Review of Economic Studies (2014) **81**, 572–607 doi: 10.1093/restud/rdt046

Advance access publication 5 February 2014

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

ABHIJIT V. BANERJEE
Department of Economics, MIT

and

ESTHER DUFLO
Department of Economics, MIT

First version received May 2008*; final version accepted July* 2013 (*Eds.*)

This article uses variation in access to a targeted lending program to estimate whether firms are credit constrained. 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, and to smaller firms that were already eligible for the preferential credit before 1998 and remained eligible in 2000. Comparing the trends in the sales and the profits of these two groups of firms, 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 in 1998, and a corresponding decline in 2000. There was no change in trends around either date for the small 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.

Key words: Banking, Credit constraints, India, misallocation.

JEL Codes: O16

1. INTRODUCTION

Limits to credit access are widely accepted today as an important part of an economist's description of the world. Credit constraints figure prominently in economic analyses of short-term fluctuations and long-term growth.[1] Cross-country/cross industry approaches (Rajan and Zingales, 1998) suggest that industries which require more financing grow more slowly in countries with poorly

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.

developed capital markets, which is *prima facie* evidence that financial constraints are particularly likely to matter in poor countries. Yet there is still very little tight micro-economic evidence of the existence of credit constraints on larger firms in developing countries. The available evidence comes mostly from farming and very small firms in the manufacturing and services sector.[2] Since such firms have a relatively small part of overall the capital stock and the share of capital in the farm sector even in relatively poor countries is not very large and declining (in India, the share of agriculture in output is 24% and its share in the capital stock is even smaller (Banerjee and Duflo, 2005), it seems unlikely that if credit constraints were confined to these sectors they would have major aggregate consequences. Therefore, it is important to have solid evidence of the presence or absence of credit constraints among larger firms. This is the goal of the present article.

We make use of two policy changes that affected the flow of directed credit to an identifiable subset of firms that first gained and then lost eligibility for a targeted lending policy. Such policies are common in many developing and developed countries. What makes this case particularly interesting is that the firms affected by the policy are formally registered firms (although not listed on the stock market), and fairly large by Indian standards. Firms with a capital stock of Rs. 6.5 to Rs. 30 million were affected by the reform. The average capital stock of firms in the 95th percentile in the median industry in India was Rs. 36 million (approximately 45 rupees to a dollar at this time).

We first show that firms are rationed with respect to the supply of directed credit. When they become eligible for directed credit they expand their borrowing from the bank relative to firms that were already eligible, and when they become ineligible they reduce their borrowing relative to firms that remain eligible. On the other hand, the interest rate they were paying to the bank did not change differentially. Since there is no reason to assume that productivity trends for firms affected by this policy change are systematically different from the firms that they are compared to (our identification assumption) this suggests that some of these firms cannot borrow as much as they want at the going interest rate—the supply curve of credit from this particular bank to its client firms *is not always horizontal*.

However, the fact that the bank was legally required to lend a certain amount to these firms at a capped interest rate makes it difficult to interpret this evidence as a sign that these firms are credit constrained, in the sense that the *aggregate supply of capital to the firm* (and not just the supply from one specific bank) is upward-sloping. It could be that there is a fixed amount that the bank is prepared to lend to these firms (just enough to meet their regulatory obligations) and that this amount is rationed among a certain set of eligible firms, with no one getting as much as they wanted (because the bank is forced to make it available at below market rates). It could still be true that the firm, while rationed with respect to bank capital, can borrow as much as it wants at the market rate, in which case they are not credit constrained in the sense suggested above. This distinction is critical because an upward-sloping aggregate supply of credit to the firm means that a firm that has more of its own money and therefore needs less credit faces a lower interest rate, which is sufficient to generate the wealth/cash flow/balance sheet effects that drive the macro consequences of credit market imperfections. Conversely, if all firms can borrow as much as they want at the common market interest rate then wealth/cash flow/balance sheet effects may be absent.

2. The estimation of the effects of credit constraints on farmers makes use of the fact that 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. McKenzie, De Mel, and Woodruff (2008) randomly allocate small ($200 or so) capital grants to microenterprises in Sri Lanka, and find that the returns to capital are very high for those firms: the average returns to capital is as high as 4% per month. Similar results are found in Mexico (McKenzie and Woodruff, 2008).

Our goal in this article is to demonstrate that these firms are in fact credit constrained in the sense defined above. Our main empirical challenge comes from the fact that we do not observe market borrowing. We therefore present multiple separate pieces of evidence to demonstrate credit constraints.

The first methodology is based on two observations we make in the first part of the theory section: 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 in response to an increase in the availability of cheap directed credit 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.

The second methodology involves directly estimating the effect of bank credit on profits, then comparing with standard estimates of the interest rate faced by firms of this type. Finally, we show that, at least locally, the firms seem to face increasing returns to working capital. We argue that this is not consistent with an interior maximum for unconstrained firms.

We implement these methodologies using firm-level data we collected from a sample of medium size firms in India. We make use of two changes in the so-called "priority sector" regulation, under which firms smaller than a certain limit are given priority access to bank lending.[3] 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 triple 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, and compare this change to the corresponding changes for firms that were already in the priority sector (for whom the reform resulted in a decline in the rate of growth of their loan). 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, were excluded again in 2000.

Our key results are as follows. First, as mentioned above, we show that bank lending expanded (relatively) for firms who were newly included in the priority sector (relative to firms that were always included, as well as to another sample of firms that were never included), and then contracted for the subsample that were excluded. Second, sales, costs, and profits followed the same pattern. An overidentification test confirms that the implied effect of bank loans on revenue and costs is the same in both cases, which bolsters our confidence in the identification assumption that there was no differential trends (unrelated to the changes in credit availability) affecting those firms. The same result is obtained when we restrict the sample to firms that did not substitute market credit for bank credit, a first indication that these firms are credit constrained. Combining the credit and the sales equations, we obtain an instrumental variable estimates of the effect of bank credit on sales; we apply the same methodology to obtain IV estimate of bank credit on costs and profits. We find that the elasticity of sales with respect to bank credit is 0.75, and that of cost is 0.70. For the average firm, this suggests that one rupee of loan increases profit net of interest payment by 0.89 rupees, and hence profit before interest payment by 1.05 rupees. Available evidence suggest that the market rate of interest for firms comparable to those in our sample ranges between 30% and 60%. Given these returns, firms should therefore want to borrow more. This is our second indication that firms are credit constrained. Finally, we propose a specific parametric form for the production function, and in this specific case, we are able to compute the return to scale for working capital for the firms in our sample. We find evidence of (locally)

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

increasing returns to scale in working capital, a third indication, as we argue, that firms must be credit constrained.

These results together tell us that firms are indeed credit constrained in the sense described above. Under our assumptions about the production function, we also show that the impact of bank loans on profit turns out to be exactly the net marginal return to working capital. This implies that the marginal returns of capital for the average firm in our sample is 105%. Meanwhile, depositors are paid only about 10% on their savings on average, a first metric of inefficiency. Furthermore, this rate of return is considerably higher than available estimates of the rate of return to capital in the economy (which, in Banerjee and Duflo (2005), we compute to be about 22% in India, and Caselli and Freyer (2007) calculate to be at most 19% for Sri Lanka which should be similar to India. It would thus seem that there are firms in the economy that have a much lower marginal product than at least some firms in our sample, and, therefore, that capital would be allocated more efficiently if it could be re-directed to those. A final piece of evidence suggests that there may be misallocation *within* the sample of firms as well: the OLS estimate of the impact of bank loan on profit is much lower than than the IV estimate, suggesting negative selection (the least productive firms are the ones getting the most credit). This is consistent with a model of bank lending we develop in the second part of the theory section of the article, where bank officers have reason to allocate bank loans to some of the least successful firms among those that they had lent to in the past in order to try to save them from bankruptcy (what is sometimes called "evergreening").

