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Firm-Level Growth Effects of Small Loan Programs: Estimates Using Universal Panel Data from Romania

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Firm-Level Growth Effects of Small Loan Programs:

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This paper evaluates the effectiveness of small business loans in improving employment, sales, and survival outcomes at recipient firms. Using kernel-weighted propensity-score matching, we analyze panel data on firms receiving USAID-funded loans and two control groups of firms drawn from the universe of firms in Romania. One control group consists of eligible firms not receiving loans, and the other includes firms that were not eligible (because of location). The availability of universal data, including detailed long panel information on financial and operating variables, implies that the usual problem of non-overlapping support between treatments and controls is mitigated. The evidence supports the contention that these loans do in fact raise employment and sales growth, although they do not have a significant effect on the probability of survival.

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1. Introduction

Microcredit, small loans provided to small businesses or individual entrepreneurs, has become a cornerstone of economic development efforts around the world and a fashionable topic for policymakers and researchers alike (Morduch, 1999). Public and international support for microcredit programs has been justified by claims of high repayment rates, salutary effects on poverty and inequality, and positive roles in fostering job creation and economic growth. While there is some evidence to support the first two claims, however, there has been remarkably little analysis of the impact of microfinance loans on employment and sales in the businesses receiving them, although such impacts could be potentially important externalities of this form of finance.

A possible reason for the lack of research on the firm-level employment and output effects of microcredit might be the lack of good data available for studies with this purpose. Data requirements include detailed time series on the recipient firms, including information on their performance, finances, and other relevant characteristics for a significant period both before and after the loan is received. Few if any studies have examined such data, which are usually confidential and expensive to collect. A further problem, however, is that any estimate derived from such a data set could be subject to selection bias, inasmuch as receiving the loan could be endogenous with respect to the firm's growth potential. To control for this possibility, it is desirable to have a comparison sample, one that is similar enough to the "treated" sample of loan recipients to permit valid comparisons of performance across the two samples. To put it differently, it is necessary that the comparison group have a common statistical support with the treatment group (e.g., Heckman et al., 1998).

In this paper, we report estimates of such microcredit treatment effects on firm-level employment, sales, and survival. Our analysis exploits a remarkable database containing detailed information on both recipients and nonrecipients of microcredit in Romania. The data are drawn from two primary sources: a special survey of firms receiving loans from three small loan programs financed by the U.S. Agency for International Development (USAID), and financial and operating data on the universe of firms in Romania. These data permit us to measure precisely the timing and magnitude of the USAID loans and the evolution of employment, sales, debt, profit, and capital stock for each year from 1992 to 2006. Although the USAID programs are small, and our sample contains only 297 recipients, the universal data comprise a large number of firms: more than 200,000 cases per year. The large number of nonrecipients provides an opportunity to select firms for a comparison group that have a common support with the recipient firms, and thus to control for selection bias in estimating the loan treatment effect.

Our econometric approach differs from traditional matching procedures, which typically use information only on a cross-section, and more recent extensions to difference-in-differences matching using panel data.[1] While these procedures have the advantage of selecting matches based on propensity scores, thus using a common support, they frequently ignore additional information on the outcome variable before and after treatment, and on the quality of the matching. Our approach uses the full time-series

[1] See Imbens (2004) for an exposition of matching methods. Abadie (2006), Arnold and Smarzynska Javorcik (2005) and Gong et al. (2006) apply difference-in-differences approaches.

information for each firm in the treatment and comparison groups for both the matching procedure and the estimation of the treatment effect. The matching regressions condition on four years of data prior to treatment, as well as industry, county (*județ*), year, and the age of the firm at treatment date. As we show, including these regressors is crucial to obtain high quality matches (very similar propensity scores). To assure still greater homogeneity of the treated and matched samples, we include matches only if they are exactly identical in several key variables: 2-digit NACE industry, year, and age.

Within this approach, we consider two alternative sets of comparison firms. The first adds county to the list of required exact matches. The rationale for selecting the matched comparison firm from the same county as the treated firm is that economic conditions are likely to be similar for firms of the same industry, age, year, and location. On the other hand, it might be argued that firms receiving loans may be systematically different from those not receiving loans; for instance, the former may self-select into loan recipiency because they have some information that we cannot observe – even after conditioning on four lagged years of performance and financial variables, as well as industry, age, year, and county – on the usefulness of the loan. So few Romanian firms actually received USAID loans (about 1 in 1000) that it seems to us that there could be many similar firms with loan impacts similar to those of the recipients.

Nevertheless, we also consider a second set of comparison firms. This approach exploits an institutional feature of the USAID programs, which made loans available only in certain counties of Romania: 18 out of 41, as of March 2000. Firms located in the 23 non-USAID counties were not eligible for the loans, and thus by definition there is no problem of self-selection. In this approach, we include only firms in these 23 counties as potential comparators in the matching regressions.

Once the matched pairs are selected, we go beyond the usual difference-in-differences method to estimating the effects of treatment on outcomes. The usual approach somewhat arbitrarily chooses a single pre-treatment period and a single post-treatment period to compute after-minus-before differences of the outcome variable for the difference between the treated and matched firm. This method has the advantage over a cross-sectional approach of controlling for common trends within a matched pair, but it suffers from sensitivity to the choice of periods – what Heckman et al. (1999) call the "alignment fallacy." An alternative is to use pre- and post-treatment averages, but this does not control for systematic differences in match quality across pairs nor does it use the full time-series information available in the data. We address the first of these issues by estimating difference-in-differences models including firm fixed effects, and we address the second by estimating models with full dynamics of the effect around treatment date. The latter method allows us to assess the extent to which pre-treatment behavior of the outcome variable is similar within matched pairs not only in levels but in dynamics. For instance, if firm performance tends to fall or rise more for the treated or non-treated firm just prior to loan receipt, this method will pick it up.[2] This method also allows us to assess the extent to which the effects of loan receipt are immediate or gradual, and short-term only or sustained for a longer period. While we have developed these approaches for the purposes of this paper, it seems to us that they may be more generally useful in studies of treatment effects where rich long panel data are available.

[2] The situation is analogous to an "Ashenfelter dip" in studies of the effect of training on wages. In our case of loan receipt, some kind of pre-treatment spike in firm performance is more likely.

Section 2 provides a brief overview of the new private sector and role of microcredit in Romania and other transition economies. Section 3 describes the sources of the data, compares the basic characteristics of the USAID loan recipients and all other firms in Romania, and discusses the employment, sales, and survival outcome measures. Section 4 presents our procedure for defining matched comparison firms and the matching results. Section 5 explains our method of estimating treatment effects using the matched data and provides the results. Section 6 concludes the paper with a summary of results and caveats.

2. Microcredit Programs and the New Private Sector

Development agencies have long recognized the potential importance of small startup firms in transition and developing economies. According to many observers, it is hard to overestimate the importance of this *de novo* private sector, particularly in the transition context where the existing enterprises inherited from central planning face difficult if not insurmountable problems in restructuring and adjusting to the demands of a market economy.[3] A number of studies have provided evidence that new private firms tend to outperform old enterprises, and, indeed, international financial institutions have proposed that new private sector growth should be a principal measure of "progress in transition."[4]

The widespread interest in entrepreneurial startups, however, has not been

[3] Kornai (1990) and Murrell (1992) were perhaps the earliest to emphasize the difficulties of restructuring old enterprises and the crucial importance of new firm growth to economic transition. Johnson and Loveman (1995) examine case studies in Poland, and McMillan and Woodruff (2002) and McIntyre and Dallago (2003) provide recent overviews.

