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martin@utalca.cl
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Microcredit Impact Assessment: The Brazilian and Chilean Cases

Evaluación del Impacto del Microcrédito: Los Casos de Brasil y Chile

Patricio Aroca[1], Geoffrey J.D. Hewings[2*]
[1]Ph.D. IDEAR, Universidad Católica del Norte, Chile, e-mail: paroca@ucn.cl [2]Ph.D. REAL, University of Illinois, Urbana, IL, USA, e-mail: hewings@uiuc.edu
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Abstract. Two different sources of data, Brazilian and Chilean banks and NGOs, were accessed to evaluate microcredit programs. Using propensity score and matching techniques, we compare the average income of individuals who received microcredit with the income of control groups, formed by people with similar characteristics. The results for the Brazilian data show a high positive impact of microcredit programs, especially for those administered by banks. In the Chilean case the evidence is weaker for the microcredit administered by banks. As for NGO-based programs, the evidence suggests that their impact on the average income of their clients is actually negative.

Keywords: Methods, microfinance, credit, impact assessment, evaluation.

Resumen. Se tuvo acceso a dos fuentes de información, bancos brasileños y chilenos, así como varias ONG, para evaluar programas de microcrédito. Usando técnicas de Propensity Score Matching (PSM), comparamos el ingreso promedio de los individuos que recibieron microcrédito con el ingreso de grupos de control formado por gente con características similares. Los resultados con los datos brasileños muestran un impacto positivo importante de los programas de microcrédito, especialmente para aquellos administrados por bancos. En el caso chileno, la evidencia es más débil para los microcréditos administrados por los bancos. En cuanto a los programas basados en las ONG, la evidencia sugiere que su impacto en el ingreso promedio de sus clientes es en realidad negativo.

Palabras clave: Métodos, microfinanzas, crédito, evaluación de impacto, evaluación.

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INTRODUCTION

The award of the Nobel Peace Prize to Bangladeshi economist Muhammad Yunus and the Grameen Bank that he created focused attention on the role of micro-credit in the economic development of many second and third-world countries. As Moore (2006) noted: “The Grameen Bank’s pioneering use of micro-credit has been duplicated across the globe since Yunus started the project in his home village three decades ago. Loans as low as $9 have helped beggars start small businesses and poor women buy cellular phones and basket-weaving materials”.

The award highlights the need to consider more extensively the fact that entrepreneurship is not the exclusive purview of either large companies, or business people in western economies but can be considered to be a more widespread phenomenon, often targeting thousands of people whose economic impact might be limited when considered in isolation – but aggregated across individuals, the impacts can often be outstanding (It should be noted that a Chicago community bank, ShoreBank, that had pioneered loans to low-income communities, was recruited by Yunus in 1983 to help him set up Grameen Bank. Grameen Bank now has over 6 million borrowers who have accessed loans totaling almost $6 billion with a 98% repayment rate). There is increasing interest in measuring the impact and viability of microcredit programs. The evidence to date is not uniformly encouraging, notwithstanding the Bangladesh experience, as Sebstad and Chen (1996) describe in their survey of the impact of microcredit programs: “*Income increased for at least half of the enterprises in most of the studies, while it remained the same or even declined for a significant proportion”*. Results are ambiguous and many programs are kept alive only by the injections of government or sponsor subsidies (Schreiner and Yaron, 2001).

Amin et al. (2003) report the results from research in northern Bangladesh. They distinguish among the poor between vulnerable and non vulnerable, concluding that in the richer villages the microcredit programs do not reach the vulnerable while in the poorer ones they are excluded from the programs. In both case, measuring the impact of the microcredit program could be biased for the reasons articulated by Coleman (2006), namely, self selection of the borrowers and endogenous program placement. Thus, microcredit does not reach the vulnerable poor in the relatively richer village B, and appears to exclude the vulnerable poor in the poorer village A. In other words, poor households that do join tend to have better access to insurance and smoothing devices than those who do not. The vulnerable poor may either choose not to join or they may be excluded by the microcredit program.

What has been the experience in Latin America? Berger (2006) makes the following observation:

> “In a region of great inequality and economic instability, microfinance is a capitalist paradox. In the past 20 years, microfinance has gone from an obscure development to a multibillion dollar enterprise bringing banking to millions of people. Although the industry has grown globally and there are star performers in every region, institutions in Latin America stand out for their integration into the formal financial system and their impressive growth, outreach and profitability indicators.”

