Research Proposal

"Microfinance Impact in Chile: A Tale of Two Cooperatives"

by

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I. Introduction

Many developing countries have persistently high poverty and unemployment and low growth rates. This has led some observers in the developed world to ask, "Why can't people in these countries improve their situation – are they just lazy?" A more informed answer to this question is that the poor and potential entrepreneurs simply do not have access to credit. This could be a few hundred dollars to buy some raw materials to make handicrafts and sell them, or a few thousand dollars to open a small store. Some preliminary studies suggest that credit constraints *do* bind for the poor and that relaxing these constraints provides tangible benefits (Karlan and Zinman, 2006a/b).

Microfinance programs have rapidly spread worldwide on the premise that they help to eradicate poverty and create economic growth; however, the impact of microcredit on poverty reduction and other outcomes is still unclear. The economics literature is inconclusive; some studies claim that micro-loans can greatly improve the welfare of the poor (McKernan, 2002) while others suggest that the impact of such programs is exaggerated because of self-selection and endogenous program placement (Coleman, 1999). Recent evidence suggests that integrating business training into micro-finance programs can improve their effectiveness and pay for itself (Karlan and Valdivia, 2006).

The success of microfinance institutions (MFIs) in Asia, such as the Grameen Bank, has led to the rapid expansion of similar programs in South America (SA); however, simply making credit available to potential entrepreneurs may not necessarily create successful

small businesses – there may be other factors that prevent economic growth. Although MFIs have spread quickly across SA over the past ten years, there are very few studies that have evaluated their impact. Unlike programs in Asia, the majority of MFIs in SA do not offer group and gender-based lending. Within the SA context, the effects of MFIs in general, and group vs. individual/male vs. female lending specifically, are unknown.

This study attempts to fill the gap in this literature by focusing on MFIs in Chile. Eighty percent of businesses in Chile qualify as microenterprises, employing forty percent of the workforce (Valenzuela and Venegas, 2001). There are currently 14 independent MFIs with 57 branches throughout the country. The large number of programs in Chile, and continuing national and international support, makes it worthwhile to evaluate whether they are achieving their purpose: to help individuals obtain employment and overcome poverty (PET, 2002). An analysis of Chilean programs would be a first step to determine whether microfinance programs have been as successful in SA as they have been in Asia, and in particular, if the policies used are appropriate given SA's unique cultural climate

No matter the region, the two key challenges in estimating the impacts of micro-loans are self-selection and nonrandom program placement (Pitt and Khandker, 1998). This study will overcome these challenges to provide unbiased estimates of program impact by exploiting an exogenous source of variation in program participation: some programs require that the applicant's business have been in operation for at least one year, others require only six months. This study will focus on the two microcredit cooperatives in the country, Cooperativa de Ahorro y Crédito Talagante Ltda. (COOCRETAL) and

Cooperativa de Servicios Financieros a la Microempresa (CREDICOOP). Each organization has eight branches; the former requires that applicant have at least one year of experience in the business activity, the later only requires six months of experience. Using a type of regression discontinuity model with village-fixed effects and controls for institutional type and policies, and household/village characteristics, I will estimate the impact of program participation on outcomes including business profits, consumption (relative to poverty line), primary schooling of children, and health expenditures.

The rest of the paper will be organized as follows: Section II will provide a brief review of the empirical literature on this subject; Section III will provide an overview of microfinance organizations in Chile; Section IV will detail the estimation strategy; Section V will discuss the data; and Section VI will conclude.

II. Literature Review

Several authors have used different methods to deal with the nonrandom program placement and self-selection issues that challenge any evaluation of the impact of microcredit on welfare. Pitt and Khandker (1998) study the impact of program participation, by gender, in the Grameen Bank, Bangladesh Rural Advancement Committee (BRAC), and Bangladesh Rural Development Board's (BRDB) Rural Development RD-12 program. In order to deal with nonrandom placement, they use village fixed-effects estimation, which "treats the village-specific component of the error as a parameter to be estimated, eliminating the endogeneity caused by unmeasured

village attributes including nonrandom program placement." To overcome the self-selection bias, they exploit a source of exogenous variation in program placement: households that own more than one-half acre of land are excluded from participating.

