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Varouj Aivazian,[1] Dipak Mazumdar,[1] and Eric Santor[2]

[1]University of Toronto
Toronto, Ontario, Canada

[2]International Department
Bank of Canada
Ottawa, Ontario, Canada K1A 0G9
esantor@bankofcanada.ca

Contents

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Abstract

The authors examine the investment behaviour of a sample of small, credit-constrained firms in Sri Lanka. Using a unique panel-data set, they analyze and compare the activities of two groups of small firms distinguished by their different access to financing; one group consists of firms with heavily subsidized loans from the World Bank, and the other consists of firms without such subsidies. The use of program-evaluation techniques reveals that the relaxation of financing constraints did not affect economic efficiency for the group of firms that received subsidized capital.

JEL classification: G00, O16
Bank classification: Development economics

Résumé

Les auteurs examinent le comportement de l'investissement d'un échantillon de petites entreprises sri lankaises ayant un accès limité au crédit. À partir d'un ensemble unique de données longitudinales, ils analysent et comparent les activités de deux groupes de petites entreprises qui se distinguent par leur capacité d'accès au crédit. Le premier groupe se compose d'entreprises disposant de prêts subventionnés largement par la Banque mondiale, tandis que le second réunit des entreprises qui ne bénéficient pas de cette forme de subvention. Les méthodes employées pour évaluer le programme de prêt montrent que le relâchement des contraintes financières n'a pas eu d'incidence sur l'efficience économique du groupe d'entreprises bénéficiaires d'un financement subventionné.

Classification JEL : G00, O16
Classification de la Banque : Économie du développement

1. Introduction

Considerable research, both theoretical and empirical, explores the consequences of market imperfections in credit markets on firm-level performance.[1] In particular, small and medium-sized enterprises (SMEs) are believed to suffer disproportionately in their ability to access credit when financial markets are undeveloped, segmented, or subject to arbitrary credit-allocation mechanisms. In these environments, asymmetric information between lenders and borrowers affects the ability of firms to access credit and therefore hinders investment, ultimately affecting aggregate economic activity. Consequently, there is keen interest on the part of policy-makers to ensure that SMEs are able to access financing when faced with credit market imperfections.

Widespread acknowledgement of the impact of credit market imperfections on economic behaviour has led to significant efforts to correct cases of market failure. While broader financial market reforms have been implemented in many developing countries, these interventions tend to favour larger, publicly traded firms over the financing needs of SMEs. In light of this fact, the World Bank initiated Small and Medium Industries (SMI) loan programs designed specifically to address the financing needs of SMEs in a number of developing countries.[2] Despite the considerable resources devoted to these programs, there is little evidence of their effectiveness. We assess the impact of a World Bank SMI loan program on the behaviour of a sample of SMEs in Sri Lanka. Using a unique panel-data set, we analyze and compare the activities of two groups of SMEs distinguished by their different access to financing; one group consists of firms with subsidized loans from the World Bank, and the other group consists of firms without such subsidies. Particular attention is paid to addressing the potential biases that stem from self-selection in the loan program. We find that the World Bank SMI loan program led to a relaxation of credit constraints and higher levels of investment for firms that received the subsidies. There is little evidence, however, to suggest that the loan program had a positive impact on economic efficiency; it did not resolve the problem of capital market imperfections. Simple empirical tests of reduced-form profit and input demand functions derived from profit maximization show that the relaxation of financing constraints did not affect the absolute or relative economic efficiency of the group of firms that received loans from the SMI program. That is, SMI recipients did not have higher profits nor were they more allocatively efficient than non-recipient firms. The use of program-evaluation techniques to correct for self-selection and sample bias confirms these results.

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1. The seminal theoretical work is Stiglitz and Weiss (1981); Povel and Raith (2001) offer recent insights. Empirical work is also very extensive. See Allayannis and Mozumdar (2001), Gallego and Loayza (2000), Bigsten et al. (1999), Mairesse et al. (1999), Kaplan and Zingales (1997), Bond et al. (1997), Bond and Meghir (1994), and Fazzari, Hubbard, and Petersen (1988) for examples. Hubbard (1998) provides a fine survey of the major issues.
2. For instance, it is estimated that over $3.7 billion was lent to SMEs by national governments and international lending institutions between 1973 and 1989 (Webster 1989). Interestingly, there is little evidence of the effectiveness of these programs, and, in particular, of the SMI program.

This paper is organized as follows. Section 2 briefly describes the SMI loan program and the survey data. Section 3 describes key regressions used in assessing the impact of the loan program. Section 4 addresses the econometric issues of program evaluation and describes solutions. Section 5 provides descriptive statistics from the survey data, and section 6 presents results. Section 7 offers some conclusions.

2. SMI Loan Program and Survey

2.1 SMI loan program

The SMI loan program for Sri Lanka was developed to assist with the reform of its economic system, initiated after 1977. The program's success clearly depended on the success of more fundamental reforms in the economy, but it was felt that the program could help develop the institutions and mechanisms that would be required by a more autonomous, market-oriented economy. Manufacturing industry in the late 1970s contributed about 17 per cent of GDP in Sri Lanka; private firms contributed about half of manufacturing value added, two-thirds of which came from unregistered small firms. Private sector firms were a fraction of the size of public sector companies. The average number of employees of the 1400 largest private companies was small, 42,[3] while the typical unregistered firm employed family labour supplemented by at most two or three hired workers. It was expected that the SMI loan program would help the growth of entrepreneurship in a sector that also supported more labour-intensive growth, through the provision of credit for capital expenditures. At the same time, it was designed to direct credit away from a banking sector that largely carried out government directives, and to encourage the development of lending institutions that could provide project-based, rather than strictly collateral-based, funding to private sector businesses that were largely ignored by the banking sector. In this respect, the program aimed to increase economic efficiency through project-based lending.

At first impression, the program might seem contradictory to the broader aim of promoting decision-making by private sector institutions. A targeted loan program could be accused of carrying on the *dirigiste* tradition. But the justification of the program was that it sought to mitigate the consequences of the old system, under which the large state-owned enterprises had set up vertically integrated operating modes that discriminated against the development of SMEs. Furthermore, the state-controlled banking system, lending at negative interest rates to loss-making public enterprises and their subsidiaries, had few funds left for the development of private firms.

3. Sri Lanka: 1980 Annual Survey of Establishments.

While targeting assistance based on the size of firms, albeit with a size limitation that increased by more than 50 per cent in real terms over the period of the four credits, the program was designed to help in the ultimate elimination of the need for such lines of credit.

Between 1979 and 1991, the World Bank provided Sri Lanka with $110 million in financing for SMIs through a series of four credit programs: SMI I, SMI II, SMI III, and SMI IV.[4] The World Bank did not lend the funds directly; rather, the program operated through an autonomous body known as the National Development Board (NDB). The NDB provided capital to participating financial institutions (PFIs), which then retailed the loanable funds to firms.[5] Under the SMI program, firms would approach the participating credit institutions and apply for a loan.[6] Upon approval, the firm's application would be forwarded to the NDB, which in turn would approve the loan and consequently refinance the PFI. Initially, 80 per cent of the loans were refinanced—but this figure fell to 75 per cent and 70 per cent, respectively—by SMI III and IV. The eligibility requirements for the SMI program restricted access to those firms from the following sectors: manufacturing, mining, construction, agriculture industries, fish processing, industrial services, horticulture, commercial transport, and animal husbandry. With respect to firm size, there was no minimum scale, but firms could not have more than a set level of fixed assets, and loan size was limited accordingly. The allowed debt-to-equity ratio was 75:25 and firms had to be prepared to contribute 25 per cent of the cost of the project themselves. There were collateral requirements and interest rates were initially fixed, but they later moved to adjustable nominal rates to ensure positive real interest rates.

The lending mechanism of the SME program was designed to ensure that market forces determined how the PFIs distributed loans. On the lending side, the NDB was wholesaling funds (not providing grants), and only for a portion of the loan; retail lenders had to provide between 20 and 25 per cent of the funds themselves, thus ensuring that loan losses were borne by the retail lender directly. This meant that retailing banks would engage in the necessary screening and monitoring activity so that only firms with good projects and strong balance sheets would receive funding. On the borrowing side, firms were required to provide 25 per cent of the project's cost from their own equity, and collateral was necessary. Thus, firms could not view the program as a cheap source of capital with no expected repayment—rather, the program was like any other

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4. While the program did not directly subsidize firms, in that the interest rates of SMI loans were below market rates, there was an implicit subsidy. The funds from the World Bank would not have been available to Sri Lanka, and thus represent a subsidy.
5. The PFIs could be existing state-owned banks, but new or developing commercial banks were encouraged to be PFIs.
6. The majority of loans were granted by the Bank of Ceylon, the People's Bank, and the DFCC. However, by SMI III, more than 10 banks were participating in the scheme.

formal financial-sector lending arrangement. Although it would appear that subsidies were not provided, an implicit subsidy did exist, since the program sought to mimic market outcomes. Given the extent of credit market failure (and credit rationing) in Sri Lanka, if funding from the SME program was not available, firms would have had to seek credit from more expensive alternative sources.

2.2 Survey data

The data used in this paper were generated by a World Bank "Small and Medium Industry Impact Evaluation (SMIIE)" survey conducted in 1996. The survey covered 300 firms ranging in size from one to over 600 employees. The sample was split evenly between those firms that received an SMI loan (treatment group) and those that did not (control group). The treatment group received their loans from SMI II and III.[7] The treatment group was chosen to reflect the proportion of firms by the value of total loans by location, sector, and firm size. The control group was chosen on the basis of similarity to the treatment group in terms of observable characteristics: size, location, and industry type. The survey data include a wide range of firm level characteristics: sales, fixed assets, employment, finances, and technology. There is also considerable information on owner characteristics and legal organization. In each case, firms provided detailed statistics for the year 1995 and recall data from 1992 and 1985.[8]

3. SMI Program Assessment

To assess the impact of the SMI program, an evaluation must be made of how firms in the treatment group changed their behaviour relative to the control group. Specifically, three research questions are of interest: (i) did the SMI loan program relax the credit constraints of financially constrained firms, (ii) did the mix of inputs change, and (iii) what was the impact of changes in input use on economic efficiency as measured by profitability? Each question will be addressed in turn.

3.1 Financing constraints and investment

Empirical modelling of firms' investment behaviour in the presence of financing constraints has followed three alternatives in the literature: a standard accelerator approach, Tobin's q method, and a Euler equation approach (Hubbard 1998). Despite the apparent heterogeneity of

7. SMI I and IV firms are excluded from the sample.
8. The survey initially sought to collect data for each year between 1985 and 1995, but many firms were unable to provide such detailed information.

econometric investment models, there is considerable similarity among them, considering their actual implementation in functional form. We follow a standard accelerator approach to assess the impact of the SMI program on investment behaviour. Bigsten et al. (1999) suggest the following functional form of the accelerator model:

$$I/K_{t-1} = \alpha_0 + \alpha_1 \Delta V/K_{t-1} + \alpha_2 (\Pi/K)_{t-1} + \alpha_3 X_t + \varepsilon_t, \tag{1}$$

where I is investment, K is capital, ΔV is change in value added, Π is profits, and the Xs are firm level characteristics (see Appendix A for the model).[9] Before we describe the testable implications of (1), a brief discussion of the difficulties in estimating investment equations is warranted.

