The Impact of Short-Term Credit on Microenterprises: Evidence from the *Fincomun-Bimbo* Program in Mexico

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I. Introduction

The United Nations declared 2005 the "International Year of Microcredit." The declaration was meant both to celebrate the achievements of microcredit and to spur its further advance. The successful microcredit organizations are well known: among others, Grameen Bank of Bangladesh, Bank Rakyat of Indonesia, and Banco Sol in Bolivia have shown that lending to the poor might be profitable. But what is the effect of microcredit among the borrowers? Does microcredit eliminate financial constraints, thereby allowing poor households and entrepreneurs to acquire assets, start businesses, finance emergency needs, and insure themselves against negative shocks? On these questions, there is surprisingly little evidence. In a recent review of impact studies, Armendariz de Aghion and Morduch (2005, 199) note that "the number of careful impact studies is small but growing, and their conclusions, so far, are [measured]." Armendariz de Aghion and Morduch's review of the existing studies suggests that those showing the strongest impacts are also those with the largest methodological flaws, while those that are cleanest methodologically generally show little or no positive impact. The review of the existing literature underscores the need for a more thorough analysis of the impact of microlending.

One reason reliable studies of impacts are few is that the growth of microlending programs is seen as evidence in itself of their impact.[1] The mere fact

We wish to thank *Fincomun* and *Bimbo* authorities for their support and Craig McIntosh, two referees, and the editor for helpful comments. Financial support from the Ford Foundation and the University of California Institute for Mexico and the United States (UC MEXUS) is gratefully acknowledged.

[1] However, striving for sustainability, many practitioners are starting to recognize the need for impact assessments as an instrument to help keep and attract new clients.

 0013-0079/2008/5604-0002$10.00

that there is a demand for their services provides clear-cut evidence of their usefulness, in this view, and therefore more detailed impact assessments are unnecessary. From this perspective, the high real interest rates charged on typical microloans is a sufficient indicator of the marginal benefits to borrowers. Given the perception of a scarcity of lendable funds, many microfinance participants believe that resources that could be used to foster microfinance activities should not be diverted to impact studies.

From the perspective of research on small-scale enterprises, the scarcity of microfinance impact assessments is unfortunate. A lack of finance is often mentioned as the main constraint to the growth of microenterprises. Many microlending organizations were formed specifically to address this need. Enterprise-focused loan programs provide one lens through which to examine the dynamics of small-scale enterprises. Do loans allow the firms to grow or only to survive? Do firms find a way to turn short-term loans into long-term capital investments, or are the funds used only in working capital investments?

This article examines the impact of a microfinance program operated by *Fincomun*, a microlender in Mexico City. The program offers loans to small-scale retailers who are also clients of *Grupo Bimbo*, the largest snack food company in Mexico. The *Bimbo* program was rolled out neighborhood by neighborhood, allowing us to gather data from clients of the program in a recently entered neighborhood and future clients in a soon-to-be-entered neighborhood. The latter serve as a comparison group against which to measure the impacts of the loans provided new clients. After discussions with *Fincomun*, we selected firms in two neighborhoods in Mexico City. The first, Naucalpan, was a neighborhood that *Fincomun* entered in July 2004, when our data collection began. The second, Texcoco, was a neighborhood where entry was planned for the spring of 2005.[2] *Fincomun* agreed to gather data on firms in Texcoco beginning in the summer of 2004 to act as a control group against which the impact of loans in Naucalpan could be measured. Using three surveys at 4–6 month intervals, we examine changes in the operations of 216 retail enterprises receiving loans in the first neighborhood with those of 188 retail enterprises selected in the second neighborhood. Screening methods to select the enterprises in both neighborhoods were identical.

The data we use in the analysis have strengths and weaknesses for analyzing the impact of new loans on the performance of small scale enterprises. The data were collected by *Fincomun* loan officers. *Fincomun* routinely collects detailed data on the enterprise performance and assets as a part of the loan application process. The loan officers are trained to spot inconsistent answers

[2] In the end, entry into Texcoco was delayed until the fall of 2005.

and to make their own estimates of the value of fixed assets and inventories used by the enterprise. They are paid incentives based on the performance of loans. Thus, there is some reason to believe that the quality of the data is higher than that typically gathered in surveys, especially on observable items such as inventories and fixed investments. Second, the treatment and control samples were selected with identical methods. As a result, the two groups should be comparable in terms of unmeasured characteristics related to demand for credit, entrepreneurial ability, and so on.

The biggest drawback to the data is the geographic divide between the treatment and control samples. All of the treatment firms are in one neighborhood, and all of the control firms are in another neighborhood that is located 37 kilometers from the treatment neighborhood. Therefore, local shocks might have affected the control and treatment groups differently. Given the temporal proximity of *Fincomun*'s entry into these two neighborhoods, we expect that these shocks would be random. Nevertheless, local shocks might affect the measured impact of the loans. Fortunately, we are able to control for neighborhood shocks by using monthly data on overall sales of the snack food company in each of the two neighborhoods. As we show below, while there is significant seasonality in the sales, the sales trends in the two neighborhoods are very similar.

Our main measures of impact relate to the operations of the enterprises: profits and gross sales and investment in inventories and fixed assets. *Fincomun* provides loans with a term of 4 months. The baseline interviews were completed before the first loan was given to the treatment group. The first follow-up survey was done after the first loan was repaid and before the second loan was given, and the second follow-up interview was conducted after the second loan was repaid and before the third loan was given. Therefore, at the time of each of the interviews, the outstanding balance on loans from *Fincomun* was zero. Still, if firms are severely credit constrained and the profitability of investments exceeds the cost of the loans, then we would expect to see a positive impact from the loans flowing as a result of profits reinvested in the enterprise. Given the 4-month term of the loan, we might expect the loans to be used primarily for purchases of inventories, and indeed about 88% of firms report using loans for inventories. These inventories were supplied by *Bimbo* or by other suppliers. Thus, we should expect to see larger impacts on sales and inventories than on fixed assets.