[4] The view that the size of the new private sector is a principal measure of progess in transition can be found, for instance, in EBRD (1999) or World Bank (2002). Analysis of the relative performance and growth of new private firms include Earle, Estrin, and Leshchenko (1996), Richter and Schaffer (1996), Bilsen and Konings (1998), Winiecki (2002), Grogan (2003), and Hellman, Jones, and Kaufmann (2003), although these papers do not analyze factors that may promote or hinder *de novo* development.

matched by anything close to a corresponding research effort. Research on East European economies has paid some attention to factors affecting self-employment decisions, some of which may be classified as entrepreneurial entry, although no such study has yet been undertaken for many of the countries of the region.[5] But what policy-relevant factors determine whether the embryonic enterprises, once they have been founded, develop into larger firms, creating jobs for workers and producing goods for consumers, or instead languish as tiny "mom-and-pop" operations with relatively few externalities for economic development?

Recent discussions of this question in transition economies have tended to focus on the possibility that aspects of the business environment – property rights, contract enforcement, efficient regulation – may be important determinants of small firm growth, perhaps more important than access to finance. In one of the few empirical studies of firm-level data, Johnson, McMillan, and Woodruff (2000) analyze employment and sales growth from 1994 to 1996 in five countries and find that "[a] lack of bank finance does not seem to prevent private-sector growth" and that "[m]ore inhibiting than inadequate finance are insecure property rights" (p. 1).[6] But these results are at variance with a number of other studies. Pissarides, Singer, and Svejnar's (2003) study of managers' perceptions in Russia and Bulgaria, for instance, finds that "constraints on external financing limit in important ways [the] ability to expand production," but insecurity of property rights is not "a major constraint" (p. 526). Our (2005) analysis of Romania survey data also supports this viewpoint.

[5] Earle and Sakova (1999, 2000) analyze self-employment in Bulgaria, Czech Republic, Hungary, Poland, Russia, and Slovakia. Grogan (2003) includes transitions to self-employment as part of a broader study of worker flows in Russia. Other surveys of small firms in Romania include IRIS (2000) and Murrell (2003).
[6] Related work on investment in small firms in Central and Eastern Europe includes Bratkowski, Grosfeld, and Rostowski (2000) and Johnson, McMillan, and Woodruff (2002).

There are strong *a priori* reasons to expect that lack of finance might stymie start-up sector growth in Romania and similar contexts. Even in economies with "well-functioning capital markets" (whatever that might mean nowadays), small and young firms tend to face much greater financial barriers than larger firms. But the conditions of the post-socialist economies could only serve to exacerbate this problem. Financial institutions hardly existed in the sense understood (or that we thought we understood) in the West: no commercial or investment banks, no venture capital or angel investors, no short-term or long-term paper, not even any loan sharks or pawn shops! Added to these problems was a general lack of savings and wealth that might be able to provide self-financing resources for a new business, a great difficulty in valuing any collateral that might exist on paper, and a poorly functioning legal system that lenders might use to pursue delinquent borrowers. The consequence is a situation not only or mainly of high borrowing rates, but one in which capital is unavailable to small firms at any price. Small firms appear to be largely rationed out of formal credit markets.

The domestic governments have had neither the financial resources nor the skills and know-how to address the capital shortage in this sector. In both the developed and developing worlds, however, experience with microcredit has been accumulating, and the results have been favorable, even laudatory (e.g., Morduch, 1999). Many of these programs (such as the Grameen Bank) focus on very small loans to own-account workers (entrepreneurs without employees) who are usually very poor and female, and who participate with groups of other borrowers in which all members can be penalized for the default of any member. Others (such as Bank Rakyat Indonesia's *Unit Desa*) provide somewhat larger loans to individual operating firms with small numbers of employees and without considering poverty and gender explicitly. Development agencies have tended to followed this latter model in their new private sector support policies in Eastern Europe.

The USAID has had the largest such program in Romania since the early 1990s. Three lending agencies were set up to provide credit to small firms: the Romanian-American Enterprise Fund (RAEF – Small Loan Program), the Cooperative Housing Foundation (CHF – Micro Loan Program), and World Vision (CAPA). The agencies for the most part operate in different regions of Romania, which we exploit in designing one of the comparison groups we employ below. But they follow similar principles, both to each other and to the *Unit Desa*, in most important characteristics: loan size and term, gender composition of borrowers, and loan practices (use of collateral, no group lending, some progressive lending, regular repayment schedules, and flexible client targeting mostly to non-poor). Also, like most microfinance agencies, the USAID lenders are profit-oriented, and they claim to provide loans on commercial terms and to make profits on these operations. Loan decisions are made on the basis of the accounting cash-flow for the past several years, and business plans are not part of the application. State-owned firms cannot receive loans under these programs, and start-ups are generally not immediately eligible.[7]

Our analysis is based on a list of all the firms that had received such a USAID loan by March 2000, a survey of most of these firms, and panel data on the universe of all Romanian firms. The next section provides more information on the firms in the USAID program and on other firms in the universal data that we draw upon to form comparison

[7] These statements about the nature of the loan programs are relevant through the period of our survey (spring 2001); they may have changed for new loans granted by these agencies subsequently.

groups.

3. Data

This section describes the sources and construction of our database and provides basic descriptive statistics on sample characteristics and on our measures of employment and sales growth.[8]

3.1 The Sample and Characteristics of USAID and Non-USAID Firms

This paper studies data from two sources. Data on the date of dispersal of a loan from an international organization comes from a survey of Romanian firm owner-managers conducted in May-June 2001. The sample was designed to cover all firms that had received a loan by March 2000 from one of three international loan agencies supported by USAID. We refer to these firms in this paper as "USAID," while the rest of the universe is referred to as "non-USAID." Out of a total of 386 firms receiving such loans, 297 were interviewed, with a refusal rate of about 15 percent.[9] The international loan agencies provided us with date of loan dispersal for XX of the nonrespondents.

These data are combined with annual balance sheet information for the universe of registered firms in Romania. We have compiled these data from the National Institute of Statistics and the Finance Ministry, checking and cleaning them for consistency.[10] From these data, we use measures of employment, sales, debt, profit, and capital stock

[8] Much more detail about the sample, questionnaire, survey organization, and data processing procedures can be found in CEU Labor Project (2002).

[9] A total of 89 could not be interviewed, for the following reasons: 4 had been bought out, 20 had closed, 5 did not have the owner-manager present, 19 could not be found, 9 had had their loan foreclosed and therefore did not cooperate, and 32 refused for other reasons. The refusal rate was thus about 15 percent.

[10] Besides removing obvious mistakes, we recoded variables as missing in a particular year if the value changed by a factor of 5 or greater from the previous year and reversed itself by the same factor in the next year. We also used all available information, including names and addresses, to reconnect longitudinal links broken by reregistrations.

from 1992 to 2006.