Exactly the same approach is used by McKernan (2002) to answer a different question: she asks, "Do the noncredit aspects of microcredit programs have a positive effect on self-employment productivity above and beyond the provision of credit?" McKernan argues that noncredit products offered by MFIs, such as vocational training, health advice, and group lending, can increase micro-enterprise productivity well beyond credit alone. She measures the total effect of microcredit by estimating a profit equation and the noncredit effect by estimating this equation conditional on productive capital. Like Pitt and Khandker, her estimation strategy employs a Limited Information Maximum Likelihood (LIML) technique with endogenous and exogenous switching.

The method employed by Pitt and Khandker is questionable, particularly the assumption that land holdings exogenously determine program participation but are uncorrelated with household outcomes. One could argue that those who own greater amounts of land have other unobservable characteristics (perhaps they are more entrepreneurial) that will affect their outcomes. The data collected by Coleman (1999/2006) allows for a superior way of dealing with the bias resulting from self-selection. Coleman conducted a survey of 444 households in 14 villages supported by two different NGOs in Northeast Thailand in 1995/1996. Six of the 14 villages had never had a village bank; however, the two NGO's planned to expand their lending programs and allowed participants to self-select one year

before the program began. This provided a natural control group to compare self-selected individuals not receiving microcredit with similar individuals in other villages that were. A stratified random sample of households was surveyed four times over the course of a year; this time series and cross-sectional data enhances the credibility of his results, and clearly produces unbiased estimates of microcredit's impact on household outcomes.

Finding a source of exogenous variation that determines program eligibility is not the only way to obtain clean estimates of program impact, however. Kaboski and Townsend (2005) collect survey data from 161 MFIs across 108 villages in rural and semi-urban Thailand. These many institutions are promoted by several different agencies and ministries, creating a heterogeneous group that provides for an important source of variation in institutional practices and form. They suggest that this variation reflects how well an institution can provide financial products; thus, this serves as an instrument to identify program impacts. To deal with the issues of self-selection and non-random program placement, they compare the outcomes of households in villages with a single MFI to those of households in villages with no MFIs, while controlling for 19 village-level variables. The household outcomes were obtained by a survey administered to 2,880 households across 192 villages – sufficient power to isolate program impact.

Some other ways that one can deal with self-selection and nonrandom placement biases introduced into estimates of program impact is by simply presuming knowledge of the distribution of the errors. For instance, "if the errors are assumed to be normally distributed, as is common, the treatment effect is implicitly identified from the deviations

from normality within the sample of treatment participants," (Pitt and Khandker, 1998). However, if the treatment and outcomes are binary variables, this cannot be used. A second alternative is to collect time-series data for households before and after receiving microloans; in this case, it is possible to obtain unbiased estimates of program impact by using individual fixed effects. This would be ideal, but it is not a very feasible approach, even if one can identify households that do not qualify to receive microloans, but will once they satisfy some exogenous rule, such as age or number of months in operation.

Finally, one might convince a MFI to randomize some of its loans and thus completely resolve the issue of self-selection by comparing individuals that are chosen randomly irrespectively of their personal unobserved characteristics. This has been the method pursued by Dean Karlan and coauthors. Karlan and Zinman (2006a) propose that "another way to expand access to credit is for existing lenders to liberalize their screening criteria." By randomizing the credit supply decisions of a South African bank to marginally rejected applicants, they find evidence that relaxing credit constraints provides tangible benefits such as stable employment and consumption smoothing. Also, they find that these loans were actually profitable for the lender. In a second example, Karlan and Valdivia (2006) use a randomized control trial to estimate the impact of business training on microcredit customers. They find that the treatment had positive effects on business practices and revenues while improving repayment and retention rates for the MFI. This implies that entrepreneurship is a skill that can, in fact, be learned.

III. Microfinance Organizations in Chile

Microenterprises in Chile have recently begun to be recognized as an important economic sector apart from small and medium-sized businesses (PET, 2002). Likewise, the need for microfinance has started to receive mainstream attention; I estimate that there are 57 MFI branches throughout the country today, in addition to the microenterprise divisions of the three largest banks (Banco Estado, Banco del Desarrollo, and Banco Santander). The banks have among the lowest interest rates among MFIs, however, they also have higher collateral requirements and in no circumstances offer uncollateralized loans. The "independent" microfinance organizations tend to be either cooperatives or non-governmental organizations (NGOs). In order to assess the impact of microcredit programs, it is preferential to analyze the independent organizations because they are available to the very poor who have no collateral or other sources of credit.