Drawing on Miller and Martínez (2005), she notes that by the end of 2004, 80 of the top microfinance institutions (both NGO and formal financial institutions) in this continent were serving 4 million clients with a loan portfolio valued as $4 billion. These are impressive numbers but also generate some important questions. The most important obviously focuses on the success of the program; but here, there are several options that could be considered as appropriate metrics – the volumes of loans, the profitability of the system, the percentage of loans repaid, jobs and businesses created by the program and so forth. One important criterion that seems to not to have received much attention is the degree to which recipients of the program are better off than non recipients. Obviously, in this case, there are a variety of appropriate welfare measures that could be employed, for example, did the microloan recipient successfully launch a new venture or extend or enhance an existing one? In this paper, we follow the suggestion of Aghion and Morduch (2005) who focused on the causal impact of microfinance on borrower income; however, as they note, there are many other measures that may be equally important (extending the viability of the enterprise, increasing the productivity of the business, or generating positive spillover effects on other enterprises).

One of the major challenges of any program evaluation that involves a subset of a population is to measure success of the program participants in comparison to a control group of non-participants. In most cases, data are only available about the program participants, generating a need to build the control group from nonsurvey information. The objective of this paper is to evaluate the impact on micro-entrepreneurs income of two Brazilian and Chilean microcredit programs. Drawing on two unique sources of data, control groups are built using the propensity score to match beneficiaries of micro-credit programs with non-beneficiaries with similar characteristics. Then, the average incomes are compared. The results for the Brazilian data show a high positive impact of the microcredit programs, especially for those programs administered by banks. In the Chilean case, the evidence is weaker for bank-based programs and NGO (non-governmental organization) based programs appear to have no positive impact at all.

In the next section, the theoretical underpinning of the microfinance programs will be explored, together with a focus on the challenges facing small-scale entrepreneurs in Latin America. The need to develop a control group is presented along with the methodology of propensity score and matching estimator is explained in the following section. The second and third section presents the results from the Chilean and Brazilian cases. The final section summarizes our main conclusions and offers some policy evaluation.

The Microfinance Program: Economic Foundations and Challenges

An emerging entrepreneur in the United States can approach a financial institution with an idea for a new business; she will be encouraged to develop a business plan and asked to consider various forms of collateral so that a loan can be underwritten. In many cases, the collateral offered may be in the form of an additional mortgage on her house or other property; in very exceptional circumstances, a loan may be granted without significant collateral if the business concept has been through a successful experimental "proof of concept" phase. Without collateral, the entrepreneur may be forced to approach venture capitalists that, for a share in the profits, might be willing to underwrite the new business. These explorations assume an efficient market where information is easily accessed, the process is quite rational and the outcome can usually be predicted rather well. Now consider an entrepreneur in Latin America with few tangible assets (i.e. little or no collateral). As Goldmark (2006) reminds us, earlier microfinance programs often offered a "bundle" of services that extended beyond the loan itself, such as training programs to help clients manage businesses and money. While there would be a reasonable expectation that the US entrepreneur might possess some of these skills (such as familiarity with a prior loan), there was little expectation of this being the case with the majority of microfinance clients in Latin America. However, the more recent experience suggests that microlenders have moved away from more extensive training programs to focus on the credit transaction itself.

In essence, Goldmark's (2006) concerns parallel those of Aghion and Morduch (2005) who aim to broaden the conversation about the role and impacts of microfinance and to address some of the prevailing myths. For example, they argue that microfinance needs to be complemented by providing better ways for low-income households to save and insure. The challenge the received wisdom that microfinance has a clear record of social impacts (e.g. poverty reduction); they note:

> "Relatively few rigorous studies of impacts have been completed, and the evidence on statistical impacts has been mixed so far. There is not yet a widely acclaimed study that robustly shows strong impacts, but many studies suggest the possibility. Better impact studies can help resolve debates…" (Aghion and Morduch, 2005, p.4).

In addition, Aghion and Morduch (2005) address the question of why capital does not naturally flow to the poor. To begin with a simple binary example, think of an enterprise with little capital and one with extensive capital. Given a concave production function, the rates of return from investment in the small capitalized firm should be higher than the larger one; however, empirical evidence does not reveal capital flowing disproportionately to poorer entrepreneurs. To the contrary, issue of risk, government rile (such as usury laws) proven a market operating since, ceteris paribus, given the expected larger marginal returns for the poorer entrepreneur, she should be capable of paying a higher interest rate. There are additional problems – such as adverse selection that may result in banks charger a risk premium than may drive customers with the ability to pay out of the market or the issue of moral hazard that posits the problem of clients not making the required effort to make their projects successful. A final problem may stem from poor enforcement of legal contracts. Collecting the requisite information about clients in poorer, rural areas presents a daunting problem; microfinance is seen as an option that may reduce risk and costs (such as information collection) for lenders, encourage broad participation and ultimately offer benefits to both lender and client.