The composition of the USAID recipients and non-USAID universe by employment size category, industry, and *judet* (county) are shown in the first two columns ("full samples") of Tables 1-3. An important difference in our analysis compared to previous research is that the firms in our data tend to be drawn from the smaller end of the size distribution. Most of the literature studies the entire "small and medium enterprise (SME)" sector, including in the analysis firms with as many as 250 employees and paying little attention to the smallest category of micro enterprises (those with fewer than 10 employees).[11] The larger SMEs may be inherited state-owned enterprises or spin-offs from such firms, and thus not genuinely new startups, or they may be extraordinarily successful new firms, but they are unlikely to be typical.[12] Micro firms represent the overwhelming majority of small firms, as the second column of Table 1 makes plain: 84 percent of all Romanian firms in 1999. The USAID firms we study are mostly micro-enterprises (fewer than 10 employees); at 61 percent the share is smaller than in the universe, but it is much larger than previous studies of SMEs. Altogether the group of micro and small firms accounts for 93 percent of the USAID sample, very similar to the 95 percent in the universe. But within this group (which will be the focus of our analysis), the mix of USAID firms is somewhat more towards small and less

[11] The standard definition of micro firms is 0-9 employees, small firms 10-49, and medium-sized 50-249. Johnson, McMillan, and Woodruff (2000 and 2002) exclude micro firms and those with employment over 270 (pp. 14-15); the average employment in their sample of Romanian startups is 45.5. Pissarides, Singer, and Svejnar (2003) exclude firms with more than 200 employees and report average employment in their samples at 33.0 in Russia and 27.3 in Bulgaria. The average in our sample is 18.6 employees. For Romanian reports on the overall SME sector, see Romanian Center for Small and Medium Size Enterprises (1998) and National Agency for Regional Development (2000), and see EBRD (1999) for information on several countries.

[12] A further problem is that some data sets do not provide enough information on the history of the firm to permit any evaluation of the firm's origins, so that the new private sector is identified with the SME sector, although the latter may include firms that are neither new nor private. World Bank (2002) evaluates the performance of 20 or so different transition economies using this approach applied to official statistics on the firm size distribution.

towards micro – an important point for estimation that we return to below.

The loan agencies showed strong preferences for particular sectors, and indeed they prohibited a few entirely (military or any weapons and tobacco manufacture or distribution). For small businesses, it is not surprising that a large share is in trade, but as shown in Table 2, the fraction is actually less in the USAID than non-USAID samples. Manufacturing firms are disproportionately important in the USAID sample (29 percent versus 11 percent in the universe).

The distribution of USAID firms by region, shown in Table 3, follows the geographic spread of the loan programs. All regions of Romania are covered, but only 18 out of 41 counties (*judeţe*). Again, this is an important point for the approach we will take to designing alternative comparison groups.

Table 4 displays the year in which firms enter the universal database, labeled “start year.” The USAID firms are fairly similar to the non-USAID, but tend to be slightly older. Indeed, as noted above, it is an explicit loan criterion that the firm should have existed for several years prior to receiving the loan. Age is a very important correlate of firm growth, as is well-known from other studies (e.g., Davis and Haltiwanger, 1999). For example, a typical growth profile is concave in age, so comparisons should focus on firms at similar points in the life-cycle.

3.2 Outcome Measures

We analyze employment and sales levels, conditional on firm survival; therefore we also study the effects of the microcredit loans on the probability of survival. This section describes the employment, sales, and survival variables that constitute the outcome variables in our analysis.

The definition of employment growth in the present study differs from previous research, which calculates the change in the number of workers from the firm's start-up to the date of interview.[13] We instead make use of the entire available time series on annual employment. This permits a more precise assessment of the timing of employment effects, rather than cumulating over a long period of time. We also use annual information on sales, but exclude the start-up year from the analysis because it is typically a highly volatile period in which firms may not fully operate.[14] Because the final year data of an exiting firm may also reflect part-year operation, we also eliminate observations on sales from this year (unless the year is 2006, the final year in our database).

The first two columns of Tables 5 and 6 show the means and standard deviations for employment and sales in each year for the USAID sample and non-USAID universe. The 1992 difference in employment between these sets of firms is striking: 70 at the mean for non-USAID, 11 for USAID. The proportionate difference in sales is similar. In both cases the average size of the non-USAID universe diminishes rapidly, as restructuring of old enterprises and entry of new businesses work together to diminish it. The difference provides further evidence, however, of the heterogeneity in the non-USAID universe.

The difference reverses by the last years in the sample, as the USAID starts to exceed the non-USAID sample average. This reversal reflects another type of

[13] The studies discussed by Liedholm and Mead (1999) appear to analyze employment growth from start-up without scaling by firm age, so that surviving older firms will almost certainly display higher "growth."
[14] Indeed, a finding reported in Liedholm and Mead's (1999) summary of research on Africa, that smaller size in the start-up year is associated with larger subsequent growth, might be accounted for by start-up year size reflecting later or only partial start-up during the first year followed by catch-up growth in subsequent years.

heterogeneity of concern for our analysis: as noted above, the USAID programs did not provide financing of entrants, but only of firms that had already been successful for several years. The reversal takes place after 2000 for employment and after 1995 for sales, probably reflecting faster productivity growth in the USAID compared to the non-USAID samples – yet another type of heterogeneity for concern.

At the same time, the rate of increase in employment (and sales, to a lesser extent) among the USAID firms displays a general decline over the period. To some extent, the changes may be influenced by the recession of the late 1990s in Romania, and they may also reflect age effects: as the firms in the sample grow older, their growth rates follow a typical life cycle decline. This pattern suggests not only that age and year should be controlled for in the statistical analysis, but also that our search for the effects of loans on subsequent outcomes faces an uphill battle in the face of the life cycle effect.

The final outcome variable we study is also related to life cycle issues: survival. Table 7 shows the exit year (last year of appearance in the universal database) for the USAID and non-USAID samples of firms existing in 1999. Exit rates are much higher among non-USAID firms in most years, with a cumulative survival rate through 2006 of 55.8 percent, while the corresponding rate is 77.0 percent for USAID firms. This relationship can hardly be interpreted causally, however, as the non-USAID sample during this period is on average younger and smaller, and both characteristics are associated with lower probabilities of survival (e.g., Davis and Haltiwanger, 1999). Creating a comparison group with similar characteristics to the treated firms is thus crucial for the survival analysis.

4. Constructing the Matched Comparison Groups

The previous section showed that the USAID sample and universe of registered firms in Romania differ considerably in terms of some observable characteristics, and we might expect that they also differ, therefore, in the average likelihood of receiving a loan. To examine the effect of the loans on employment growth of the USAID firms, we need to select a comparison group from the universe that has a common support with the USAID sample. Our procedure has five steps. The first is to define more precisely the treatment we will focus upon, which raises some caveats and potential biases for our analysis. Second, we truncate the original samples to exclude some firms, based on *a priori* considerations. Third, we impose some exact criteria within which we will consider possible matches. Fourth, we estimate probability of treatment functions. The fifth step is to select matches within a propensity score bandwidth surrounding the treated firm's propensity score. Throughout, we adopt the language of an experiment, referring to the USAID loan recipients as the "treatment group" (T), all other firms in the universe as the "nontreatment group" (N), and the selected comparison firms as the "matched comparison group" (M).