In economies characterized by significant transaction and information costs (such as Sri Lanka), it becomes difficult to distinguish empirically between investments caused by changes in capital market constraints and investments caused by changes in future growth opportunities. Thus, an increase in a firm’s current cash flow or internal net worth may reduce moral hazard and adverse-selection problems in the capital market, increasing the supply of financing to the firm and, as a result, investment. Alternatively, the increase in net worth may just be due to improvements in investment opportunities. In fact, empirical observations of a positive association between net worth (cash flow) and investment may simply reflect the positive relationship between investment and expected future profitability, and are fully consistent with the neoclassical investment model (and the model developed by Modigliani and Miller 1958) with perfect capital markets.

Improvements in growth opportunities shift to the right the marginal efficiency of investment schedule, thereby increasing investment, but the attendant increase in profitability (or internal net worth) serves to reduce transaction and information costs, which increases the supply of financing to the firm, again increasing investment. Thus, it becomes difficult to decompose investment into demand-induced and supply-induced components when there are capital market imperfections.

In a cross-section of firms, the impact of profitability or net worth on the supply of credit should be more pronounced for firms that face higher transaction and information costs in financial markets; we can call such firms financially “constrained,” while those facing low information costs can be called financially “unconstrained.” Ideally, empirical research should identify firm characteristics that affect the transaction and information costs that face a firm in financial markets that are at the same time independent of demand (or marginal efficiency of investment) parameters. Cash flow (or net worth) does not satisfy this criterion. We must therefore find

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9. To account fully for firm level heterogeneity, (1) is also estimated using first differences.

additional firm attributes in order to distinguish, in a cross-sectional study, between firms facing significant transaction and information costs in capital markets (constrained firms) and those for whom the disparity between the costs of internal and external funds is not significant (unconstrained firms). If this can be done, it may help to distinguish empirically between investment caused by changes in financial market constraints and that caused by changes in future growth opportunities. Possible firm attributes relating to information costs in capital markets include the firm's age, industry type, size, and past financing record. Using such attributes in conjunction with cash flow (or net worth) may distinguish investment expenditures caused by changes in expected growth opportunities from investment induced by changes in transaction and information costs facing the firm in the capital market.

As discussed above, in a world with asymmetric information in credit markets, the testable implications of (1) are that the coefficients for changes in value added and past profits should be positive, since they can proxy for future investment opportunities. In a world characterized by perfect capital markets, perfect information, and no uncertainty, the demand for funds would depend entirely upon the perfect forecasts of future investment opportunities, and therefore estimating (1) should produce only significant relationships with respect to the adjustment cost of capital. That is, the demand for credit would not depend upon current or past measures of profitability, cash flow, or net worth, nor would other firm characteristics that signal firm quality or creditworthiness matter. Under imperfect but symmetric information, value added and profitability may be important, since they could signal the quality of future investment opportunities. If information is asymmetric, then these variables could also signal a firm's creditworthiness. That is, value added, profits, and the *X*s could capture the firm's ability to access credit, which directly affects the investment process.[10] Lagged profits could be positively related to future investment if firms rely on internal funds and the investment process is "lumpy." Likewise, the coefficient for current and past profitability should signal to the market the firm's creditworthiness. Lastly, if firm characteristics such as age, location, industry type, or owner ability are seen as potential sources of information to the lender, then these variables should also matter. That is, if some firm characteristics can mitigate information asymmetries, then firms that possess those characteristics should have easier access to credit and therefore higher levels of investment.

We can use the accelerator framework to assess whether firms in the treatment group behaved differently given that each was able to access credit. Estimating (1) separately for each group of

10. For a good discussion of the empirical problems encountered in estimating determinants of investment when there are capital market imperfections, see Hubbard (1998).

firms, respectively, can provide evidence of whether the SMI program was successful in relaxing the financial constraints of the firms that did receive loans. Specifically, the estimation of (1) should provide different estimates of the coefficients for change in value added and lagged cash flow for the two groups of firms.[11] Firms that received loans from the SMI program should exhibit lower coefficient values for changes in value added and lagged profits, relative to the control group.[12] This conjecture is based upon the notion that those firms that were able to access the program were less financially “constrained” than those that did not.

3.2 Input use and profitability

Most empirical studies of financial constraints and firm level behaviour focus on the first criterion of the program evaluation: the effect of financial constraints on investment. The inability of the accelerator framework to address the more fundamental question of efficiency, however, is a shortcoming of much of the current literature.[13] There are numerous ways in which to measure economic efficiency. Most studies assess improvements in efficiency by measuring changes in total factor productivity (TFP), which requires estimation of a stochastic production function frontier model. Estimation of TFP by these methods, however, relies heavily on functional form and on strong assumptions about the nature of the production function.[14] An alternative to measuring the impact of the SMI program is the dual approach of profit maximization and/or cost minimization. Specifically, the effect of the SMI program on input use can be viewed through its influence on the shadow cost of capital for recipient firms. That is, under profit maximization, firms set the level of capital such that the marginal product of capital equals its marginal shadow price:

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11. There may be differences between those firms that access the SMI program and those that do not, and these sources of self-selection would necessarily bias the results of estimating (1). That is, we would expect that firm level characteristics that are correlated with access to SMI credit would also be correlated to investment behaviour, thereby producing biased results (Hubbard 1998). However, these sources of self-selection, at least in terms of observables, can be accommodated within a standard switching regression framework (Maddala 1988).
12. Fazzari, Hubbard, and Peterson (1988) argue that financially constrained firms should have higher sensitivity to cash flow than financially unconstrained firms. However, their results have been challenged by Kaplan and Zingales (1997). Replies and comments from both sets of authors (Fazzari, Hubbard, and Peterson 2000; Kaplan and Zingales 2000) offer further explanations regarding the use of the accelerator model to assess the sensitivity of investment to cash flow. Their discussion centres on the question of how to distinguish the difference between financially “constrained” and “unconstrained” firms. Often, the distinction is arbitrary and leads to spurious results.
13. For instance, Gallego and Loayaza (2000) use the accelerator framework to explore the impact of financial liberalization and macroeconomic policy on firm level behaviour in Chile. They find strong evidence that financial market liberalization leads to a reduction of a firm’s reliance on internal funds for investment and higher rates of investment growth.
14. See Greene (2002) for an extensive discussion of the issues in estimating TFP using frontier methods.

$$\rho \frac{\partial V^j}{\partial X_i^j} = k_i^j c_i^j, \qquad k_i^j \geq 0, \tag{2}$$

where V is output, X is a vector of inputs, k_i^j indexes the decision rule, c_i^j is the price of the jth input, and ρ is the price of output (Yotopoulos and Lau 1971). In a world of imperfect markets, firms will face differential shadow input prices, depending on the value of k. Access to the SMI program implies that the treatment group should face a lower shadow price of capital than the control group ($k_{SMI}^K < k_{non\text{-}SMI}^K$). If capital markets are characterized by asymmetric information, then the shadow cost of capital would be greater than its market price, which implies that, for all firms, $k^K > 1$. If firms are able to access the program (and its subsidized capital), then this treatment group should experience a higher rate of fixed-asset growth than the control group, given the lower shadow cost of capital for these firms. Likewise, if financial capital is fungible within the firm, it could also be the case that firms in the treatment group adjust the use of other inputs in the production process. That is, treatment firms will follow a different behavioural rule than the control group with respect to the use of variable inputs. Specifically, how firms adjust the quantity of labour will also be explored.

If firms are able to adjust their input use given the relaxation of credit constraints, then this should translate directly into higher levels of profitability. Yotopoulos and Lau (1971) describe a simple test of this proposition. Under Cobb-Douglas[15] assumptions, two simple reduced-form equations for firm profits and variable input (labour) demand can be derived:

$$\ln \Pi = \alpha_0 + \alpha_1 SMI + \alpha_2 \ln W + \beta_1 R + \beta_2 \ln K + \sum_{n=1}^{5} \delta_n ISIC + \sum_{m=1}^{4} \delta_m LOC, \tag{3}$$

$$\frac{-WL}{\Pi} = \alpha_3 SMI + \alpha_4 NSMI, \tag{4}$$

where Π is actual profits, *SMI/NSMI* indicates that the firm received/did not receive an *SMI* loan, W is the average wage rate, R is the rental rate of capital, K is the capital stock, *ISIC* is industry, *LOC* is the firm's location, and L is the quantity of labour used by the firm (see Appendix B for the derivation). The expected signs for the inputs are straightforward: higher wages and rental rates for capital should lead to lower profits. The estimation of (3) and (4) can be used to test three hypotheses. The first is the equal relative economic efficiency of SMI and non-SMI firms: $H_0{:}\alpha_1 = 0$. Rejection of H_0 implies that SMI firms differ in economic efficiency from non-SMI firms. If $\alpha_1 > 0$, then the SMI program resulted in higher levels of profitability for the firms that received the treatment. The second hypothesis is the equal relative price efficiency of SMI and non-SMI firms: $H_1{:}\alpha_3 = \alpha_4$. If one rejects H_1, then firms differ in terms of price efficiency

15. Interestingly, one cannot reject constant returns to scale (CRS) when one estimates a production function for this set of firms.

(that is, k_i^L differs). The third hypothesis is absolute price efficiency: $H_3{:}\alpha_2 = \alpha_3$ and $H_4{:}\alpha_2 = \alpha_4$. Rejection of H_3 or H_4 implies that the firms are not absolutely price-efficient.[16]

4. Econometric Issues

The means by which firms accessed the SMI program naturally raises questions regarding the potential biases that stem from endogeneity. That is, how does placement into the treatment group bias the estimates from the models described above? Selection can originate from two features of the SMI program: who applies and who is accepted. In terms of the application process, the self-selection problem can be two-sided. If firms that want to access the program do so because they have favourable investment opportunities that require access to external funds, then the selection bias will be positive. However, if firms apply to the program because they are unable to access credit from the formal financial sector, since they have unfavourable projects, then the selection bias could be negative. In either case, conditional on application, the screening process would imply that accepted firms are positively selected, because banks would choose only the best credit risks from the pool of applicant firms. For both the estimation of the profit function and the accelerator model, the direction of the expected bias will be discussed below and, more importantly, solutions proposed to account for these potential biases.

4.1 Evaluating program effects

The problem of endogeneity described above makes it difficult to evaluate the impact of program participation for the accelerator and profit function equations. In both cases, estimation by ordinary least squares (OLS) will produce only unbiased estimates of program effects if program participation is exogenous. That is, firms that access the program would have to be identical to firms that did not, other than the fact that the program is exogenously available to the recipients. It is evident that participation is not exogenous in the case of the SMI program, because only those firms that have good projects and sufficient collateral would be able to access the program. Nevertheless, OLS can still produce unbiased estimates of program effects if the characteristics that determine participation are observable.[17] It is most likely, however, that program participation is a function of firm characteristics that are typically unobservable. Firm

16. An empirical stochastic production frontier model could also be estimated to determine whether the treatment group had higher levels of efficiency. Preliminary estimates were consistent with the profit function estimation results—the treatment group did not become more efficient (in fact, they were less so).