The best estimate of returns to capital available for firms comparable to those in the treatment and control groups comes from McKenzie and Woodruff (2006), who examine returns on capital using a Mexican microenterprise survey. The firms in the current sample are fairly large firms relative to those in the sample used by McKenzie and Woodruff. Median sales are Mex$56,000 (about US$5,000) per month. Initial median total assets are Mex$43,500 (about US$4,000), divided almost evenly between fixed assets (Mex$18,800) and inventories (Mex$24,400). For firms of this size, McKenzie and Woodruff find returns of about 2%–3% per month. A return of 2.5% per month equates to an annual return of 34% with profits reinvested. *Fincomun*'s interest rate equates to an annual rate of 39%. However, the McKenzie and Woodruff estimates come from the full cross section of firms. The take-up rate on loans from this microlender is around 40%. We would expect that those with the most profitable opportunities would be those most likely to take a loan. Therefore, returns among the subset of firms taking loans may be well in excess of the interest rates charged by the microlender, even if the average returns of all firms are not.

This article is structured as follows. Section II reviews the literature and shows the methodological problems one encounters when dealing with impact analysis studies as well as the results that have been found regarding credit's impact. Section III describes the empirical setup and the *Fincomun-Bimbo* loan program from which the data come. In Section IV, the impact results are presented and discussed. Section V contains our conclusions.

II. Literature Review

The central challenge in assessing the impact of access to credit is identifying a control group against which changes resulting from new loans can be measured. Those taking loans will not be randomly drawn from the population. They must first want the loan; this implies that their demand for credit is higher than average. They must also be granted the loan when they apply; this implies that they are seen by the lender as having better than average prospects for success. If these factors could be measured directly, one could simply control for them in regressions. But while proxies for the demand for credit can sometimes be found, it is seldom possible convincingly to control for selection effects. There are generally factors affecting the use of credit that are known to the borrower or lender but that are unknown to the researcher.

To understand the potential bias in the loan's impact estimate, consider the following regression on a sample of both borrowers and nonborrowers:

$$Y_i = a + bP_i + cX_i + \varepsilon_i, \tag{1}$$

where Y_i is the outcome of interest for person i (e.g., her income), P indicates participation in the lending program, X measures relevant personal characteristics, and ε_i is a residual capturing unobservable variables that includes other determinants of income. With this setting, b is a naive measure of the impact of participating in a credit program.

However, for b to represent an unbiased measure of the program's impact, ε_i must be identical in both the borrower and nonborrower groups. That is, ε_i must be uncorrelated with participation in the lending program. This assumption may be violated for many reasons. Most important, unmeasured factors such as entrepreneurial ability may affect the demand for credit. For example, the value of the parameter b is generally thought to be upward biased if the more entrepreneurial households are more likely to apply for credit. Second, lenders may choose to operate in areas where the demand for credit is higher. In either case, the positive correlation between an individual's participation choice and entrepreneurial ability not captured by X_i may bias the estimate b.

Attempts to address the selection problem have met with limited success, underscoring the nature of the challenge. A common methodology for estimating the impact of microlending programs is to compare a group of existing clients (the treatment group) with a group of new applicants who are deemed to be qualified to receive credit by the lender but who have not yet received credit (the control group).[3] The new applicants are seen as a good group against which to measure impacts because presumably they have been selected into the lending program in a manner similar to the way the existing clients were selected.[4] Hence, it would appear that we could attribute differences between the two groups to the loans themselves. The problem with this comparison is that some existing clients drop out during each loan cycle, either because they decide not to apply for another loan or because the lender declines to offer them another loan. The existing clients thus have been screened twice. Since dropout rates are often quite high, this is not a trivial matter. In our data, for example, about a quarter of the borrowers do not receive a second loan, either because they choose not to apply or because the lender chooses not to grant them a loan. Alexander-Tedeschi and Karlan (2006) attempt to quantify the bias coming from attrition in the treatment group by including clients who stopped borrowing in the treatment group. They reanalyze data

[3] See, e.g., Copestake, Bhalotra, and Johnson (2001) and Mosley (2001), who use matched samples of current clients and applicants. Vogelsang (2001) implements a related approach, using firms whose loans were not approved as a control group. He attempts to correct for selection using a Heckman procedure, identifying the first step with a variable indicating that the applicant has been blacklisted for past defaults with other credit institutions and the amount of the loan requested. Both of these would appear to be important indicators of the ability of the applicants.

[4] Of course, there are several reasons why the selection process used for the two groups might differ. For example, the microlender may have changed its selection criteria over time.

from Peru used by Dunn and Arbuckle (2001), showing that the failure to take attrition into account led to significant upward bias in Dunn and Arbuckle's estimate of the impact of loans. Karlan (2001) discusses other problems related to using new clients as a comparison group.

Coleman (1999) reports one of the more reliable studies using this methodology. He uses a sample of households from 14 villages in Thailand. Eight of the villages had community banks at the time of the baseline survey—all of which were established between 1990 and 1995—and six added the banks 1 year later. The community banks allowed individuals to join the bank before the branch was open in their village. These households serve as a control against which the impacts of lending in the village with banks can be measured. They would be expected to have unmeasured characteristics similar to those households who previously joined the bank in villages with an existing branch. The issue of attrition is addressed in two ways. First, the banks in treatment villages are fairly new at the time of the survey, reducing the impact of attrition bias. Second, Coleman includes nonmember households in the survey in all 14 of the villages. This allows him to measure village fixed effects and to control for differences between members and nonmembers in villages with new and established banks. Coleman finds that the loan programs had very little impact on measures of household well-being.