The data set admits various possibilities for defining a loan access treatment. In this paper, we focus on the first international loan received by a USAID firm (i.e., a firm that ever received a loan through the USAID agencies). In all but four cases, the first loan was received from one of the USAID agencies, but in those four a different international agency provided the loan, also on microcredit terms. We choose to focus on the first loan without regard to the provider because the terms are very similar, and the first loan is likely to start a process of growth and credit history that makes later loans easier to obtain. It should be noted that this definition of treatment ignores the effects of later loans as well as the size of all loans. While these assumptions are conventional in the evaluation and matching literatures, they are limitations on the analysis in this paper.

This discussion raises the point that USAID was not the only provider of microcredit to small Romanian firms during this period. Our understanding is that it was the largest, but some of the firms in the N group certainly, and some in the M group possibly, received loans from other international agencies. If the effects of these loans are positive, then this would imply that our estimates of the effects among USAID firms are understated.

Table 8 shows the distribution of the first year of an international loan through 2001. The year 1999 is the most common outcome, and in cases where information on the timing of the first loan is missing – particularly for nonrespondents to our survey – we impute 1999 as the year. Our USAID list includes only firms receiving loans before March 2001, so 1999 is a conservative choice: if the true loan dates are prior to 1999, then imputing 1999 will tend to understate the true loan effects. Moreover, we do not include firms receiving first international loans from USAID after March 2001 in T, so if they appear in M, again our treatment effect estimates will be understated.

In defining the T group, we first eliminate two types of firms that are unusual in the set of loan recipients and for which we therefore have little hope of estimating treatment effects. The first type is firms that ever have any state ownership. The USAID programs prohibited state-owned firms from receiving loans, but the USAID sample contains three that had been privatized. Because of the many differences between old and new firms in the transition context, we eliminate them from the sample. Secondly, we eliminate firms with 50 or more employees in the year prior to loan reception. This elimination amounts to less than 7 percent of the sample, as column 1 of Table 1 shows. Again, these firms were not the target group of the loan programs, and there are too few to carry out reliable estimates. The characteristics of the USAID sample after this truncation are shown in column 3 of each Table 1 to 7. The number of USAID firms is reduced by about 10 percent, from 339 to 308 in the case of employment in 1999, for example.

The same two deletions are also applied to the non-USAID universe. In addition, we eliminate firms operating in 3-digit NACE industries where no USAID loans were received. We also eliminate non-USAID firms where any time series observation violates the basic cleaning rules described in Section 3.1, where either sales or employment is missing in the year prior to the loan, and where industry or county are missing in the year of the loan. From this point, we construct two versions of the N group. The first eliminates all firms in counties where no USAID loans were available, so that we analyze a homogeneous set of locations. The assumption is that economic conditions are likely to be similar for firms in the same county, just as they tend to be for firms of the same industry.

The characteristics of firms in this N group (the non-USAID same country sample) after this set of truncations are shown as column 4 in Table 1 to 7. The number of non-USAID observations falls by about 50 percent, for instance from 219,000 to 108,000 for employment in 1999. Comparing the characteristics shows that, while the truncated N samples are much more similar to each other than the full samples, the differences are still substantial.

A potential problem with this comparison group is that firms receiving loans may be systematically different from those not receiving loans; for instance, the former may self-select themselves into treatment because they have information, beyond what we observe, on the usefulness of the loan. So few Romanian firms actually received USAID loans (about 1 in 1000) that it seems to us that there could be many comparable firms with potential loan impacts similar to those of the recipients.[15] Anecdotal information suggests competition for the loans was fierce. But we do not have a list of firms that applied and were rejected.

Therefore, we also consider a second set of comparison firms. This approach exploits an institutional feature of the USAID programs, which made loans available only in certain counties of Romania: 18 out of 41, as of March 2000. Firms located in the 23 non-USAID counties were not eligible for the loans, and thus by definition there is no problem of self-selection. In this approach, we include only firms in these 23 counties as potential comparators in the matching regressions. This sample constitutes the second N group.

We next impose some exact criteria on which firms in N can be in the matched comparison group M. We require exact matches on 3-digit NACE industry and age, and in the case of the same county comparison group, we also require exact match on county. In one set of results (denoted by "imposing pre-loan employment level restriction" or "imposing pre-loan sales level restriction" in the results below), we also impose a further

[15] Moreover, the relevant policy question is unlikely to be whether the loan program could or should be extended to the universe of firms, but rather whether it should be scaled up or down. For instance, policymakers might consider eliminating the program, or doubling or tripling it, but they are unlikely to increase it by a factor of 100 or 1000. Our objective, as is conventional in the treatment literature, is thus to estimate the effect of treatment on the treated (and on very similar firms), not to estimate an average effect across all firms in Romania.

restriction that the control firm's value of the performance variable fall between one-half and twice the size of the treated firm's value in the year prior to treatment.

The next step is to estimate equations for the probability of treatment for these restricted samples. While some matching estimates rely on very little pre-treatment information, we feel it is important to condition on a significant history of the outcome variables and other regressors in order to obtain reliable matches. Appendix Table 1 shows the estimates of the marginal effects from probit estimation of the treatment probability among firms in the 18 USAID counties.[16] The year of treatment is *t*, and only observations from the N group from this year are considered. The employment and sales samples differ because of more outliers in the sales time series and the deletion of the first and last observations on sales for any firm in any year except 1992 and 2006 (because of the part-year operation problem described above).

The variables in the probits include the lagged outcome variables: the natural logs of the performance variable in the year prior to treatment, its square, and the changes in the performance variable going back to *t*-4. They also include log(total assets), the profit/sales ratio, log(debt), and log(wage) in *t*-1, age (number of years of operation) at *t*, age squared, and industry, county, and year dummies.[17] The probits are weighted such that the T group and the N group are weighted equally, i.e., firms receiving an international loan receive a weight of N/T and potential controls receive a weight of one. The results in Appendix Table 1 suggest that firms with higher growth and profitability, more assets and debt, and lower wages are more likely to get a loan. In the specifications

[16] Firms in non-USAID counties are not included in the probits, even when the matched comparison group M comes from among them. We use out-of-sample predictions from the probits to obtain non-USAID firm propensity scores.
[17] County dummies are excluded when the matched comparison group M is firms in non-USAID counties.

not imposing a restriction on the performance variable in t-1 (other than that the firm have fewer than 50 employees) before running the probit, larger firms are more likely to get a loan.[18]

We impose a common support and a 0.9-1.1 bandwidth of the treated to nontreated propensity score ratio. Epanechnikov kernel weights are assigned to control firms within the bandwidth.[19] The sampling is done with replacement, so if there is a nontreated firm with a propensity score within the bandwidth of x treated firms, this firm will be included as a control firm x times.[20] Two of these M groups include matched pairs within the same counties, and the other two match T firms only with firms in counties not eligible for the USAID loans. Appendix Table 2 shows the number of treated firms and matched controls, broken down by propensity score quintile.