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.[4]

While banks in India occasionally provide longer-term loans, financing fixed capital is primarily the responsibility of specialized long-term lending institutions such as the Industrial Finance Corporation of India. Banks typically provide short-term working capital to firms. These loans are given as a credit line with a pre-specified limit and an interest rate that is set a few percentage points above prime. The borrower draws from the limit when needed, and reimburses on a quarterly basis. This article 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%. The average interest rate in our sample is about 16%, and this is representative of the interest rates charged by banks in India. 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

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

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. If they lend to the priority sector, they lend at the same interest rate as in other sectors.

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 target 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% (for example 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 labour and administrative costs associated with lending to the priority sector were about 0.015 rupees higher per rupee lent 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 fulfil 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. Note that this means that we do not estimate the effect of priority lending on credit (since the group of small firms is not a control group: it is affected as well). Our main focus in this article is to trace and quantify the impact that the differential growth in loans for big versus small firms has on their revenues, costs and profits.

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. Therefore, 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 could not collect data on larger firms, that are not generally handled by those branches. 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, 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 sample selection bias would emerge from the closing of those accounts.[5]

Table 1 presents the summary statistics for all data used in the analysis (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.

Bank credit is quite important. In this sample (as well as in the nationally representative Annual Survey of Industries), bank credit is around one half of total current liability. The rest is financed, we presume, by a combination of more informal loans, accumulated cash and trade credit.

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 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, 57% to 81% of the accounts reached or exceeded the credit limit at least once in the year. This means that the borrower effectively overdrawn from their account.

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. There is no evidence that firms were using any other formal source of credit. On average 98% of what is reported as working capital borrowing from formal sources (this excludes trade credit) by the firms in our sample come from this one bank and, in any case, the same kind of inertia shows up if we look at loans from all banks. Sales, on the other hand, increased from year to year for most firms (row 2),

5. 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.

TABLE 1
Descriptive statistics

	Levels		Change(t)-(t-1)	
	Entire sample (1)	Change in loans not missing (2)	Entire sample (3)	Change in loans not missing (4)
Panel A: Loans and interest rates				
Working capital Loan limit (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 limit) (this bank)	3.39	3.44	0.07	0.07
	(1.47)	(1.5)	(0.24)	(0.24)
	1208	928	928	928
Working capital Loans limit (all banks)	87	97	10	7
	(246)	(273)	(69)	(67)
	1102	807	842	807
log(working capital loans limits) (all banks)	3.36	3.41	0.06	0.06
	(1.48)	(1.51)	(0.26)	(0.26)
	1085	807	807	807
Other bank loans Positive	0.0120	0.004	0.0000	−0.007
	(0.11)	(0.06)	(0.14)	(0.1)
	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)	(0.94)	(0.94)
	1142	896	876	856
log(interest rate)	2.75	2.74	−0.02	−0.02
	(0.18)	(0.19)	(0.16)	(0.17)
	1142	896	878	858
Panel B: Credit utilization and firm performance				
Account reaches the limit	0.72	0.69	−0.01	−0.01
	(0.45)	(0.46)	(0.44)	(0.44)
	522	380	247	233
log(account turnover/ granted limit)	2.15	2.15	0.09	0.11
	(0.95)	(0.96)	(0.72)	(0.71)
	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)	(0.53)	(0.45)
	1248	740	1029	732
Net profit	36.53	42.49	6.08	4.00
	(214.11)	(237.16)	(61.33)	(58.32)
	1259	747	1043	741
log(costs)	5.45	5.61	0.16	0.09
	(1.45)	(1.45)	(0.51)	(0.43)
	1245	739	1022	729

Notes: 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. 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. All values are expressed in current Rs.10,000. The account turnover is the sum of all the payments that have been made using the firm's credit line over the year.

TABLE 2
Characteristics of loans

	1997 (1)	1998 (2)	1999 (3)	2000 (4)	2001 (5)	2002 (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
Projected 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*projected 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
	(0.061)	(0.05)	(0.054)	(0.056)	(0.126)	(0.07)
Ratio granted limit/(0.20*predicted sales)	0.62	0.63	0.68	0.63	0.68	0.71
	(0.041)	(0.037)	(0.034)	(0.055)	(0.064)	(0.062)
Number of loans	175	208	205	175	163	124

Notes: Each column present the data on the limit approved in a given year (to be used in the following year). Limits from other banks were not collected in year 2002.

as did the maximum authorized lending (a function of projected sales based on the bank's official lending policies, which the loan officer dutifully records on the file). The change in the credit limit that was actually sanctioned systematically fell short of what the bank itself determined to be the firm's needs (which is also reported in the file). On average, the granted limit was 89% of the recommended limit based on needs.

It is possible that some of the shortfall was covered by informal credit. While we have no evidence on the interest rate at which the firms in our sample borrow (since all the records we have come from the bank, which does not collect this type of information), we review a number of descriptive studies in Banerjee and Duflo (2005). This literature suggest that, in urban areas, entrepreneurs can get credit from non-bank financial institutions, or informal lenders, at rates that vary a lot, but are in the 30%–60% range. A recent paper by De and Singh (2011) estimates, based on survey data, that the maximum interest rate charged in informal relationships in India ranges between 50% and 58%.[6]

In Table 3, we examine in more detail whether the bank's lending behaviour seems related to factors affecting a firm's need for credit. Column (3) shows that no variable that 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 U.S., of how secure a working capital loan is) had increased. Turning to the direction of the changes, only an increase in projected sales or current sales predicts an increase in granted limit. The only variable that predicts the size of the increase in the credit limit is projected sales but this could well be due to reverse causality: the bank officers could be more likely to predict an increase in sales when they are preparing to give a larger credit extension to the firm. The same is true for old and young clients, suggesting that lack of information does not explain the result.

6. The average interest rate for trade credit in their sample is 34% and the median is 22%.

TABLE 3
Changes in working capital limits, by firm characteristics

	Proportion	Propoportion of cases where Limit was increased	Limit was not changed	Mean of: log(current limit) –log(past limit)	Proportion of cases where limit was changed Client<=5 years	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	0.69
Difference		0.05	−0.05	0.03	−0.05	−0.02
		(0.054)	(0.056)	(0.04)	(0.081)	(0.059)
B-Have projected sales increased?						
Yes	0.71	0.43	0.52	0.19	0.54	0.56
No	0.29	0.25	0.61	0.06	0.50	0.73
Difference		0.18	−0.09	0.13	0.04	−0.17
		(0.076)	(0.079)	(0.053)	(0.114)	(0.083)
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	0.74
Difference		0.08	−0.06	0.02	−0.09	−0.06
		(0.041)	(0.043)	(0.029)	(0.059)	(0.04)
D-Has profit over sale increased?						
Yes	0.56	0.29	0.67	0.11	0.64	0.71
No	0.44	0.35	0.61	0.16	0.61	0.70
Difference		−0.06	0.06	−0.05	0.03	0.01
		(0.042)	(0.044)	(0.028)	(0.059)	(0.043)
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	0.72
Difference		0.03	−0.05	−0.02	−0.06	−0.03
		(0.038)	(0.04)	(0.027)	(0.052)	(0.039)

Notes: Each panel divides the sample in two subsamples, according to the answer to the question asked in the panel title. Column 1 gives the proportion of the sample that falls into each category. The first two rows in Columns (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). Row 3 is the difference between row 1 and 3 in each panel. The standard errors are in parentheses in row 4.

3. THEORY: THE EFFECT OF SUBSIDIZED BANK CREDIT ON FIRM OUTCOMES

The goal of this section is to develop some intuition about how we expect firms to react to an increase in the supply of subsidized bank credit. We study 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.