A list of the independent MFIs in Chile with their application requirements is presented in Figure I. Loans range from less than $200 up to $3,000 USD with maturities of up to three years. Some programs require collateral or cosigners, others, such as Fundacion Ayuda y Esperanza, only require a referral from a church or other social service agency. Some programs, such as FINAM and CICADES are primarily funded from the U.S.; others receive government support and grants through organizations such as the Inter-American Development Bank (IADB). Figure II details the locations of the 57 MFI branches in Chile, Figure III contains a map of the regions of Chile, and Figure IV presents a histogram showing the distribution of MFIs across regions.

This investigation will focus on the two major microfinance cooperatives in the country, Cooperativa de Ahorro y Crédito Talagante Ltda. (COOCRETAL) and Cooperativa de Servicios Financieros a la Microempresa (CREDICOOP). The advantage of focusing on these two institutions is that they both have similar policies and organization; they require that borrowers be members of the cooperative and maintain pledged savings accounts, and each organization has eight branches located throughout the country, making them among the largest independent MFIs in Chile. Lastly, these organizations have the most data available, which suggests that they may be more institutionalized than the others.

These organizations are fairly similar; however, there is one notable difference – COOCRETAL requires that applicants have operated their business for at least one year in order to receive a loan, but CREDICOOP only requires six months of experience. This allows for a natural experiment; among entrepreneurs with six to 12 months of experience that self-select to participate in a microcredit program, those that apply to CREDICOOP for loans are denied but those who apply to COOCRETAL are accepted, regardless of individual-specific unobservables. Thus, by comparing the outcomes of the "treatment" group against those of the "control" group, while controlling for observable individual/village characteristics, we can obtain an unbiased estimate of program impact.

IV. Estimation Strategy

Karlan and Goldberg (2006) provide a review of the methodological issues in evaluating program impact. They outline two methods of program evaluation: randomized control

trials and quasi-experimental approaches. The first category encompasses experimental credit scoring, the method employed in Karlan and Zinman (2006b). The authors note, however, that "this approach, if sample sizes permit, does not necessarily require randomization. A regression discontinuity design may also be possible if enough individuals are at or near the threshold." Randomized control trials also include randomly assigning individuals to receive microloans, but this method may not account for spillovers to non-participants. The second category, quasi-experimental design, includes the approaches of Coleman (1999/2006), which is a "prospective" method in which treatment and control groups are selected in advance, and Pitt and Khandker (1998), which employs weighted exogenous sampling LIML.

All of these methods can be used more or less successfully to produce estimates of program impact. Given the source of exogenous variation in program participation identified between COOCRETAL and CREDICOOP, there are several possible alternatives; however, the best approach would be a variant of the regression discontinuity model mentioned as an alternative to randomization by Karlan and Goldberg. The common regression discontinuity design in this case would involve comparing marginally denied applicants (due to the exogenous rule) to marginally accepted applicants across one or many MFIs. However, this study proposes that the applicants, who otherwise qualify for loans, who are denied loans by COOCRETAL for having less than one year's experience in business be compared with entrepreneurs with similar experience who are granted loans by CREDICOOP because of their less stringent requirement. This is a superior method because a larger number of observations can be

obtained, including many individuals who are far from the margin; i.e. comparisons can be made between individuals with seven, eight, nine, ten, eleven, and twelve month's of experience, the only difference is that one group was given loans and the other wasn't.

Although I don't think this approach has been used to evaluate the impact of microcredit before, it is precisely the one employed by Tyler, Murnane, and Willett (2000) in order to estimate the impact of GED certification on future earnings. They "take advantage of interstate variation in GED passing standards to generate variation in GED status among individuals with the same observed levels of human capital." Tyler, Murnane, and Willett compare individuals with exactly the same test scores in states with higher and lower passing standards, controlling for local testing conditions, and then compare the mean earnings between the two groups to get an estimate of having the GED. This would be the best way to take advantage of the variation we identified between Chilean MFIs.

The model below expands on that used by Tyler, Murnane, and Willett (TMW) to include variation across j villages, a village-fixed effects variable, and other unique explanators specific to the question we are trying to answer. What each variable represents is explained beneath the equation. Vectors are in bold face.

$$(1) \qquad Y_{ij} = \beta_0 + \beta_1{}'\mathbf{X}_{ij} + \beta_2{}'\mathbf{M}_{ij} + \alpha(\mathbf{M}_{ij}*C_{ij}) + \beta_3 SEX_{ij} + \beta_4 GROUP_{ij} + \mu_j + \varepsilon_{ij}$$

Y_{ij} is the outcome (business profits, consumption, primary schooling of children, and health expenditures) for individual i in village j.