We perform several balancing tests for the treated firms and matched controls. Appendix Table 3 shows kernel-weighted means of the independent variables in the probits (except age, age squared, industry, county, year, and missing value dummies)[21] for the M and T groups, the Rosenbaum and Rubin (1985) test of standardized differences, and a *t* test for the difference in kernel-weighted means. We also run kernel-weighted regressions of the probit independent variables on a quartic of propensity scores and a quartic of propensity scores interacted with treatment dummies and show *F* tests for the joint significance of the interaction terms in the table.[22] Appendix Table 4 shows

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[18] As a robustness check, we have performed the matching and performance regression exercises with variations on these probit specifications, and the results are qualitatively similar.
[19] We have also performed estimations using radius matching, single nearest neighbor matching, and four nearest neighbor matching and have received qualitatively similar results to kernel matching.
[20] Each set of nontreated firm values in the probit and performance regressions is specific to the treated firm to which it is matched.
[21] Note that potential controls are limited to ones with exact matches on age, two-digit industry, year, and sometimes county, so balance on these variables is already assured.
[22] This test was performed in Smith and Todd (2005).

Hotelling T^2 test of the joint null of equal means of all the probit independent variables included in Appendix Table 3, separately by propensity score quintile. Finally, in Appendix Tables 5 and 6 we show a generalized version of the Heckman and Hotz (1989) "pre-program" test, calculating *F*-tests for the joint significance of dummies for treated firms in the three years prior to treatment.

The matching and reweighting significantly reduces the standardized differences except for the profit/sales ratio with one of the eight sets of controls. The standardized difference values are all below 20, a value Rosenbaum and Rubin (1985) consider large. The *t* tests, *F* tests, and Hotelling T^2 tests are quite significant when not imposing a restriction on *t-1* performance values, however. The tests are either insignificant or at least less significant after imposing the restriction, motivating us to display performance regressions below with the restriction. We also show the results without the restriction, as the imposition of this restriction reduces the number of treated firms with matches (see Appendix Table 2).

The Heckman-Hotz *F*-tests are all insignificant, implying no systematic pre-treatment differences in performance between the treated and matched samples. Just two individual dummies are significant – the *t-1* employment values when using non-USAID county controls. Only one of the eight specifications (sales with controls from non-USAID counties and a pre-loan sales level restriction) shows any increase in treatment firm performance relative to controls in the pre-treatment period.

5. Estimating Loan Effects

One approach to estimating the treatment effect would involve a simple comparison of the mean group rates of the T and M groups post-treatment. Another possibility is a difference-in-differences approach where the pre-treatment outcomes are also taken into account. These approaches frequently ignore additional information on the outcome variable before and after treatment, and on the quality of the matching. Our approach uses the full time-series information for each firm in the treatment and matched comparison groups to estimate the treatment effect.

The usual approach somewhat arbitrarily chooses a single pre-treatment period and a single post-treatment period to compute after-minus-before differences of the outcome variable for the difference between the treated and matched firm. This method has the advantage over a cross-sectional approach of controlling for common trends within a matched pair, but it suffers from sensitivity to the choice of periods – what Heckman et al. (1999) call the "alignment fallacy." An alternative is to use pre- and post-treatment averages, but this does not control for systematic differences in match quality across pairs, nor does it use the full time-series information available in the data. We address the first of these issues by estimating difference-in-differences models including matched-pair-specific fixed effects (FE), and we address the second by estimating models with full dynamics of the effect around the treatment date. The latter method allows us to assess the extent to which pre-treatment behavior of the outcome variable is similar within matched pairs not only in levels but in dynamics. For instance, if firm performance tends to fall or rise more for the treated or non-treated firm just prior to loan receipt, this method will pick it up. This method also allows us to assess the extent to which the effects of loan receipt are immediate or gradual, and short-term only or sustained for a longer period.

For comparison purposes, we also show OLS regressions with covariates potentially related to firm growth.

Standard errors are bootstrapped with 500 repetitions to adjust for additional variability sources introduced by propensity score estimation and the matching process.[23]

For the survival analysis, we estimate Cox-Proportional-Hazard models with number of years the firm survives after receiving an international loan as the dependent variable and a treatment dummy as the independent variable. One model includes covariates potentially related to survival.

Tables 9-13 show results. The estimated effects on employment and sales are positive and statistically and substantively significant for both comparison groups, with and without a *t-1* performance variable restriction in the matching, and in both OLS and FE specifications. The estimated effects on survival, however, are statistically insignificant. We speculate that the loan may raise growth but also riskiness for recipient firms.

Dynamic specification employment and sales results are shown in Figures 1-8 and Appendix Tables 5-6. The base category is observations four or more years prior to the treated firm's reception of its first international loan. The loan effect is positive already in the year of the loan for both employment and sales, and it increases further in the year after the loan. In several of the specifications there is a further increase in the treatment effect four years after the loan. The effects are long lasting, as significant differences between the treated and matched firms are maintained five and more years after reception of the loan.

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[23] According to Imbens and Wooldridge (2009), bootstrapping is likely to be a valid method for calculating standard errors for kernel estimators, as the number of matches increases with sample size.

6. Conclusion

Despite all the interest in microfinance as a tool for economic development, there has been relatively little research on the externalities of microfinance loans for nonparticipants and the broader economy. Potential externalities include job creation and sales growth (increasing consumer surplus): as their financial constraints are relaxed, entrepreneurs may expand their operations and hire others to work with them. But we have little knowledge of how effective the programs are in promoting employment growth, and thus of how important financial constraints are for small firms.

In this paper, we have analyzed a database containing both recipients of loans from USAID-funded programs and a comparison group of firms drawn from the universe in Romania. Detailed data on financial and operating histories of all firms permit us to construct a matched comparison group with treatment propensities that follow a distribution essentially identical to that of the treatment group. The use of this procedure mitigates any selection bias associated with observables, including four years of pre-treatment financial variables. We exploit the long panel – 15 years of data – not only to construct better matches, but to evaluate remaining selection bias based on unobservables prior to treatment, to control for matched-pair-specific fixed differences in the outcome variables, and to study the dynamics of the effects post-treatment.

The results suggest that the USAID loans have indeed increased employment and sales, thus implying that these programs may generate important externalities beyond their effects on the livelihoods of the entrepreneur-recipients. The estimated effects remain sizable and statistically significant even when we include firm fixed effects, and they are similar when we exploit the uneven geographic coverage of the USAID loan availability to construct a matched comparison group in counties where firms could not receive such loans. Overall, the point estimates lie in the range of 0.2 to 0.4, implying substantial job creation and sales expansion. We do not find a significant effect on survival probabilities, however. A possible interpretation of this result is that while the loan increases the financial resources available to the firm, it may also increase risk.

Following nearly all the matching literature, such as standard evaluations of training and other labor market programs, we have treated the first international loan receipt as a dummy variable. This approach takes into account neither the size of the loan, its term, interest rate, or collateral requirement, nor the presence of subsequent international loans or any other loans that affect the financial resources of the firm. A more general approach would consider the strength and frequency of the dosage, but we leave this to future research.