Aghion and Morduch (2005) also advance the idea of a shift in nomenclature, from microcredit to microfinance; while the two are often regarded as synonyms, they argue that the former refers to loans while the latter embraces part of what Goldmark (2006) would term "business services" – collecting savings, providing insurance or even assisting in the marketing of products.

The issues raised here are extensive; our access to the available data precludes our addressing many of them but we feel, following Aghion and Morduch's (2005) comments about the dearth of impact studies, that it is important to at least begin the analysis of the economic impacts at the individual level. Hence, the purpose of this paper is very simple: is it possible to demonstrate that recipients of microfinance were better off (in terms of income) than non recipients? As we noted in the introduction, formal survey data are only available for recipients; our task was to create a control group using nonsurvey methods. The method adopted is described in the next section. Essentially, the goal was to define two groups – recipients and non-recipients of microcredit – who were "identical" in terms of a set of non-income related characteristics thus affording the opportunity to claim that differences in income could be attributed to the microcredit program.

METHODOLOGY

Impact assessment requires a group affected by the program intervention (microcredit recipients), and a control group (non recipients) to compare the outcomes. Then, the differences between the two groups will provide an important component of the total impact of the program. Hulme (2000) notes that:

> *"Impact Assessments assess the difference in the values of key variables between the outcomes on «agents» (individuals, enterprises, households, populations, policy-makers, etc) which have experienced an intervention against the values of those variables that would have occurred had there been no intervention.The fact that no agent can both experience an intervention and at the same time not experience an intervention generates many methodological problems."*

In our study, the intervention will be the microcredit program. However, one of the main obstacles to assessing the impact is finding or building the adequate control group. Mosley (1997) suggests several different alternatives, although all of them present serious limitations of the kind discussed by Hulme (2000).

Coleman (2006) described two kinds of bias in the measurement of a microcredit program impact when the control group is not properly chosen. The first is associated with the comparison of people with initially different characteristics such as education, skill, wealth, etc., that might lead to the identification of differences that are not necessarily caused by the microcredit program. He also noted a tendency for microcredit to go to people with some particular sets of skills. A second source is associated with the microcredit institutions that chose the village in which to launch their microcredit program, ones that may have more entrepreneurship capacity installed that increase the success of the programs (endogenous program placement). Coleman (2006) addressed those bias issues in his study through the survey design and sample process.

In this paper, we will use a methodology that was initially applied in the health literature and has recently been adapted by a research group led by Heckman (Heckman et al., 1997 and 1998) for evaluating programs in economics. It addresses the problems described by Coleman (2006) and it is especially suitable when there is a data base available (household survey) from which to build the control group in a way that significantly reduces the research cost. This methodology is known as a matching estimator and it is based on Rosenbaum and Rubin (1983). The basic idea is that using a set of similar attributes (X: characteristic, variables or regressors) for two groups of people, one subject to the treatment and the other not; a propensity score for each individual in each group can be calculated. Then, the balancing property of the propensity score makes it possible to obtain the same probability distribution of X for treated and non-treated individuals in matched samples. Following Sianesi (2001), we will evaluate the causal effect of microcredit programs on household incomes of the participants, relative to a constructed control group, which has not received credit. Note that the evaluation is limited to income – other metrics could not be compared (such as job creation, success in marketing, participating in supplier networks etc.).

Let Y_1 be the outcome that would result if the individual receives microcredit and Y_0 the outcome that would result if the same individual does not receive microcredit. Let $D = \{0, 1\}$ denote the binary indicator of microcredit ($D = 1$ if microcredit, 0 otherwise). For a given individual, i, the observed household income is then $Y_i = Y_{0i} + D_i (Y_{li} – Y_{0i})$. In addition, assume that X, the set of attributes, is not affected by the microcredit program.

As Hulme (2000) noted, no individual can both receive and not receive microcredit at the same time, so that either Y_{li} or Y_{0i} is missing for each i. Since it is impossible to observe the *individual* microcredit effect and thus to make causal inference without making generally un-testable assumptions, we attempt to identify the *average* treatment effect in the population, or in a sub-population, which requires generally less stringent assumptions.

Thus, following Heckman (1997 and 1998) and Sianesi (2001), we could attempt to identify the following parameters:

- The average treatment effect: $E(Y_1 – Y_0)$

is the average income difference between the two groups: the micro-entrepreneurs that receive microcredit and the rest that does not.