3.1. *Defining credit constraints*

Consider a firm with the following fairly standard production technology: the firm pays a sunk cost Ψ before starting production (say the cost of setting up a factory and installing machinery).

Once the cost is sunk, its production can be described by a production function

$$R = f(I_1, I_2, \ldots . I_n). \tag{1}$$

Now assume that inputs $1, .., m, m < n$, are paid out of working capital. Working capital partly comes from the bank and partly from the firm's market borrowing. Therefore:

$$I_1 p_1 + I_2 p_2 + \ldots . + I_m p_m \leq k_b + k_m = k \tag{2}$$

where p_i is the price of the i-th input, k_b is total bank capital available to the firm, k_m is total market capital, and $k = k_b + k_m$ is total available working capital. We will assume that the working capital constraint above always binds.

The rest of the inputs, $I(m+1), \ldots, I(n)$, do not have to be purchased out of working capital, perhaps because they are financed by trade credit (the interest on which is included in the price). All inputs are supplied elastically at the given price.

We assume that the firm maximizes:

$$f(.) - I_1 p_1 + I_2 p_2 + \ldots . + I_n p_n \tag{3}$$

subject to the working capital constraint given above. This maximization problem leads to a solution $R = f_t(k_t), f_t'(k_t) > 0$, where k_t is total working capital (the time subscript recognizes that prices also enter into the production function and vary over time). We interpret this as a reduced form production function. Let $f_t(k_t)$ be increasing but bounded in k_t, reflecting the idea that once the fixed investment is sunk, the firm can only expand its output so much (this does not rule out locally increasing returns, but ensures that for any r there is a finite optimal level of k that maximizes $f_t(k) - rk$).

Given this production function, it is easy to define $k_t(r)$, the firm's total investment if it can borrow as much as it wants at interest rate r. This is the level of k_t that maximizes $f_t(k) - rk$.

To define credit constraints, consider a firm that is borrowing at multiple interest rates: $r(1) < r(2), \ldots .. < r(N)$. Let the corresponding amounts of borrowing be $k_t(1), k_t(2), \ldots ... k_t(N)$. In addition let $k_t(0)$ be the amount of capital that comes from the firm's own resources.

Then we say that the firm is *credit rationed* at interest rate $r(i)$ if the amount of the borrowing at or below that rate, $\sum_{0 \leq j \leq i} k_t(j) < k_t(r(i))$. The firm would want more credit at that rate than it is getting. We say that the firm is *credit constrained* if $\sum_{0 \leq j \leq N} k_t(j) < k_t(r(N))$. In other words, the total capital that the firm has is less than the amount it would want at the highest interest rate that it is currently paying.

The distinction between credit rationing and credit constraints is key: most theoretical models that build on credit constraints rely on there being wealth, balance-sheet or cash flow effects on investment, and our definition of credit constrained aims to capture this: It should be clear from the definitions that if the firm is not credit constrained (even if it is credit rationed), a small shock to $k_t(0)$ has no effect on its total capital stock, which remains at $k_t(r(N))$. Nevertheless, firms that are credit rationed but not credit constrained might rightly claim that credit access is a problem for them when asked the standard survey question about the problems they face—since cheaper credit is always preferable.

Of course under the standard perfect capital markets assumption, the firm faces a horizontal supply curve of capital at a single fixed interest rate, $k_t(N) = k_t(r(N))$ and is neither credit constrained nor credit rationed. On the other hand, a firm can also be credit rationed at every interest rate it is paying except the highest and not be credit constrained. This is what makes the identification of credit constraints challenging unless we observe all sources of credit—which is generally not possible. In the next subsection, we develop a theoretical argument that will enable us to get around this problem.

3.2. *The demand side: the key to identifying credit constraints*

To get the basic intuition behind our argument, it suffices to focus on 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 analyse involves the firms 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 at the bank interest rate*. However, this in itself does not imply that they would have borrowed more at the market interest rate. A possible scenario is depicted in Figure 1. The horizontal axis in the figure measures k while the vertical axis represents output. The downward sloping curve in the figure represents the marginal product of capital, $f'(k)$. The step function represents the supply of capital. In the case represented in the figure, we assume that the firm has access to k_{b0} units of capital at the bank rate r_b but was free to borrow as much as it wanted at the higher market rate r_m. As a result, it borrowed additional resources at the market rate until the point where the marginal product of capital is equal to r_m. Its total outlay in this equilibrium is k_0. Now consider what happens if the firm is now allowed to borrow a greater amount, k_{b1}, at the bank rate. 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 as 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

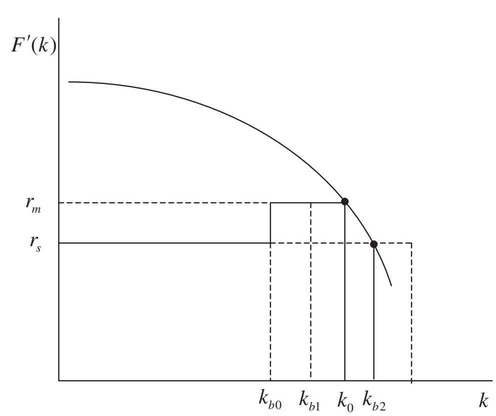

FIGURE 1

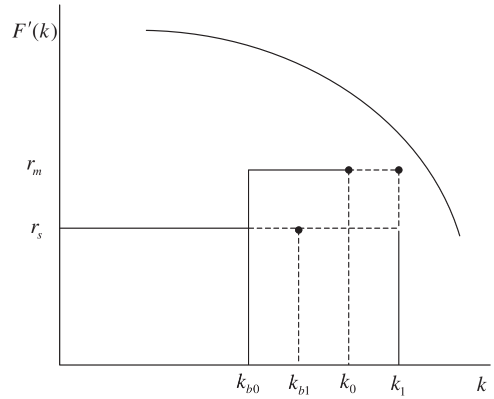

FIGURE 2

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.[7]

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

It is worth pointing out that what this gives us is the marginal product of an extra dollar of credit, not the marginal product of working capital (or the marginal product of inputs). It could be that when credit increases, the firm hires more labourers and then buy more inputs with another form of capital. In Section 6, we spell out a set of assumptions under which we can calculate the marginal product of capital in our settings.

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

7. 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.

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, or for convenience). 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 was 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 was removed. We observe in the subsection on data collection that there was no systematic difference in exit rates between large and small firms in the 2000–2002 period. Indeed, rather surprisingly, attrition is, if anything, slightly lower for bigger firms in this period. This gives us some confidence that the results we show below are not driven by firm exit resulting from the withdrawal of the subsidy.

3.3. *The supply side: understanding lending behaviour 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 *et al*., 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.

3.3.1. 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 subsection). 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 two 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 decided upon by loan officers. Each loan officer's tenure is also two 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.[8] This assumption is a part of the reason why there are bailouts—it says 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 firm types, which makes them attractive in large bureaucracies.

8. When a loan in an Indian public sector bank (like the 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 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).

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 analysing 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 in the Appendix). 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.

In the Supplementary Appendix we prove the following result:

Result 3. Under the assumptions that firms take available credit and that the bail out option dominates for loan officer, loan officer's optimal allocation of second period credit is to give known type H firms an amount $1+g-(1-p_0)(1-p_L)l^*/p_0\pi$, 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 bail out 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 (except in the special period when they get kicked out of the priority sector). 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 the same assumptions as result 3 (spelt out in Appendix) the loan officer's optimal allocation of second period credit is to give type H firms an loan increment of $g-(1-p_0)(1-p_L)(l^*-1)/p_0\pi$, 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.

3.3.2. Implications of results. Under these two conditions, 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. 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 that there is no systematic relation between measures of firm performance and the probability of an increment in the loan or the 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 than the average firm to fail again . 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 an 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 is available) and therefore has no reason to try to spread it around. All the effects of the reform should therefore be on the intensive margin.