$\mathbf{X}_{ij}$ is a vector of household-specific observable characteristics (e.g. age/years of schooling of head of household) for individual i in village j.

$\mathbf{M}_{ij}$ is a vector of dummies representing the months that the applicant's business has been in operation (six dummies: seven, eight, nine, ten, eleven, twelve months).

C_{ij} represents program participation; it is the value (in USD) of the initial loan the applicant receives (for applicants exogenously denied loans, $C_{ij} = 0$). The product $\mathbf{M}_{ij}*C_{ij}$ will yield the amount borrowed for individual i in the ith row of the resulting vector. The coefficient α will capture the impact of the loan.

SEX_{ij} is a dummy variable taking the value 1 if the applicant is female and 0 if male.

$GROUP_{ij}$ is a dummy variable taking the value 1 if the loan was received as part of a group application and a value of 0 if not.

μ_j is an unmeasured determinant of Y_i that is fixed within a village.

ε_{ij} is a nonsystematic error that captures unmeasured determinants that change between households such that $E[\varepsilon_{ij}|\mathbf{X}_{ij}, \mu_j] = 0$.

Because program participation is uncorrelated with the error term (all individuals in the sample self-selected themselves into, and qualify for, the microcredit program), the

estimate of α provided by the model will be unbiased. This treats the problem of self-selection; the village-fixed effects variable eliminates the endogeneity caused by nonrandom program placement because it picks up unmeasured village attributes that determine both the MFI's decision to locate there and any other unobservable characteristics that influence individual's ability or desire to participate in the program.

V. Data

Sources

In obtaining the data to carry out this investigation, the participation of COOCRETAL and CREDICOOP administrators is imperative. Although I have not contacted them directly, I think that the benefit of this study would be particularly valuable to them and that they would be willing to share basic information about loan disbursements, pledged savings account balances, repayments, and approval/denial decisions. The benefit of dealing with these two organizations is that one would have access (hopefully) to centralized information covering eight different branches each; however, if participation can not be secured, this study could easily proceed with the participation of other MFIs in Chile because the source of variation exploited here is not unique to these two programs.

The more important source of data regards the outcomes of the treatment and control groups. Assuming that one can obtain the contact information from the coops for individuals in both groups, a household survey would have to be administered to a sample for each village. Although it would be feasible to collect monthly time-series

data, this study will probably employ a cross-section to begin with. The survey should be administered long enough after the credit is disbursed to the treatment group so that the funds can be put to productive use (maybe a few weeks). Since applications are ongoing, and it is probably more common for short-lived businesses to apply for microcredit, one should be able to amass a substantial sample of individuals over a short time period.

The survey will be designed to obtain information on the outcome and control variables mentioned above, with every effort to obtain honest responses while maintaining the anonymity of the respondents. This information will be coupled with information from the MFI and any other potential sources (such as the municipality, etc.) so that various hypotheses may be tested. Particularly whether or not there is a significant relationship between program participation and the outcome variables. It is also of interest whether gender is important in explaining the outcomes, as well as group vs. individual lending.

Power

Given that this is a quasi-experimental design in which "subjects" are separated into treatment and control groups, there are some calculations that need to be done to determine the number of observations needed per village so that the results have significant power. "In a study comparing two groups, power is the chance of rejecting the null hypothesis that the two groups share a common population mean and therefore claiming that there is a difference between the population means of the two groups, when in fact there is a difference of a given magnitude," (Optimal Design User's Manual).

The values of the following parameters must be known (or assumed) in order to determine the power of the experiment: 1.) the cluster size, n (the number of participants in each cluster), 2.) the total number of clusters, J, 3.) the intra-class correlation coefficient, ρ (the variability between clusters), and 4.) the standardized effect size δ. In order to make an informed decision about the parameter values without the luxury of having done a pre-test, I needed to make some assumptions and use existing aggregate data on regional demographics, income, etc., for the regions where the MFIs are located. The parameter values that I determined will be presented below, followed by a brief explanation of how I obtained the numbers or why I made a particular assumption.

The number of participants, n, per cluster is the unknown in my optimization problem. Given that I chose to focus my attention on the eight branches, respectively, of COOCRETAL and CREDICOOP, there are 16 clusters (J), half of which will comprise the control group and half the treatment group. Thus, I need to determine how many households I should survey in each of the 16 villages as a function of ρ and δ.

