Microfinance Impact: Bias from Dropouts

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Cross Sectional Impact Analysis: Bias from Dropouts*

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Abstract

Several microfinance organizations have begun using a management tool, developed by Assessing the Impact of Microenterprise Services (AIMS) at the United States Agency for International Development (USAID), to assess impact. This tool recommends comparing veteran members to new members of a microcredit program, and attributes any difference to the impact of the program. The tool introduces a potential source of bias into estimates of impact by not instructing organizations to include program dropouts in their calculations. This paper uses data from a longitudinal study in Peru of Mibanco borrowers and non-borrowers to quantify some, but not all, of the biases in the cross-sectional approach. In these data, not including dropouts overestimates the impact of the credit program. Furthermore, we find that the sample composition shifted over the two years, introducing further bias into a cross-sectional impact assessment.

Keywords: microfinance, impact methodologies, program evaluation, attrition, Peru

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1. Introduction

Microfinance institutions receive tremendous amounts of development aid and publicity.[1] Studying the impact of such programs always has been a critical question from both a policy perspective (e.g., should funds be allocated to such programs) as well as a monitoring perspective (e.g., is a particular organization achieving its mission). In an attempt to conduct impact evaluations with limited resources, some organizations now use a cross-sectional empirical methodology (hereinafter referred to as the 'cross-sectional approach') developed by the United States Agency for International Development (USAID), under its Assessing the Impact of Microenterprise Services (AIMS) program. A few examples of such studies using this methodology also exist in academic journals and conference papers (Copestake, Bhalotra et al. 2001; Maldonado, Gonzales-Vega et al. 2002; Noponen 2005).[2] This paper will compare the results obtained using a slight but important change: we include includes dropouts in the analysis. We refer to this approach as the 'recalculated cross-sectional approach.' The point of this paper is *not* to estimate the impact of a program, but rather to provide an empirical example of the consequence of ignoring the biases inherent in a cross-sectional impact assessment for microfinance institutions.

The fundamental goal of any impact assessment is to estimate the effect of an intervention in relationship to the counterfactual—what would have happened in the absence of the intervention. A common technique to achieve this is to compare program participants to a group of similar non-participants. The difficulty is to find groups that are sufficiently comparable. The best way to achieve this is through a randomized controlled trial, which ensures that the treatment group, on average, is identical to the control group except for the impact of the intervention, as in Karlan and Zinman (2006). However, we do not generally observe MFIs randomly giving credit to some members of a community. With most impacts assessments in microfinance, the major hurdle is that the treatment group was self-

[1] CGAP, the association of donor agencies investing in microfinance, estimates in its *Donor Guidelines on Good Practice in Microfinance* that the donor community spends US$800 million - $1 billion per year on microfinance.

[2] Note that these studies, in particular Copestake et al (2001) and Noponen (2005), employ a triangulation of methodologies, only one of which is the cross-sectional approach discussed in detail here.

selected—borrowers chose to enter a credit program in a non-random way. If unobservable characteristics, such as entrepreneurial spirit, also cause an individual to choose to borrow, any observed differences in outcomes between borrowers and non-borrowers will be biased. Quasi-experimental techniques based on program expansion have been used on cross-section data with some success (Coleman 1999). Alternatively, panel data is another way to control for initial differences between the groups (Copestake, Dawson et al. 2005; Khandker 2005; Tedeschi 2006).

However, quasi-experimental methods are not always available, panel data is expensive and too time-consuming for most MFIs to gather, and experimental methods sometimes are not feasible and other times require expertise not available to the microfinance institution. Thus, the AIMS program, along with the Small Enterprise Education and Promotion Network (SEEP, a consortium of non-governmental organizations that support microfinance), developed a set of low-cost tools for practitioners to use to study impact (Nelson et. al., 2004). One of these tools, the cross-sectional impact assessment, is what we study in this paper. The cross-sectional approach argues that one can measure impact by comparing veteran borrowers of a microcredit program to incoming clients that have yet to receive loans. The difference between the two groups is labeled impact due to the program. One drawback of these types of studies is the lack of a control group of non-borrowers, meaning that the entire sample has self-selected into the treatment group. Without a control group of non-borrowers, we cannot say if credit is beneficial, *in general*, or if it is successful because it has been given to the most entrepreneurial people. This type of assessment, by averting one self-selection problem, can tell the lending institution if they are assisting the microentrepreneurs who *choose* to become clients.