17. This depends on whether one has enough “controls” to account for the determinants of program participation.

characteristics such as managerial competence, social ties, and/or the availability of good projects may determine which firms gain access to the program. Consequently, it is necessary to determine how endogenous program participation will bias the results, and how this bias can be accounted for in the estimation procedure.

The effects of endogenous program participation with respect to the estimation of the accelerator model are clear: firm characteristics that are correlated with access to the SMI program will also be correlated with investment behaviour (Hubbard 1998). Given that selection is positive, the coefficients of value added and profitability will be biased upwards for the treatment group when estimating (1) by OLS. In order to account for self-selection, switching regression techniques can be used to account for the potential endogeneity of SMI program participation (Appendix C). Similarly, self-selection affects the assessment of program effects on profitability. From (3), the Yotopoulos and Lau (1973) framework estimates the average impact of program participation as:

$$\alpha_1 = E(\Pi_1 \mid SMI = 1) - E(\Pi_0 \mid SMI = 0), \tag{5}$$

where Π_1 are the profits of the firms in the state when they receive the program and Π_0 when they do not; *SMI* = 1 indicates program eligibility, 0 otherwise; and the "return" from accessing credit is equivalent to α_1. If only the best firms access the program, the estimate of the "return" to an SMI loan will be biased upwards. That is, the positive effect on profits by unobservable attributes, such as better project quality and managerial competence, will be captured by the coefficient for program participation, leading to an incorrect assessment of program impacts. Following Greene (2000), one can estimate a "treatment-effects" model to account for endogeneous program participation (see Appendix D for the full model).

4.2 Matching methods

The non-experimental techniques described above rely on the fact that the treatment and control groups share common supports for the distribution of firm characteristics. That is, firms in the treatment and control groups are comparable across a range of characteristics, such as firm size, age, and profitability. Heckman et al. (1996), however, show that if the supports of the distribution are not similar, then implementation of standard non-experimental techniques may produce biased estimates of program impacts, because OLS estimates of program effects assume that the impact of the program can be captured entirely by the single index *X*'ß, which may not be related to the firm's propensity to participate in the program. Furthermore, OLS implies a common program effect across all firms. If there were substantial differences between the control and treatment groups, then the estimates of the program would be biased, because the treatment group could respond differently to the treatment. For example, the treatment group may consist of young, growth-oriented firms located in urban areas, and the control group may consist of older, established firms that operate in rural areas; the effect of access to subsidized credit may therefore differ substantially between firms, and these differences are not resolved by the standard treatment-effects models and switching regression techniques described above. For example, the treatment-effects model estimates the difference between participants and non-participants as:

$$\alpha = E(\Pi_1 | SMI = 1) - E(\Pi_0 | SMI = 0). \tag{6}$$

To accurately assess the impact of the program, it is necessary to calculate the effect of the treatment (the SMI program) on the treated (those who accessed the program):

$$\alpha_T = E(\Pi_1 | SMI = 1) - E(\Pi_0 | SMI = 1). \tag{7}$$

That is, it is necessary to observe the outcomes of the firms that received the treatment and compare them with a control group of firms that are otherwise identical, except for the fact that they did not have access to the program (but are eligible to take up the treatment and would do so, given its availability). Unfortunately, the second term of the right-hand side of (7) does not exist in the data, since it is not observed. A solution is to create $E(\Pi_0 | SMI = 1)$ through the implementation of a randomized experiment: firms would apply to the SMI program and a proportion of the accepted firms would be randomly denied access. This would create a true-control group sample analogue that could be used to determine the difference between the outcomes of those firms that accessed the program and the outcomes if the program had not existed. While randomized experiments have been successfully implemented in certain settings, techniques of this sort are not readily accepted by development practitioners for evaluating the impact of credit programs.[18]

A solution to this evaluation problem is to create the counterfactual $E(\Pi_0 | SMI = 1)$ by matching treatment and control firms along observable characteristics. For every firm in the treatment group, a firm in the control group can be found that is identical in every respect except for the availability of an SMI loan. For instance, if the treatment group consisted of young, urban-based, and highly profitable firms in the machinery business, one would like to find similarly profitable firms, from the same industrial type and location, in the control group. Typically, there are many dimensions along which to match firms, and if the dimensionality of the match becomes very large, it becomes difficult if not impossible to find matches. Fortunately, there is a solution to this problem, known as “matching methods.” Rosenbaum and Rubin (1983) show that, instead of matching along X, one can match along $P(X)$, the probability that the firm participated in the

18. A large literature has evolved around the use of randomized experiments to evaluate job training programs. See Heckman, Lalonde, and Smith (1999) for a complete survey.

treatment group, and still estimate consistent and unbiased estimates of the effect of program participation on the treated.

Several methods of matching can be considered: “without replacement,” “with replacement,” and nearest-neighbour techniques (Dehejia and Wahba 1998). The standard technique, matching without replacement, is conducted as follows. First, a logit and/or probit regression is run to generate a scalar measure of the probability of loan-program participation, $P(X)$. Then, the data are sorted according to the estimate of $P(X)$, from highest to lowest. Each firm in the treatment group is matched to a control firm, in descending order, and this technique is repeated until each treatment firm is matched with a firm from the control group. This technique can also be done “with replacement,” in which case $P(X)$ is estimated and the data randomly ordered. Then each firm in the treatment group is matched with the firm from the control group that is its nearest neighbour. In this way, different treatment firms may have the same control-group analogue. Lastly, each treatment firm is matched with those control firms within some radius δ of $P(X)$ and the weighted average is taken of the characteristics of those firms in the radius.[19]

The ability of matching-method techniques to construct a suitable control-group sample analogue depends on the following crucial assumption:

$$E(\Pi_0 \mid P(X), SMI = 1) = E(\Pi_0 \mid P(X), SMI = 0). \tag{8}$$

Conditional on the propensity score, the outcome in the non-participation state is independent of participation. That is, if the Xs capture the participation decision, then the control group will have the same characteristics as the treatment group, and thus the outcome in the non-participation state will be the same in the two groups, conditional on the propensity score. For this result to hold, Smith and Todd (2001) suggest that the data must possess three criteria: (i) the data for the control and treatment group must come from the same source; (ii) the outcomes must occur in the same geographic region; and (iii) the data must be “sufficiently rich” that (8) holds. The limitations of matching methods are a function of these conditions. In particular, the matching technique relies heavily on the third criterion, the availability of a rich set of conditioning variables. The ability to create suitable counterfactuals to the treatment group depends on the ability to match along observable characteristics. If the process of selection into the participation and non-participation states is a function of unobservables that are not captured by the observable data, then the control group may not be properly specified. In this sense, the limitation of using the propensity score as a measure of “comparability” is determined by the availability of sufficient

19. The size of δ is determined by the researcher. Likewise, one can use local linear regression or kernel estimator methods to generate the control-group analogue within the range of δ.

conditioning variables. If the decision to participate in the program is poorly measured, the treatment and control groups will be poorly matched, and any inferences on the effect of the "treatment on the treated" will be biased in an undetermined manner. In this way, matching may actually accentuate the biases caused by selection on unobservables (Smith and Todd 2001).

Conversely, if the observable data sufficiently determine participation, then the benefits of matching are large. By reducing the dimensionality of the match to a univariate measure, it is possible to generate for each firm in the treatment group its sample analogue in the control group. Matching methods allow for a straightforward assessment (along $P(X)$) to determine whether the supports of the distribution of the control-group characteristics differ from those of the treatment group. Firms in the control group that fall outside the support of the treatment group are discarded from the sample. Likewise, treatment-group firms that have no comparable control-group analogues are removed from the assessment procedure, since no counterfactual exists. In this way, the most directly comparable sample analogue control group is used to assess the impacts of program participation. Section 5 describes the results of applying matching methods to the sample of treatment and control groups in addition to the standard descriptive statistics and regression results.

5. Data

5.1 SMI loan size, term, interest rates, and usage

The average loan size was 425,000 rupees for SMI II, rising slightly to 456,000 rupees for SMI III. The interest rate was 18 per cent for SMI II and slightly lower for SMI III.[20] The loan term remained unchanged at 10 years for both SMI II and SMI III. The majority of SMI loans were used to purchase equipment or to accumulate fixed assets. Across all firm sizes, roughly 60 per cent of all SMI loans were used for equipment purchases, 15 per cent were used for buildings and renovations, and 16 per cent were used for materials. These statistics are consistent with those of firms from the control group that borrowed from other sources during the same period. In terms of access requirements, the collateral type did not vary substantially across year or treatment/control-group status. In all cases, roughly 85 per cent of all firms used equipment, buildings, or land as the collateral for the loan, and, consequently, there does not appear to be any systematic difference in collateral requirements across firms that received loans and/or credit from other sources.[21]

20. The survey data reveal that the interest rates of the SMI loan program are comparable with market interest rates over this period, which implies a lack of subsidization at the retail level.

21. This would suggest that selection was based on market characteristics and that therefore selection would be positive.

5.2 Firm characteristics: treatment and control groups

The sample is split into two groups of firms: those that received SMI loans (the treatment group) and those that did not (the control group). Table 1 provides descriptive statistics for the treatment and control group firms for 1985. The treatment group is smaller than the control group in terms of sales, fixed assets, equipment, and employment at the mean, and the firms are much younger (columns 1 and 2). These differences are significant, as highlighted by the *t*-statistics and standardized differences. The treatment group, however, has a higher initial debt-to-equity ratio, operating margin, value added to capital, and profits to capital, although the differences in the last two measures are not significant. With respect to firm characteristics, SMI loan recipients tend to be sole proprietorships located outside Colombo, and are also more likely to have Sinhalese owners, but have similar education levels as the owners of firms in the control group (Table 1).

5.3 How did input use change?

A first glance at the data would suggest that the SMI loan program was having the desired effect. The impact of the SMI program appears to have influenced the mean growth rates of firms (Table 2). SMI recipients had faster growth rates in fixed assets, equipment, and total employment (although not significantly different).[22] Interestingly, employment growth was faster than asset growth over this period, and therefore fixed assets per worker fell for both groups. Equipment growth per worker was positive for the treatment group and significantly higher than for the control group: this reflects the emphasis placed on equipment investment by the SMI program. The impact of the SMI program on growth in value added, however, was negligible relative to the control group. Interestingly, while average nominal wages rose over the period, due to high inflation, real wages fell in a similar fashion for both types of firms.

