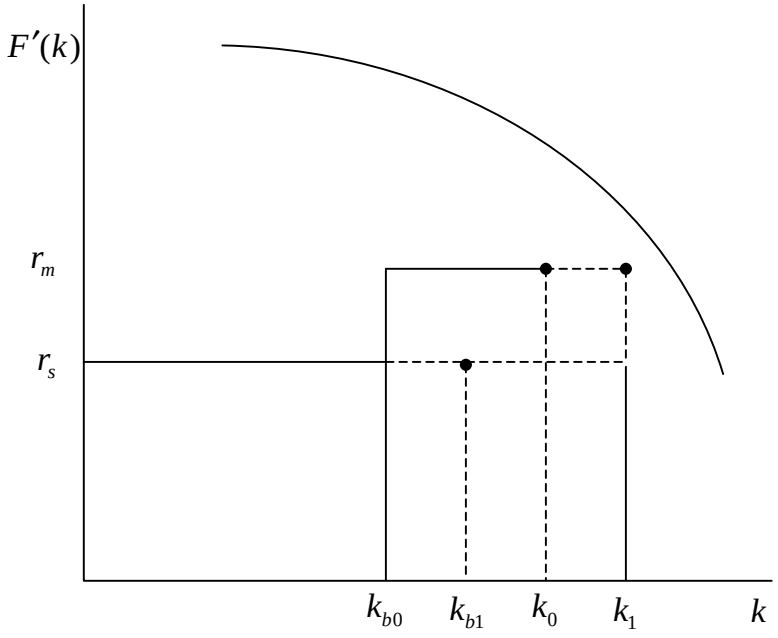

Table 1: Descriptive statistics

	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 loan (this bank)	87.66	96.29	10.29	7.46
	(237.04)	(258.2)	(59.92)	(55.32)
	1226	928	966	928
log(working capital loan) (this bank)	3.39	3.44	0.07	0.07
	(1.47)	(1.5)	(.24)	(.24)
	1208	928	928	928
working capital loans (all banks)	87	97	10	7
	(246)	(273)	(69)	(67)
	1102	807	842	807
log(working capital loans) (all banks)	3.36	3.41	0.06	0.06
	(1.48)	(1.51)	(.26)	(.26)
	1085	807	807	807
other bank loans positive	0.0120	0.004	0.0000	-0.007
	(.11)	(.06)	(.14)	(.1)
	1748	807	1748	807
other bank loans (level)	1.65	2.23	0.00	-0.62
	(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. All Values are expressed in current Rs.10,000.

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 limit	0.72	0.69	-0.01	-0.01
	(.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

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. All Values are expressed in current Rs.10,000.

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	217	213	175	163	124

Note:

1.Each column present the data on the limit approved in a given year (to be used in the following year).

2.Limits from other banks were not collected in year 2002.

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- HAS PAST UTILIZATION REACHED MAXIMUM ?						
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
		(.054)	(.056)	(.04)	(.081)	(.072)
B-HAVE PROJECTED SALES INCREASED?						
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
		(.076)	(.079)	(.053)	(.114)	(.101)
C-HAVE ACTUAL SALES INCREASED?						
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
		(.041)	(.043)	(.029)	(.059)	(.05)
D-HAS PROFIT OVER SALE INCREASED?						
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
		(.042)	(.044)	(.028)	(.059)	(.053)
E- HAS CURRENT RATIO INCREASED?						
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
		(.038)	(.04)	(.027)	(.052)	(.049)

Note:
1. Each panel divides the sample in two subsamples, according to the answer to the question asked in the panel title.
2.Column 1 gives the proportion of the sample that falls into each categorie. The first two rows in Column 2 to 6 display the mean of the relevant variables in the subsample where the answer to the question in the panel title is yes (row 1 in each panel) , and no (row 2 in each panel).
3.Row 3 is the difference between row 1 and 3 in each panel. The standard errors are in parenthese in row 4.

Table 4: Average change in limit

	Years		
	1996-1997	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)	(.030)	(.025)
biggest	0.093	0.147	0.000
	(.064)	(.040)	(.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)	(.040)
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 Rs 6.5 million . "Medium firms" are firms with investment in plant and machinery above Rs 6.5 million. and below Rs 10 million. "Biggest firms" are firms with investment in plant and machinery above Rs 10 million.