- The average treatment effect on the non-treated: $E(Y_1 - Y_0 I D=0)$ is the average income difference between the potential or expected income that the entrepreneurs who did not receive microcredit (D=0) would get if they had $[E(Y_1)]$ and the real income that they earned (Y_0).
- The average treatment effect on the treated $E(Y_1 - Y_0 I D=1)$. This parameter is the one receiving the most attention in the evaluation literature and measures the average income difference between the income that the entrepreneurs earned who received microcredit and the income that they would obtained if they had not received credit.

Two unknown values: $E(Y_1 I D=0)$ and $E(Y_0 I D=1)$ prevent direct inference. Therefore, we need to make estimates based on some usually un-testable identifying assumptions that justify the use of the observed $E(Y_1 I D=1)$ and $E(Y_0 I D=0)$.

Sianesi (2001) notes that treated individuals may not be a random sample of the population, but they may receive treatment on the basis of characteristics, which also influence their outcomes. For example, microcredit institutions may try to pick out the best candidates such that micro-entrepreneurs who receive microcredit are of better quality than the rest; hence, these individuals could be expected to perform well, generating selection bias. This would result in an over-estimate of the impact of the microcredit program: $E(Y_1 I D=1)–E(Y_0 I D=0)$ would in general be an upward-biased estimate of the effect of treatment on the treated.

Statistical matching offers a way to construct a control group to partially address the selection bias issue (see Sianesi 2000). Rosenbaum and Rubin (1983) show that treatment and non-treatment observations with the same value of the propensity score have the same distribution as the full vector of regressors. It is thus sufficient to match exactly on the propensity score to obtain the same probability distribution of the explanatory variables for treated and non-treated individuals in matched samples.

In the next two sections, we will review, in turn, the Chilean and Brazilian cases.

THE CHILEAN CASE

In order to obtain the information about the Chilean micro-entrepreneurs, we developed a questionnaire following the methodologies developed by Barnes (1996), Chen (1997), Hulme and Mosley (1996), Hulme (2000), Mosley (1997), Sultana and Nigam (1999) and Tsilikounas (2000). The survey was run in February and March of 2002. The sample was obtained randomly from the databases of the bank Bandesarrollo and the NGO Propesa: respectively 56 observations for Antofagasta (II Region) and 30 observations for Melipilla (Metropolitan Region).

The CASEN (the Chilean survey of national socio-economic characterization) for the year 2000 was used to build the control group. This survey was run in November 2000. A set of variables from both surveys (location, age and employment status) was used to identify the control group from CASEN. We selected people who were living in Melipilla or Antofagasta, who were employees and who were older than 17 and younger than 66. The final control group sample contained 715 observations. After cleaning the sample extracted from the Bandessarollo database, we were left with 81 cases of "treated" individuals leaving us with a total of 796 observations. The variables dictionary for both groups is shown in **Table 1**.

The set of variables was chosen according to the matching techniques' requirements. First, it was important that these variables should not be affected by the microcredit program. With the exception of income, all the other variables comply with this requirement. To build the control group, we estimated the propensity score as a function of those variables. We used a probit specification and the results are shown in **Table 2** for the total sample, the bank clients (Bandesarrollo) and the NGO clients (Propesa). To measure the impact of the microcredit program we compare the average income of the people who received microcredit with the average income of the "similar" people who did not receive microcredit in the constructed control group: $E(Y_1–Y_0 I D=1)$.

The results shown in **Table 2** are as expected and with a high value for the explained variance for these kinds of models. In general, older women who do not have a husband, with some education and household heads are the ones with the highest probability of receiving microcredit. However, there are some significant differences between the bank and NGO clients. While the bank clients who belong to the productive or service sectors have a higher propensity to receive microcredit, the NGO clients that belong to the same sectors have a lower than average propensity to receive microcredit. This fact is reflected in the sign change of the coefficients on the variables productive sector (*spro*) and service sector (*sserv*) for the bank clients (positive) and the NGO clients (negative).

Evidence for the quality of the matching for the total sample, the bank clients and the NGO clients is presented in Appendix A, where the mean for each variable is calculated for the treated and the control groups, for the whole sample and for the matched sub sample. The difference between the treated and control group is lower for the matched sub sample than for the whole sample. The results further show that the matching procedure worked better for the whole sample and for the NGO clients, while for bank clients, the improvement from matching were not as significant. The main explanation is that even though there was some improvement in matching the individual according to the chosen variables, the matching of household head and household head spouse was not good.