The stated justifications for the cross-sectional approach are twofold: (1) since both veteran borrowers and new borrowers are self-selected into the program, evaluators can avoid the selection bias inherent in comparing clients to non-clients; (2) the evaluation will be less costly since all analysis is performed on existing clients only, not on a control group of non-clients. Karlan (2001) discusses why this approach is flawed. In its attempt to correct for one bias it introduces several new biases. He we demonstrate a method to improve upon the cross-section approach by adding program dropouts to the analysis. Of course, many biases still exist; we do not put forward this approach as an actual recommended approach to generate unbiased estimates, but merely as an improvement to an otherwise popular approach.

This paper uses a longitudinal data set to produce cross-sectional impact results, and then we quantify one of the biases inherent in this approach. To measure dropout bias, we recalculate impact on a data set that includes dropouts: rather than comparing those who have been in the program for two years (veterans) to those who just entered, one could compare those who *joined* two years ago (veterans and dropouts) to those who just entered. This change is one suggested by Karlan (2001) as a means to address some of the biases inherent in the cross-sectional approach. In several cases, we find that correcting the sample frame by including the dropouts lowers the estimated impact. This could be because those who are made worse off from participating drop out of the program or perhaps because the poor are more likely to drop out of the program. Either way, the cross-sectional impact strategy upwardly biased impact. While the suggested change to the methodology will undoubtedly improve the accuracy of the impact estimates using the cross-sectional approach, it should be stressed that it does not correct all the biases of the cross-sectional approach.

Section 2 summarizes the biases discussed in Karlan (2001). Section 3 describes the data. Section 4 analyzes the data using first the cross-sectional approach (without accounting for dropout), and then with the recalculated cross-sectional approach (after correcting for dropout). This section also examines whether evidence exists to support other assumptions inherent in the cross-sectional methodology, such as the requirement that selection criteria (whether by the lender or by individual self-selection) do not change over time. Section 5 concludes and makes policy recommendations.

2. Summary of Perils of Cross-Sectional Impact Assessment

Karlan (2001) details three types of problems with the cross-sectional impact assessment. This section summarizes the arguments put forth in that paper. First, a dropout problem exists in which dropouts are included in the comparison group but excluded from the treatment group. Second, a timing problem exists, in which the cross-sectional methodology assumes that individuals in the comparison group do not differ from those who chose to join the program earlier, except for the effect of the program. Third, the methodology requires the following assumption: no changes occur over time in program placement strategy, targeting or credit evaluation.

Incomplete Sample Bias

Suppose there are two types of participants, those who benefit from participation and those who are made worse off. Those who benefit stay in the program. Those who are made worse off leave the program. By only including in the treatment group those who remain in the program, those with negative impact are ignored. The cross-sectional impact analysis would find a positive impact, whereas the true impact depends entirely on the relative size of these two groups and their relative impacts. If one wants to know the net impact of a microfinance program, one must include the failures along with the success stories. After all, we would all be perfect if we ignored our failures when evaluating ourselves.

On the other hand, the most successful entrepreneurs may 'graduate' from the program. In this case, the cross-sectional methodology underestimates the impact. Which is right? We cannot say, but fortunately there is a relatively simple fix which works either way: include the dropouts in the treatment group. The next section of this paper will explain how to do that.

Attrition Bias

Suppose for the moment that the program has no impact whatsoever, neither positive nor negative, on any participant. Yet some individuals dropout and others remain. If the rich are more likely to leave (perhaps the weekly meetings are more costly to them than they are to the poor), then the cross-sectional approach will underestimate the impact. The story behind this is different than the incomplete sample problem, but the net result is the same: the sample composition of the treatment and control groups is fundamentally different because the treatment group does not include those who left, whereas the control group includes those who will eventually leave. The solution is the same as for the incomplete sample problem: include the dropouts in the treatment group.

Timing of Decision Problem

Why does someone join a credit program now rather than 2 years ago? We do not know, but we intuit that there is a reason, and it is significant. We will examine this in the empirical analysis of this paper by comparing the two groups of individuals from the same baseline survey: (a) individuals who joined two years ago (at the time of the baseline survey), and (b) individuals who did not join two years ago, but did join by the time of the follow-up survey.

The empirical analysis is necessary in order to address several conceptual problems. Imagine that individuals join after coming to an epiphany that they must grow their business in order to pull themselves out of poverty. Or perhaps participants join when everyone in their household is healthy, and hence does not need constant care in the home. Such a situation suggests that perhaps access to credit is not the problem, but rather access to good health care. If ample opportunities exist for credit and savings in their community, then attributing the improvement in their lives to the microfinance institution would be erroneous. Their epiphany or their family's health may be the *causal* agent that led to the improved welfare, not the access to that particular source of credit.