Pitt and Khandker (1998) use a different approach to identify a control group. They make use of the lending rules of the Grameen Bank in Bangladesh, which requires that borrowing households own no more than one-half an acre of land. Controlling for village fixed effects (to account for endogenous program placement), Pitt and Khandker identify the impacts of the program across households within villages, using land ownership as an instrument for participation. This is a very clever identification strategy. However, Morduch (1998) points out two major problems with using the land rule as an instrument for participation: the data make clear that the rule was not followed all the time, and the rule itself was not entirely exogenous. While Pitt and Khandkar find positive impacts of Grameen Bank loans on household outcomes, Morduch finds that the positive result disappears when the identification problems are taken into account.[5]

Theoretically, with a perfect control group, cross-sectional data are sufficient for measuring impacts. Since individuals in the perfect treatment and control groups are identical in all respects except for access to the lending program, any differences in outcomes of interest can be attributed to the loan program itself. In practice, treatment and control groups are not likely

[5] Pitt (1999) responds to Morduch's criticisms.

to be so perfect nor sample sizes so large that cross-sectional data will be sufficient. Some—but not all—imperfections in treatment and control can be addressed with panel data, which measure an individual's experience across time. In short, panel data allow for perfect control of time- and treatment-invariant characteristics. But panel data will not resolve problems caused by variation across individuals across time. As an example of the limits of panel data, differences in the entrepreneurial ability of those in the treatment group and the control group will generally not be fully solved with panel data. Entrepreneurial ability will affect the impact of the treatment itself. Therefore the changes associated with the treatment will not be generalizable to the control group population.

To see where panel data allow one to measure an impact that could not be measured in the cross section, consider the study of a change in Indian lending laws examined by Banerjee and Duflo (2004). By law, Indian banks must make a certain portion of their loans to small enterprises. A 1998 reform changed the definition of "small enterprise" from an enterprise with roughly US$150,000 in assets to an enterprise with roughly US$750,000. In 2000, the law was changed again, and the upper limit on "small" was lowered to about US$250,000. Thus, firms with from US$250,000 to US$750,000 experienced an increase in the availability of capital in 1998 and a reduction in the availability in 2000. Banerjee and Duflo use these shocks to identify the impact of access to credit, comparing the changes in outcomes of firms in the affected range with changes in outcomes of firms just outside the affected range. They estimate that returns to invested capital are "at least 74%" per year, which is considerably higher than the 30%–40% rates that McKenzie and Woodruff find for firms with US$5,000 in assets in Mexico. The Banerjee and Duflo study is a case where differences in the characteristics of the treatment and control samples necessitate the use of panel data to study the impacts. Firms that experienced no change in access to capital are used as controls for changes in profitability across time. The implicit assumption is that the time-variant changes affected both the treated and the untreated firms in a similar manner.

Armendariz de Aghion and Morduch (2005, chap. 8) summarize a few other studies of the impact of microcredit programs. The literature is surprising in at least two respects. First, given the popularity of microfinance as a strategy to assist the poor, there are relatively few studies examining the impacts. Second, the general pattern is that the studies showing the most positive impacts are methodologically flawed and the studies that use the most defensible methods show little or no positive impact. The latter finding is puzzling given the increasing number of studies that show very high returns to capital among microenterprises.

III. Empirical Design

In this section, we describe *Fincomun*'s data gathering, loan decision, and program placement road map. The description will help us explain the manner in which the design of this project addresses the major methodological problems discussed in the preceding section.

A. The Fincomun-Bimbo Program

Fincomun is a credit union that serves small retail enterprises in the eastern part of Mexico City. It began offering savings and lending services in Iztapalapa, a neighborhood in the southeastern part of Mexico City in 1994. By 2004, it had expanded to cover much of the area in the eastern part of the federal district, with 21 branches and 61,000 clients.

In 2002, *Fincomun* started a business alliance with the snack food company *Grupo Bimbo*.[6] The two entities agreed to work together to provide *Fincomun* credit to clients of *Bimbo*.[7] The program operates as follows. First, *Fincomun* promoters travel in *Bimbo* supply trucks. From the drivers, they learn the payment history of the *Bimbo* clients on the route. While drivers deliver the goods, the *Fincomun* promoters discuss the loan program with those *Bimbo* clients having an adequate payment record. If the clients express an interest in the loan program, the promoters arrange for a more extensive interview during business hours. *Fincomun* reports that about 60% of potential clients arrange an interview after learning the details of the program from the promoter.[8]

The longer interviews involve a series of questions contained on a Palm hand-held computer and are intended to establish a risk profile and decide whether the *Bimbo* clients satisfy the *Fincomun* loan criteria. Of those interviewed, about 70% desire and are offered a loan. The program has become a significant part of the *Fincomun* portfolio: 2 years after the program started, *Bimbo* clients represented more than 20% of *Fincomun*'s business.[9]

[6] *Grupo Bimbo* was established in 1945 in Mexico City and is currently considered one of the most important bakeries in the word. It is present in Mexico, the United States, Argentina, Brazil, Colombia, Chile, all Central American countries, Peru, Venezuela, and the Czech Republic. Its net sales for 2004 amounted to US$430 billion. The group has the most extensive network in Mexico and one of the largest on the American continents.

[7] According to data of 2003, the owners of these stores had a family income that put them between the third and the seventh deciles of Mexico's income distribution.

[8] We do not have an estimate of the portion of *Bimbo* clients screened out on the basis of information from the *Bimbo* delivery truck drivers.

[9] *Fincomun* has started to mediate in the remittances business. Also, it has signed an agreement with Infonavit, a government institution in charge of offering housing credit, so that savings deposits in *Fincomun* may be used by depositors as collateral to get credit from this public institution.