These implications can be tested in the data, and we examine them below.

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 were no more likely to take on big firms after the reform and that our results will not be affected by sample selection.

It is important to note that the small firms are not a standard "control" group in our empirical strategy, since they are also expected to be affected by the reform. Since the bank's lending to the priority sector remained around 40% during the entire period, the increase in lending to the large firms during the phase of expansion of the priority sector happened at the expense of the small firms, and vice versa during the contraction phase, as we will show below. Thus, our estimates do not identify the "effect" of the reform on aggregate lending (which did not really change) or sales, or profit. Instead, we exploit this reform as an exogenous source of differential growth in loans for different types of firms, and trace whether there is a corresponding differential increase in sales and growth. Since we are interested in investigating the effects of changes in loans, the fact that the small firms are potentially affected by the reform as well does not invalidate the identification strategy.

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
	(0.021)	(0.013)	(0.014)
Medium	0.040	0.093	0.011
	(0.032)	(0.030)	(0.025)
Biggest	0.093	0.147	0.000
	(0.064)	(0.040)	(0.031)
Firms with fixed capital	0.135	0.062	0.153
between Rs30 and 450 million (4)	(0.017)	(0.017)	(0.019)
B. Proportion of cases where limit was not changed			
Small	0.701	0.701	0.724
	(0.043)	(0.031)	(0.027)
Medium	0.667	0.608	0.798
	(0.088)	(0.055)	(0.040)
Biggest	0.625	0.692	0.769
	(0.183)	(0.075)	(0.053)
C. Average change in limit, conditional on change			
Small	0.366	0.252	0.253
	(0.045)	(0.035)	(0.045)
Medium	0.119	0.237	0.053
	(0.093)	(0.068)	(0.124)
Biggest	0.248	0.479	−0.002
	(0.137)	(0.062)	(0.138)
D. Fraction of cases where the limit was decreased			
Small	0.026	0.051	0.052
	(0.015)	(0.015)	(0.014)
Medium	0.067	0.076	0.087
	(0.046)	(0.03)	(0.028)
Biggest	0.000	0.000	0.123
			(0.041)

Notes: The first row of each panel presents the average of log(working capital limit granted at date t)-log(working capital limit granted at date t-1). Standard errors in parentheses below the average. "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 and below Rs. 30 million Firms with fixed capital between Rs. 30 million and Rs. 450 million: PROWESS data set, Center for Monitoring the Indian Economy.

Since the granted limit, as well as all the outcomes we will consider, are very strongly autocorrelated, we focus on the proportional change in this limit, *i.e.* log(limit granted in year t) − log(limit granted in year t-1).[9] 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, and after January 2000) separately for the largest firms (investment in plant and machinery measured before 1998 between Rs. 10 million and Rs. 30 million), the medium-sized firms (investment in plant and machinery measured before 1998 between Rs. 6.5 and Rs. 10 million), and the smaller firms (investment in plant and machinery measured before 1998 below Rs. 6.5 million).

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

For limits granted in 1996–1997, the average increment in the limit over the previous year's limit was 11% for the small firms, 4% for the medium firms, and 9% for the biggest firms. For limits granted in 1998 and 1999, it was 7.5% for the small firms, 9% for the medium firms, and 15% for the largest firm. We therefore see movement in opposite directions for the small firms and the medium firms, consistent with a reallocation of credit from one group to the next. After 2000, limit increases were smaller for all firms, but the biggest decline happened for the larger firms, whose enhancement declined from an average of 15% in 1998 and 1999 to 0% in 2000.

The last row in Panel A presents comparable data for a sample of larger firms (fixed assets between Rs. 30 and 450 million), which were never included in the priority sector. The data is from the PROWESS data set, collected by the Center for Monitoring the Indian Economy. It shows that the trend in loan size for the "big" firms in our sample does not appear to be driven by a differential trend affecting larger firms irrespective of the priority sector regulation. The increase in loan for these larger firms was 14% in the first period, 6% in the second period, and back to 15% in the third period: in other words, they follow a trend that is U-shaped over time (somewhat similar to the small firms in the first two periods), in contrast to the inverted U-shaped relationship for the medium and big firms in our sample.

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 was 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. Not surprisingly, the patterns are the same as in Table 1, but more pronounced. 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 0.48 to an average of slightly less than 0).

Panel D shows the probability of a decline in the credit limit. Declines are rare in usual times, but they increase steeply (from 0% to 12%) for large firms after the reversal in the reform, suggesting that bank officers specifically targeted those firms to reduce their credit.

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 an equation of the form[10]:

$$\log k_{bit} - \log k_{bit-1} = \alpha_{1kb} BIG_i + \beta_{1kb} POST + \gamma_{1kb} BIG_i * POST_t + \epsilon_{1kbit}, \tag{4}$$

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 $t-1$[11]), *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 1999 and 2000. (The reform was passed in 1998. It therefore affected the credit decisions for the revision

10. All the standard errors are clustered at the sector level.

11. 70% of the credit reviews happen during the last 6 months of the year, including 15% in December alone.

conducted during the year 1998 and 1999, affecting the credit available in 1999 and 2000.) We focus on working capital loans from this bank.[12] We estimate this equation in the entire sample for those years 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 (4) 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:

$$\log k_{bit} - \log k_{bit-1} = \alpha_{2kb} BIG2_i + \beta_{2kb} POST2 + \gamma_{2kb} BIG2_i * POST2_t + \epsilon_{2kbit}, \quad (5)$$

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.[13] 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:

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

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 larger firms, we run a number of other regressions that exactly parallel equations (4) to (6). First, we use the sample 1997–2000 to estimate:

$$y_{it} - y_{it-1} = \alpha_{1y} BIG_i + \beta_{1y} POST_t + \gamma_{1y} BIG_i * POST_t + \epsilon_{1yit}, \quad (7)$$

where y_{it} is an outcome variable (log(credit), interest rate, credit utilization, log(sales), log(cost), log(profit) for firm i in year t. Second, we estimate:

$$\log y_{it} - \log y_{it-1} = \alpha_{2y} BIG2_i + \beta_{2y} POST2 + \gamma_{2y} BIG2_i * POST2_t + \epsilon_{2yit}, \quad (8)$$

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

$$\begin{aligned} \log y_{it} - \log y_{it-1} = {} & \alpha_{3y} BIG2_i + \alpha_{4y} MED_i + \beta_{3y} POST + \beta_{4y} POST2 + \\ & \gamma_{3y} BIG2_i * POST_t + \gamma_{4y} MED_i * POST_t + \\ & + \gamma_{5y} BIG2_i * POST2_t + \gamma_{6y} MED_i * POST2_t + \epsilon_{3yit} \end{aligned} \quad (9)$$

in the 1997–2003 sample.

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

13. 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.

The key identification assumption in estimating these equations is that there are no differential trends in y that are not due to the change in bank loan availability: we assume that there was no difference in the rate of change in productivity, or access to markets, for large and small firms.

Denoting log(sales) with the subscript R, 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 (7) 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.

Our model predicts that the only impact of the reform is on the intensive margin: firms pre-identified as good will now get a larger increase in their loan (when the reform is reversed, some firms may get a decline in loans). Some firms which had previously failed will also get an increase in loans 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 subsamples: 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 Tables 3 and 4. In particular, there is no 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 our 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 predetermined 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.

A final piece of evidence comes from looking at profit (revenue minus costs). Profits are expected to increase regardless of whether the firm is credit constrained or not (since the interest payments go down), but the magnitude of the increase matters, as we discuss in our last section.