One way to address this problem is to analyze the alternatives for credit and savings that borrowers have in their communities. Since social networks can create both credit (such as informal loans) and savings (Rotating Savings and Credit Associations, ROSCAs) opportunities, evaluating a client's next best alternative is not an easy task. Further research to understand the informal opportunities to borrow and save is essential for understanding the seriousness of the timing problem.

<u>Program & Credit Evaluation Changes</u>

The program itself may change in the way it finds new clients. This could be because the lender entered the poorest neighborhoods first, or perhaps those closest to the major highway first, or perhaps because the lender changed their credit evaluation process as they became more experienced in the area. For example, the lender could start to check potential clients against lists provided by a credit bureau. Or a seemingly unrelated change could cause the groups to differ, such as a change in the interest rate charged, which could attract a different profile of client, (for example, lowering interest rates might attract wealthier clients, who have more financial alternatives). Any of these changes would cause the comparison of current clients to incoming clients to be biased. See Karlan (2001) for further discussion of these issues.

In the data, this problem would exhibit itself similarly to the 'timing of decision' problem. If changes in program and credit evaluation affected the type of client who joined two years ago, then we will observe differences in the baseline data between those who joined initially and those who joined later. We examine this in the empirical section below.

3. Data

The data used in this study were also collected through the AIMS program but are not related to the cross-sectional studies we discuss here. These longitudinal data were gathered in a more intense exercise to assess impact on individuals who borrow, as well as the effects on their households and microenterprises.[3] We use these data because they allow us to reconstruct the cross-sectional approach with and without the inclusion of dropout, and to compare the profiles of clients who join the program earlier to those who joined later. The sample consists of borrowers from *Mibanco*, a microfinance institution (MFI) operating in Lima, Peru, and a control group of households which did not borrow from any source but were eligible to receive a Mibanco loan.[4] Two waves of data were collected: a baseline in 1997, and a follow-up on the same households in 1999. In both years two surveys were administered—a household survey that measured variables including employment, education, consumption and savings data at the household level, and a microenterprise survey that gathered data such as profits, assets, and business practices, on up to three microenterprises operated by the household. The variables chosen for this study follow those that Dunn and Arbuckle (2001) measured in the project's final report, which breaks the analysis down into three categories: individual, household and enterprise.

[3] More information on AIMS and the data used in this survey can be found at http://www.MicroLinks.org. Data and surveys are also available at this site.
[4] To be eligible for a Mibanco loan, the borrower's business must have been in operation for at least six months.

A random sample of borrowers in three credit districts throughout Lima was selected, matching the sectoral composition of the sample to that of Mibanco's entire borrower base.[5] In the baseline survey, the sample consisted of 400 borrower households and 301 non-borrower (control) households. In the second round of data collection, enumerators found 305 original borrower households (76%) and 213 non-borrower households (71%) that agreed to be resurveyed.[6] Of these, 92 households chose to close their microenterprises due to insufficient revenue, lack of capital, illness, death or to take a full time or salaried job. These households are excluded from the enterprise and individual analysis due to an absence of data, but are included for household variables.

These data allow us to test for dropout bias because it contains dropouts in the 1999 round of the survey and several members of the 1997 control group who began borrowing by 1999. We use the 1999 round of survey data to reproduce the cross-sectional impact analysis, using the 1997 information only to categorize the observations as veterans (Group #1), dropouts (Group #2) and new entrants (Group #3).

The number of observations in each group differs by the level of analysis. Sample sizes are lowest for the individual outcomes (found in Table 3) since most of these questions were taken from the enterprise survey and not all primary respondents had enterprises in 1999. The number of observations for the savings variable is higher, as it was asked of all respondents in the household survey. For individual-level impacts, the primary respondent must be a borrower in a given period for the observation to be categorized as 'borrower.' For household and enterprise-level results (Tables 1 and 2), if any member of the household has a loan the observation is considered 'borrower' due to fungibility of money within household. Some enterprise impact variables are about the primary enterprise only or aggregated for each household, leading to the smaller sample sizes (Panel A). Other enterprise impacts are measured at the firm level (Panel B), leading to significantly more observations since up to three enterprises were recorded per household. For the household data, sample sizes are lower for coping questions, which were

[5] See Dunn and Arbuckle (2001) for complete details on the survey process.