According to the Optimal Design User's Manual, "The intra-class correlation, ρ, is a ratio of the variability between clusters to the total variability: $\rho = \frac{\tau}{\tau + \sigma^2}$ where τ is the variation between clusters, σ^2 is the variation within clusters, and $\tau + \sigma^2$ is the total variation. Because $\tau + \sigma^2$ is the total variation, we can constrain it to be 1. Algebraic manipulation of the formula then reveals $\rho = \tau$ and $1 - \rho = \sigma^2$." Thus, our estimate of ρ will depend on the variance across villages of our outcome variable(s). Since village-

level data is difficult to obtain, I can use regional data to calculate the variance of one of the outcome variables across clusters as a rough approximation. For the percent of the population living under the poverty line, the variance between clusters is 0.00065; if we normalize the total variation to be 1, then $\rho = 1 - \sigma^2 = 0.99935$. This would be nice, but it cannot be true. This occurred because several of the MFI branches are in Santiago and the data is not disaggregated, so it creates a deceivingly invariant series. According to the Optimal Design User's Manual, ρ typically ranges between 0.05 and 0.10 for U.S. data sets on school achievement. It seems natural that there is a reasonable amount of variance between clusters, but not too much because the program locations are targeted to areas with similar demographics. Since I can't conduct a pre-test to get more accurate estimates, I'll check whether the optimal n changes much over different parameter values.

In situations where a lack of funds prevents an experiment from having sufficient power (which applies here), one can include a cluster-level covariate to decrease the number of clusters needed to obtain a certain power level. In this study, I will include the covariate for the fraction of the regional population that is indigent, which has a 0.97 correlation with the dependent variable that I used to calculate the intra-cluster correlation coeff. ρ.

The last parameter to be estimated is δ, the standardized effect size. This parameter is "the population means difference divided by the standard error of the outcome:

$$\delta = \frac{\gamma_{01}}{\sqrt{\tau + \sigma^2}}$$

where $\gamma_{01} = \mu_E - \mu_C$, μ_E is the population mean for the experimental group, and μ_C is the population mean for the control group." It is common for small standardized effects, such as 0.20 to 0.30, to be worth reporting (Optimal Design User's Guide). Karlan and Goldberg (2006) imply that even a standardized effect of 0.10 is important in this context.

Given the number of clusters, J, equal to 16, values of the intra-class relation, ρ, of 0.10 and 0.20, standardized effect sizes, δ, of 0.20 and 0.40, and a 95% confidence level, Figure V graphs the power of the experiment over different numbers of participants per cluster, n. Given that acceptable power levels are generally 0.80 or above, I would need to sample between 15 and 60 households *per village* in order to have sufficient power depending on the value of the parameter δ. Clearly, I need to obtain more precise data.

VI. Conclusion

This proposed research presents a unique opportunity to provide unbiased estimates of the impact of microfinance programs on outcomes such as business profits, consumption, child education, and health by exploiting differences in program requirements between two large cooperatives in Chile. While the literature on MFIs in Asia is often contradictory, the literature on MFIs in SA is much smaller and even less rigorous, emphasizing the need for more research on this region (Weiss and Montgomery, 2005). As microcredit programs continue to expand all over the continent, NGO's and policymakers praise them for reducing poverty and creating employment; maybe there's something they're hiding from us economists. I'd like to find out.

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Figure I. Independent Microfinance Institutions (MFIs) in Chile

Source: Catastro de Instituciones Crediticias (PET, 2002)