The data reveal the large degree of "covariate imbalance" that exists in the control group. For key firm-level measures, such as fixed assets, sales, and employment, the standardized differences are well outside acceptable ranges (Table 1).[23] To correct this imbalance, "with replacement" matching is implemented. First, probit results are generated to determine which characteristics are correlated with receiving an SMI loan (Table 3). The results reveal that profit rates do not figure

22. The results may be affected if the accounting rules employed by the survey tend to underestimate the value of existing capital relative to new capital. Fortunately, the survey calculated the value of fixed assets that account for the asset's replacement value, as opposed to simple accounting measures that assume a constant rate of depreciation.
23. Rosenbaum and Rubin (1983) suggest that standardized differences greater than 10 indicate "covariate imbalance." In other words, the distribution of characteristics between the treatment and control group differs significantly.

prominently in the determination of who received credit. Firm age and size, however, were significant but negative. This result contradicts the underlying notion that lenders use firm age and size as a signal for quality. It could simply be the case that the survey did not match treatment and control groups effectively across these dimensions. Past borrowing is correlated with credit access in the sample. The positive sign on the linear term and the negative sign on the squared term suggest that being moderately leveraged is a signal of credit-worthiness, while highly leveraged firms are less likely to access credit.[24] The remaining control variables, such as location, industry classification, and ownership structure, are not significant. Previous credit history (as proxied by the debt-to-equity ratio) is a significant predictor of access. Interestingly, the squared term is negative, which suggests that firms with high debt levels were not able to access the program. As noted above, the efficacy of the matching results depends heavily on the observable conditioning variables. If credit access depended on unobservable characteristics, then the estimate of the propensity score may be biased. For instance, credit access could depend on future investment opportunities, managerial ability, or the degree of social connections between firm owners and bank managers. If these characteristics are unaccounted for in the observables, then condition (8) will not hold.[25] While much of the heterogeneity can be captured by the rich set of conditioning variables in the data, proxies for future investment opportunities remain elusive.[26] Thus, while the matching-method approach resolves many of the problems associated with program evaluation, it is not a complete solution. Consequently, the matching-method results must be viewed as an alternative means of verifying the results generated from standard non-experimental techniques.

Given values of the propensity scores, treatment firms are matched to the nearest neighbour with replacement. Tables 1 and 2 show that propensity score matching dramatically reduces the degree of covariate imbalance. In most cases, the standardized differences are under 10 per cent (similarly, the per cent reduction in bias is also very large). Matching methods reveal differences in SMI program impacts. Whereas use of the initial control group revealed that the treatment group grew faster in terms of fixed assets and other outcomes, this is not the case for the matched sample. For instance, sales and employment actually grew more slowly for the treatment group.

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24. This specification is consistent with evidence that suggests that firms with some debt, and therefore a credit history, can provide more information to lenders than firms with no credit history. At high levels of debt, however, the informational benefits of previous levels of debt are swamped by the implied risk of potential insolvency.

25. The owner's education, father's and mother's occupation, province of birth, and other characteristics were included in the selection equation to account for some of these typically "unobservable" characteristics, but the matching results were not sensitive to further inclusion of these conditioning variables.

26. The inability to control for future investment opportunities is a recurring problem for most studies that use the accelerator framework. Tobin's q is often used to account for this problem, but it too presents empirical problems. Unfortunately, the SMI survey does not include publicly listed firms and thus measures of q are not available.

Equipment and equipment per worker growth was still faster, however, even when matching is conducted. Importantly, while treatment firms experienced slower employment growth, they had higher average nominal-wage growth (but real wages fell over the period), although these differences are not statistically significant.[27] Nevertheless, these results show that total wage and employment growth occurred at the same time as equipment growth for the treatment firms.

6. Regression Results

6.1 Did the SMI program relax financial constraints?

The accelerator model is estimated separately for the treatment and control groups, to test the effectiveness of the SMI program in relaxing credit constraints. If the SMI program was successful in relaxing credit constraints, then the accelerator model should produce different estimates of the parameters for the respective groups. Table 4 shows the results for the switching regression using an unmatched control group and treatment group for fixed assets and equipment investment, respectively. First, a probit regression is estimated to determine program participation. The investment equation (1) is estimated by OLS with inclusion of the mills ratio from the first stage (Maddala 1988).[28] This technique naturally raises questions regarding how program participation is identified: if the characteristics that determine access to credit also determine investment, then identification of the selection correction will depend entirely on functional form.[29] Naturally, it would be ideal to find firm characteristics that would predict program access but not investment; however, it is not clear ex ante which variables satisfy this requirement. Fortunately, the survey's structure provides possible identifying variables, notably the firm's age, the owner's ethnicity, and previous credit history characteristics, which are not predictors of firm profitability.[30] In the first stage, firm characteristics, including owner ethnicity, debt-to-equity, debt-to-equity squared, and age are used to identify participation in the program.[31] Table 4 shows the results of the second-stage estimation by OLS. Controlling for firm-level heterogeneity, the control-group firms' investment rate responded to changes in value added and past profits more significantly than treatment firms' investment rates (compare columns 3 and 4, or columns 7 and 8). This suggests that the SMI program relaxed the credit constraints of the

27. Inflation averaged over 10 per cent per year.
28. This methodology follows Nabi (1989), Fazzari, Hubbard, and Petersen (1988), and Cleary (1999).
29. In this case, the non-linearity of the probit regression is used to identify selection.
30. The leverage criteria were not utilized as part of the matching mechanism of the survey and thus one can use those characteristics to identify SMI program access. Owner ethnicity appears to be a positive determinant of access to credit; this would be consistent with the fact that Sri Lanka's Sinhalese majority was being served by a Sinhalese-dominated banking system.
31. This section follows the methodology laid out by Greene (2000).

treatment-group firms, because their investment decisions are less dependent on current or past levels of firm performance. The regressions are repeated in Table 5 using only those firms with positive cash flow: the results remain essentially unchanged.[32] Again, the SMI program appears to have reduced the financing constraints for the treatment group.

To check the results further, a matched control group is used in place of the unmatched survey data (Table 6). For fixed-asset and equipment growth, the matched sample control group reports lower levels of sensitivity to changes in cash flow, but similar sensitivity to lagged profits, than the original control group. Interestingly, the differences in sensitivity to cash flow between the treatment and control group for fixed assets are negligible, but still significantly different for equipment investment. The exercise is repeated using first differences (Table 7). The matched control group exhibits much higher cash-flow sensitivity than the treatment group.[33]

The SMI program reduced the credit constraints facing firms with respect to equipment investment expenditures, according to standard regression techniques. Matching methods reduced the differences between the control group and the treatment group; however, equipment investment was still less sensitive to changes in cash flow and lagged profits for the treatment group. Although this evidence would suggest, at first blush, that the SMI program achieved its goal of relaxing the credit constraints of the SMEs it served, it cannot be claimed that the program was a "success." A closer look at the impact of the program using alternative performance measures, such as economic efficiency, is required.

6.2 Did the SMI program lead to higher profitability?

The impact of the SMI program on profitability is not apparent from the descriptive statistics: the treatment group did not experience faster growth in value added, or profits, over the sample period. Likewise, there was no statistically significant difference in the growth rates of value added to capital, profits to capital, or operating margins, even when matching methods were employed (results not shown). To confirm these results, the profit function and input-demand

32. The exercise is repeated for the data between 1992 and 1995, and for first differences, with qualitatively similar results (which are not shown). The accelerator model is also estimated for the pooled sample, but the results are similar: the control group shows a higher sensitivity to cash flow to investment. Likewise, the first-differences model included lagged values of the dependent variable, instrumented with levels of the explanatory variables (Arellano and Bond 1991). However, this did not change the results.

33. As noted earlier, there is considerable disagreement regarding the usefulness of cash-flow sensitivities in determining the presence of financial constraints. Evaluation of the SMI loan program provides a natural experiment to test the "cash-flow sensitivity" model, in that the splitting criterion between "constrained"and "unconstrained" firms is program participation.

function (3) and (4) are estimated jointly using seemingly unrelated regressions (Table 8). Examining column 4 for the pooled sample, the results suggest that the firms that pay higher wages are the firms that are more profitable, despite the prediction of the model that profits should be decreasing in the wage rate.[34] It is a well-known empirical fact, however, that more profitable firms pay higher wages, which suggests that there are firm-level unobservables that are correlated to wages and profitability.[35] Similarly, the coefficient on the interest rate should be negative, but the results show a statistically insignificant relationship. After controlling for location and industry dummies, the results indicate that SMI recipient firms were not relatively economically efficient when compared with non-SMI firms: the coefficient for SMI recipient dummy α_1 does not differ significantly from zero (in fact, the sign is incorrect, because we would expect it to be positive), and thus the hypothesis of equal economic efficiency cannot be rejected. Firms that applied for and received the treatment did not have different levels of profitability than firms that did not, which suggests that the relaxation of credit constraints for a group of credit-constrained firms did not lead directly to an increase in economic efficiency. Estimation of the labour-demand function reveals that the hypothesis that the treatment and control groups are equally relatively price efficient ($\alpha_3 = \alpha_4$) can be rejected, although it is not rejected for the latest year in the sample.[36] SMI firms had lower labour-demand growth (as normalized by value added) than non-SMI firms, which suggests that the treatment group was following a different behavioural rule $(k^L_{SMI} > k^L_{non-SMI})$ than the control group. This result is consistent with the notion that, although SMI firms increased the labour input, they did less so than non-SMI firms. This may be due to the fact that SMI firms invested more in equipment than non-SMI firms, which indicates that they were substituting away from labour in favour of capital. Setting firms to be equally relatively price efficient $(\alpha_3 = \alpha_4)$, both types of firms fail the test of absolute price efficiency, since the hypotheses $(\alpha_2 = \alpha_3)$ and $(\alpha_2 = \alpha_4)$ are strongly rejected. This last finding is not surprising, however, given the positive coefficient for the wage variable in the profit function. Consequently, no strong conclusions should be drawn regarding the absolute price efficiency of SMI vs. non-SMI firms. Before we provide a deeper interpretation of the lack of impact of the SMI program on firm profitability, it is important to determine whether a failure to control for the potential endogeneity of program participation is biasing the results.

34. This result directly contradicts the typical results found by Yotopoulos and Lau (1973).

35. One potential explanation for this result is an efficiency-wage story: firms that have higher profits pay workers higher wages, who then work harder, leading to higher profitability. An alternative hypothesis is that firms are not strictly competitive but earn rents, which are then shared with employees.

36. The survey data disaggregates labour by family and hired-in status, as well as by production, administrative, and seasonal categories. Consequently, the labour input is well measured and does not suffer from typical problems associated with underreporting by family-run firms.

6.3 First differences, selectivity correction, and matching methods

The profit function is estimated using first differences to remove firm-level fixed effects, but the results are similar (Table 9): SMI firms are not more profitable than non-SMI firms.[37] These results, however, do not account for the problem of self-selection and should be treated with caution.[38] A treatment-effects model is estimated following Greene (2000) using a standard two-step procedure. First, a probit regression is estimated to determine program participation, similar to the results in equation (3).[39] The participation hazard is included in the second-stage OLS regression of the profit function. Table 10 shows the treatment effect estimates. To account for the notion that the benefits from investment may take time to be realized, the profit functions are estimated for both 1992 and 1995. In either case, the coefficient for program participation is insignificant. Using this result and the value of the hazard, Table 10 reports the impact of the program: $\alpha = E(\Pi_i | SMI_i = 1) - E(\Pi_i | SMI_i = 0)$. Interestingly, the treatment-effects model reveals that the program did not have a positive impact on profitability for the treatment firms. The estimation of the profit function is repeated using the matched data for the control group (Table 11). Again, the impact of the program on profitability is not significant.