[6] Slightly over half of the panel leavers could not be located, meaning that the household had moved and their microenterprises either closed or moved. For those that were located, but not surveyed, the most common reason was distrust of the surveyors, although other reasons such as lack of time, being away from the household for extended periods, sickness and death, were also reported.

only asked of households who had reported a negative economic shock, and for school and intergenerational launching (helping a daughter or son start their own enterprise) questions, which were only asked of households with school age children.

4. Empirical Analysis

Dropout Bias

In the first analysis, we address the incomplete sample bias and sample attrition bias using the 1999 survey data to construct the impact assessments. The 1997 data is only used to identify groups, so that we can compare results excluding dropouts ('cross-sectional approach'), and including dropouts ('adjusted cross-sectional approach'). The cross-sectional methodology compares mean outcomes in 1999 for Group #1 to Group #3, or the veterans to the new entrants. To create the adjusted cross-sectional approach, we replace the Group #1 veterans, with a weighted average of Groups #1 and #2 (veterans and dropouts) in order to include *all* who were borrowers in 1997, not just those who stayed in the program.[7]

Comparing means is equivalent to the following OLS specification:

$Y_i = \alpha+\beta T_i+\varepsilon_i$,

where T_i is an indicator variable equal to one if the weighted observation is from Groups #1 or #2, and is is equal to zero if the observation is from Group #3.

Tables 1 through 3 show the results for this analysis, for enterprise outcomes, household outcomes and individual outcomes, respectively. Column 1 shows the mean outcome for the veterans (Group #1), Column 2 shows the mean outcome for the dropouts (Group #2) and Column 3 shows the mean outcome for the new entrants (Group #3). Column 5 then shows the cross-sectional results comparing veterans to new entrants. Column 6 shows the adjusted cross-sectional approach, comparing 'veterans *and* dropouts' to new entrants. Column 7 reports the t-statistic on the null hypothesis that no dropout bias exists in the cross-sectional methodology.

[7] Weights are used because the sample of dropouts (Group #2) was smaller for this sample than it is estimated to be for the population as a whole. Dropout for non-delinquent Mibanco borrowers is reported as 56% per cent in Dunn and Arbuckle (2001, p: 88).

First, note that for the many of outcomes no measurable difference is observed between veterans and new entrants. Although the difference between methodologies is often not significant statistically (the sample is very small), there is typically an upward bias for the cross-sectional method without taking dropouts into account.

The most striking change is in the first row of Table 1, Yearly Enterprise Profit from All Microenterprises. The cross-sectional methodology estimates an increase of 4083 nuevos soles per year[8], which is about US$1200. The recalculated methodology estimates a *decrease* of 588 nuevos soles per year. Another example: if we look at total employment over the top three enterprises operated by a veteran household, the cross-sectional methodology finds that veterans have 14.6 more days worked per month, whereas the recalculated methodology estimates an increase of only 4 days per month.

Table 2 shows parallel results, a systematically upward bias to the cross-sectional methodology. In the first row, Household Income from All Sources, we see a change from 6569 nuevos soles impact to only 2062 nuevos soles impact. Similarly stark differences are found for answers to qualitative questions about coping strategies. The cross-sectional methodology finds borrowers 1% less likely to employ 'bad' coping strategies, such as selling or pawning productive assets, whereas the adjusted methodology finds borrowers 7% *more* likely to employ bad coping strategies. For poor households, the difference is even starker, going from 0% likely to employ bad strategies to 12%.

Selection Bias

Next, we examine whether the composition of new borrowers changed over the two years, hence invalidating another key assumption of the cross-sectional methodology. This assumption could fail due to the 'timing of life' decision to participate, or due to changes in program placement or credit screening and client solicitation procedures. To do this analysis, we will compare the 1997 measures of two groups, those who entered in 1997 and those who entered in 1999. If we find that those who entered in 1999 were significantly wealthier (or poorer) in 1997 than those who entered the program in 1997, then this suggests that the selection process (whether intentional or not) indeed changed over the two years.

[8] The exchange rate at the time was 3.50, hence this is equivalent to US$1166.

Table 4 shows these results. Several differences exist. First, new entrants in 1999 are more likely to have had a formal location in 1997 than the new entrants in 1997. This is likely to underestimate the impact, since the control group members (new entrants in 1999) are likely to have larger business if the businesses are more formal. On the other hand the veterans (new entrants in 1997) were more likely to own their business premise in 1997, and this would be likely to overstate impact. The largest differences are found in the household outcomes, specifically expenditure on appliances and per pupil expenditures on education. Both indicate that the 1999 new entrants were poorer in 1997 than the 1997 new entrants were in 1997.