The survey asks detailed questions on the value of assets and inventories and on sales and costs of the enterprise. Because the microenterprise activities are usually embedded in the household economic portfolio, the survey also includes questions on household income and expenditure. For inputs that are visible—fixed assets and inventories, for example, the survey includes both the owner's response and the loan officer's estimated value. The Palm software also prompts the interviewers when the respondent provides answers that are inconsistent with previous answers, helping to improve the quality of the data. Using these data, the software calculates the overall net income of potential borrowers. The most important criterion is that new borrowers must have a net income that is 50% higher than the monthly payment they would have to make to *Fincomun*. Using this criterion, loans officials decide in situ how much to offer.

Firms of this size rarely have audited accounting records. The data reported by the firms are more likely to be from memory or from informal written records.[10] Consequently, any data available from these firms will be noisy at best, and they may be biased as well. This is particularly true for data reported by a loan applicant. In this regard, we take some comfort in the fact that the data are verified by the loan officers whose pay depends on the outcome of the decisions they base on the data. Bonuses make up as much as half of a loan officer's total compensation. The bonus is based both on the value of loans made during the period and the loan repayment rates. We believe that the incentives provided to loan officers likely reduced both the noise and the bias in the data, though, of course, these are unlikely to be eliminated.

Loans are provided for a period of 4 months and are renewable after that time. The average size of *Fincomun*'s loans in 2005 was US$580, with a fixed monthly interest rate of 6% on the amount of the current loan balance.[11] Evidence of collateral is required. Loans are made to an individual, and in most cases they must be paid in 18 weekly fixed-amount installments.

For the purpose of our research, *Fincomun* agreed to provide us with data from a set of new clients in Naucalpan, a neighborhood it entered in the summer of 2004. The company also agreed to follow the same regimen to

[10] Mexico's National Survey of Microenterprises (ENAMIN) asks microenterprises with between 1 and 15 employees how they keep the accounts of the business. Among firms with less than US$5,000 in capital stock, only 16% report using outside accountants. More than half (60%) say that they keep no records at all. The remainder rely on personal notes (23%) or a notebook provided by tax authorities (2%).

[11] This "declining balance" interest rate is equivalent to a fixed rate of 3.25% per month, or 39% per year. *Fincomun*'s rate was close to the rate that commercial banks charged on credit card debt, which averaged about 35% during this period. By comparison, rates on short-term government debt were in the 5%–8% range.

gather data from a similar group of potential clients in Texcoco, a neighborhood it then planned to enter in the spring of 2005. The latter was to serve as a control group. From the perspective of *Fincomun*, these two neighborhoods had similar business potential. Both are located in the northern part of Mexico City.

To select the control group, *Fincomun* promoters rode on the *Bimbo* delivery trucks in Texcoco, identified *Bimbo* clients with adequate payment histories, and provided them with information on the loan programs. That is, they followed the same procedure they had used in Naucalpan. Where the *Bimbo* client expressed interest in a loan, the *Fincomun* promoter explained that *Fincomun* was not yet operating in the neighborhood but was conducting a marketing study and considering entering the neighborhood in the "near future." An appointment with a loan officer was arranged to conduct the 30-minute survey. *Fincomun* reported that a somewhat higher percentage (about 80%) of the firms approached agreed to the full interview and that 70% of those interviewed expressed an interest in receiving a loan and were deemed qualified for a loan by *Fincomun*, a rate similar to *Fincomun*'s general experience. In sum, since members of both the treatment group and the control group were selected through similar screens, we expect that they possess similar entrepreneurial spirit and face similar economic restrictions and opportunities.

One additional concern is that control firms might have adjusted their borrowing from other lenders given the prospect of *Fincomun*'s entry in the area. While the *Fincomun* interest rates are much lower than interest rates changed by informal lenders for daily or monthly loans, the short-term nature of these loans and the uncertainty of *Fincomun*'s entry make it less likely that they would change their borrowing from short-term lenders. *Fincomun*'s rates are comparable to those of other formal lenders. Finally, we note that, if we expect that firms borrow for profitable opportunities, any reduction in borrowing by the control group would bias the results in favor of finding a positive effect of the loans.

The baseline interviews were conducted in Naucalpan and Texcoco between July and November 2004. A total of 216 *Bimbo* clients in Naucalpan were interviewed and received loans; 188 *Bimbo* clients were interviewed in Texcoco during the same period. We selected neighborhoods some distance apart to avoid displacement problems of the sort identified by de Mel, McKenzie, and Woodruff (2007). While the neighborhoods differ in some ways, data from *Bimbo* on sales to all customers in these areas indicate that sales grew at similar rates in the two neighborhoods both prior to and during the period of analysis.[12]

[12] Sales of bread and snacks provided by *Bimbo* represent about 20% of a typical small grocer's

Between the second half of 2003 and the second half of 2004, sales grew by 8.2% in Naucalpan and 7.8% in Texcoco. Between the second half of 2004 and the first half of 2005 (i.e., roughly between the times of baseline and the first follow-up survey), sales decreased by 1.2% in Naucalpan and by 3.0% in Texcoco. Between the first half and the second half of 2005 (i.e., between the times of the first and the second follow-up surveys), sales increased by 9.3% in Naucalpan and 13.1% in Texcoco.

As a normal practice, *Fincomun* reinterviews clients only when they ask for a larger loan. For the purpose of our study, *Fincomun* agreed to resurvey all of the initial borrowers around the time that they would have requested a second loan. The second interview was done between January and May of 2005. During the second half of 2005, *Fincomun* resurveyed everyone who had been given a second loan. Thus, we have an unbalanced panel of three rounds of interviews. As we discuss in more detail later in this article, there is very little attrition between the first and second rounds but more between rounds 2 and 3 since those receiving only the first loan were not surveyed a third time.