6.4 Program impacts

The failure of the SMI program to positively affect the efficiency (as defined by profitability) of the treatment group is clear: loan program recipients did not outperform the control group, when self-selection is accounted for or matching methods used. The first issue is whether the program allocated credit efficiently. One of the underlying assumptions of the SMI program (and this analysis) is that the efficient allocation of credit is equivalent to the efficient allocation of resources. That is, by adhering to market forces, the program hoped to direct credit to those firms that could best use the funds. It could be the case, however, that the criteria by which credit was allocated did not achieve this result. Rather, PFIs allocated SMI loans to firms that were most able to overcome the problems of asymmetric information (i.e., they had the highest likelihood of repayment as viewed by the bank). This group of firms, while having characteristics that made them safe credit risks, was not necessarily more efficient than a set of comparable firms. The results confirm this intuition: firms that received credit from the program did experience a relaxation of credit constraints and faster equipment (and equipment/worker) growth than firms in the control group. This is compatible with the goals and design of the program, which are to

37. A fixed-effects model was also estimated, with similar results.
38. If selection is positive, then the coefficient for the SMI dummy could be biased upwards.
39. Identification of program participation follows the same logic as the switching regression.

enhance capital equipment expenditure by credit-constrained firms. However, this did not translate into better performance. Despite considerably faster equipment/worker growth, firms in the treatment group did not become more profitable.[40]

This raises the question: why did the firms that received the program fail to become more profitable, despite their ability to access subsidized credit? Simple economic theory suggests that access to subsidized credit should lead to higher profits, primarily in two ways: first, capital is cheaper; and second, a substitution away from labour, which is made feasible by cheaper (and more accessible) capital.[41] The effect of the cheaper capital alone should lead to higher profitability, for a given level of capital. However, the ability to access cheaper capital may have led to overinvestment: firms may have viewed the program as a "one-off" government program, and thus deliberately overinvested in equipment for fear that they would miss the opportunity in the future. In this sense, firms can be seen as investing for precautionary reasons, not to increase profitability immediately, but in the future. At the same time, the availability of cheaper capital should have led to a substitution away from labour. But the empirical analysis suggests that the increase in equipment investment by SMI recipients was not offset by a sufficiently large relative decline in their labour input. Two factors contributed to this effect. First, despite higher capital investment, SMI firms' employment growth was similar to that of the control group for the 1985–92 and 1992–95 periods (Table 12). Second, although firms in the treatment and control groups exhibited similar negative real wage growth from 1985 to 1992, treatment firms experienced larger increases in average real wages from 1992 onwards. This real wage increase occurred even though treatment and control groups experienced similar growth in other outcomes during this period (such as value added, fixed asset, and employment growth; results not shown). The real wage increase itself cannot be explained by changes in the composition of labour within firms: family labour constituted only a small proportion of overall labour for both types of firms, and fell over time (Table 12). Furthermore, although full recall data are not available, records from 1995 reveal that treatment and control groups had similar labour composition with respect to the mix of administrative and production workers. Two possible explanations for the higher real wages paid by the treatment group, especially after 1992, can be attributed to the introduction of new

40. An alternative explanation for the lack of positive impact on profitability may stem from the survivor bias of the sample. It could be the case that by accessing the program, the treatment firms were able to survive longer than otherwise possible, since they were receiving an implicit subsidy (through access to the SMI credit facility.) If they had lacked such access, failure would be a more likely outcome. Consequently, the treatment group is a negatively self-selected group that would show lower levels of profitability than the control group (which also survived despite not accessing the program). Fortunately, the matching-method technique overcomes this problem by ensuring that the control group is an appropriate sample analogue.

41. Greater access to capital could also allow firms to produce more output (a supply effect).

equipment technology as facilitated by the SMI loan program.[42] First, as firms used their new access to credit to purchase new equipment, they needed to add new, more highly skilled labourers. One would expect that the treatment firms would have simultaneously reduced their low-skill labour component: but labour market rigidities may have prevented firms from reducing the use of their existing low-skill workforce (or, in the case of some of the firms in this sample, family workers). Thus, while SMI firms were increasing their capital input substantially (relative to the control group), they were also increasing their labour input, even though they did not experience increases in sales relative to the control group. This suggests that, for the treatment group, labour and capital are complements, as opposed to substitutes, in the production process. While profit-maximizing behaviour would be expected to lead to a reduction in labour demand for the recipient firms, the opposite occurred: firms had to increase their labour input, given its complementarity to capital.[43] Second, workers may have been able to seize the rents of the credit subsidy. The increased profitability induced by access to subsidized capital was not kept by the firm: rather, workers were able to capture a proportionately large share of this benefit, due in part to labour market rigidities in the Sri Lankan economy that benefit workers. Consequently, the SMI program did not enhance the economic efficiency of the treatment firms.

6.5 Indirect effects

The evaluation exercise conducted above estimated the returns to program participation for those firms that were eligible for and took up the treatment. Underlying the evaluation technique was the assumption that the program did not affect the outcomes of those firms that did not participate. An economy-wide intervention, however, such as the SMI program, may have had an effect on firms that were eligible to take up the program and did not (the control group). In a Walrasian world, such effects need not be considered, since the impact of pecuniary externalities is irrelevant, and any change in factor prices fully reveals the social cost of that input. But, in a second-best world, any intervention that causes a distortion in factor markets could have negative externalities. For instance, given that the SMI program was associated with higher average wages, this could potentially spill over to other firms by affecting the wages paid to workers in similar industries. In this case, the potential benefits of the program, although inconclusive for the treatment group, may actually undermine the economic efficiency for the untreated group, since they are forced to pay higher wages. Similarly, the SMI program, while purporting to reduce

42. This result is striking, given that SMI firms are, on average, smaller than non-SMI firms, and it is well-known that larger firms pay higher wages.

43. This also suggests that, when financial capital is fungible within the firm, access to credit implies that firms that use capital as “working capital” to finance the purchase of variable inputs.

credit constraints, may have led to a reduction in credit to firms that were not eligible to access the program. That is, firms that could access formal credit previously, but were unable to access the SMI program because of non-market program eligibility requirements, might have faced credit rationing as PFIs directed their own capital to SMI lending (since PFIs had incentives to direct their own capital to the program, given the lower capital requirement).[44] Such indirect effects place an upward bias on the estimated treatment effect. Because of the negative or insignificant effect of the treatment, however, the results are robust to this bias. In fact, it is possible the evaluation method has underestimated the SMI loan program's negative consequences.

7. Conclusions

This paper has examined the investment behaviour of a sample of small, credit-constrained firms in Sri Lanka. Using a unique panel-data set, we analyzed and compared the activities of two groups of small firms distinguished by their different access to financing; one group consisted of firms with heavily subsidized loans from the World Bank, and the other consisted of firms without such subsidies. We have found that the World Bank loan program did lead to higher levels of investment for financially constrained firms, but the impact of the loan program on economic efficiency is inconclusive. The use of program-evaluation techniques revealed that the relaxation of financing constraints did not affect economic efficiency for the group of firms that received subsidized capital. Although the program allowed firms to invest in more capital goods, firms were not able to reduce their labour component enough to achieve allocative efficiency. That is, SMI recipient firms did substitute from capital to labour, as they hired more workers and generally paid them higher wages. There are two potential explanations for this phenomena: (i) that labour and capital are complements in the production function, and (ii) that workers were able to capture rents from firms that accessed the SMI loans. Any improvements in profitability inevitably went to workers. Lastly, we cannot discount the possibility that the SMI program was seen as a "one-off" program, and that firms simply took advantage of the program while it existed. The lack of positive impacts suggests that the implementation of credit schemes, while relaxing financial constraints, may not necessarily lead to higher levels of economic efficiency.

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44. On the contrary, it could also be the case that the SMI program reduced credit constraints for the economy as a whole, and thus the control group benefited from lower financing constraints. However, we believe this effect to be small. Similarly, one should also consider the positive impact of higher wages on the economy through an aggregate demand effect. Given that the SMI program was quite small relative to the entire economy, however, this income effect is very limited, and there is no reason to believe that the control group would enjoy benefits in excess of the treatment group.

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Table 1: Firm Characteristics (levels)

Variables	Mean (treatment group)	Mean (control group)	Std. diff.*	Two-sample *T*-stat	Mean (matched-control group)	Std. diff.*	Two-sample *T*-stat
Fixed assets	7582	17849	33.80	2.53	6378	12.21	1.03
Equipment	3131	6875	29.40	2.53	3205	1.10	0.09
Employment	30	47	38.50	3.33	40.3	16.21	1.38
Sales	12272	32866	46.67	2.60	8800	25.93	2.19
Value added	4984	11117	32.20	2.50	5205	2.31	0.19
Debt/equity	0.62	0.48	7.85	0.70	0.71	5.24	0.40
Age	6.05	12.41	41.00	3.55	6.35	0.22	2.58
Wages	1519	3259	38.19	3.30	2218	21.46	1.82
Avg Wage	183	212	25.12	2.09	172	9.99	0.83
Value added capital	1.78	1.61	2.83	0.24	1.65	1.98	0.17
Profits/capital	0.56	0.38	9.50	0.78	0.43	6.16	0.53
Profits/sales	0.10	0.07	26.07	2.09	0.08	24.95	2.01
Location							
Colombo	0.47	0.59	24.24	2.04	0.46	2.00	0.14
Gampaha	0.25	0.20	12.45	1.07	0.16	22.31	1.79
Kurungala	0.13	0.09	12.50	1.08	0.20	19.23	1.80
Ownership type							
Sole prop.	0.62	0.38	48.97	4.36	0.63	2.04	0.12
Partnership	0.16	0.22	15.16	1.37	0.14	5.62	0.62
Priv. lim. liab.	0.20	0.35	33.95	2.82	0.21	2.50	0.09
Ethnicity							
Sinhalese	0.74	0.44	61.58	5.55	0.77	9.29	0.68

Note: The standardized difference in per cent is the absolute value of the mean difference as a percentage of the average standard deviation: $100(x_1 - x_2)/[(s_1^2 + s_2^2)/2]^{/2}$,where, for each variable, x_1 and x_2 are the sample means in the treated group and the control group, and S_1^2 and S_2^2 are the corresponding sample variances.

Table 2: Outcomes (growth rates 1985–92)

Variables	Mean (treatment)	Mean (control group)	Two-sample *T*-stat	Mean (matched-control group)	Two-sample *T*-stat
Fixed assets	5.64	4.07	0.78	5.63	0.17
Equipment	10.31	4.53	1.61	4.23	1.90
Employment	6.52	5.45	1.04	8.85	1.15
Fixed asset/worker	–0.50	–1.01	0.30	–1.13	0.03
Equipment/worker	3.83	–1.00	1.69	–4.20	2.53
Sales	4.68	6.38	0.71	8.55	1.93
Value added	5.87	4.50	0.61	8.56	1.11
Total wages	4.48	3.46	0.64	6.65	0.70
Total wages/ employment	0.62	0.56	0.14	0.42	0.60
Average real wage	–1.81	–1.89	0.04	–2.20	0.12

Note: The standardized difference in per cent is the absolute value of the mean difference as a percentage of the average standard deviation: $100(x_1 - x_2)/[(s_1^2 + s_2^2)/2]^{/2}$, where, for each variable x_1 and x_2 are the sample means in the treated group and the control group, and S_1^2 and S_2^2 are the corresponding sample variances.