4.2. *Empirical strategy: instrumental variable estimates*

The discussion above suggests that equation (4) to (6) and (7), to (9), 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):

Specifically, assume the following log-log relationship between bank loans and sales (or costs):[14]

$$y_{it} = \alpha_{it} + \theta^* log k_{bit} + \epsilon_{it} \quad (10)$$

In this equation, if $y = log(R)$, θ^* is the elasticity of sales with respect to bank credit. Taking the difference across pre and post periods, and substituting k_{bit} by its expression in equations (4)

14. In Section 5, we propose a form of the production function above that will justify this formulation.

and (5), it can easily be seen that an estimate of θ^* i the ratio $\frac{\gamma_{1y}}{\gamma_{1kb}}$, and hence that assuming that the only impact of the reform on sales is due to its impact of credit, controlling for *BIG* and *POST*, *BIG∗POST* is a valid instrument for the impact of bank loans on the sales.[15] Similarly, *BIG*2∗ *POST*2 is another instrument when using the later period, and both can be used in combination when combining the periods.

4.3. *Empirical strategy: testing the identification assumptions*

The interpretation of the central result on sales growth crucially depends on the assumption that there are no differential changes in the productivity trends for small and large firms, either during the expansion or during the contraction. Nonetheless, 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).

The fact that we have two experiments affecting different sets of firms helps distinguish the effect of the priority sector regulation from trends affecting different groups of firms differentially. The two reforms went in different directions and difference in trends would have had to reverse at exactly the same time to match our results. Specfically, we predict γ_{1R} in equation (7) to be positive and γ_{2R} in equation (8) to be negative and moreover, we expect the effects to be symmetrical, *i.e*. the ratio $\frac{\gamma_{1R}}{\gamma_{1kb}}$ and $\frac{\gamma_{2y}}{\gamma_{2kb}}$ should be equal.

The same reasoning of course applies to equations (6) and (9) as well (which combine the two experiments), 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: we have several instruments for θ^*, and they should all give us the same result. 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, 43% 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 (7) to (9) for all the different outcome 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. Under the assumptions of our model, the subsample where there was no change is a true comparison group, in that it is unaffected by the reform. 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

15. More specifically, in our model of the supply of credit, this is an estimate for the firms that are known to be “good”.

did not get a change. This also provides an extra test that the effects are not due to differential trends.

5. RESULTS

5.1. *Credit*

Credit expansion: Panel A in Table 5 presents the results of estimating equation (4) for several credit variables.[16] 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.[17] 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. 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). This appears to have come to some extent at the expense of the small firms, since the coefficient of the dummy for *POST* is negative (−0.11), although not quite significant (of course, it could be a general trend in credit).

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 of the interaction is almost the same (0.26) and still significant.

Credit contraction: In panel B, we present the result of estimating equation (5). 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)).[18] In this case, small and medium firms did not seem to have benefitted from the reversal:

16. The standard errors in all regressions are adjusted for heteroskedasticity and clustering at the firm and sector levels.

17. 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.

18. 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.

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
	(0.050)	(0.052)	(0.024)	(0.026)	(0.074)	(0.028)	(0.071)
Big	−0.043	0.016	0.027	−0.059	−0.218	−0.055	−0.206
	(0.052)	(0.051)	(0.041)	(0.028)	(0.088)	(0.028)	(0.082)
Post*big	−0.022	0.050	−0.028	0.095	0.271	0.087	0.259
	(0.087)	(0.079)	(0.044)	(0.033)	(0.102)	(0.032)	(0.099)
	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
	(0.032)	(0.037)	(0.024)	(0.024)	(0.075)	(0.026)	(0.077)
Biggest	0.017	0.041	−0.058	0.067	0.232	0.057	0.251
	(0.129)	(0.131)	(0.017)	(0.059)	(0.063)	(0.058)	(0.057)
Post2*biggest	0.008	−0.127	0.119	−0.121	−0.442	−0.128	−0.549
	(0.179)	(0.172)	(0.033)	(0.082)	(0.191)	(0.080)	(0.171)
	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
	(0.150)	(0.150)	(0.024)	(0.059)	(0.146)	(0.059)	(0.145)
Post*medium (γ_{4kb})	−0.059	0.076	−0.016	0.088	0.233	0.083	0.221
	(0.098)	(0.090)	(0.051)	(0.041)	(0.122)	(0.042)	(0.119)
Post2*biggest (γ_{5kb})	0.054	−0.176	0.122	−0.142	−0.482	−0.150	−0.581
	(0.175)	(0.170)	(0.033)	(0.077)	(0.181)	(0.076)	(0.157)
Post2*medium (γ_{6kb})	0.168	−0.177	0.010	−0.077	−0.185	−0.078	−0.170
	(0.034)	(0.052)	(0.040)	(0.044)	(0.167)	(0.040)	(0.159)
	924	924	924	924	265	718	215

Notes: Each panel is a separate regression. Each column presents a regression of column heading on the variables listed in each panel. For consistency of notation across Tables 5 to 9, we display credit available in year t (granted in year t-1). 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. 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. In addition to the coefficients displayed, the regression in panel C includes the dummies “post”, “post2”, “medium”, “biggest”. Standard errors (corrected for clustering at the sector level) are in parentheses below the coefficient.

the coefficient for them is close to zero: priority sector lending as a whole contracted over these years.

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 latter is significant.)[19]

5.2. *Evidence of credit rationing*

Table 6 presents evidence on rationing of bank credit. 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.[20] 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 expansion and less after the contraction is not explained by a differential fall in the interest rate charged. To complete the argument we also need to show that firms actually use the additional credit they get when there is an expansion.[21] To look at this, we compute limit utilization, as the ratio of turnover (sum of the payment made on the account) over loan limit and use it as dependent variable. The coefficient of $BIG*POST$ is negative and insignificant both during the expansion and during the contraction.

These results are far from definitive, due to the limited number of observations for which the data on loan utilization is available.[22] However, the evidence available suggests that firms did

19. 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.

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

21. 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.

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

TABLE 6

Effect of the reforms 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)	(4)	(5)	(6)	(7)
A. t = 1997–2000							
Post	−0.165	−0.010	0.280	0.154	−0.127	−0.007	0.279
	(0.128)	(0.008)	(0.074)	(0.174)	(0.249)	(0.015)	(0.151)
Big	−0.002	0.000	0.098	0.412	−0.036	−0.002	0.052
	(0.132)	(0.008)	(0.106)	(0.188)	(0.241)	(0.014)	(0.153)
Post*big	0.073	0.002	−0.135	−0.145	0.163	0.009	−0.144
	(0.169)	(0.011)	(0.125)	(0.249)	(0.337)	(0.020)	(0.225)
	430	430	430	93	141	141	141
B. t = 1999–2002							
Post2	0.035	−0.009	−0.029	0.050	−0.146	−0.008	0.225
	(0.072)	(0.013)	(0.038)	(0.102)	(0.167)	(0.013)	(0.068)
Biggest	−0.062	−0.007	−0.010	0.986	−0.077	−0.004	0.039
	(0.110)	(0.008)	(0.063)	(0.580)	(0.188)	(0.011)	(0.140)
Post2*biggest	0.099	0.020	0.001	−0.872	0.206	0.013	−0.036
	(0.147)	(0.017)	(0.098)	(0.871)	(0.385)	(0.026)	(0.184)
	719	721	721	139	203	203	203

Notes: Each panel is a separate regression. Each column presents a regression of column heading on the variables listed in each panel. The interest rate is the interest rate on credit used at date t (granted at date t-1). The dummy "post" is equal to 1 for year 1999 and 2000 (limit granted in years 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. 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 "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. Standard errors (corrected for clustering at the sector level) are in parentheses below the coefficient.

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 firms' activities are constrained by their limited access to credit.

5.3. *Impact on revenues, costs, and profits*

Table 7 present evidence on revenues, costs, and profits.