General Information			Loan Requirements										
	Acronym	Full Name	Credit?	Pledged Savings?	# Branches	Collateralized?	Years in Oper.*	# Workers***	Location?	Other Sources?^	Loan Limit^^	Maturity Limit^^^	Interest Rate^^^^
Savings and Credit Cooperatives:													
1.)	COOCRETAL	Cooperativa de Ahorro y Credito Talagante Ltda.	Yes	Yes	8	Yes	1.00	5	Yes	No	$2,000	18	2.1%
2.)	CREDICOOP	Cooperativa de Servicios Financieros a la Micro.	Yes	Yes	8	Yes	0.50	n/a	Yes	Yes	n/a	24	2.8%
Non-Governmental Organizations (NGOs):													
1.)	CECADES	Centro de Capacitacion y Desarrollo Economico	Yes	No	1	Yes	1.00	n/a	n/a	n/a	n/a	n/a	n/a
2.)	FINAM	Financiera de la Mujer	Yes	No	1	No	1.00	n/a	n/a	n/a	n/a	n/a	n/a
3.)	n/a	Fundacion Ayuda y Esperanza	Yes	No	2	No	n/a	n/a	n/a	n/a	$200	n/a	n/a
4.)		Fundacion Contigo	Yes	No	3	Yes	0.50	n/a	n/a	n/a	$1,000	12	n/a
5.)	FUNDA	Fundacion para el Desarrollo Regional de Aysen	Yes	No	1	Yes	n/a	n/a	n/a	n/a	$1,000	36	n/a
6.)	SOINTRAL	Fundacion SOINTRAL	Yes	No	5	No	n/a	n/a	n/a	n/a	n/a	n/a	n/a
7.)	TPH	Fundacion Trabajo para un Hermano Atacama	Yes	No	9	Yes	0.25	n/a	Yes	n/a	$500	n/a	n/a
8.)	TPH	Fundacion Trabajo para un Hermano Concepcion	Yes	No	3	Yes	0.50	n/a	n/a	n/a	n/a	n/a	n/a
9.)	TPH	Fundacion Trabajo para un Hermano Santiago	Yes	No	4	Yes	n/a	n/a	Yes	n/a	n/a	n/a	n/a
10.)	INDES	Inversiones para el Desarrollo	Yes	No	2	No	n/a	n/a	Yes	n/a	n/a	n/a	n/a
11.)	OCAC	Oficina Coordinadora de Asistencia Campesina	Yes+	No	7	No	n/a	n/a	n/a	n/a	n/a	n/a	n/a
12.)	PROPESA	Corporacion de Promocion para la Pequena Em.	Yes	No	3	No	1.00	n/a	n/a	n/a	$300	12	n/a

+This organization makes loans in goods only (no cash).

*Businesses must currently be in operation to apply; **Monthly figures in U.S. dollars; ***Number of salaried workers only.
^Does borrower have access to conventional loans; ^^Maximum loan in U.S. dollars; ^^^In months; ^^^^Monthly interest rate.

Figure II. Catalogue of Microcredit Enterprises Operating in Chile

Source: Catastro de Instituciones Crediticias (PET, 2002)