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

	Dummy equal to 1 if			Log(working capital limit availble at t)-log(working capital limit available at t-1)			
	limit was changed between t and t-1	limit increased between t and t-1	limit decreased between t and t-1	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: t=1997-2000							
post	0.000	-0.026	0.026	-0.034	-0.115	-0.025	-0.102
	(.050)	(.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: t= 1999-2003							
post2	0.069	-0.073	0.004	-0.027	-0.038	-0.028	0.001
	(.032)	(.037)	(.024)	(.024)	(.075)	(.026)	(.077)
biggest	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)	(.080)	(.171)
	769	769	769	769	217	569	168
PANEL C: t= 1997-2003							
post*biggest (γ_{3kb})	0.067	-0.041	-0.026	0.089	0.346	0.076	0.352
	(.150)	(.150)	(.024)	(.059)	(.146)	(.059)	(.145)
post*medium (γ_{4kb})	-0.059	0.076	-0.016	0.088	0.233	0.083	0.221
	(.098)	(.090)	(.051)	(.041)	(.122)	(.042)	(.119)
post2*biggest (γ_{5kb})	0.054	-0.176	0.122	-0.142	-0.482	-0.150	-0.581
	(.175)	(.170)	(.033)	(.077)	(.181)	(.076)	(.157)
post2*medium (γ_{6kb})	0.168	-0.177	0.010	-0.077	-0.185	-0.078	-0.170
	(.034)	(.052)	(.040)	(.044)	(.167)	(.040)	(.159)
	924	924	924	924	265	718	215

Notes:
1. Each panel is a separate regression. Each column presents a regression of column heading on the variables listed in each panel.
2. For consistency of notation across tables 5 to 9, we display credit available in year t (granted in year t-1).
3. The dummy "post" is equal to 1 for credit available in 1999 and 2000 (granted in year 1998 and 1999), zero othertwise.
The dummy "post2" is equal to 1 for credit available in 2001-2002-2003 (granted in years 2000,2001 and 2002), zero otherwise.
4. The dummy "big" is equal to 1 for firms with investment in plant and machinery larger than Rs 6.5 millions, zero otherwise.
The dummy "medium" is equal to 1 for firms with investment in plant and machinery between Rs 6.5 and Rs 10 million.
The dummy "biggest" is equal to 1 for firms with investment in plant and machinery larger than Rs 10 million
5. In addition to the coefficients displayed, the regression in panel C includes the dummies "post", "post2", "medium", "biggest".
6-Standard errors (corrected for clustering at the sector level) are in parentheses below the coefficient.

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

	Complete sample				Sample where limit was changed		
	interest rate$_t$ - interest rate$_{t-1}$	log(interest rate)$_t$ -log(interest rate)$_{t-1}$	dummy for interest rate decline	log(turnover/limit)$_t$ -log(turnover/limit)$_{t-1}$	interest rate$_t$ - interest rate$_{t-1}$	log(interest rate)$_t$ -log(interest rate)$_{t-1}$	dummy for interest rate decline
	(1)	(2)	(3)	(8)	(5)	(6)	(7)
A. t=1997-2000							
post	-0.165	-0.010	0.280	0.154	-0.127	-0.007	0.279
	(.128)	(.008)	(.074)	(.174)	(.249)	(.015)	(.151)
big	-0.002	0.000	0.098	0.412	-0.036	-0.002	0.052
	(.132)	(.008)	(.106)	(.188)	(.241)	(.014)	(.153)
post*big	0.073	0.002	-0.135	-0.112	0.163	0.009	-0.144
	(.169)	(.011)	(.125)	(.260)	(.337)	(.020)	(.225)
	430	430	430	93	141	141	141
B. t=1999-2002							
post2	0.035	-0.009	-0.029	0.018	-0.146	-0.008	0.225
	(.072)	(.013)	(.038)	(.116)	(.167)	(.013)	(.068)
biggest	-0.062	-0.007	-0.010	0.971	-0.077	-0.004	0.039
	(.110)	(.008)	(.063)	(.578)	(.188)	(.011)	(.140)
post2*biggest	0.099	0.020	0.001	-0.840	0.206	0.013	-0.036
	(.147)	(.017)	(.098)	(.868)	(.385)	(.026)	(.184)
	719	721	721	139	203	203	203

Notes:
1. Each panel is a separate regression. Each column presents a regression of column heading on the variables listed in each panel.
2. The interest rate is the interest rate on credit used at date t (granted at date t-1).
3. The dummy "post" is equal to 1 for year 1999 and 2000 (limit granted in yaers 1998 and 1999), zero otherwise.
The dummy "post2" is equal to 1 for years 2001-2002-2003 (limit and interest rate granted in years 2000,2001 and 2002), zero otherwise.
4. The dummy "big" is equal to 1 for firms with investment in plant and machinery larger than Rs 6.5 million, zero otherwi
The dummy "medium" is equal to 1 for firms with investment in plant and machinery between Rs 6.5 and Rs 10 million.
The dummy "biggest" is equal to 1 for firms with investment in plant and machinery larger than Rs 10 million.
5-Standard errors (corrected for clustering at the sector level) are in parentheses below the coefficient.