5. Conclusion and Recommendation

The purpose of this paper is to make one point: if conducting a cross-sectional impact assessment, then the definition of 'veterans' should *not* be `has been in the program for two years,' but rather should be *'joined the program two years ago.'* This small correction can improve the accuracy and hence reliability of the results. This is not to say all problems are removed, nor to say that a cross-sectional methodology (even one that includes dropouts) would satisfactorily measure the impact of a microcredit program. Furthermore, although it costs additional money to interview individuals who have left the program, this additional cost buys two things: (1) a more accurate impact assessment, and (2) valuable information on the causes of dropout. We believe the additional expense is money well spent, but exactly how well spent can be investigated empirically.

By replicating our analysis in more settings and determining the size and direction of the bias the microfinance community can get a clearer picture of the pattern of dropouts and the extent to which not including them will affect the results. The dropout rate among Mibanco clients, at 56% over a two year period for this sample, is quite normal, and it would be especially interesting to see results for MFIs with a different dropout rates. If for MFIs with lower dropouts rates the bias is found to be insubstantial, such MFIs can decide whether it is worthwhile to include dropouts in their analysis. Or, if in certain cases (such as MFIs with low dropout rates) the bias is found to be negative—implying either that dropouts are ‘successful graduates’ or that better off clients tend to drop out more often—such MFIs could legitimately decide not to include dropouts because the biased results would be a conservative estimate. Until further evidence exists, however, our results make it clear that not including dropouts makes a flawed methodology even more suspect. Although this paper does not discuss these issues, other articles on best practices for MFIs suggest that systematic interviewing of dropouts can help tremendously with ongoing quality control and improvement of the services the MFI is offering (Micro-Save Africa 2002).

It may be possible to distinguish between ‘dropouts’ and ‘successful graduates’ quantitatively, however. Our sample size did not allow for subgroup analysis, but future researchers could examine the direction of bias by initial income. Indeed, among clients with similar initial incomes, the direction of bias could serve as the very definition of the distinction between the two groups. If the measured impact decreases when dropouts are included, then on average the dropouts are less successful—perhaps because the program is failing them in some way—while if the inclusion of dropouts increases the estimate of impact, then perhaps clients are exiting the program because they feel their needs have been met. Evidence suggesting one or the other would be enormously valuable to MFIs.

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TABLE 1: Enterprise Outcomes