B. *The Baseline Data*

All of the enterprises sell groceries at the retail level. Very few have paid employees. The enterprises reported median assets (fixed assets and inventories) in the baseline survey of about Mex$43,600 and monthly revenues of about Mex$55,600. This would appear to place the firms at the large end of microenterprises in Mexico. Data from the 1998 National Survey of Microenterprises indicate that, among retail firms with from one to five workers, the median asset level is around Mex$15,000 and the 75th percentile is Mex$45,000. Our firms fall between those two levels, adjusting for inflation between 1998 and 2004. Their total net income and number of dependents indicate that the average entrepreneur in the sample belongs to the sixth decile of Mexico's per capita income distribution.

Table 1 shows the means of several variables for the treatment group and the control group, along with *t*-tests indicating the significance of those differences. Sales and profits are almost identical in the two groups at the mean. Figure 1 shows the kernel densities of net sales (total revenues minus cost of goods sold) for the treatment sample and the control sample. The two distributions are quite similar throughout. However, the control group has a larger level of both fixed assets (Mex$21,300 vs. Mex$16,600; significant at

sales. The remaining products—soft drinks, canned goods, dairy products, etc.—represent the remaining sales. Given that the stores in the sample all sell almost exclusively food products, we expect that the sales of *Bimbo* products is positively correlated with the total sales of the store.

TABLE 1
CONTROL AND TREATMENT POPULATION AT THE BASELINE

	Control Group Mean	Treatment Group Mean	*t*-Test for Difference in Means
Gross sales (pesos)	57,302	54,141	.85
Net income (pesos)	8,823	8,584	.64
Fixed assets (pesos)	21,336	16,559	3.54
Inventories (pesos)	26,326	22,854	1.81
Family expenditure (pesos)	3,305	3,144	1.29
Number of dependents	3.26	3.34	.55

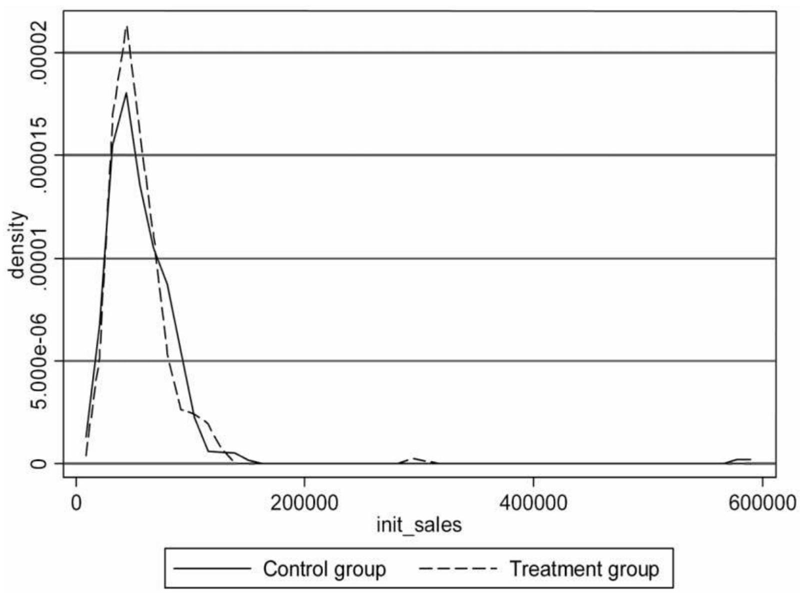

Figure 1. Kernel distribution of initial net sales (pesos [Mex$])

the .01 level) and inventories (Mex$26,300 vs. Mex$22,900; significant at the .10 level). In both cases, the difference in median levels is much smaller than the difference in means, suggesting that the differences come from more large firms in the control group. For inventories, a Mann-Whitney test suggests that the differences in median levels are not significant at the .10 level, but a similar test indicates significant differences in fixed asset levels throughout the distribution.

While the control group was selected in a manner that provides a good match on unmeasured characteristics, the treatment and control groups are located in geographically distinct areas. The performance of the firms in the two groups is thus subject to differences in changes to local demand in the

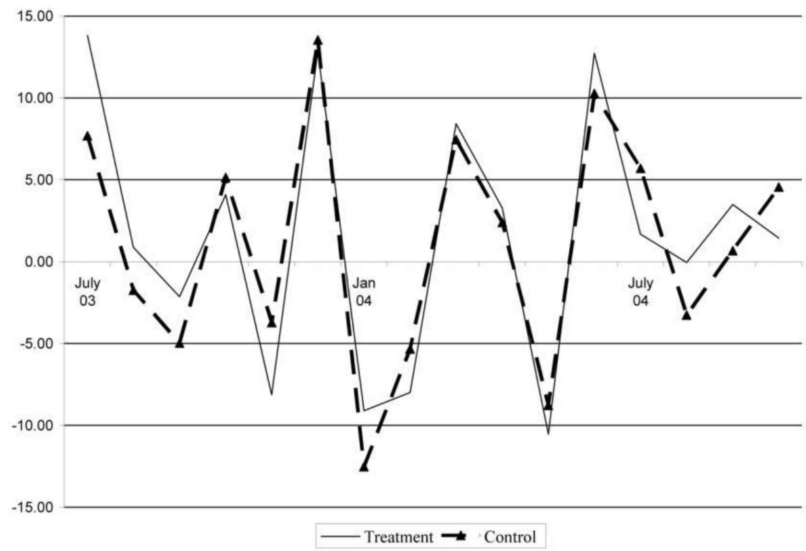

Figure 2. Monthly growth rate of *Bimbo* sales, July 2003 to October 2004

two neighborhoods. Figure 2 shows the growth rate of *Bimbo* sales in both Naucalpan and Texcoco for the year leading up to the baseline survey, from July 2003 through October 2004. These data are total sales on the delivery routes from which the two samples are drawn, not sales to the subset of firms included in the sample. Thus, they reflect general demand conditions in the areas. The sales in the two neighborhoods are very similar over this period, as figure 2 indicates. On average, sales grew by 5.6% between interviews in the treatment neighborhood (Naucalpan), while they grew only 1.7% between interviews in the control neighborhood (Texcoco).[13] In the regressions, we control for the change in the rate of sales in the neighborhood over the period between interviews. Note that the *Bimbo* sales data for each neighborhood include the clients who are in our treatment and control samples. However, our firms represent only a small percentage of the clients in each neighborhood. *Bimbo* has about 2,000 retail clients in Naucalpan and 1,500 in Texcoco. Controlling for neighborhood sales will dampen any positive effects of the loan program on sales but only to a very minor extent.