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 and the coefficient of the "POST" variable (the other coefficients are available upon request). Interestingly, the coefficient of the POST variable is small in absolute value and insignificant in all specifications and for all dependent variables: this means that, over the period, there was no change in the trend of the rate of growth of the small firms (which remained constant about 15% a year over the entire period). All the results are thus driven by changes occuring to the medium and large firms. While this was not an identification requirement, this is reassuring, as difference in differences estimates are easier to interpret in cases where the main effects are small. Unless the effects of the reform on small firms and the economy-wide trend cancelled out, this also suggests that the small firms were relatively unaffected by the reform in either direction, which is consistent with the fact that they experienced only a small decrease in loan increase in 1998, and no increase in 2000.[23]

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.

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 market credit. We do not have reliable data on market credit, but we have a possible proxy, 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 non bank 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). 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.

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.

23. We have also computed the increase in sales for firms with fixed assets above 30 million rupees in the PROWESS data base. We find that, during this period, a *decline* in the rate of growth for these firms—from 12% to 7%. Using these firms as an indication of what the trend would have been in our sample would thus lead to an even larger difference in difference.

TABLE 7

Effect of the reforms on sales, sales to loan ratios, and profits (OLS regressions)

	Dependent variables				
	Log(sales)_{t+1}-log(sales)_t			Log(cost)_{t+1}	Log(profit)_{t+1}
	Complete sample	Sample without substitution	$\text{log(sales/loans)}_{t+1}$ $\text{log(sales/loans)}_t$	-log(cost)_t	-log(profit)_t
	(1)	(2)	(3)	(4)	(5)
A. 1996–1999					
1. Sample with changes in limit					
Post	0.013	0.000	0.124	0.035	0.063
	(0.085)	(0.095)	(0.101)	(0.101)	(0.145)
Post*big	0.194	0.168	−0.126	0.187	0.538
	(0.106)	(0.118)	(0.094)	(0.097)	(0.281)
	152	136	152	151	141
2. Sample without change in limit					
Post	0.012	0.023	0.031	0.022	−0.316
	(0.052)	(0.045)	(0.048)	(0.049)	(0.177)
Post*big	0.007	0.022	0.016	0.004	0.280
	(0.074)	(0.081)	(0.065)	(0.064)	(0.473)
	301	285	301	301	250
3. Whole sample					
Post*big	0.071	0.071	−0.032	0.067	0.316
	(0.068)	(0.069)	(0.065)	(0.054)	(0.368)
	453	421	453	452	391
B. 1998–2001					
1 Sample with changes in limit					
Post2	−0.041	−0.041	−0.039	−0.072	0.030
	(0.088)	(0.087)	(0.121)	(0.088)	(0.263)
Post2*biggest	−0.403	−0.387	0.143	−0.374	−0.923
	(0.207)	(0.196)	(0.206)	(0.279)	(0.639)
	168	150	169	168	151
2. Sample without change in limit					
Post2	−0.012	−0.044	−0.033	−0.047	0.042
	(0.043)	(0.043)	(0.039)	(0.049)	(0.124)
Post2*biggest	−0.092	−0.045	−0.101	−0.047	0.170
	(0.108)	(0.128)	(0.088)	(0.086)	(0.56)
	401	380	401	399	321
3. Whole sample					
Post*big	−0.143	−0.113	−0.023	−0.100	−0.253
	(0.111)	(0.134)	(0.153)	(0.093)	(0.496)
	569	530	570	567	472

Notes: Each panel is a separate regression. Each column presents a regression of column heading on the variables listed in each panel. 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. 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. Standard errors (corrected for clustering at the sector level) are in parentheses below the coefficient. In addition to coefficient displayed, the regressions in panels A1-A3 include a dummy for big. In addition to coefficient displayed, the regressions in panels B1-B3 include a dummy for biggest.

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 (5). 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). The $POST2$ coefficient is negative but small for the small firm (−0.041).[24]

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 presents the results of estimating equation (9) 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 C, 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 0.44 and 0.83), and the test does not reject the hypothesis that they are equal. This result makes it very implausible that the estimated coefficients 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 were affected by the expansion, but not the contraction, behaved like the affected firms in the expansion, but like 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. They are not concentrated among firms that have fully substituted bank credit for market credit.

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

24. In the larger firms in the PROWESS data, we similarly find a decline of 4% in the rate of growth of revenues between the two periods: the rate of growth was 7% during the period 1999–2000 and 3% during the period 2001–2002.

TABLE 8

Effect of the reform on sales and cost and overidentification tests

	Dependent variables		
	$\text{Log(sales)}_t\text{-log(sales)}_{t-1}$		Log(costs)_t
	Complete sample	Sample without substitution	-log(cost)_{t-1}
	(1)	(2)	(3)
Post*biggest (γ_{3y})	0.238	0.235	0.205
	(0.153)	(0.162)	(0.151)
Post*medium (γ_{4y})	0.182	0.146	0.183
	(0.121)	(0.134)	(0.109)
Post2*biggest (γ_{5y})	−0.421	−0.400	−0.384
	(0.197)	(0.186)	(0.279)
Post2*med (γ_{6y})	−0.091	−0.095	−0.072
	(0.113)	(0.115)	(0.112)
	215	193	215
Ratio 1: γ_{3y}/γ_{3kb}	0.676	0.666	0.583
Ratio 2: γ_{4y}/γ_{4kb}	0.825	0.662	0.829
Ratio 3: γ_{5y}/γ_{5kb}	0.725	0.689	0.660
Ratio 4: γ_{6y}/γ_{6kb}	0.535	0.561	0.424
Test ratio 1=ratio2	0.06	0.00	0.17
(*p* value)	(0.81)	(0.99)	(0.68)
Test ratio 1=ratio2=ratio3	0.07	0.00	0.22
(*p* value)	(0.97)	(1.00)	(0.90)
Test ratio 1=ratio2=ratio3=ratio4	0.24	0.02	0.59
(*p* value)	(0.97)	(1.00)	(0.90)

Notes: All the regressions are estimated in the sample were the limit was changed. 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. 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. Standard errors (corrected for clustering at the sector level) are in parentheses below the coefficient. In addition the the coefficients displayed, the regressions include dummies for post, post2, medium, and biggest. 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.

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 5% of the loans to small firms, became NPA. 7% 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 Rs. 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, costs, and profits*

In this last sub-section, we present (in Tables 9 and 10) instrumental variable estimates of the effect of bank loans on sales, costs, and profit, in the whole sample and in selected subsample.

5.4.1. Main results. Column (1) of Table 9 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

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

	Dependent variables					
	2SLS	2SLS	2SLS	2SLS	2SLS	OLS
	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	0.20
-log(working capital limit_t-1)	(0.37)	(0.35)	(0.32)	(0.35)	(1.12)	(0.09)
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	0.23
-log(working capital limit_t-1)	(0.36)	(0.44)	(0.4)	(0.5)	(0.82)	(0.08)
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)	(0.94)	(1.49)	(0.996)	(0.99)	(3.26)	(0.19)
observations	141	151	192	166	598	598

Notes: Standard errors (corrected for clustering at the sector level and heteroskedasticity) in parentheses below the coefficients. Each panel and each column present the result for a separate regression. The regressions in column 1 control for the "post" and "big" dummy (defined as in previous tables) and use the interaction big*post as instrument. The regressions in column 2 control for the "post2" and "biggest" dummy (defined as in previous tables) and use the interaction biggest*post2 as instrument. 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 instruments. The regressions in column 6 control for the dummies "post", "post2", "big" and "biggest" (defined as before).

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).

Column (4) restricts the sample to firms that do not produce SSI products, since, as we mentionned, 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's predictions.

Panel B presents 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: in column (3), we find a coefficient of 0.70 on cost (in the sample with change in limits).