We also leave open the question of the proper interpretation of the finding that employment and sales are positively affected by access to microcredit. One possibility is that the microcredit programs enable firms to raise capital more cheaply than they could otherwise, for instance by lowering the interest rate spread. A second possibility is that the credit market is so highly rationed that firms cannot raise capital at just about any price. In favor of the second possibility is the loan agencies' assertion that the loans are commercially based in terms of interest and collateral and that, indeed, they are profitable operations for the lenders. On the other hand, the survey of USAID firms suggested they do receive other loans, so the USAID is apparently not the only source of formal finance. Further progress on this question might benefit from a comparison of the terms of international versus other types of loans, and from an analysis of the timing of the loans, in particular whether receipt of a USAID loan tends to open up other financial sources – or possibly vice versa. Future research could also exploit the set of debt variables included in the universal data to improve understanding of the role played by microcredit in the process by which successful start-up firms gradually gain access to a wider variety of financial markets.

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Table 1: Composition of USAID and Non-USAID Firms
By Employment Size Category in 1999 (Percent)

Number of Employees	Full Samples		Truncated Samples	
	USAID	Non-USAID	USAID	Non-USAID
0-9 (micro)	61.36	83.54	65.91	88.46
10-49 (small)	31.86	12.14	34.09	11.54
50-249 (medium)	6.78	3.29	0.00	0.00
250+ (large)	0.00	1.03	0.00	0.00
Number of firms	339	218,759	308	108,189

Note: "Full samples" are the firms for which information on number of employees is available in 1999; "truncated samples" are 1999 observations excluding firms that are ever state-owned, have more than 50 employees, and operate in 3-digit industries or counties (*județe*) where no USAID loans were available. "USAID" refers to firms that received a loan from one of the three USAID lending agencies by March 2000; "non-USAID" refers to all other firms in the universal database. The truncated non-USAID sample forms the basis for the selection of the same-county comparison group.

Table 2: Composition of USAID and Non-USAID Firms By Industry in 1999 (Percent)

	Full Samples		Truncated Samples	
	USAID	Non-USAID	USAID	Non-USAID
Agriculture & Forestry	1.39	2.90	1.30	1.98
Fishing	0.00	0.04	0.00	0.00
Mining and Quarrying	0.00	0.08	0.00	0.00
Manufacturing	28.61	11.16	26.30	10.75
Elect., Gas, & Water Supply	0.00	0.08	0.00	0.00
Construction	1.39	3.10	1.62	3.81
Wholesale & Retail Trade; Repair	54.44	66.94	56.82	68.51
Hotels and Restaurants	3.33	3.00	3.25	3.44
Transportation & Commun.	5.56	3.43	4.87	3.87
Financial Intermediation	0.28	0.45	0.32	0.39
Real estate	2.78	4.74	2.92	4.27
Pub. Admin. & Defense	0.00	0.02	0.00	0.00
Education	0.28	0.16	0.32	0.03
Health and Social Work	1.11	1.04	1.30	1.12
Other Community, Social, & Personal Service Activities	0.83	2.85	0.97	1.83

Note: "Full samples" are the firms for which information on industry affiliation is available in 1999; "truncated samples" are 1999 observations excluding firms that are ever state-owned, have more than 50 employees, and operate in 3-digit industries or counties (*județe*) where no USAID loans were available. "USAID" refers to firms that received a loan from one of the three USAID lending agencies by March 2000; "non-USAID" refers to all other firms in the universal database. The truncated non-USAID sample forms the basis for the same-county comparison group.

Table 3: Composition of USAID and Non-USAID Firms By County in 1999 (Percent)

Judeţ	Full Samples		Truncated Samples	
	USAID	Non-USAID	USAID	Non-USAID
Alba	1.67	1.37	1.30	2.98
Arad	3.61	2.03	3.57	4.43
Arges	2.78	2.46	2.60	4.86
Bacau		2.55		
Bihor		3.76		
Bistrita-Nasaud		1.25		
Botosani		1.10		
Brasov		3.52		
Braila	0.56	1.55		
Buzau	1.67	2.13	0.97	3.18
Caras-Severin	11.11	1.02	12.01	2.16
Cluj	18.61	4.10	20.78	7.32
Constanta	2.78	4.08	2.60	7.73
Covasna		1.04		
Dimbovita		1.66		
Dolj	11.94	3.55	12.34	5.85
Galati	0.56	2.81	0.65	5.01
Gorj		1.50		
Harghita		1.33		
Hunedoara	10.00	2.04	9.74	4.14
Ialomita		1.05		
Iasi	4.17	3.12	3.57	6.11
Ilfov		1.02		
Maramures		1.85		
Mehedinti	1.67	1.26	1.95	2.15
Mures	2.78	2.33	2.60	4.68
Neamt		1.77		
Olt		1.56		
Prahova		3.69		
Satu-Mare		1.57		
Salaj		0.81		
Sibiu	3.33	2.13	3.90	4.08
Suceava	0.28	2.05		
Teleorman		1.32		
Timis	18.13	3.18	18.18	6.62
Tulcea		1.26		
Vaslui		0.95		
Vilcea		1.71		
Vrancea		1.45		
Bucuresti	4.17	19.31	3.25	28.71
Calarasi		0.89		
Giurgiu		0.89		

Table 4: Composition of USAID and Non-USAID Firms as of 1999
By Start Year (Percent)

	Full Samples		Truncated Samples	
	USAID	Non-USAID	USAID	Non-USAID
1992	25.75	22.57	24.68	23.30
1993	14.63	16.76	14.94	16.21
1994	22.22	17.99	23.05	18.38
1995	14.91	14.12	15.26	13.44
1996	7.32	6.73	7.14	6.94
1997	8.40	7.10	8.77	7.26
1998	4.34	8.05	4.22	8.53
1999	2.44	6.67	1.95	5.94

Table 5: Mean (SD) Employment By Year for USAID and Non-USAID Firms

	Full Samples		Truncated Samples	
	USAID	Non-USAID	USAID	Non-USAID
1992	10.94	70.19	7.48	6.13
	(17.64)	(1,026.08)	(8.36)	(53.70)
1993	14.35	41.48	11.73	5.38
	(33.39)	(754.81)	(31.51)	(41.59)
1994	10.76	41.72	8.26	4.59
	(21.54)	(804.25)	(16.38)	(29.80)
1995	10.94	30.51	7.31	4.40
	(23.11)	(628.92)	(9.47)	(9.72)
1996	11.57	25.85	8.60	4.67
	(19.88)	(539.15)	(10.03)	(10.06)
1997	12.36	23.16	9.61	4.69
	(19.22)	(516.23)	(10.61)	(8.73)
1998	14.69	20.13	10.22	4.67
	(23.40)	(360.19)	(11.05)	(7.69)
1999	16.06	19.02	10.45	4.61
	(26.26)	(332.32)	(11.14)	(6.27)
2000	18.23	18.60	12.06	5.38
	(30.86)	(316.86)	(15.17)	(8.91)
2001	20.93	19.42	14.41	6.05
	(36.06)	(316.96)	(19.24)	(11.31)
2002	22.44	19.11	15.95	6.32
	(41.63)	(274.40)	(23.86)	(12.79)
2003	25.95	14.99	17.96	5.61
	(50.63)	(216.52)	(28.24)	(13.23)
2004	28.29	12.10	19.37	4.90
	(54.45)	(175.43)	(31.64)	(12.19)
2005	29.80	11.16	19.87	4.86
	(63.89)	(158.94)	(33.28)	(12.20)
2006	33.75	10.88	23.52	5.06
	(80.39)	(144.10)	(63.88)	(13.37)