[13] Between the first and second interviews, sales grew by 4.4% in the treatment neighborhood and fell by 2.2% in the control neighborhood. The interview dates for the third survey were not recorded. However, between the date of the second interview and 5 months later, sales grew by 7.3% in the treatment neighborhood and by 9.1% in the control neighborhood.

TABLE 2
IMPACT OF RECEIVING A LOAN ON FIRM PERFORMANCE

	Reported Profits	Reported Revenues	Inventories	Fixed Assets
Loan (0/1)	−520.7	467	2,800	1,967
	(2.01)	(.34)	(2.01)	(3.63)
Index of *Bimbo* sales	1,088	−12,580	−4,708	−5,911
	(.57)	(1.33)	(.57)	(1.61)
Number of observations	1,099	1,100	1,050	1,099
Number of firms	404	404	403	404
R_2 (within)	.02	.04	.06	.02

Note. All regressions include firm-level effects; *t*-statistics are in parentheses.

IV. The Estimation Strategy and Results

We begin by estimating the impact of a loan on several measures of the firm's performance. With panel data, we estimate the impact of the loan across time. The basic regression is

$$Y_{i,t} = b\text{Loan}_{i,t} + \gamma X_{i,t} + \sum_{t=2}^{T} \omega_t \delta_t + a_i + \varepsilon_{i,t}, \quad (2)$$

where the a_i are firm fixed effects, X_{it} is a variable or vector of characteristics that vary across both firms and time, and ω_t are survey-wave fixed effects.

The basic regression presumes that the treatment affects all firms in a similar manner. There may be reason to believe that this is not the case. Perhaps, for example, the smallest firms are more constrained, so that the loans affect very small firms differently than they affect larger firms. If the treatment effects are heterogeneous, we can estimate a more general specification:

$$Y_{i,t} = b\text{Loan}_{i,t} + (\gamma_s \text{Loan}_{i,t} \times X_i) + \gamma X_{i,t} + \sum_{t=2}^{T} \omega_t \delta_t + \sum_{t=2}^{T} (\omega_t \delta_t \times X_i) + \alpha_i + \varepsilon_{i,t}. \quad (3)$$

Here the treatment is interacted with characteristics of the firms (measured at the baseline survey). As in the basic regression, we allow for firm and wave fixed effects. We also allow for the effects of the X's to vary across waves by including a set of wave × X interactions. Taking the example above, we allow the outcome of small and large firms to vary across waves for reasons that are independent of the treatments.

A. Basic Results

Table 2 shows the results of estimating equation (2), the simplest fixed effects regression, controlling only for wave effects and an index of *Bimbo* sales in the neighborhood and at the time of each interview. We use the full sample of firms, including those that exited from either the treatment sample or the control sample before the third interview. The regression in column 1 indicates that loans had a negative effect on profits, and this is significant at the .05 level. The implication is that the interest costs exceeded any positive effect the loan might have had on sales, at least in the final month of the loan. Column 2 shows that the measured effect of loans on gross sales is positive, but this is far from statistically significant. Recall that the interviews are carried out after the firm has fully repaid the loan, with the data reflecting the previous month. Assuming that the interviews come just after the last loan payment, the firm would have only a small positive loan balance outstanding during the reference month. One interpretation of the lack of a significant positive effect of the loan on sales is that firms invest the funds in inventories but then reduce inventories in order to pay back the loans.

While we find no significant impact of the loan on total sales, columns 3 and 4 of table 2 show significant effects of loans on both fixed investments and inventories. Loans are associated with an increase in inventories of about Mex$2,800 and an increase in fixed assets of almost Mex$2,000. Combined, these increases amount to 56% of the Mex$8,500 average loan size. Contrary to the profit and sales data, then, the results in columns 3 and 4 suggest that the enterprises with loans have grown compared to those without, even though the loans have been fully repaid at the time of the survey.

One could argue that inventories are a better measure of the impact of the loan on the firm's operation because inventories are more easily verified by the loan officers than are sales and profits. Fixed assets are also more easily verifiable. The association between loans and investment in fixed assets is surprising given that the loans have a term of only 4 months. These results suggest that firms are using other resources—reinvested profits, informal loans, or their own savings—to invest in fixed assets even when using the short-term loans.[14] Note, however, that the change in fixed assets is smaller than the change in inventories.

The index of *Bimbo* sales is never significant, and indeed it is negative in all but the profits regression. This may reflect the fact that *Bimbo* merchandise represents only a part of the sales of the sample firms. We will see in some of the later regressions that this variable behaves more in line with our expectations than it does in the regressions in table 2. In the aggregate, *Bimbo*

[14] Few of the firms are likely to have other formal loans because *Fincomun* enters neighborhoods in which no other microlender is operating and the *Bimbo* clients in the sample are smaller than typical commercial bank clients in Mexico. However, the data do not allow us to see if some firms may have formal loans through lenders operating outside the neighborhood in which the store is located.