The impacts of the microcredit programs are shown at the end of **Table 2**. The first two results are positive although not statistically significant. The microcredit program as a whole has a positive impact on the average income of the micro-entrepreneurs. This positive impact means that those receiving microcredit earn on average 25% more than those that did not. This impact is about 38% (Ch$119,432 or about US$ 220) for those who received credit from the Bandesarrollo. Those receiving credit from the NGO program appear to have a negative and significant impact on incomes of about 50%.

Alternatives explanation can be given for this finding. First, it could result from bias in the quality of the matching. Appendix A shows that we only obtain positive results from the worst matching.

Table 1. Variable dictionary

Variables	Description
Ide	Identification
Tret	=1 if the individual microcredit, =0 otherwise
Sex	=1 if females, =0 if male
Age	in years
Ms	marital status: =1 if married, =0 otherwise
Hhs	household size
Nemp	number of employees in the individual enterprise
Hxw	hours per week worked by the individual
Hh	head of household = 1, =0 otherwise
Hhsp	head of household spouse = 1, =0 otherwise
Mfinc	income from micro-firm or the main job
Hhinc	household income
Loc	Location
Spro	= 1 if the firm is in the productive sector, except retail
Sserv	= 1 if the firm is in the services sector, except retail
Edbas	= 1 if the education level is at most primary
Edsec	= 1 if the education level is at most secondary
Edhig	= 1 if the education level is higher than secondary

Table 2. Probit Estimate to predict the Propensity Score and Program Effect

	Total Sample		Bank		ONG	
Sample size	796		424		372	
Number of Treated	81		51		30	
LR $\chi^2(13)$	263		214		100	
Prob. > χ^2	0.00		0.00		0.00	
Pseudo R^2	0.50		0.69		0.48	
	Coefficients	**z**	**Coefficients**	**z**	**Coefficients**	**Z**
Sex	0.9357	4.30	1.3932	3.93	0.8492	2.25
Age	0.0276	3.06	0.0546	2.92	0.0181	1.22
Ms	-0.3581	-1.43	-0.2505	-0.58	-0.2378	-0.57
Hhs	0.1212	2.93	0.0734	1.13	0.1919	2.77
Edbas	2.0487	3.08	7.4690	10.89	0.7929	1.12
Edsec	2.0278	3.05	6.7686	10.37	1.2891	1.78
Edhig	2.2302	3.12			2.1819	2.80
Spro	-0.1797	-0.72	0.7833	1.56	-0.6928	-1.88
Sserv	-0.2736	-1.23	0.7560	1.74	-1.3144	-3.48
Nemp	-0.7796	-6.95	-0.7842	-4.80	-1.0002	-4.28
Hxw	0.0207	4.45	0.0394	4.66	0.0063	0.98
Hh	1.1692	3.73	6.9145	16.87	0.9015	2.00
Hhsp	0.4463	1.11			-0.0237	-0.03
Constant	-5.3795	-5.75	-19.1409	-12.88	-2.7455	-2.40
Income Mean of Matched Treateds	$342,062		$436,608		$181,333	
Income Mean of Matched Controls	$273,494		$317,176		$400,817	
Microcredit Program Effect	$68,568		$119,432		-$219,484	
T-statistics for Ho: Effect = 0	0.92		1.30		-2.49	

[1]Calculated using the psmatch routine written by Sianesi (2001a). The one-to-one matching with replacement was used in all the calculations.

THE BRAZILIAN CASE

The Brazilian data were collected in February and March 2002. Five institutions provided information about their micro-entrepreneur clients: Microcred (bank from São Paulo), Socialcred (bank from Rio de Janeiro), CEAPE (NGO from Goias), Bancri (NGO from Santa Catarina) and Bco Povo Sto Andre (NGO from São Paulo). In addition, the information collected by PNAD (Brazilian National Survey on Households) 1999 was used to build the control group.

Table 3 shows the variables that were used in the analysis. These variables are not exactly the same as the ones used in the Chilean case because the Chilean CASEN uses a different questionnaire than the Brazilian PNAD. However, we tried to provide proxies for the same concepts, even if measured by different variables. The only variable that was not available in the PNAD is marital status. In addition, for hours worked by week and number of employees in the firm, PNAD provides ranges and not number of hours and workers respectively. It is worth noting, however, that PNAD does provide information on variables that are not available in the Chilean case, such as the location of the micro-firm (home or outside).