Name	Full Name	Type	Products Offered	Est.	HQ*/Branch City	Reg.
OCAC	Oficina Coordinadora de Asistencia Campesina	NGO	Goods/Training	-	Arica	I
OCAC	Oficina Coordinadora de Asistencia Campesina	NGO	Goods/Training	-	Iquique	I
SOINTRAL	Fundacion Sointral	NGO	Credit	1988	Antofogasta	II
SOINTRAL	Fundacion Sointral	NGO	Credit	1988	Chuquicamata	II
SOINTRAL	Fundacion Sointral	NGO	Credit	1988	Copiapo	III
TPH	Fundacion Trabajo para un Hermano Atacama	NGO	Credit	1988	Copiapo	III
SOINTRAL	Fundacion Sointral	NGO	Credit	1988	La Serena	IV
COOCRETAL	Cooperativa de Ahorro y Credito Talagante Ltda.	Coop.	Credit/Savings	1960	San Antonio	V
CREDICOOP	Cooperativa de Servicios Financieros a la Microempresa	Coop.	Credit/Savings	1986	Valparaiso	V
INDES	Inversiones para el Desarrollo	NGO	Credit	-	Valparaiso	V
CECADES	Centro de Capacitacion y Desarrollo Economico y Social	NGO	Credit/Capacitation	1998	Santiago*	RM
FINAM	Financiera de la Mujer	NGO	Credit/Capacitation	1989	Santiago*	RM
-	Fundacion Contigo	NGO	Credit/Capacitation	1990	Santiago*	RM
-	Fundacion Contigo	NGO	Credit/Capacitation	1990	Lo Espejo	RM
-	Fundacion Contigo	NGO	Credit/Capacitation	1990	Buin	RM
SOINTRAL	Fundacion Sointral	NGO	Credit	1988	Santiago*	RM
TPH	Fundacion Trabajo para un Hermano Atacama	NGO	Credit	1988	Tierra Amarilla	RM
TPH	Fundacion Trabajo para un Hermano Atacama	NGO	Credit	1988	Caldera	RM
TPH	Fundacion Trabajo para un Hermano Atacama	NGO	Credit	1988	Vallenar	RM
TPH	Fundacion Trabajo para un Hermano Atacama	NGO	Credit	1988	Huasco	RM
TPH	Fundacion Trabajo para un Hermano Atacama	NGO	Credit	1988	Chanaral	RM
TPH	Fundacion Trabajo para un Hermano Atacama	NGO	Credit	1988	Diego de Almagro	RM
TPH	Fundacion Trabajo para un Hermano Atacama	NGO	Credit	1988	Alto del Carmen	RM
TPH	Fundacion Trabajo para un Hermano Atacama	NGO	Credit	1988	Freirina	RM
TPH	Fundacion Trabajo para un Hermano Santiago	NGO	Credit/Capacitation	1988	Penalolen*	RM
TPH	Fundacion Trabajo para un Hermano Santiago	NGO	Credit/Capacitation	1988	Cerro Navia	RM
TPH	Fundacion Trabajo para un Hermano Santiago	NGO	Credit/Capacitation	1988	San Joaquin	RM
TPH	Fundacion Trabajo para un Hermano Santiago	NGO	Credit/Capacitation	1988	Huechuraba	RM
INDES	Inversiones para el Desarrollo	NGO	Credit	-	Santiago	RM
OCAC	Oficina Coordinadora de Asistencia Campesina	NGO	Goods/Training	-	Santiago*	RM
PROPESA	Corporacion de Promocion para la Pequena Empresa	NGO	Credit/Capacitation	-	Santiago*	RM
COOCRETAL	Cooperativa de Ahorro y Credito Talagante Ltda.	Coop.	Credit/Savings	1960	Talagante*	RM
COOCRETAL	Cooperativa de Ahorro y Credito Talagante Ltda.	Coop.	Credit/Savings	1960	Santiago	RM
COOCRETAL	Cooperativa de Ahorro y Credito Talagante Ltda.	Coop.	Credit/Savings	1960	Maipu	RM
COOCRETAL	Cooperativa de Ahorro y Credito Talagante Ltda.	Coop.	Credit/Savings	1960	Penaflor	RM
COOCRETAL	Cooperativa de Ahorro y Credito Talagante Ltda.	Coop.	Credit/Savings	1960	El Monte	RM
COOCRETAL	Cooperativa de Ahorro y Credito Talagante Ltda.	Coop.	Credit/Savings	1960	Melipilla	RM
CREDICOOP	Cooperativa de Servicios Financieros a la Microempresa	Coop.	Credit/Savings	1986	Recoleta	RM
CREDICOOP	Cooperativa de Servicios Financieros a la Microempresa	Coop.	Credit/Savings	1986	Quinta Normal	RM
CREDICOOP	Cooperativa de Servicios Financieros a la Microempresa	Coop.	Credit/Savings	1986	San Joaquin	RM
CREDICOOP	Cooperativa de Servicios Financieros a la Microempresa	Coop.	Credit/Savings	1986	Santiago Centro	RM
COOCRETAL	Cooperativa de Ahorro y Credito Talagante Ltda.	Coop.	Credit/Savings	1960	Rancagua	VI
OCAC	Oficina Coordinadora de Asistencia Campesina	NGO	Goods/Training	-	Litueche	VI
OCAC	Oficina Coordinadora de Asistencia Campesina	NGO	Goods/Training	-	Lolol	VI
PROPESA	Corporacion de Promocion para la Pequena Empresa	NGO	Credit/Capacitation	-	Rancagua	VI
CREDICOOP	Cooperativa de Servicios Financieros a la Microempresa	Coop.	Credit/Savings	1986	Curico	VII
CREDICOOP	Cooperativa de Servicios Financieros a la Microempresa	Coop.	Credit/Savings	1986	Concepcion	VIII
-	Fundacion Ayuda y Esperanza	NGO	Credit	-	Concepcion	VIII
TPH	Fundacion Trabajo para un Hermano Concepcion	NGO	Credit	1988	Concepcion	VIII
TPH	Fundacion Trabajo para un Hermano Concepcion	NGO	Credit	1988	Curanilahue	VIII
TPH	Fundacion Trabajo para un Hermano Concepcion	NGO	Credit	1988	Barrio Norte	VIII
OCAC	Oficina Coordinadora de Asistencia Campesina	NGO	Goods/Training	-	Los Angeles	VIII
PROPESA	Corporacion de Promocion para la Pequena Empresa	NGO	Credit/Capacitation	-	Concepcion	VIII
CREDICOOP	Cooperativa de Servicios Financieros a la Microempresa	Coop.	Credit/Savings	1986	Temuco	IX
-	Fundacion Ayuda y Esperanza	NGO	Credit	-	Temuco	IX
OCAC	Oficina Coordinadora de Asistencia Campesina	NGO	Goods/Training	-	Osorno	X
FUNDA	Fundacion para el Desarrollo Regional de Aysen	NGO	Credit/Capacitation	1976	Coyhaique	XI