		Group 1: Client in 1997 & 1999 (1)	**Group 2:** 1997 Clients, 1999 Dropouts (2)	**Group 3:** New Client in 1999 (3)	**All 1997 Clients** (weighting columns 1 & 2) (4)	Cross-Sectional Impact: Col 1 - Col 3 & t-statistic (5)	Recalculated Impact, including dropouts: Col 4 - Col 3 & t-statistic (6)	"Cross Sectional" Impact Bias H_o: (5) = (6) (7)
Panel A								
Revenue Variables	Yearly Enterprise Profit from all microenterprises	21956.03 (22513.19)	13613.82 (12446.91)	17872.68 (32255.37)	17284.39 (18049.10)	4083.35 0.84	-588.29 -0.13	**2.11** 0.04
Revenue Variables	Monthly Gross Revenue from top 3 enterprises per household	7363.69 (12671.48)	6769.23 (23080.95)	5542.59 (11354.01)	7030.80 (19181.02)	1821.10 0.96	1488.20 0.78	0.21 0.84
Revenue Variables	Monthly gross revenue for primary microenterprise	5070.37 (9518.09)	6360.18 (24503.02)	5264.04 (11803.27)	5792.66 (19369.28)	-193.68 -0.10	528.62 0.25	-0.47 0.64
Fixed Assets	Value of fixed assets for primary microenterprise	9115.50 (18628.31)	7065.07 (13351.90)	9903.37 (30366.99)	7967.26 (15892.31)	-787.87 -0.17	-1936.11 -0.44	0.55 0.58
Fixed Assets	Value of fixed assets for all microenterprises owned by a household	14285.86 (23862.47)	9134.95 (15376.06)	10006.64 (29499.56)	11513.73 (19882.34)	4279.22 0.93	1507.09 0.35	1.04 0.30
Employment of HH and Non-HH Members	Total days worked per month for all workers in the primary microenterprise	59.94 (35.80)	51.12 (30.81)	55.85 (34.31)	55.00 (33.33)	4.09 0.69	-0.85 -0.16	1.19 0.23
Employment of HH and Non-HH Members	Total days worked per month for non-household members in the primary microenterprise	8.53 (25.76)	7.43 (20.25)	5.96 (17.17)	7.92 (22.80)	2.57 0.76	1.96 0.68	0.23 0.81
Employment of HH and Non-HH Members	Total Wages per month in Primary Microenterprise	154.89 (449.47)	157.29 (467.59)	173.23 (618.03)	156.23 (458.86)	-18.34 -0.19	-17.00 -0.18	-0.05 0.96
Employment of HH and Non-HH Members	Total days worked per month for all workers in the top 3 microenterprises	87.40 (51.44)	68.59 (43.46)	72.81 (44.20)	76.87 (47.98)	14.59 1.93	4.05 0.60	1.47 0.14
Employment of HH and Non-HH Members	Total days worked per month for non-household members in the top 3 microenterprises	11.74 (28.85)	9.39 (22.89)	9.25 (23.80)	10.42 (25.66)	2.49 0.60	1.18 0.33	0.26 0.80
	Number of Observations, max(min)	129(125)	157(135)	53(47)	286(260)			
Panel B								
Transactional Relationships	Buy mostly from wholesalers, proportion	0.75 (0.43)	0.65 (0.48)	0.78 (0.42)	0.69 (0.46)	-0.08 -1.54	-0.13 -2.17	1.40 0.16
Transactional Relationships	Sell mainly to businesses, proportion	0.09 (0.29)	0.08 (0.28)	0.07 (0.25)	0.09 (0.28)	0.03 0.83	0.02 0.70	0.26 0.79
Transactional Relationships	Operate in a formal location, proportion	0.26 (0.44)	0.19 (0.40)	0.36 (0.48)	0.22 (0.42)	-0.10 -1.57	-0.13 -2.28	0.93 0.35
Transactional Relationships	Ownership of premise, proportion*	0.68 (0.47)	0.68 (0.47)	0.52 (0.50)	0.68 (0.47)	0.16 2.24	0.16 2.40	-0.01 0.99
Transactional Relationships	Sell mostly by contract, proportion	0.25 (0.44)	0.20 (0.40)	0.14 (0.35)	0.22 (0.42)	0.11 2.15	0.08 1.72	0.80 0.42
Transactional Relationships	Business is licensed, proportion	0.49 (0.50)	0.40 (0.49)	0.45 (0.50)	0.44 (0.50)	0.04 0.66	-0.01 -0.15	1.21 0.23
	Number of Observations, max(min)	222(177)	242(190)	76(65)	464(367)			

Note: Number of observations varies due to missing observations.
* This variable has several "does not apply" responses, which causes the large differences in observations for this sectior
Columns 1 - 4: Standard deviation reported in parenthesis
Column 5: Difference between Col 1 and Col 3 reported on top line. t-statistic reported for Ho: Col 1 = Col 3 on second line.
Column 6: Difference between Col 3 and Col 4 reported on top line. t-statistic reported for Ho: Col 3 = Col 4 on second line.
Column 7: t-statistic for Ho: Cross-sectional impact (Col 5) = Recalculated cross-sectional impact (Col 6) reported on top line. p-value reported on second lin