In total, we were able to use 198 observations from

Table 3. PNAD Variable Descriptions

Variables	Description (PNAD code)
Ide	Identification
Tret	=1 if the individual microcredit, =0 otherwise
Sex	=1 if females, =0 if male (v0302)
Age	in years (v8005)
Hhs	household size (v4725)
Nemp1	= 1 employee, 0 otherwise
Nemp2	= 1, 2 employees, 0 otherwise
Nemp3	= 1, 3-5 employee
Nemp4	= 1, more than 5 employee
Hxw1	= 1, if Hours per week is lower than 15 (v4707)
Hxw2	= 1, if Hours per week is larger than 14 and lower than 40
Hxw3	= 1, if Hours per week is larger than 39 and lower than 45
Hxw4	= 1, if Hours per week is larger than 44 and lower than 49
Hxw5	= 1, if Hours per week is larger than 48
Hh	head of household = 1, =0 otherwise (v0402)
Hhsp	head of household spouse = 1, =0 otherwise (v0402)
Mfinc	income from micro-firm or the main job
Hhinc	Income from all the sources
Spro	= 1 if the firm is in the productive sector, except retail
Sserv	= 1 if the firm is in the services sector, except retail
Edbas	= 1 if the education level is at most primary
Edsec	= 1 if the education level is at most secondary
Edhig	= 1 if the education level is higher than secondary
Locmf	=1 if microfirm is located at home, 0 otherwise (v9054)

micro-entrepreneurs of five different states that had received microcredit, either from NGOs (152) or from banks (46). For the control group, we selected 34.887 observations from PNAD, for the same five states.

In order to make income comparisons across treatment and control groups, we need to take into account the different reference periods for the income information available for microcredit clients (January 2000) and the control group from PNAD (September 1999). The price variation in Brazil during these 4 months is shown in **Table 4**: income differences of that order of magnitude across treated and control groups can be explained purely by price changes.

The large sample collected in Brazil allowed for better comparisons than in Chile. We estimated the effect of microcredit programs using the total sample and comparing the entrepreneurs that received microcredit, first, with the whole set of employers and self-employed in the PNAD sample, and secondly with the PNAD sample of salaried workers.

We are aware of the possibility that some of the employer or self-employed individuals in the control group may also be clients of a microcredit program. However, we do not have the information required to recognize them. Therefore, when the PNAD sample of individuals in the employer and self-employed status (we will hereafter use the term employer to refer to both employers and self-employed individuals) is used as a control group, we can expect the difference between treated and non-treated to be a downwardly biased estimate of the impact of the microcredit program hereby considered. On the other hand, estimates based on the control group built using only salaried workers from the PNAD sample could overestimate the effect of the microcredit program because those workers may lack the entrepreneurial skills that account for a share of the income of individuals in the treatment group.

Table 5 shows the results for the three alternative control groups (total PNAD sample, employers only

Table 4. Price Index and Variation between September 1999 and January 2000

Period	IGP - M geral (ago.1994 = 100) General Market Price Index	IPCA - geral - índice (dez.1993 = 100) Broad Consumer Price Index	IPC – geral - índice (ago.1994 = 100) Consumer Price Index - Brazil	IPC - geral - índice (jun.1994 = 100) - RMSP Consumer Price Index-SP
1999 09	167.997	1545.83	176.344	173.8829
2000 01	180.301	1598.41	182.871	180.3469
Index Var.	7.32%	3.40%	3.70%	3.72%

and salaried workers only), while Appendix B presents evidence on the respective quality of the matching. From Appendix B, we see that in all three cases the matching procedure generally leads to significant reductions in the average differences among treated and control groups. This is especially the case when the matching is performed using the PNAD sample of employers as a control group. It is worth noting, however, that although gains are considerable for most variables, there are some variables for which average differences across the two groups actually increase after the matching.

In contrast to our results on Chile, the microcredit programs examined in Brazil appear highly effective, with high and statistically significant increases in the average income of their clients. If we adjust the mean income in the PNAD sample for the inflation between September 1999 and January 2000 (see **Table 4**), the difference between entrepreneurs who received microcredit and other entrepreneurs or workers with similar characteristics is still above 100%. When we compare the estimates obtained with control groups of, respectively, employers and salaried workers, we find, as expected, that the impact of microcredit is lower in the first case. However, this impact is still high (R$ 1,351 or about US$ 350 of the average monthly income) and highly significant (t- statistic = 5.68).

As in the Chilean case, we compare the differences between those who received microcredit from a bank-based program to those who were clients of an NGO-based program. Results are shown in table 6 for both types of control groups (employers and salaried workers). Before further commenting on these results, note that the average income of bank clients is about 20% higher than that of NGO clients. As for the matched control groups, in the case of bank-based programs, the average income of the employers control group is 80% higher than that of the salaried workers control group (R$ 934 versus R$ 516). This is different from the NGO case, where the average income of the employers matched control group is lower than that of the salaried workers control group (R$860 and R$1,053, respectively). Moreover, the average income of the employers matched control group is higher for bank- than for NGO-based programs, while the opposite is true when comparisons are made across salaried workers control groups. The main conclusion arising from these results is that banks and NGOs have very different clients in terms of their incomes, a finding that was also observed in Chile.