Table 6: Mean (SD) Sales By Year for USAID and Non-USAID Firms (2006 ROL)

	Full Samples		Truncated Samples	
	USAID	Non-USAID	USAID	Non-USAID
1992	9,796 (18,597)	37,117 (1,340,216)	8,241 (17,039)	8,999 (54,137)
1993	10,044 (16,848)	41,548 (2,174,576)	8,723 (15,389)	8,366 (49,697)
1994	12,484 (21,216)	24,075 (1,055,427)	11,897 (21,290)	8,206 (44,198)
1995	15,303 (50,815)	18,940 (807,400)	11,091 (21,136)	8,600 (56,741)
1996	16,718 (49,708)	17,176 (768,918)	13,639 (27,609)	9,004 (70,123)
1997	17,106 (45,983)	14,510 (722,301)	14,501 (32,336)	6,842 (41,567)
1998	16,420 (47,377)	11,893 (423,008)	12,894 (30,111)	6,038 (32,428)
1999	17,757 (48,428)	12,352 (476,833)	12,387 (28,167)	6,242 (27,545)
2000	17,869 (44,298)	13,097 (496,803)	13,314 (29,550)	6,627 (34,950)
2001	21,296 (54,196)	13,716 (419,215)	15,404 (36,132)	7,446 (36,026)
2002	25,438 (64,694)	14,875 (372,943)	19,084 (52,339)	8,740 (47,907)
2003	32,367 (89,308)	15,666 (327,297)	21,729 (64,158)	9,269 (61,926)
2004	37,475 (99,859)	16,640 (400,431)	25,306 (72,947)	8,764 (64,070)
2005	38,222 (101,177)	15,828 (380,974)	26,939 (73,247)	8,418 (63,861)
2006	43,946 (12,433)	15,912 (374,553)	30,215 (78,125)	9,085 (86,991)

Table 7: Year of Exit for USAID and Non-USAID Firms Existing in 1999 (Percent)

	Full Samples		Truncated Samples	
	USAID	Non-USAID	USAID	Non-USAID
2000	0.81	5.09	0.00	0.00
2001	2.44	6.42	2.27	3.63
2002	2.98	6.18	2.92	3.28
2003	1.63	1.30	1.62	1.24
2004	4.88	10.45	5.52	6.64
2005	5.15	5.41	5.19	4.58
2006	5.15	9.36	5.84	8.21
Survived thru 2006	76.96	55.80	76.62	72.41

Table 8: Distribution of Year of First International Loan

	Number of Firms	Percent of Firms
1993	1	0.3
1994	1	0.3
1995	4	1.1
1996	7	1.9
1997	8	2.2
1998	62	16.9
1999	200	54.4
2000	82	22.3
2001	3	0.8
N	368	100.0

Table 9: Estimates of the International Loan Impact on Employment

	Matches in Same Counties		Matches in Non-USAID Counties	
	OLS	FE	OLS	FE
Post Loan	0.242***	0.221***	0.323***	0.176***
	(0.034)	(0.034)	(0.034)	(0.041)
Age	0.157***		0.139***	
	(0.017)		(0.023)	
Age Squared	-0.011***		-0.010***	
	(0.001)		(0.002)	
Micro	-1.655***		-1.784***	
	(0.031)		(0.032)	
Small	-0.709***		-0.858***	
	(0.035)		(0.039)	
Firms	6,026	6,026	52,373	52,373
Obs.	57,407	57,592	503,658	504,794

Notes: "Matches in same counties" refers to the matched comparison group where the matched pair is in the same county (non-USAID counties are excluded); "matches in non-USAID counties" refers to the case where matches are drawn only from counties where USAID loans were unavailable. Loan timing and year effects are included in all specifications. Two-digit industry effects are included in the OLS specifications. County effects are included in OLS specifications with matches in same counties. Standard errors, bootstrapped with 500 repetitions, are in parentheses. * = significant at 10-percent level. ** = significant at 5-percent level. *** = significant at 1-percent level.

Table 10: Estimates of the International Loan Impact on Sales

	Matches in Same Counties		Matches in Non-USAID Counties	
	OLS	FE	OLS	FE
Post Loan	0.326***	0.295***	0.601***	0.376***
	(0.044)	(0.051)	(0.052)	(0.058)
Age	0.180***		0.165***	
	(0.031)		(0.034)	
Age Squared	-0.013***		-0.015***	
	(0.002)		(0.002)	
Micro	-1.879***		-2.068***	
	(0.048)		(0.058)	
Small	-0.655***		-0.864***	
	(0.057)		(0.064)	
Firms	4,209	4,209	48,182	48,182
Obs.	46,980	46,980	521,993	521,993

Notes: "Matches in same counties" refers to the matched comparison group where the matched pair is in the same county (non-USAID counties are excluded); "matches in non-USAID counties" refers to the case where matches are drawn only from counties where USAID loans were unavailable. Loan timing and year effects are included in all specifications. Two-digit industry effects are included in the OLS specifications. County effects are included in OLS specifications with matches in same counties. Standard errors, bootstrapped with 500 repetitions, are in parentheses. * = significant at 10-percent level. ** = significant at 5-percent level. *** = significant at 1-percent level.

Table 11: Estimates of the International Loan Impact on Employment, Imposing Pre-Loan Employment Level Restriction

	Matches in Same Counties		Matches in Non-USAID Counties	
	OLS	FE	OLS	FE
Post Loan	0.255***	0.254***	0.206***	0.142***
	(0.032)	(0.036)	(0.041)	(0.042)
Age	0.159***		0.191***	
	(0.018)		(0.024)	
Age Squared	-0.012***		-0.013***	
	(0.001)		(0.002)	
Micro	-1.817***		-1.603***	
	(0.030)		(0.038)	
Small	-0.810***		-0.616***	
	(0.037)		(0.045)	
Firms	3,368	3,368	23,250	23,250
Obs.	32,487	32,500	241,029	241,409

Notes: "Matches in same counties" refers to the matched comparison group where the matched pair is in the same county (non-USAID counties are excluded); "matches in non-USAID counties" refers to the case where matches are drawn only from counties where USAID loans were unavailable. Loan timing and year effects are included in all specifications. Two-digit industry effects are included in the OLS specifications. County effects are included in OLS specifications with matches in same counties. Standard errors, bootstrapped with 500 repetitions, are in parentheses. * = significant at 10-percent level. ** = significant at 5-percent level. *** = significant at 1-percent level.

Table 12: Estimates of the International Loan Impact on Sales, Imposing Pre-Loan Sales Level Restriction

	Matches in Same Counties		Matches in Non-USAID Counties	
	OLS	FE	OLS	FE
Post Loan	0.231***	0.285***	0.327***	0.322***
	(0.052)	(0.063)	(0.060)	(0.063)
Age	0.219***		0.256***	
	(0.037)		(0.040)	
Age Squared	-0.015***		-0.018***	
	(0.002)		(0.003)	
Micro	-1.767***		-2.046***	
	(0.064)		(0.068)	
Small	-0.748***		-0.865***	
	(0.075)		(0.080)	
Firms	1,469	1,469	13,433	13,433
Obs.	16,374	16,374	160,086	160,086

Notes: "Matches in same counties" refers to the matched comparison group where the matched pair is in the same county (non-USAID counties are excluded); "matches in non-USAID counties" refers to the case where matches are drawn only from counties where USAID loans were unavailable. Loan timing and year effects are included in all specifications. Two-digit industry effects are included in the OLS specifications. County effects are included in OLS specifications with matches in same counties. Standard errors, bootstrapped with 500 repetitions, are in parentheses. * = significant at 10-percent level. ** = significant at 5-percent level. *** = significant at 1-percent level.