Table 3: Probit Results

Dependant variable: Firm received an SMI loan			
Variables	(1)	(2)	(3)
Profit rates $_{(t-1)}$	0.0321	0.0380	0.0535
	(0.0471)	(0.0481)	(0.0533)
Ln (size) $_{(t-1)}$	–0.3536*	–0.3676*	–0.2346*
	(0.0747)	(0.0756)	(0.0921)
Age	–0.0176*	–0.0160*	–0.0187*
	(0.0055)	(0.0056)	(0.0061)
(Borrowing/Capital) $_{(t-1)}$		0.3063*	0.2945*
		(0.1237)	(0.1315)
(Borrowing/Capital)^2 $_{(t-1)}$		–0.0280*	–0.0250**
		(0.0134)	(0.0140)
[Galle]			
Colombo			–0.4063
			(0.3533)
Gampaha			–0.2228
			(0.3787)
Kurunegala			–0.1380
			(0.4120)
[Limited liability]			
Sole proprietorship			0.2856
			(0.2294)
Partnership			–0.1910
			(0.2618)
Sinhalese			0.4727*
			(0.1941)
Constant	1.3084*	1.2494*	0.8320
	(0.2490)	(0.2517)	(0.5266)
LR Chi^2 (14)	45.77	53.37	71.61
Pseudo R^2	0.12	0.14	0.19
N	278	278	278

Notes: *, ** indicate significance at the 5 per cent and 10 per cent levels, respectively. Robust standard errors are reported.

Table 4: Accelerator Model (unmatched data)

Dependant variable: 1985–92	**Investment/(Capital) $_{(t-1)}$**				**Equipment investment/(Capital) $_{(t-1)}$**			
Variables	non-SMI (1)	SMI (2)	non-SMI (3)	SMI (4)	non-SMI (5)	SMI (6)	non-SMI (7)	SMI (8)
Δ Value added/capital $_{(t–1)}$	0.2513* (0.0707)	0.1965** (0.1054)	0.2183* (0.0633)	0.1472 (0.1134)	0.1714* (0.0473)	0.0531 (0.0477)	0.1679* (0.0492)	0.0216 (0.0853)
Profit rates $_{(t–1)}$	0.1472* (0.0303)	0.0649* (0.0267)	0.1534* (0.0304)	0.0711* (0.0268)	0.0883* (0.0157)	0.0251* (0.0123)	0.0891* (0.0156)	0.0282* (0.0127)
Ln (size) $_{(t–1)}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls								
Mills ration	No	No	Yes	Yes	No	No	Yes	Yes
Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ownership	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	–0.0371 (0.0550)	0.1143 (0.1072)	0.4022 (0.2903)	–0.0003 (0.1506)	–0.0083 (0.0330)	0.0106 (0.0423)	0.0385 (0.1755)	–0.0481 (0.0911)
F	4.61	2.85	4.47	2.77	4.33	2.24	4.05	2.34
R^2	0.4477	0.2003	0.4628	0.2077	0.4548	0.1414	0.4554	0.1423

Notes: *, ** indicate significance at the 5 per cent and 10 per cent levels, respectively. Robust standard errors are reported. N = 278.

Table 5: Accelerator Model for Firms with Positive Cash Flow (unmatched data)

Dependant variable: 1985–92	**Investment/ (Capital) $_{(t\text{-}1)}$**				**Equipment investment/ (Capital) $_{(t\text{-}1)}$**			
Variables	non-SMI (1)	SMI (2)	non-SMI (3)	SMI (4)	non-SMI (5)	SMI (6)	non-SMI (7)	SMI (8)
Δ Value added/Capital $_{(t-1)}$	0.2275* (0.0853)	0.1341 (0.1126)	0.2258* (0.0854)	0.0855 (0.1200)	0.1706* (0.0552)	0.0697 (0.0589)	0.1743* (0.0548)	0.0430 (0.0675)
Profit rates $_{(t-1)}$	0.1498* (0.0287)	0.0717* (0.0287)	0.1517* (0.0294)	0.0772* (0.0284)	0.0917* (0.0164)	0.0256** (0.0130)	0.0874* (0.0158)	0.0286* (0.0134)
Ln size $_{(t-1)}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls								
Mills ratio	No	No	Yes	Yes	No	No	Yes	Yes
Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ownership	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.0542 (0.0607)	0.0960 (0.1290)	0.0550 (0.3086)	-0.0157 (0.1565	-0.0078 (0.0337)	0.0192 (0.0508)	-0.2492 (0.2228)	-0.0422 (0.0960)
F	4.04	2.15	3.77	2.20	5.00	1.88	5.17	2.09
R^2	0.4409	0.1535	0.4413	0.1618	0.4587	0.1534	0.4658	0.1617

Notes: *, ** indicate significance at the 5 per cent and 10 per cent levels, respectively. Robust standard errors are reported. N = 278.

Table 6: Accelerator Model (matched data)

Dependent variable: 1985–92	Investment/ $(\text{Capital})_{(t-1)}$		Equipment investment/ $(\text{Capital})_{(t-1)}$	
Variables	non-SMI (1)	SMI (2)	non-SMI (3)	SMI (4)
Δ Value added/Capital $_{(t-1)}$	0.1605**	0.1965	0.2180*	0.0531
	(0.0849)	(0.1324)	(0.0615)	(0.0589)
Profit rates $_{(t-1)}$	0.1058**	0.0649**	0.0514**	0.0251
	(0.0552)	(0.0353)	(0.0281)	(0.0178)
Ln (size) $_{(t-1)}$	-0.0232	-0.0056	-0.0034	-0.0021
	(0.0150)	(0.0219)	(0.0068)	(0.0120)
Age	0.0009	0.0006	0.0000	-0.0009
	(0.0010)	(0.0015)	(0.0006)	(0.0006)
Controls				
Mills ratio	No	No	No	No
Location	Yes	Yes	Yes	Yes
Ownership	Yes	Yes	Yes	Yes
Ethnicity	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes
Constant	0.0343	0.1143	-0.0933*	0.0106
	(0.0940)	(0.1072)	(0.0412)	(0.0423)
F	32.85	2.85	14.26	2.24
R^2	0.6581	0.2003	0.5645	0.1414

Notes: *, ** indicate significance at the 5 per cent and 10 per cent levels, respectively. Bootstrapped standard errors are reported.

Table 7: Accelerator Model for First Differences (matched data)

Dependent variable	Investment/ (Capital)$_{(t-1)}$		Equipment investment/ (Capital)$_{(t-1)}$	
Variables	non-SMI (1)	SMI (2)	non-SMI (3)	SMI (4)
Δ Value added/ Capital $_{(t-1)}$	0.2291* (0.0787)	0.1290 (0.1382)	0.1731* (0.0697)	0.0505 (0.0933)
Profit rates $_{(t-1)}$	0.1551* (0.0623)	0.0950** (0.0507)	0.0837* (0.0365)	0.0571* (0.0189)
Ln (size) $_{(t-1)}$	–0.1256** (0.0812)	–0.0056 (0.0426)	–0.0659* (0.0334)	–0.0301 (0.0332)
Constant	–0.0195 (0.0417)	–0.0774* (0.0232)	–0.0216** (0.0110)	–0.0216** (0.0130)
F	16.81	4.65	28.75	6.80
R^2	0.2479	0.1268	0.3703	0.1313

Notes: *, ** indicate significance at the 5 per cent and 10 per cent levels, respectively. Bootstrapped standard errors are reported.

Table 8: Estimation of Profit Function (using seemingly unrelated regressions)

Dependent variable: value added				
Variables	1985 (1)	1992 (2)	1995 (3)	Pooled (4)
SMI recipient (α_1)	–0.1679 (0.1328)	–0.0588 (0.1217)	–0.1431 (0.1187)	–0.1653* (0.0829)
Ln wage rate (α_2)	0.8624* (0.1073)	0.9293* (0.1311)	0.9275* (0.1342)	0.9161* (0.0700)
Interest rate (β_1)	0.0277 (0.0240)	0.0150 (0.0168)	–0.0205 (0.0159)	0.0047 (0.0100)
Ln fixed assets (β_2)	0.4612* (0.0450)	0.5764* (0.0432)	0.6206* (0.0418)	0.5405* (0.0240)
Location dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Constant	–4.5871* (1.1275)	–5.8170* (1.3028)	–5.5161* (1.3669)	–5.4145* (0.7114)
N	278	278	278	834
SMI recipient (α_3)	–0.4213* (0.0321)	–0.4310* (0.0529)	–0.4607* (0.1097)	–0.4438* (0.0500)
Non-SMI recipient (α_4)	–0.5424* (0.0325)	–0.5973* (0.0522)	–0.6703* (0.1051)	–0.5596* (0.0350)
$H_0\colon\alpha_1 = 0$	not rejected	not rejected	not rejected	rejected
$H_1\colon\alpha_3 = \alpha_4$	rejected	rejected	not rejected	rejected
$H_2\colon\alpha_2 = \alpha_4$	rejected	rejected	rejected	rejected
$H_3\colon\alpha_2 = \alpha_4$	rejected	rejected	rejected	rejected

Note: * indicates significance at the 5 per cent level. Robust standard errors are reported. N = 278.

Table 9: Profit Function Estimation for First Differences

Dependent variable: change in value added				
	OLS	OLS	OLS	Fixed effects
Variables	(1)	(2)	(3)	(4)
Ln fixed assets	0.3406*	0.3290*	0.3291*	0.3170*
	(0.0448)	(0.0460)	(0.0461)	(0.0392)
Interest rate	0.0053	0.0030	0.0032	0.0030
	(0.0074)	(0.0075)	(0.0075)	(0.0064)
Ln wage rate	0.5140*	0.5605*	0.5616*	0.5634*
	(0.0869)	(0.0930)	(0.0930)	(0.0634)
SMI recipient	No	No	–0.0291	–0.0357
			(0.0472)	(0.0510)
Year dummy	No	Yes	Yes	Yes
Constant	0.1357*	0.2083*	0.2228*	0.2390*
	(0.0256)	(0.0463)	(0.0527)	(0.0465)
F	33.59	26.67	22.09	162.04**
R^2	0.2220	0.2306	0.2312	

Notes: *, ** indicate significance at the 5 per cent and 10 per cent levels, respectively. Robust standard errors are reported. N = 278.

Table 10: Profit Function Estimation for Treatment Effects

Dependent variable: value added

Variables	1992 (1)	1995 (2)	1992 (3)	1995 (4)
Ln fixed assets (β_2)	0.6279* (0.0517)	0.6844* (0.0517)	0.6472* (0.0518)	0.7034* (0.0517)
Interest rate (β_1)	0.0008 (0.0187)	–0.0213 (0.0192)	0.0029 (0.0187)	–0.0166 (0.0191)
Ln wage rate (α_2)	0.9395* (0.1497)	0.9522* (0.1538)	0.9483* (0.1492)	0.9540* (0.1531)
SMI recipient (α_1)	–0.1858 (0.2976)	–0.0917 (0.3196)	0.1320 (0.3144)	0.2287 (0.3388)
Location/industry controls	Yes	Yes	Yes	Yes
Constant	–5.7142 (1.5639)	–6.1575* (1.6557)	–6.2408 (1.5739)	–6.6811* (1.6569)
Hazard	0.0166 (0.1932)	–0.0708 (0.2111)	–0.2082 (0.2032)	–0.2978 (0.2221)
ρ	0.0177	–0.0737	–0.2189	–0.3037
σ	0.9412	0.9610	0.9513	0.9808
Chi^2 (13)	425.36	434.89	440.64	444.25
α	–0.1574	–0.2119	–0.2285	–0.1850

Notes: * indicates significance at the 5 per cent level. The treatment effect, α, is calculated as in Greene (2000). N = 278. Specifications (1) and (2) utilize the estimates from the probit regression in Table 3, column (2), while specifications (3) and (4) utilize the estimates from the probit regression in Table 3, column (3).