We can use these estimates to get a sense of the average increase in profit caused by every rupee in loan. The average loan for the treated firms (large and post reform) is Rs. 11,739,000 (about 45 days of sales). Therefore, using the coefficients in column (3), an increase of Rs. 100,000 in

TABLE 10
Which firms are most affected by the priority sector reforms?

	Sample cuts			
	By ownership		By ratio number of employees/investment in plant and machinery	
	Privately owned	Partnerships or Ltd companies	Below median	Above median or missing
	(1)	(2)	(3)	(4)
Panel A: First stage—Dependent variable: log(working capital limit_t)-log(working capital limit_t-1)				
Big* post (sample with change in limit)	0.40	0.090	0.65	0.15
	(0.13)	(0.06)	(0.3)	(0.12)
Panel B: Reduced form—Dependent variable: log(salest+1)-log(salest)				
B.1 sample with change in limits				
big*post	0.32	0.029	0.73	0.16
	(0.13)	(0.22)	(0.35)	(0.12)
B.2 sample without change in limit				
big*post	−0.078	0.155	−0.18	−0.09
	(0.13)	(0.12)	(0.26)	(0.06)
B.3 overall sample				
big*post	0.070	0.084	0.58	0.00
	(0.097)	(0.087)	(0.38)	(0.05)
Panel C: IV—Effect of loans on profit (estimated in the sample with change in limit)				
log(working capital limit_t)	0.79	0.478	1.13	1.19
-log(working capital limit_t-1)	(0.39)	(3.81)	(0.31)	(1.31)
-log(working capital limit_t-1)				

the loan corresponds to an increase in Rs. 829,000 in sales, and Rs. 739,000 increase in costs. This implies an Rs. 89,500 increase in profit for the average firm, after repaying interest.[25]

In panel C, we present the direct IV estimate of loans on log(profit), despite the fact that these regressions suffer from the sample selection induced by the omission of the firms with negative profits. The estimates vary between 1.79 and a little over 2.00. Taking 2.00 (the estimate that uses both periods) as the estimate of the effect of the log increase in loan on log increase in profit, at the mean profit (which is Rs. 5,069,000 for the average large firm in the post period), this would correspond to an increase in profit of Rs. 85,900 after repaying interest, which is very similar to what we found using cost and sales as the dependent variables.[26]

Thus, using the sales and cost estimate, a dollar of extra loans increases firm profits net of interest by 89.5%. Since the interest rate on priority sector loans at this time was 16%, the implied increase in profits before interest was 105.5%.

5.4.2 Which firms are particularly responsive to an inflow of credit?

In Table 10, we investigate whether firm characteristics that make them more likely to be credit constrained are associated with higher elasticity of profits with respect to loans. There is only a limited number

25. One alternative would be to use the average for all firms in the sample. The average loan (averaging across years and firms) is Rs. 8,680,000. 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.

26. Using the whole sample average and the same coefficient we find Rs 84,000, again similar to both this estimate and the one previous calculated.

of variables available in the data set, but two are particularly interesting. The first one is the form of ownership: one would expect private firms to be more credit constrained than partnerships and limited companies, because they have no investors to draw on (other than the sole proprietor). The second variable is the ratio of employment to plant and machinery: credit that can pay for wages (like bank credit) tends to be more difficult to obtain than trade credit. Firms that have fewer employees compared to their capital stock may be that way because they are short of credit that can be used to pay wages and therefore have the most scope to expand in the short run.

Table 10 confirms both presumptions. The interaction $BIG*POST$ is significant in the loan equation only for private firms, and only for firms with the ratio of employee over investment in plant and machinery below the median (this is consistent with our model, where loan officers prefer to give extra credit to firms that have the most scope to use it). Correspondingly, the coefficient of $BIG*POST$ in the sales equation is only significant for those firms.

6. QUANTIFYING AND INTERPRETING THE EFFECTS: PRODUCTION FUNCTION ESTIMATES

We estimated in the previous section that the marginal effect of bank loans on profit before interest is 105.5%. In other words, if there were a perfectly elastic supply of market credit available to the firm at any rate below 105.5%, it would expand till the marginal rupee of loans generates an increase in profits net of interest equal to the interest rate. Therefore either the market rate is below 105.5% and these firms are unable to borrow as much as they want and are therefore credit constrained, or the market rate at which they can borrow as much as they want is more than 105.5%. We argued earlier that the market interest rate is more plausibly in the 30% to 60% range. This suggests that these firms are likely to be credit constrained.

6.1. *Estimating the marginal product*

The marginal effect of a rupee of bank loan is not directly a production function parameter. The marginal product of capital is more useful to link these results to the macro growth literature—for example, to get at the broader question of misallocation. It turns out, however, that under some additional assumptions, the marginal effect of an additional rupee of loan on profits is very closely tied to the marginal product of capital. In the Supplementary Appendix to this article, we show that under the production function we introduced in Section 3, the marginal product of capital MPK, which we define to be the multiplier on the working capital constraints in the firm maximization problem, is equal to

$$\frac{dR}{dk}-\frac{dC}{dk}+r_b \tag{11}$$

Note that this is exactly the same as the marginal effect of a rupee of bank loans on profits before interest, which we computed above to be 105.5%.

Some caveats are in order: first, we take the multiplier on the working capital constraint to be our measure of the marginal product of capital. This is a natural choice, since it is what the owners of the firm would get if they save an extra rupee and put it into the firm. However there is no presumption that this money gets spent on capital goods. Second, the assumption that the entire working capital is always spent is quite strong. The loan in our particular setting is a credit line and the idea is that the firm draws on it as and when it needs money—it is an option rather than a fixed amount. The actual amount drawn on average over the year is likely to be less than the amount of the loan. This means that the physical marginal product of capital is probably higher than what we estimate.

6.2. *Marginal product estimates and misallocation of capital*

The immediate efficiency implications of the marginal product estimates have to do with the margin between investors and savers. Savers who put their money in bank accounts or government securities were earning 10% or less during this period. On the other hand, there were firms where that same money would have earned over 100%. This enormous wedge is clearly one measure of inefficiency.

It does not directly tell us anything about the efficiency of the allocation of capital across firms in our sample (or similar firms in the economy), however. In principle they could all have the same marginal product. Nevertheless, there are some indirect suggestions of misallocation. First recall the fact, noted above, that the OLS estimates of the effect of an extra rupee of loans on profits is smaller than the Instrumental Variables estimate. This suggests that the marginal product of capital in the average firm is less than what it is in the firms that benefit from the policy change (which is also what our model predicts). Second, in Banerjee and Duflo (2005) we argue that the inverse of the Incremental Capital Output Ratio (ICOR) provides an upper bound on the marginal product of capital for the economy as a whole, and that number for India is 22%. A somewhat more formal approach to that question is taken by Caselli and Feyrer (2007) who estimate the marginal product of capital for a number of economies based on fitting an aggregate production function to country level data. Their results do not cover India, but for Sri Lanka their highest estimate is 19%. Comparing either of these numbers with the firm level estimate of 105%, it is clear that capital is more productive in these firms than in the economy as a whole, which indicates misallocation.

6.3. *Returns to scale*

6.3.1. Estimating returns to scale. Under some additional assumptions this evidence can also inform us about returns to scale in production.

Assume that the production function takes the Leontief form (we choose units appropriately to get rid of the coefficients on individual goods):

$$f(I_1,I_2,...I_n)=A[min(I_1,I_2,...I_n)]^{\theta}. \tag{12}$$

Total production is therefore given by:

$$A\left(\frac{k}{p}\right)^{\theta}. \tag{13}$$

where,

$$p=p_1+..+p_m$$

is the cost of a unit of working capital (we continue to assume that I_1 to I_m are paid out of working capital). It is the parameter θ that we want to estimate.