Figure III. The Regions of Chile
Source: Wikipedia

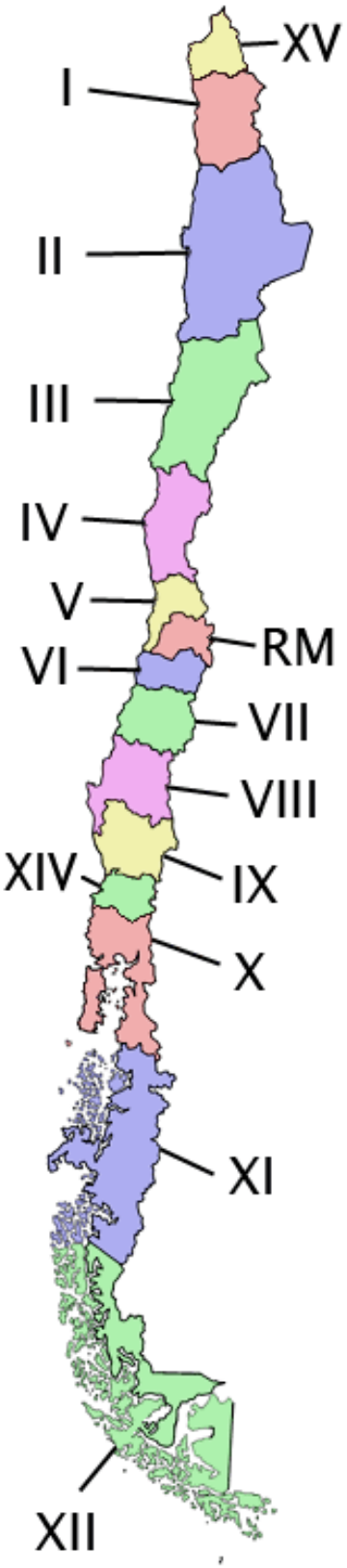

Figure IV. Distribution of MFI branches across Chile

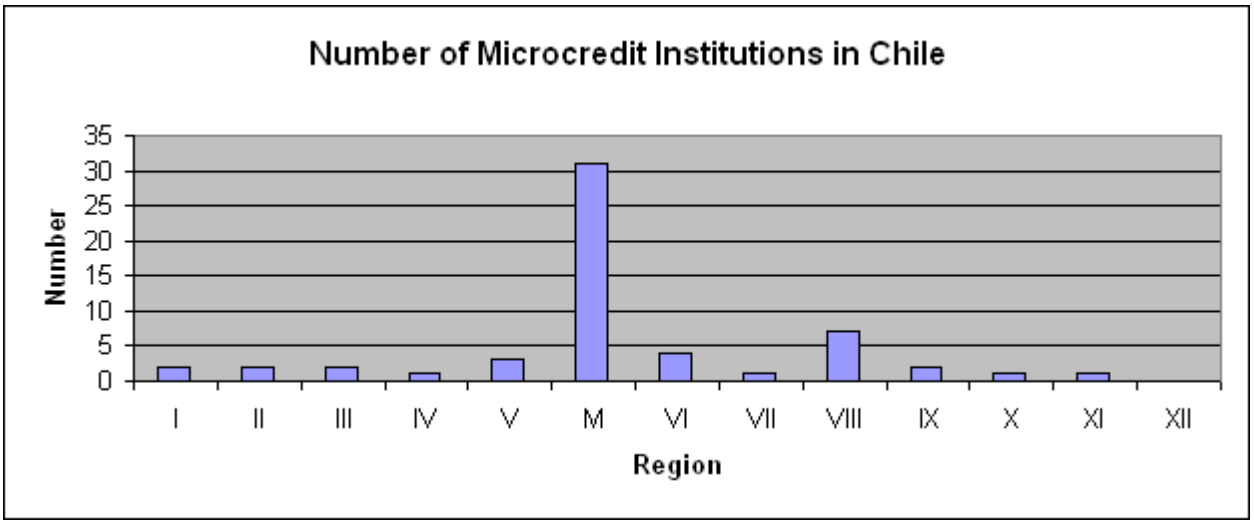

Figure V. Power