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. t=1997-2000					
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. t=1999-2002					
1 Sample with Changes in limit					
post2*biggest	-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					
post2*biggest	-0.092	-0.045	-0.092	-0.048	0.170
	(.108)	(.128)	(.108)	(.086)	(.56)
	401	380	401	399	321
3. Whole sample					
post2*biggest	-0.143	-0.113	-0.016	-0.101	-0.253
	(.111)	(.134)	(.162)	(.094)	(.496)
	569	530	570	567	472

Notes:
1. Each panel is a separate regression. Each column presents a regression of column heading on the variables listed in each panel.
2. The dummy "post" is equal to 1 in years 1999 and 2000, zero otherwise.
The dummy "post2" is equal to 1 in years 2001-2002 zero otherwise.
3. The dummy "big" is equal to 1 for firms with investment in plant and machinery larger than Rs 6.5 million, zero otherwise.
The dummy "biggest" is equal to 1 for firms with investment in plant and machinery larger than Rs 10 million.
4-Standard errors (corrected for clustering at the sector level) are in parentheses below the coefficient.
5-In addition from coefficient displayed, the regressions in panels A1-A3 include a dummy for post and a dummy for big.
5-In addition from coefficient displayed, the regressions in panels B1-B3 include a dummy for post2 and a dummy for biggest.

Table 8: Credit constraints: Effect of the reform on sales and cost and overidentification tests

	Dependent variables			
	$Log(sales)_t$-$log(sales)_{t-1}$		$log(sales/loans)_t$	$Log(costs)_t$
	Complete Sample	Sample without substitution	$log(sales/loans)_{t-1}$	$-log(cost)_{t-1}$
	(1)	(2)	(3)	(4)
post*big (γ_{3y})	0.238	0.235	-0.114	0.205
	(.153)	(.162)	(.256)	(.151)
post*medium (γ_{4y})	0.160	0.122	0.000	0.165
	(.118)	(.13)	(.106)	(.105)
post2*big (γ_{5y})	-0.421	-0.400	0.156	-0.384
	(.197)	(.186)	(.204)	(.279)
post2*med (γ_{6y})	-0.074	-0.080	-0.139	-0.058
	(.112)	(.114)	(.202)	(.105)
	215	193	216	215
ratio 1: γ_{3y}/γ_{3kb}	0.676	0.666	-0.324	0.583
ratio 2: γ_{4y}/γ_{4kb}	0.725	0.555	-0.001	0.749
ratio 3: γ_{5y}/γ_{5kb}	0.725	0.689	-0.269	0.660
ratio 4: γ_{6y}/γ_{6kb}	0.439	0.470	0.820	0.340
test ratio 1=ratio2	0.39	0.46	0.388	0.38
(p value)	(0.53)	(0.50)	(0.53)	(0.54)
test ratio 1=ratio2=ratio3	0.20	0.28	0.20	0.19
(p value)	(0.82)	(0.75)	(0.81)	(0.83)
test ratio 1=ratio2=ratio3=ratio4	0.16	0.21	0.16	0.13
(p value)	(0.92)	(0.89)	(0.92)	(0.94)

Notes:
1.All the regressions are estimated in the sample were the limit was changed.
2. The dummy "post" is equal to 1 in years 1999 and 2000, zero otherwise.
The dummy "post2" is equal to 1 in years 2001-2002 zero otherwise.
3. The dummy "big" is equal to 1 for firms with investment in plant and machinery larger than Rs 6.5 millions, zero otherwise.
The dummy "biggest" is equal to 1 for firms with investment in plant and machinery larger than Rs 10 million.
4-Standard errors (corrected for clustering at the sector level) are in parentheses below the coefficient.
5-In addition from coefficient displayed, the regressions include dummies for post, post2, medium, and biggest.
6-The parameters in parenthesis refer to equation (6) in the text. The ratios are computed using the parameters of equation (5) in the text, displayed in column 7 and panel C of table 5

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
	1997-2000	1999-2002	1997-2002	1997-2002	1997-2002	1997-2002
	no ssi products					
Regressor:	(1)	(2)	(3)	(4)	(5)	(6)
A. $\log(sales_t)-\log(sales_{t-1})$						
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. $\log(cost_t)-\log(cost_{t-1})$						
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. $\log(profit_t)-\log(profit_{t-1})$						
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-Each panel and each column present the result for a separate regression.
3-The regressions in column 1 controls for the "post" and "big" dummy (defined as in previous tables) and use the interaction big*post as instrument
4-The regressions in column 2 controls for the "post2" and "biggest" dummy (defined as in previous tables) and use the interaction biggest*post2 as instrument
5-The regressions in columns 3, 4 and 5 control for the dummies "post", "post2", "big" and "biggest" (defined as before) and use the interactions "post*med" "post2*biggest" and "post*biggest" as insturments
6-The regressions in colum 6 control for the dummies "post", "post2", "big" and "biggest" (defined as before)