Our empirical exercise identifies a parameter

$$\theta^*=\frac{dlogR}{dlogk_b}=\frac{dlogR}{dlogk}\frac{dlogk}{dlogk_b}=\theta\frac{k_b}{k} \tag{14}$$

Therefore,

$$\theta=\theta^*\frac{k}{k_b}, \tag{15}$$

which, since $k_b \leq k$, implies that θ^* is a lower bound on θ. In the Supplementary Appendix, we show that:

$$\frac{k_b}{k} = \frac{\gamma^* + C(r_m - r_b)}{1 + \frac{k_b}{C}(r_m - r_b)} \tag{16}$$

where γ^* is the elasticity of cost with respects to bank loans.

We observe $\frac{k_b}{C}$ in the data (it is 0.25 on average, both among treated firms and in the sample as a whole) and have an estimate of γ^* from table 9. The only parameter we do not observe is r_m. We argued above that a conservative range for r(m) is 30% to 60%. Within this range $\frac{k_b}{k}$ is maximized at 60%. We can therefore bound $\frac{k_b}{k}$. An upper bound, corresponding to a market interest rate of 60%, is 0.73. Using this bound we can get a lower bound on θ, which turns out to be 1.03.

6.3.2. Implications of local increasing returns for credit constraints. Given this lower bound on θ we have local increasing returns (at least based on our point estimates). We now argue that a natural implication is that at least some firms must be credit constrained.

Suppose not: then the firm can get as much k as it wants at the interest rate r. Its choice of k therefore maximizes:

$$Ak^{\theta} - rk. \tag{17}$$

But when $\theta > 1$, this maximization does not have an interior solution, since the second order condition cannot hold–the second derivative of this expression is:

$$A\theta(\theta - 1).k^{\theta - 2} > 0 \tag{18}$$

In other words, if the firm wants to borrow at all, it will want to borrow an unlimited amount.

More generally, even if the Cobb–Douglas assumption of a constant beta does not hold and the estimated beta is only locally correct, it remains true that the second-order condition for the above maximization cannot hold in the neighbourhood of the observed values of R and k, if $\theta > 1$. If the firm is credit constrained, on the other hand, increasing returns per se do not pose a challenge, as long as there are decreasing returns when k is fixed with respect to the inputs that can be freely varied ($I_{m+1} \ldots. I_n$). Hence the firm has to be credit constrained.

A few points are worth emphasizing. First, as we mentioned, marginal returns are estimated locally. We do not argue that the θ is above 1 everywhere and for every firm: ultimately, returns must be decreasing again. What we can say is that there are firms that are credit constrained, and they have locally increasing returns. Second, returns to scale are heterogenous across firms. Our experiment identifies the returns for the compliers in our experiment, *i.e.* those firms to who the bank was induced to lend because of the reform. Our model of the supply side of the bank suggests that this is likely to be a set of firms with higher returns than the firms to whom the bank officer typically lends to. Finally, while finding that $\theta > 1$ implies that at least some firms must be credit constrained, the converse is not true: it would of course be possible for firms to be credit constrained even with $\theta < 1$ for every firm.

7. CONCLUSION

The evidence presented in this article suggests that many relatively large firms in India were severely credit constrained during 1998–2002, and that there are many high-return investment opportunities that were not taken advantage of.

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. 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 interesting question is why, nevertheless, the firms did not invest much more, especially given the enormous profitability of additional investment.

One possible answer, as argued by Stein (2002), is that the inability to lend effectively to anyone but the largest 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 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, 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, makes his lending less effective, and may lead to evergreening but it covers the bank. An obvious social cost is that small firms have a hard time borrowing.

Forces that make lending difficult are thus not specific to India or to public lending. 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. This is not to say that some characteristics of developing economies are not important in understanding why lending to these firms is harder: in countries where it is harder to verify profits, or to enforce loan contracts, giving loan officers the right incentives in lending to firms is all the more difficult. This sheds light on results such as Rajan and Zingales (1998), who find that the growth of sectors that rely more on external capital in the U.S. is slower in countries that are not financially developed. Reforms that improve the functioning of the credit market (fast track tribunals, better record of property rights, enforcement of liens on property) would potentially have significant impact on total factor productivity and growth in developing economies.

Acknowledgments. We thank Tata Consulting Services for their help in understanding the Indian banking industry, Sankarnaranayan for his work collecting the data, Dean Yang Niki Klonaris and Madeline Duhon for excellent research assistance, and Robert Barro, Sugato Battacharya, Gary Becker, Shawn Cole, Ehanan Helpman, Sendhil Mullainathan, Kevin Murphy, Raghuram Rajan, Christopher Udry, and three anonymous referees 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 article.

Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

REFERENCES

ANGRIST, J. (1995), "Conditioning on the Probability of Selection to Control Selection Bias" (NBER Technical Working Paper, No. 181).

BANERJEE, A. and DUFLO, E. (2000), "Efficiency of Lending Operations and the Impact of Priority Sector Regulations" (MIMEO, MIT).

BANERJEE, A. and DUFLO, E. (2005), "Growth Theory through the Lens of Development", in *Economics Handbook of Economic Growth*, Vol. 1, Part A, (North Holland: Elsevier B.V.) 473–552.

BANERJEE, A., COLE, S. and DUFLO, E. (2008), "Are the Monitors Over-monitored: Evidence from Corruption, Vigilance, and Lending in Indian Banks" (MIMEO, MIT).

BANERJEE, A. and NEWMAN, A. (1993), "Occupational Choice and the Process of Development", *Journal of Political Economy*, **101** 274–298.

BERGER, A., MILLER, N., PETERSON, M., RAJAN, R. and STEIN, J. (2001), "Does Function Follow Organizational Form: Evidence from the Lending Practices of Large and Small Banks" (MIMEO, Harvard University).

BERNANKE, B. and GERTLER, M. (1989), "Agency Costs, Net Worth, and Business Fluctuations", *American Economic Review*, **79**, 14–31.

CASELLI, F. and FEYRER, J. (2007), "The Marginal Product of Capital", *The Quarterly Journal of Economics*, **122**, 535–568.

DE, S. and SINGH, M. (2011), "Credit Rationing in Informal Markets: The Case of Small Firms in India" (MIMEO, CAF, Indian School of Business, Hyderabad).

GALOR, O. and ZEIRA, J. (1993), "Income Distribution and Macroeconomics", *Review of Economics Studies*, **60**, 35–52.

HECKMAN, J. (1979), "Sample Selection Bias as a Specification Error", *Econometrica*, **42**, 670–693.

HECKMAN, J. and ROBB, R. (1986), "Alternative Methods for Solving the Problem of Selection Bias in Evaluating the Impact of Treatment on Outcomes", in Wained, H. (ed.) *Drawing Inferences from Self-Selected Samples*, (New York: Springer Verlag).

KIYOTAKI, N. and MOORE, J. H. (1997), "Credit Cycles", *Journal of Political Economy*, **105**, 211–248.

MCKENZIE, D., DE MEL, S. and WOODRUFF, C. (2008), "Returns to Capital: Results from a Randomized Experiment", *Quarterly Journal of Economics*, **123**, 1329–1372.

MCKENZIE, D. and WOODRUFF, C. (2008), "Experimental Evidence on Returns to Capital and Access to Finance in Mexico", *The World Bank Economic Review*, **22**, 457–482.

RAJAN, R. and ZINGALES, L. (1998), "Financial Dependence and Growth", (joint with R. Rajan), *The American Economic Review*, **88**, 559–586.

ROSENZWEIG, M. and WOLPIN, K. (1993), "Credit Market Constraints, Consumption Smoothing, and the Accumulation of Durable Production Assets in Low: income Countries: Investment in Bullocks in India", *Journal of Political Economy*, **101**, 223–244.

STEIN, J. (2002), "Information Production and Capital Allocation: Decentralized versus Hierarchical Firms", *Journal of Finance*, **LVII**, 1891–1921.