Although in the case of the control groups used for bank clients, salaried workers earn much less than employers with similar characteristics, the behavior of average incomes in the matched control groups constructed for the sample of NGO clients is very different. While the average income of employers matched to NGO clients is 9% lower than the average income of employers matched to bank clients, the average income of salaried workers matched to NGO clients is larger than the average income of employers matched to NGO clients by 22%. However, this last result is not surprising if one takes into consideration the educational level and number of workers in the firm, both of which are positively and significantly related to the propensity to receive microcredit in the sub-sample of salaried workers.

One characteristic of the Brazilian labor market is that people tend to have more than one job to increase their income. On the other hand, micro-entrepreneurs tend to over-estimate the number of hours that they really work, especially in the case when they use their home as the business location. These elements have two implications for our study. First, we decided to compare average monthly income rather than average hourly income. Secondly, we re-estimated average income of the control group using all the income sources of salaried workers and employers.

Table 7 shows the results of comparing the income of the micro-entrepreneurs that received micro-credit with the total income, from all sources, of matched salaried workers and employers. Although the microcredit program effect is lower than the one shown in **Table 6**, it is still high and significant both for clients of bank- and NGO-based programs.

Table 5. Probit and Microcredit program Effect Estimates

	Total Sample		Employers		Workers	
Sample Size.		35085		9384		25899
Number of Treated		198		198		198
LR $\chi^2(13)$		1164		917		1454
Prob. > χ^2		0.00		0.00		0.00
Pseudo R^2		0.48		0.48		0.63
Variables	**Coefficients**	**z**	**Coefficients**	**z**	**Coefficients**	**z**
Sex	0.1440	1.72	0.5776	5.87	-0.0153	-0.15
Age	0.0382	11.12	0.0292	7.34	0.0453	10.66
hhs	-0.0855	-3.07	-0.0603	-1.79	-0.0947	-2.83
edbas	0.5011	2.09	0.2528	0.93	0.7836	2.68
edsec	1.1084	4.52	0.8036	2.90	1.4264	4.69
edhig	1.0941	4.33	0.6784	2.37	1.6094	5.15
spro	-0.4542	-4.43	-0.3995	-3.40	-0.6959	-5.22
sserv	-0.8680	-9.78	-0.6287	-6.37	-1.1953	-10.45
nemp2	1.2708	9.65	0.7293	5.27	3.2287	9.74
nemp3	1.3606	11.32	0.8174	6.35	3.3233	12.31
nemp4	0.6491	3.39	0.1770	0.87	2.5137	5.93
hxw2	-1.2849	-5.10	-1.3854	-5.00	-1.2668	-4.49
hxw3	-0.5385	-3.16	-0.1425	-0.75	-0.9162	-4.44
hxw4	0.0349	0.21	0.5649	3.00	-0.3026	-1.53
hxw5	-0.2105	-1.28	0.1034	0.56	-0.3520	-1.78
hh	-1.1156	-13.22	-1.3687	-14.61	-0.9226	-9.25
hhsp	-2.0802	-8.59	-2.5279	-10.47	-2.0582	-5.78
locmf	1.3989	15.29	1.1695	11.61	1.6433	13.88
Constant	-3.7593	-10.60	-3.0041	-7.58	-3.8119	-8.84
Income Mean of Matched Treateds	$2,118		$2,118		$2,118	
Income Mean of Matched Controls	$930		$767		$684	
Microcredit Program Effect	$1,188		$1,351		$1,434	
T-statistics for Ho: Effect = 0	4.41		5.68		4.95	