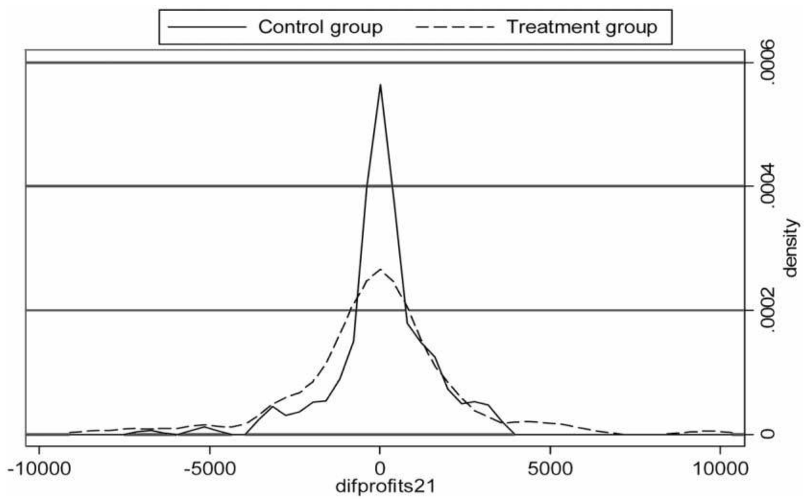

Figure 3. Change in profits, round 2 versus baseline (peso [Mex$])

sales grew at a slightly faster rate in the treatment neighborhood (Naucalpan) than in the control neighborhood (Texcoco). This suggests that, if anything, a more precise measure of demand might result in less positive (or more negative) impacts of loans. The finding that loans are negatively associated with profits is thus unlikely to be caused by the imperfect measurement of changes in demand. Since fixed assets are not likely to adjust quickly to modest changes in market demand, the greatest concern relates to the possible overstatement of the measured effect of the loan on investment in inventories.

The regressions in table 2 do not allow for the heterogeneity of the loan impacts. To see why this might matter, we plot in figure 3 the distribution of changes in profits between the baseline survey and the first follow-up survey for the treatment group and the control group separately. There is clearly much more dispersion in the treatment group than in the control group. Indeed, the greater dispersion is the overwhelming feature of the figure.

We next explore this heterogeneity by allowing for differences in impact by initial firm size. Since McKenzie and Woodruff (2006) suggest that returns to capital are higher among smaller firms, we interact initial firm size, measured by the total assets, with the indicator of having a loan. Table 3 shows the results of estimating equation (3), with loan interacted with the baseline size of the firm, measured by total assets. Because larger firms may have different seasonal patterns or may be subject to different shocks, we also include the wave–initial asset interactions in the regressions. The loan–initial size interactions thus measure the differential effect of loans on firms of different sizes across the full sample period.

TABLE 3
IMPACT OF RECEIVING A LOAN ON FIRM PERFORMANCE, HETEROGENEOUS BY INITIAL FIRM SIZE

	Reported Profits	Reported Revenues	Inventories	Fixed Assets
Loan (0/1)	775	4,262	7,450	1,933
	(1.20)	(1.32)	(2.57)	(1.85)
Loan × initial assets	−.037	−.141	−.15	−.0078
	(2.32)	(1.82)	(2.16)	(.35)
Index of *Bimbo* sales	2,541	−2,010	5,722	−3,416
	(1.32)	(.23)	(.78)	(.94)
Number of observations	987	987	978	987
Number of firms	363	363	363	363
R_2	.10	.20	.19	.009

Note. All regressions include firm-level effects; *t*-statistics are in parentheses.

Taken together, the results in table 3 show that loans had more positive impacts on smaller firms than on larger firms. The loan variable is positive and the interaction term is negative with all four of the dependent variables. The loan effect is significant only in the fixed asset and inventory regressions; the interaction effect is significant in all but the fixed asset regression. In the inventory regression (col. 4), the effect of the loan is measured positive for firms with initial asset levels below Mex$50,000 —just above the 75th percentile of the distribution—and negative for firms with initial assets above this level. Although the level effects are not significant for either profits or revenues, the implied negative profits crossover points are much lower, occurring at the 5th percentile for profits and the 25th percentile for revenues. These results suggest, then, that loans had a homogeneous effect on investment in fixed assets and a heterogeneous effect on investment in inventories, with smaller firms showing larger impacts.

In using a 0/1 variable to indicate the loan, we are losing some efficiency in exchange for reducing bias. While two-thirds of the loans were between Mex$5,000 and Mex$7,500, 5% of the loans exceeded Mex$15,000 and the maximum loan size was Mex$42,000. Larger loans would be expected to have larger impacts on outcomes. But of course the loan size is endogenously determined. Moreover, we feel comfortable comparing the full sample of treated firms with the full sample of untreated firms because some of the untreated firms would have been offered larger loans as well. But we do not have any way to separate the untreated firms by loan size. In spite of the potential endogeneity, we examine the effect of loans measured by size in table 4. We report results with the interaction between initial assets and loan size.

As expected, the results are somewhat more positive when we use loan size in place of the indicator variable. The level effect is still not significant in the profits regression, but it is now positive and significant in the sales regression.

TABLE 4
IMPACT OF LOAN AMOUNT ON FIRM PERFORMANCE, HETEROGENEOUS BY INITIAL FIRM SIZE

	Reported Profits	Reported Revenues	Inventories	Fixed Assets
Amount	.062	.54	.939	.241
	(1.46)	(2.65)	(3.87)	(2.76)
Amount × initial assets	−.0000013	−.0000043	−.000007	−.0000014
	(1.95)	(1.33)	(2.21)	(1.73)
Index of *Bimbo* sales	280	−15,778	−7,130	−3,346
	(.15)	(1.86)	(1.02)	(.90)
Number of observations	987	987	978	987
Number of firms	363	363	363	363
R_2	.09	.22	.22	.11

Note. All regressions include firm-level effects; *t*-statistics are in parentheses.