TABLE 2: Household Outcomes

		Group 1: Client in 1997 & 1999 (1)	**Group 2:** 1997 Clients, 1999 Dropouts (2)	**Group 3:** New Client in 1999 (3)	**All 1997 Clients** (weighting columns 1 & 2) (4)	Cross-Sectional Impact: Col 1 - Col 3 & T-statistic (5)	Recalculated Impact, including dropouts: Col 4 - Col 3 & T-statistic (6)	"Cross Sectional" Impact Bias H_o: (5) = (6) (7)
Income	Household Income from All Sources	30686.71	22638.66	24117.38	26179.81	6569.33	2062.43	**1.88**
		(24363.88)	(19314.86)	(32545.37)	(22009.85)	1.34	0.45	0.06
	Per Capita Income from All Sources	6779.46	5263.00	5117.42	5930.24	1662.04	812.82	1.58
		(5422.23)	(4929.56)	(6907.67)	(5198.26)	1.59	0.83	0.12
Diversification	Inverse Simpson Index for All Households	2.36	2.32	2.06	2.33	0.30	0.28	0.25
		(0.98)	(1.07)	(1.08)	(1.03)	1.79	1.77	0.81
	Inverse Simpson Index for Non-Poor Households*	2.36	2.22	2.05	2.28	0.31	0.23	0.69
		(1.00)	(1.04)	(1.26)	(1.03)	1.30	1.00	0.49
	Inverse Simpson Index for Poor Households*	2.32	2.61	2.06	2.48	0.27	0.42	-0.82
		(0.84)	(1.10)	(0.77)	(0.99)	1.04	2.05	0.42
Food Expenditure	Per Capita Daily Food Expenditure for All Households	4.31	4.02	3.74	4.15	0.57	0.41	0.64
		(2.57)	(2.20)	(2.01)	(2.37)	1.63	1.36	0.52
	Per Capita Daily Food Expenditure for Non-Poor Households*	4.64	4.62	4.60	4.63	0.04	0.03	0.04
		(2.61)	(2.20)	(2.13)	(2.38)	0.09	0.07	0.97
	Per Capita Daily Food Expenditure for Poor Households*	2.28	2.32	2.40	2.30	-0.13	-0.10	-0.15
		(0.71)	(0.94)	(0.59)	(0.84)	-0.61	-0.64	0.88
Household Investments	Expenditure on Household Improvements in previous year	1077.17	592.12	945.80	805.54	131.37	-140.26	1.10
		(2539.89)	(1712.78)	(2859.58)	(2126.67)	0.30	-0.35	0.27
	Expenditure on Household Appliances in last 2 years	2264.48	1298.39	674.20	1723.47	1590.28	1049.28	0.98
		(5655.78)	(3707.63)	(1700.73)	(4682.06)	2.91	2.98	0.33
School	Expenditure on Education per Child in School	713.48	718.76	433.99	716.44	279.49	282.44	-0.04
		(658.30)	(793.28)	(381.44)	(735.56)	3.35	3.96	0.97
Launch	Intergenerational Launching, proportion	0.13	0.09	0.09	0.11	0.04	0.02	0.72
		(0.34)	(0.28)	(0.29)	(0.31)	0.87	0.39	0.47
	Number of observations, max(min)	129(109)	176(147)	56(49)	305(256)			
Coping Strategies	Bad Coping Strategies for All Households, proportion	0.02	0.16	0.03	0.09	-0.01	0.07	**-3.17**
		(0.12)	(0.36)	(0.17)	(0.29)	-0.41	1.81	0.00
	Bad Coping Strategies for Non-Poor Households, proportion**	0.02	0.14	0.04	0.09	-0.02	0.04	**-2.31**
		(0.13)	(0.35)	(0.20)	(0.28)	-0.52	0.90	0.02
	Bad Coping Strategies for Poor Households, proportion**	0.00	0.21	0.00	0.12	0.00	0.12	**-2.63**
		(0.00)	(0.41)	(0.00)	(0.32)	0.00	2.23	0.01
	Number of observations, max(min)	65	109	35	174			

*There are 111, 130, 241, and 34 non-poor and 18, 46, 64, and 22 poor hourseholds in *Group 1* , *Group 2* , *All 1997* , and *Group 3, r* espectively.
**There are 55, 80, 135, and 24 non-poor and 10, 29, 39, and 11 poor households in *Group 1, Group 2, All 1997, and Group 3* respectively.
Columns 1 - 4: Standard deviation reported in parenthesis.
Column 5: Difference between Col 1 and Col 3 reported on top line. t-statistic reported for Ho: Col 1 = Col 3 on second line.
Column 6: Difference between Col 3 and Col 4 reported on top line. t-statistic reported for Ho: Col 3 = Col 4 on second line.
Column 7: t-statistic for Ho: Cross-sectional impact (Col 5) = Recalculated cross-sectional impact (Col 6) reported on top line. p-value reported on second lin