Table 6. Probit and Microcredit program Effect Estimates for Type of Clients

	Bank Clients				NGO Clients			
	Employers		Workers		Employers		Workers	
Sample Size	9232		25747		9338		25853	
Number of Treated	46		46		152		152	
LR $\chi^2(13)$	255		458		497		1006	
Prob. > χ^2	0.00		0.00		0.00		0.00	
Pseudo R^2	0.44		0.68		0.32		0.54	
Variables	**Coef.**	**z**	**Coef.**	**z**	**Coef.**	**z**	**Coef.**	**z**
Sex	0.1539	0.89	-0.2243	-1.08	-0.1143	-1.23	-0.2833	-2.77
Age	0.0330	4.69	0.0555	6.29	0.0192	5.02	0.0357	8.65
Hhs	0.0274	0.48	-0.0564	-0.85	-0.1607	-4.72	-0.1426	-4.03
Edbas	4.8480	9.85	4.8428	0.00	0.1324	0.57	0.5374	2.07
Edsec	5.7572	12.94	6.2103	23.99	0.6402	2.70	1.1069	4.05
Edhig	5.5011	11.95	6.0831	18.74	0.5653	2.30	1.3203	4.70
Spro	-0.6685	-2.68	-1.1787	-2.81	-0.2983	-2.73	-0.6389	-5.01
Sserv	-0.5875	-3.58	-1.3011	-5.72	-0.6186	-6.48	-1.1834	-10.10
Nemp2	1.0359	4.65	3.5136	7.96	0.5498	3.98	2.9577	9.03
Nemp3	0.8969	3.91	3.0321	7.33	0.6823	5.52	3.1419	12.17
Nemp4	0.5513	1.75	2.1335	3.18	-0.0396	-0.19	2.3862	5.88
hxw2					-0.8479	-3.37	-0.8918	-3.24
hxw3	0.3979	1.74	-0.3244	-1.16	0.1503	0.79	-0.5031	-2.38
hxw4	0.4415	1.63	-0.1715	-0.57	0.9214	4.95	0.1348	0.67
hxw5	0.5068	2.30	0.2795	1.11	0.3299	1.81	-0.0111	-0.05
Hh	-1.2760	-7.61	-1.1987	-5.46	-0.7681	-8.46	-0.5480	-5.59
Hhsp	-1.5785	-5.80	-1.3463	-3.58				
Locmf	1.3491	7.06	1.9800	8.16	1.1263	11.39	1.5529	12.84
Constant	-9.3971	0.00	-9.9487	-16.81	-2.8169	-7.73	-3.6477	-8.86
Income Mean of Matched Treateds		$2,428		$2,428		$2,024		$2,024
Income Mean of Matched Controls		$934		$516		$860		$1,053
Microcredit Program Effect		$1,494		$1,913		$1,164		$971
T-statistics for Ho: Effect = 0		2.59		3.48		4.14		2.94

Table 7. Microcredit program effect estimates using income from all sources

Comparison Using Incomes From All Sources	Bank Clients		NGO Clients	
	Employers	Workers	Employers	Workers
Income Mean of Matched Treateds	$2,428	$2,428	$2,024	$2,024
Income Mean of Matched Controls	$1,131	$597	$996	$1,152
Microcredit Program Effect	$1,297	$1,831	$1,028	$872
T-statistics for Ho: Effect = 0	2.34	3.84	5.10	3.86

CONCLUSIONS

We estimate the income impacts of two Chilean and five Brazilian microcredit programs, attempting to provide a small impacts study to contribute to the dearth of such studies noted by Aghion and Morduch (2005). We evaluated the bank and the NGO programs separately because the previous literature suggests that they address different shares of the market. We use a relatively new method to build control groups, in order to deal with some of the more serious problems that have plagued previous assessments of microcredit programs. In many cases, the program was evaluated in terms of metrics that focused on the volume of loans, number of clients, repayment rates and so forth but did not address a fundamental issue – did the program make a difference in the incomes of recipients in comparison to the incomes of non-recipients?

We find weak evidence of positive impacts for the Chilean bank-based program. As for the Chilean NGO clients, it seems that the impacts of microcredit on income are not positive but negative. On the other hand, the Brazilian evidence shows a highly positive and significant impact of microcredit programs on clients' income, especially in the case of those administered by banks. The next logical steps would be to assess the impacts of these programs on development more broadly defined – beyond the calculation of just the impacts on clients' incomes. Did they create sustainable enterprises that in turn employed innovative methods/techniques to develop products or services that extended to the community as a whole? Did the successes, such as those in the Brazilian case, generate a milieu that encouraged others to assume the risk of obtaining a loan? Were these entrepreneurs able to participate in formal supply chains in marketing their goods or services? Entrepreneurial development at this level is likely to be modest in scope when viewed from the perspective of the individual micro-loan, but as noted in the Bangladeshi case, summed over thousands or millions of clients, the impacts begin to accumulate to reach rather impressive levels.

Finally, a one-time evaluation of a program needs to be complemented by an on-going monitoring process: Were the income gains noted in Brazil for program participants sustained over time? To what degree does location play a role – Are there critical communities whose participation and success in these programs lead to a greater and more successful evolution (in terms of spread)? There are other issues that need to be considered – especially the role of the suppliers of credit – before a full evaluation of the impacts can be assessed.

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