Table 11: Profit Function Estimation for Matching Data

Dependent variable value added					
Variables	Pooled OLS (1)	Pooled OLS (2)	Pooled OLS (3)	First diff. (4)	First-diff. random effects (5)
Interest rate (β_1)	0.0148 (0.0119)	–0.0049 (0.0143)	–0.0056 (0.0147)	0.0194* (0.0065)	0.0213* (0.0078)
Ln fixed assets (β_2)	0.5502* (0.0299)	0.5312* (0.0302)	0.5343* (0.0308)	0.2256* (0.0350)	0.2289* (0.0487)
Ln wage rate (α_2)	1.0308* (0.0865)	1.0896* (0.0884)	1.0860* (0.0888)	0.7178* (0.0970)	0.7194* (0.1023)
SMI recipient (α_1)			–0.0677 (0.0823)	–0.0126 (0.0432)	–0.0102 (0.0427)
Location/industry controls	Yes	Yes	Yes	No	No
Year dummy	No	Yes	Yes	No	Yes
Constant	–6.6360* (1.4191)	–6.9066* (1.4053)	–6.8879* (1.4260)	0.2919* (0.0724)	0.2886* (0.0543)
F	38.81	36.62	34.34	10.68	278.16‡
R^2	0.6045	0.6123	0.6126	0.3317	0.33

Notes: * indicates significance at the 5 per cent level. Bootstrapped standard errors are reported. ‡ indicates Wald $chi^2(5)$ statistic.

Table 12: Labour Market Characteristics

Real wage growth	1985–92	1992–95	
SMI	–1.81	2.25	
Non-SMI	–2.20	–0.34	
Employment growth	1985–92	1992–95	
SMI	6.52	6.28	
Non-SMI	8.85	5.61	
Employee composition: 1995			
	Administrative personnel (% of total)	Production workers (% of total)	Casual workers (% of total)
SMI	8.3	67.0	24.7
Non-SMI	10.1	66.6	23.2
Family workers (% of total employees)	1985	1992	1995
SMI	3.7	2.6	2.4
Non-SMI	1.4	1.3	0.9
Family workers (total employees/firm)	1985	1992	1995
SMI	1.14	1.33	1.45
Non-SMI	0.83	1.03	0.83

Appendix A: Accelerator Model[1]

The investment decisions of a firm in a world of certainty can be analyzed under the framework of the accelerator model. First, assume that it is costly to invest: for investment *I*, the cost of investment is convex, such that $F'(I) > 0$, $F''(I) > 0$ for all $I > 0$. Next, assume standard production function relationships such that profits are a strictly concave function of the capital stock $\Pi = \Pi(K, t)$,where $\Pi_k > 0, \Pi_{kk} < 0$. The optimal path of investment can be described by the following Euler equation:

$$\Pi_k(K, t) = rF'(I) - F''(I)\dot{I}. \tag{A1}$$

Under the assumption of constant prices, (A1) implies the following flexible accelerator model:

$$I_t = \beta[K^*\text{-}K_t], \tag{A2}$$

where *K** satisfies $\Pi_K(K^*) = rF'(0)$. The implications are straightforward. If credit rationing occurs (that is, if firms are constrained), then the user cost of capital increases, implying a higher marginal product of capital and slower movement in adjusting capital stocks over time.

The accelerator model describes the path of investment when there are costs associated with different speeds of capital stock adjustment. Firms will behave according to (A2) if there are perfect capital markets or if they have sufficient internally generated funds. Otherwise, the investment process will also be determined by the availability of internal funds, or credit market constraints. The existence of capital stock adjustment costs and credit market constraints can be built into an empirical specification of the accelerator model, as follows. Given that the firm forms expectations of future output *Q**, where $\alpha Q^* = K^*$, (A2) can be expressed to incorporate the potential effects of capital stock adjustment costs to investment and credit market constraints:[2]

$$I_t = \beta[\alpha Q_t^* - K_{t-1}] + \eta\Pi_{t-1}. \tag{A3}$$

Furthermore, if there are firm-specific characteristics that may differentially affect access to credit markets differently, (A3) can be augmented to account for this firm-level heterogeneity:

$$I_t = \beta[\alpha Q_t^* - K_{t-1}] + \eta\Pi_{t-1} + \delta X_t + \varepsilon_t, \tag{A4}$$

1. This appendix follows Tybout (1983) in terms of notation. The accelerator model can be thought of as a special case of the Euler equation approach if the cost function depends solely on investment (Bigsten et al. 1999.)
2. This specification assumes the separability of financial variables from real variables in the investment process, but this strong assumption is clearly utilized as an empirical necessity.

where the *X*s can include firm location, industry classification, owner education and socio-economic status, and other firm-specific characteristics. This leads to the following functional form, suggested by Bigsten et al. (1999):

$$I/K_{t-1} = \alpha_0 + \alpha_1 \Delta V/K_{t-1} + \alpha_2 (\Pi/K)_{t-1} + \alpha_3 X_t + \varepsilon_t , \qquad \text{(A5)}$$

where I is investment, K is capital, ΔV is change in value added, Π is profits, and the *X*s are firm-level characteristics.

Appendix B: Profit Function (Yotopoulos and Lau 1973)[1]

Let $H^*(c, Z)$ be the profit function corresponding to the production function $F(X,Z)$, where c is the price of the jth input, X is a vector of variable inputs, and Z is the vector of fixed inputs. The production function and corresponding profit function is $V = AF(X,Z)$ and $\Pi^* = AH^*(c/A, Z)$. If the shadow cost of variable inputs varies across firms by the index function, k_i^j, then the profit function for the ith firm is:

$$\Pi_i = A_i H^*(k_i^l c_i^l / A_i, \ldots\ldots. k_i^j c_i^j / A_i; Z_i^l, \ldots., Z_i^k). \tag{B1}$$

Using Shepard's Lemma, one can derive the input demand functions and supply functions from (B1):

$$X_i^j = -A_i \frac{\partial H^*(k_i c_i / A_i; Z_i)}{\partial k_i^j c_i^j}$$

$$V_1 = A_i H^*(k_i c_i / A_i; Z_i) - A_i \sum_{j=1}^{m} k_i^j c_i^j \frac{\partial H^*(k_i c_i / A_i; Z_i)}{\partial k_i^j c_i^j}, \tag{B2}$$

and thus profits can be represented as

$$\begin{aligned} \Pi_i &= V_i - \sum_{j=1}^{m} c_i^j X_i^j \\ &= A_i H^*(k_i c_i / A_i; Z_i) - A_i \sum_{j=1}^{m} \frac{(1-k_i^j) c_i^j}{k_i^j} \frac{\partial H^*(k_i c_i / A_i; Z_i)}{\partial c_i^j}. \end{aligned} \tag{B3}$$

The implication of (B3) is that one can test relative economic efficiency between two firms. This framework, in turn, has several implications. First, $\partial \Pi_i / \partial A_i > 0$, and thus actual profits are increasing in technical efficiency for a set of given input shadow prices. Second, if $k_i^j = 1$ for $j = 1, \ldots.., m$, then the firm is maximizing profits. Third, if $A_i = A_n$ and $k_i = k_n$, then the actual profit functions are identical. Consequently, one can test the relative economic efficiency of two firms given a functional form for H.

The above model can be used to test relative economic efficiency for a given functional form. In this case, a Cobb-Douglas production function with the usual properties can be used. There are m variable inputs characterized by decreasing returns, and n fixed inputs for the production function:

$$V = A\left(\prod_{j=1}^{m} X_i^{a_j}\right)\left(\prod_{j=1}^{n} Z_j^{\beta_j}\right), \tag{B4}$$

1. This appendix presents the profit function model as developed by Yotopoulos and Lau (1973). This exposition follows their model in terms of notation and model specification.

where $\mu = \sum_{j=1}^{m} \alpha_j < 1$. The profit function is

$$\Pi^* = A^{1-\mu}(1-\mu)\left(\prod_{j=1}^{m}(c_j/\alpha_j)^{-\alpha_j(1-\mu)^{-1}}\right)\left(\prod_{j=1}^{n} Z_j^{\beta_j(1-\mu)^{-1}}\right). \tag{B5}$$

From (B12), one can compute the actual profit and input demand functions for the representative firm:

$$\Pi_y^i = \left[(A^i)^{(1-\mu)^{-1}}(1-\textstyle\sum_{j=1}^{m}\alpha_i/k_j^i)\right]\left[\prod_{j=1}^{m}(k_j^i)^{-\alpha_{j(1-\mu)-1}}\right]\left[\prod_{j=1}^{m}\alpha_j^{-\alpha_j(1-\mu)^{-1}}\right] \tag{B6}$$

$$\times\left[\prod_{j=1}^{m}(c_j^i)^{-\alpha_i(1-\mu)^{-1}}\right]\left[\prod_{j=1}^{n}(Z_j^i)^{\beta_j(1-\mu)^{-1}}\right]$$

$$X_l^i = \left[(A^i)^{(1-\mu)^{-1}}(\alpha_l/k_l^i c_l^i)\right]\left[\prod_{i=1}^{m}(k_j^i)^{-\alpha_i(1-\mu)^{-1}}\right]\left[\prod_{i=1}^{m}\alpha_i^{-\alpha_i(1-\mu)^{-1}}\right]$$

$$\left[\prod_{i=1}^{m}(c_j^i)^{-\alpha_i(1-\mu)^{-1}}\right]\left[\prod_{j=1}^{n}(Z_j^i)^{\beta_j(1-\mu)^{-1}}\right], \tag{B7}$$

then define

$$A_*^i = (A^i)^{(1-\mu)^{-1}}\left(1-\sum_{j=1}^{m}\alpha_j/k_j^i\right)\left[\prod_{j=1}^{m}(k_j^i)^{-\alpha_j(1-\mu)^{-1}}\right]\left[\prod_{j=1}^{n}\alpha_j^{-\alpha_j(1-\mu)^{-1}}\right]. \tag{B8}$$

Thus, the profit function becomes

$$\Pi^i = (A_*^i)\left[\prod_{j=1}^{m}(c_j^i)^{-\alpha_j(1-\mu)^{-1}}\right]\left[\prod_{j=1}^{n}(Z_j^i)^{\beta_j(1-\mu)^{-1}}\right]. \tag{B9}$$