TABLE 3: Individual Outcomes

		Group 1: Client in 1997 & 1999 (1)	Group 2: 1997 Clients, 1999 Dropouts (2)	Group 3: New Client in 1999 (3)	All 1997 Clients (weighting columns 1 & 2) (4)	Cross-Sectional Impact: Col 1 - Col 3 & T-statistic (5)	Recalculated Impact, including dropouts: Col 4 - Col 3 & T-statistic (6)	"Cross Sectional" Impact Bias Ho: (5) = (6) (7)
Control	Total control over enterprise profits, proportion	0.47	0.52	0.55	0.50	-0.08	-0.05	-0.58
		(0.50)	(0.50)	(0.51)	(0.50)	-0.64	-0.42	0.56
Self Esteem and Respect	Feels own contribution to household is important, proportion	0.97	0.97	1.00	0.97	-0.03	-0.03	-0.02
		(0.18)	(0.18)	(0.00)	(0.18)	-2.03	-2.87	0.98
	Thinks others feel his/her contribution is important, proportion	0.88	0.89	0.90	0.89	-0.02	-0.01	-0.14
		(0.32)	(0.31)	(0.31)	(0.32)	-0.22	-0.17	0.89
Attitude toward the future	Feel prepared for the future, proportion	0.78	0.76	0.65	0.77	0.13	0.12	0.21
		(0.42)	(0.43)	(0.49)	(0.42)	1.08	1.02	0.83
	Taking action to prepare for the future, proportion	0.88	0.80	0.80	0.84	0.08	0.04	1.15
		(0.32)	(0.40)	(0.41)	(0.37)	0.86	0.38	0.25
Number of Observations, max(min)		121(120)	124	20	245(244)			
Savings	Presence of savings, proportion	0.57	0.46	0.52	0.50	0.05	-0.02	1.19
		(0.50)	(0.50)	(0.51)	(0.50)	0.41	-0.14	0.23
Number of Observations		129	175	56	304			

Note: Number of observations varies due to missing observations.
Columns 1 - 4: Standard deviation reported in parenthesis.
Column 5: Difference between Col 1 and Col 3 reported on top line. t-statistic reported for Ho: Col 1 = Col 3 on second line.
Column 6: Difference between Col 3 and Col 4 reported on top line. t-statistic reported for Ho: Col 3 = Col 4 on second line.
Column 7: t-statistic for Ho: Cross-sectional impact (Col 5) = Recalculated cross-sectional impact (Col 6) reported on top line. p-value reported on second l

TABLE 4: Selection Bias Estimates

Compares characteristics in 1997 of new clients of Mibanco in 1997 to clients who joined between survey periods.

		1997 characteristics of clients who joined in 1997 (1)	1997 characteristics of clients who joined in 1999 (2)	Comparison of 1997 Characteristics: Col 1 - Col 2 (3)
Basic	Female, proportion	0.62	0.48	0.14
		(0.49)	(0.51)	1.31
	Primary Respondent has Baby (under 3), proportion	0.14	0.24	-0.10
		(0.34)	(0.44)	-1.16
	Number of Observations, max(min)	258(258)	25(25)	
Enterprise	Electricity, proportion	0.81	0.75	0.05
		(0.40)	(0.43)	0.89
	Yearly enterprise profit from all microenterprises	17304.07	17872.68	-568.61
		(18220.77)	(32255.37)	-0.12
	Buy mostly from wholesalers & manufacturers	0.69	0.78	-0.09
		(0.46)	(0.42)	-1.64
	Formal location	0.22	0.36	-0.14
		(0.41)	(0.48)	-2.30
	Ownership of business premise	0.68	0.52	0.15
		(0.47)	(0.50)	2.25
	Number of Observations, max(min)	385(249)	76(53)	
Household	Per capita income from all sources	5733.5	5117.4	616.06
		(4768.70)	(6907.67)	0.63
	Expenditure on household appliances	1852.1	674.2	1177.90
		(5001.71)	(1700.73)	3.05
	Per pupil expenditure	723.01	433.99	289.02
		(721.72)	(381.44)	3.96
	Bad coping strategies for all households	0.11	0.03	0.08
		(0.31)	(0.17)	2.01
	Bad coping strategies for non-poor households*	0.10	0.04	0.05
		(0.30)	(0.20)	1.08
	Bad coping strategies for poor households*	0.12	0.00	0.12
		(0.33)	(0.00)	2.20
	Number of Observations, max(min)	257(221)	56(49)	
Individual	Feels own contribution to HH is important	0.97	1.00	-0.03
		(0.18)	(0.00)	-2.66
	Feel prepared for the future	0.77	0.65	0.12
		(0.42)	(0.49)	1.06
	Taking action to prepare for the future	0.83	0.80	0.03
		(0.38)	(0.41)	0.32
	Number of Observations, max(min)	207(205)	20(20)	

Note: Number of observations varies due to missing observations. *There are 110 non-poor and 35 poor households among new clients in 1997, and 24 non-poor and 11 poor households among new clients in 1999.
Columns 1 - 2: Standard deviation reported in parenthesis.
Column 3: Difference between Col 1 and Col 2 reported on top line. T-statistic reported for H_0: Col 1 = Col 2 on second line.