The interaction effect is negative and significant in all but the sales regression. Moreover, the implied negative-outcome crossover points are much higher when we use the loan amount. For profits, almost 75% of firms have an implied positive effect for sales, and in the two assets regressions, all but the largest 2% of firms have implied positive effects. Given the endogeneity bias, we should interpret these results as representing an upper bound of the effect of loan on the various outcomes.

B. Attrition

There is very little attrition between the baseline and first follow-up. We lose only three firms from the control group and two from the treatment group. Between the second survey and the third survey, however, attrition is a bigger problem. We lose 90 firms from the sample of 399 in the second survey. However, the attrition rates are very similar in the treatment and control samples. The treatment sample falls by 23.4%, from 214 to 164 firms; the control sample falls by 21.8%, from 185 to 145 firms. Lee (2005) proposes methods for dealing with issues caused by differential attrition rates in the treatment and control samples. The methods suggested by Lee are not likely to help much in our case. The overall attrition rates are nearly identical. But the reasons for attrition may be quite different, and hence some bias may still be present. Firms in the treatment sample most commonly attrite because they decline to take (or are not offered) a second loan. That decision is based on real experience. Those in the control sample may have been discouraged by *Fincomun*'s delay in entering the area. In any case, they have no real experience with loans upon which to base their refusal to participate in the third follow-up survey.

Instead of attempting to adjust for any bias that might be caused by these differences, we address the issue of attrition by limiting the analysis to the

TABLE 5
IMPACT OF RECEIVING A LOAN ON FIRM PERFORMANCE, FIRST TWO WAVES ONLY

	Reported Profits	Reported Revenues	Inventories	Fixed Assets
Loan (0/1)	1,209	5,431	3,964	1,601
	(2.45)	(2.00)	(1.61)	(1.96)
Loan × initial assets	−.0433	−.1647	−.1368	−.0247
	(3.66)	(2.47)	(2.07)	(1.62)
Index of *Bimbo* sales	3,884	8,274	14,175	−2,676
	(1.67)	(.70)	(1.96)	(.60)
Number of observations	714	714	706	714
Number of firms	363	363	363	363
R^2	.08	.05	.06	.03

Note. All regressions include firm-level effects; *t*-statistics are in parentheses.

first two waves of the sample, where attrition is not an issue. The fact that we now measure very short-term impacts should be kept in mind, but since the majority of the loans are invested in inventories, we do not see this as a fatal handicap. Table 5 reports results of regressions with the equation (3) specification. The number of observations is smaller because we have eliminated the third wave. However, in many respects, the results are cleaner. The level effect of receiving a loan is now positive in all of the regressions and is significant in all but the inventories regression, where it falls just below the .10 level. The interaction effect is negative with all four dependent variables and is significant in all but the fixed assets regression, where again it falls just below the .10 level. Finally, the index of *Bimbo* sales now has the expected positive sign in three of the regressions, and it is significant at least at the .10 level for profits and inventories.

While the level effects are positive in all four of the regressions, the interaction terms are somewhat larger than those of table 3 in all but the inventories regression. As a result, the point at which the effect of the loan passes from positive to negative is lower. For both profits and inventories, the crossover point is around the 20th percentile, and for sales, the crossover point is around the 35th percentile. Only for fixed assets is the implied crossover point above the median initial firm size. This suggests that, in the short term, the loans had positive impacts only for the smallest firms in the sample.

V. Conclusions

The staggered expansion of the *Fincomun-Bimbo* loan program allows for an analysis of the program's impact on its small-scale retail client base. The analysis provides some insight into the use of funds received by borrowers and the impacts of those funds on the operations of the borrowing enterprises. Given that the loans are provided for very short time frames, it is not surprising that

85% of borrowers say all of the funds were invested in inventories. Given the fungibility of resources, however, responses to the direct questions about the use of loan funds may not match the actual experience. We find that about 40% of the funds that remain in the enterprise after the loan has been repaid are invested in fixed assets rather than inventories.

We also find that the loans are associated with modestly lower profits in the full sample of firms. The finding that profits are lower for firms receiving loans is consistent with returns to capital being lower than the effective annual interest rate of about 40%. When we allow for returns to vary with initial firm size, we find higher profits and sales for the smallest firms but lower profits and sales for larger firms. Noting the significant share of the loans that finds its way into fixed investments, it may also be the case that returns on these investment are not realized in the short run. Our data are limited to a period of less than 1 year after the initial loan.

Another notable characteristic of the data is the high rate of attrition after the first loan—between the second and third survey rounds. The majority of this attrition is due to firms not receiving a second loan, either because the lender declined to offer them a loan or because the firm declined to reapply. The firms exiting the sample saw smaller gains (or larger falls) in measured outcomes between periods 1 and 2. This suggests that any study assessing the impact of a loan program over a longer term must find a way to continue gathering data from those firms that exit the loan program over time.

The *Fincomun-Bimbo* case provides one data point in understanding the impact of microlending on small-scale enterprises. No single study will confirm or deny the accomplishments of microlending. We have noted several caveats to the interpretation of our results. The most serious of these is that the treatment on control samples comes from geographically distinct neighborhoods. We should be concerned that neighborhood-specific shocks might affect the measured impact of the loans. We address this issue by showing that the pattern of sales of *Bimbo* products is similar in the two neighborhoods and that indeed sales grow slightly faster in the treatment neighborhood. But we cannot be certain that more nuanced neighborhood-level shocks are not driving the results.

Our results, like those of many others, represent a snapshot of one particular program at one particular point in time. Collectively, like dots on an impressionistic painting, the individual studies provide a picture of the impact that microlending has on microenterprises and development more generally. We are many dots short of a coherent picture. The impacts in the particular lending program we analyze do not appear to be large, but they do appear to be present for the smallest firms in the sample.

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