If there are two firms, i = 1,2 and thus A_*^1 and A_*^2, take the ratio of the constant terms described above:

$$\frac{A_*^2}{A_*^1} = \left[\frac{A^2}{A^1}\right]^{(1-\mu)^{-1}}\frac{(1-\sum_{j=1}^{m}\alpha_j/k_j^2)}{(1-\sum_{j=1}^{m}\alpha_j/k_j^1)}\left[\prod_{j=1}^{m}\left[\frac{k_j^2}{k_j^1}\right]^{-\alpha_j(1-\mu)^{-1}}\right]. \tag{B10}$$

Then, one can take (B10) to get:

$$\Pi^1 = (A_*^1)\left[\prod_{j=1}^{m}(c_j^1)^{-\alpha_j(1-\mu)^{-1}}\right]\left[\prod_{j=1}^{n}(Z_j^1)^{\beta_j(1-\mu)^{-1}}\right]$$

$$\Pi^2 = (A_*^1)(A_*^2/A_*^1)\left[\prod_{j=1}^{m}(c_j^2)^{-\alpha_j(1-\mu)^{-1}}\right]\left[\prod_{j=1}^{n}(Z_j^2)^{\beta_j(1-\mu)-1}\right], \tag{B11}$$

and, defining

$$\alpha_j^* \equiv -\alpha_j(1-\mu)^{-1}$$

$$\beta_j^* \equiv \beta_j(1-\mu)^{-1}, \tag{B12}$$

taking natural logs of (B11) to get

$$\ln\Pi^1 = \ln A_*^1 + \textstyle\sum_{j=1}^{m}\alpha_j^*\ln c_j^1 + \sum_{j=1}^{n}\beta_j^*\ln Z_j^1$$

$$\ln\Pi^2 = \ln A_*^1 + \ln\frac{A_*^2}{A_*^1} + \textstyle\sum_{j=1}^{m}\alpha_j^*\ln c_j^2 + \sum_{j=1}^{n}\beta_j^*\ln Z_j^2. \tag{B13}$$

If $A^1 = A^2$ and $k^1 = k^2$, then Π^1 should be equal to Π^2 and this implies that $\ln A_*^2/A_*^1 = 0$. Within this framework, it is possible to test the hypothesis of equal relative economic efficiency by inserting a dummy variable into (B13) and determining whether the coefficient is zero. In previous studies, the emphasis has focused on assessing the relative economic efficiency of large and small firms. In the context of this paper, the test is straightforward. A dummy variable can be inserted to capture the relative economic efficiency of SMI recipients.

The empirical implementation is straightforward. For a Cobb-Douglas production function with labour as the variable input and capital fixed, the profit function (B13) can be expressed as:

$$\ln\Pi^1 = \ln A_*^1 + \alpha_1^*\ln w + \beta_1^*\ln R + \beta_2^* K$$

$$\ln\Pi^2 = \ln A_*^1 + \ln(A_*^2/A_*^1) + \alpha_1^*\ln w + \beta_1^*\ln R + \beta_2^* K,$$

where Π^i is the actual profit (total revenue minus total variable costs), w is the wage rate, R is the interest rate on capital, and K is the fixed assets of the firm. Allowing for regional and industry-specific price differences, the final estimating equation is:

$$\ln\Pi = \alpha_0 + \alpha_1 SMI + \alpha_2\ln w + \beta_1 R + \beta_2\ln A + \textstyle\sum_{i=1}^{5}\delta_i ISIC_i + \sum_{i=1}^{4}\delta_i LOC_i, \tag{B14}$$

where *SMI* is a dummy variable taking the value of 1 if the firm received an SMI loan, and 0 otherwise; *ISIC* is the industry code and *LOC* captures the firm's location. These last two vectors of dummy variables control for different output prices for firms in different industries or regions.

For the input demand function, define

$k_*^i \equiv (1 - \sum_{j=1}^{m} \alpha_j / k_j^i)(1-\mu)^{-1}$.

Then,

$$X_l^i = -A_*^i \alpha_l^* (k_l^i)^{-1} (c_l^i)^{-1} (k_*^i)^{-1} \left[\prod_{j=1}^{m} (c_j^i)^{-a_j(1-\mu)^{-1}} \right] \left[\prod_{j=1}^{m} (Z_j^i)^{\beta_j(1-\mu)^{-1}} \right]. \tag{B15}$$

Substituting from (B9),

$$\frac{-c_l^i X_l^i}{\Pi^i} = (k_l^i)^{-1} (k_*^i)^{-1} \alpha_l^* \equiv \alpha_l^{*i}. \tag{B16}$$

The input demand function differs across firms by a constant factor. This result can be used to test the hypothesis of relative price efficiency: if $k_1 = k_2$, then $\alpha_l^{*1} = \alpha_l^{*2}$. Likewise, one can test the hypothesis of profit maximization. If the *i*th firm maximizes profits, then $k_l^i = 1$ and $k_*^i = 1$. From this condition, it follows that $\alpha_l^{*i} = \alpha_l^*$. The empirical implementation of (B16) is straightforward:

$$\frac{-wL}{\Pi} = \alpha_l^{*SMI} + \alpha_l^{*non-SMI} non-SMI, \tag{B17}$$

where *w* is the wage rate, *L* is the quantity of labour, and *SMI* indicates the status of the *SMI* loan recipient.

Appendix C: Switching Regression[1]

A first approximation of the impact of financing constraints on firm behaviour can be obtained by estimating equation (1) using switching regression techniques (Maddala 1988). That is, one can divide the sample into those firms that receive credit and those that do not. Define the two possible regimes as follows:

$$I_i = \beta_1' X_{1i} + u_{1i} \quad iff \ \gamma' Z_i \geq u_i, \tag{C1a}$$

$$I_i = \beta_2' X_{2i} + u_{2i} \quad iff \ \gamma' Z_i < u_i, \tag{C1b}$$

where I_i is investment, X_i are the determinants of investment described above, and Z is a matrix of characteristics that predict the firms' participation in the SMI loan program. Then, defining a dummy variable,

$$\begin{aligned} SMI_i &= 1 \ \ if \ \gamma' Z_i \geq u_i \\ SMI_i &= 0 \ \ otherwise, \end{aligned} \tag{C2}$$

and where u_{1i}, u_{2i}, and u_i are distributed trivariate normal with mean zero and covariance matrix,

$$\Sigma = \begin{bmatrix} \sigma_1^2 & \sigma_{12} & \sigma_{1u} \\ & \sigma_2^2 & \sigma_{2u} \\ & & 1 \end{bmatrix},$$

the following likelihood function is obtained:

$$L(\beta_1, \beta_2, \sigma_1^2, \sigma_2^2, \sigma_{1u}, \sigma_{2u}) = \prod \left[\int_{\infty}^{\gamma Z_i} g(y_i - \beta_1' X_{1i}, u_i) du_i \right]^{SMI_i} \times \left[\int_{\gamma Z_i}^{\infty} f(y_i - \beta_2' X_{2i}, u_i) du_i \right]^{1 - SMI_i}. \tag{C3}$$

Lee (1978) provides a simple two-stage process to estimate (C3). First, the expected value of u_{1i} and u_{2i} must be estimated from (C1a) and (C1b). That is, obtain $E(u_i \leq \gamma_i' Z_i)$. Lee shows that:

1. This appendix follows Maddala (1988) in terms of notation.

$$E\left(u_{1i} | \; u_i \leq \gamma' Z_i\right) = E\left(\sigma_{1u} u_{1i} | \; u_i \leq \gamma' Z_i\right)$$

(C4)

$$= -\sigma_{1u}\frac{\phi(\gamma' Z_i)}{\Phi(\gamma' Z_i)},$$

and

$$E(u_{2i} \mid u_i \geq \gamma' Z_i) = E(\sigma_{21u} u_{1i} \mid u_i \geq \gamma' Z_i)$$

(C5)

$$= -\sigma_{2u}\frac{\phi(\gamma' Z_i)}{1 - \Phi(\gamma' Z_i)},$$

where $\phi(.)$ and $\Phi(.)$ are the pdf and cdf of the standard normal. Thus equations (C1a) and (C1b) can be written as:

$$I_i = \beta_1' X_{1i} - \sigma_{1u} W_{1i} + \varepsilon_{1i} \quad for\ SMI_i = 1, \tag{C6a}$$

$$I_i = \beta_2' X_{2i} + \sigma_{2u} W_{2i} + \varepsilon_{2i} \quad for\ SMI_i = 0, \tag{C6b}$$

where

$$W_{1i} = \frac{\phi(\gamma' Z_i)}{\Phi(\gamma' Z_i)}, \tag{C7a}$$

$$W_{2i} = \frac{\phi(\gamma' Z_i)}{1 - \Phi(\gamma' Z_i)}, \tag{C7b}$$

and ε_{1i} and ε_{2i} are the residuals with zero means:

$$\varepsilon_{1i} = u_{1i} + \sigma_{1i} W_{1i}$$

$$\varepsilon_{2i} = u_{2i} + \sigma_{2i} W_{2i}.$$

The estimation of the second stage by OLS for equations (C6a) and (C6b) is straightforward.

Appendix D: Treatment Effects

The treatment-effects model (Greene 2000) estimates the following equation:

$$\Pi_i = X_i\beta + \alpha SMI_i + \varepsilon_i, \tag{D1}$$

where Π are profits, X is a vector of firm characteristics, SMI is a dummy variable indicating whether the firm participated in the SMI program, and there is an error term with the usual properties. The binary dummy variable is characterized by a latent process:

$$SMI_i^* = Z_i\gamma + u_j, \tag{D2}$$

and thus the decision to take the treatment is made according to the following rule:

$$SMI_i = \left\{ \begin{array}{l} 1,\ if\ SMI_i^* > 0 \\ 0,\ \ otherwise \end{array} \right\}, \tag{D3}$$

and the error terms u and ε are bivariate normal with the following covariance matrix:

$$\begin{bmatrix} \sigma & \rho \\ \rho & 1 \end{bmatrix}.$$

The two-estimator is derived by Maddala (1988). First, a probit is estimated for participation in the treatment:

$$Pr(SMI_i = 1 \mid Z_i) = \Phi(Z_i\gamma). \tag{D4}$$

From (D4) the hazard ratio for each I can be calculated:

$$h_i = \left\{ \begin{array}{ll} \phi(Z_i\hat{\gamma})/\Phi(Z_i\hat{\gamma}), & SMI_i = 1 \\ -\phi(Z_i\hat{\gamma})/(1-\Phi(Z_i\hat{\gamma})), & SMI_i = 0 \end{array} \right\}, \tag{D5}$$

where ϕ and Φ are the standard normal density and cumulative distribution functions. Thus,

$$E(\Pi_i \mid Z_i) = X_i\beta + \delta Z_i + \rho\sigma h_i. \tag{D6}$$

The difference between participants in the SMI program and non-participants can then be estimated as:

$$E(\Pi_i \mid Z_i = 1) - E(\Pi_i \mid Z_i = 0) = \delta + \rho\sigma\left[\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)\{1 - \Phi(Z_i\gamma)\}}\right]. \tag{D7}$$

If $\rho = 0$, then (D4) can be estimated by OLS and the treatment effect is δ. If selection were positive, then $\rho > 0$ and the OLS estimate of δ would be biased upwards.

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