Table 13: Estimated Impact of International Loan on Survival

	Matches in Same Counties		Matches in Non-USAID Counties	
	No Pre-Loan Employment Restriction	Pre-Loan Employment Restriction	No Pre-Loan Employment Restriction	Pre-Loan Employment Restriction
Without Covariates				
USAID	0.911	0.954	0.945	1.120
	(0.150)	(0.166)	(0.190)	(0.242)
With Covariates				
USAID	0.919	0.936	0.977	1.137
	(0.154)	(0.165)	(0.204)	(0.252)
Age	1.021	0.911	1.109	1.171
	(0.293)	(0.271)	(0.407)	(0.586)
Age Squared	0.999	1.007	0.997	0.992
	(0.025)	(0.026)	(0.031)	(0.039)
Micro	0.859	0.988	0.879	1.242
	(0.224)	(0.296)	(0.253)	(0.443)
Small	0.904	0.996	0.957	1.256
	(0.281)	(0.345)	(0.339)	(0.512)
Firms	5,188	3,100	33,961	16,966

Notes: These are Cox Proportional Hazard regressions with kernel weights. Survival is measured as the number of years a firm operates after the treated firm receives an international loan. "Matches in same counties" refers to the matched comparison group where the matched pair is in the same county (non-USAID counties are excluded); "matches in non-USAID counties" refers to the case where matches are drawn only from counties where USAID loans were unavailable. USAID is a dummy variable equal to one if the firm received an international loan. Two-digit industry effects are also included in the specifications with covariates. Standard errors are in parentheses. * = significant at 10-percent level. ** = significant at 5-percent level. ** = significant at 1-percent level.

Appendix Table 1: Loan Probits

	Employment without Pre-Loan Restriction	Emp. with Pre-Loan Emp. Restriction	Sales without Pre-Loan Restriction	Sales with Pre-Loan Sales Restriction
Log Employment$_{t-1}$	0.052***	-0.110***		
	(0.007)	(0.011)		
Log Employment$_{t-1}$ Squared	0.017***	0.066***		
	(0.002)	(0.004)		
Log Emp$_{t-1}$ -Log Emp$_{t-2}$	0.084***	0.085***		
	(0.005)	(0.008)		
Log Emp$_{t-2}$ -Log Emp$_{t-3}$	0.070***	0.050***		
	(0.005)	(0.008)		
Log Emp$_{t-3}$ -Log Emp$_{t-4}$	0.025***	0.032***		
	(0.006)	(0.008)		
Log Sales$_{t-1}$			0.371***	-0.513***
			(0.024)	(0.059)
Log Sales$_{t-1}$ Squared			-0.011***	0.018***
			(0.001)	(0.002)
Log Sales$_{t-1}$ -Log Sales$_{t-2}$			0.101***	0.104***
			(0.004)	(0.008)
Log Sales$_{t-2}$ -Log Sales$_{t-3}$			0.051***	0.055***
			(0.004)	(0.007)
Log Sales$_{t-3}$ -Log Sales$_{t-4}$			0.041***	0.030***
			(0.004)	(0.008)
Log Total Assets$_{t-1}$	0.008**	0.013**	0.013***	0.005
	(0.004)	(0.006)	(0.005)	(0.009)
Profit$_{t-1}$/Sales$_{t-1}$	0.304***	0.304***	0.255***	0.334***
	(0.013)	(0.023)	(0.018)	(0.040)
Log Debt$_{t-1}$	0.103***	0.105***	0.106***	0.121***
	(0.004)	(0.005)	(0.004)	(0.008)
Log Wage$_{t-1}$	-0.043***	-0.043***	-0.071***	-0.077***
	(0.003)	(0.005)	(0.004)	(0.008)
Age	-0.033***	-0.052***	0.181***	0.122***
	(0.009)	(0.014)	(0.014)	(0.027)
Age Squared	0.002**	0.003***	-0.012***	-0.008***
	(0.001)	(0.001)	(0.001)	(0.002)
N	102,305	42,846	71,610	19,778

Notes: These are marginal effects from probits with an international loan in year *t* as the dependent variable. The regressions also include dummies for missing total assets, profit/sales, debt, and wage, as well as three-digit industry, county, and year effects. Standard errors are in parentheses. * = significant at 10-percent level. ** = significant at 5-percent level. *** = significant at 1-percent level.

Appendix Table 2: Frequency Distribution of Treated and Untreated Firms by Propensity Score Quintile

Employment, Matches in Same Counties, with no Pre-Loan Employment Restriction

Quintile	Treated	Untreated
First	9	449
Second	35	1,731
Third	49	1,491
Fourth	47	1,012
Fifth	140	1,237
Total	280	5,920

Employment, Matches in Same Counties, with Pre-Loan Employment Restriction

Quintile	Treated	Untreated
First	13	491
Second	37	1,042
Third	48	701
Fourth	39	395
Fifth	129	526
Total	266	3,155

Employment, Matches in Non-USAID Counties, with no Pre-Loan Employment Restriction

Quintile	Treated	Untreated
First	12	8,231
Second	28	22,880
Third	53	30,632
Fourth	36	8,111
Fifth	102	6,776
Total	231	76,630

Employment, Matches in Non-USAID Counties, with Pre-Loan Employment Restriction

Quintile	Treated	Untreated
First	12	3,574
Second	34	13,886
Third	40	6,225
Fourth	29	1,785
Fifth	79	1,898
Total	194	27,368

Sales, Matches in Same Counties, with no Pre-Loan Sales Restriction

Quintile	Treated	Untreated
First	9	314
Second	31	987
Third	37	995
Fourth	57	969
Fifth	129	885
Total	263	4,150

Sales, Matches in Same Counties, with Pre-Loan Sales Restriction

Quintile	Treated	Untreated
First	12	151
Second	32	425
Third	35	233
Fourth	47	188
Fifth	86	250
Total	212	1,247

Sales, Matches in Non-USAID Counties, with no Pre-Loan Sales Restriction

Quintile	Treated	Untreated
First	9	4,561
Second	27	21,015
Third	43	24,858
Fourth	51	13,912
Fifth	96	7,980
Total	226	72,326

Sales, Matches in Non-USAID Counties, with Pre-Loan Sales Restriction

Quintile	Treated	Untreated
First	6	1,225
Second	37	9,724
Third	29	2,470
Fourth	29	1,244
Fifth	58	838
Total	159	15,501

Appendix Table 3: Kernel Matching Balancing Tests Employment, Matches in Same Counties, with no Pre-Loan Employment Restriction

	Mean				t-test	Regression-based tests
	Treated	Control	% bias	% bias reduction	t-stat (p-value)	F-stat (p-value)
Log Emp_{t-1}	1.731	1.662	6.15	91.42	2.66 (0.008)	4.92 (0.001)
Log Emp_{t-1} sq.	4.066	3.758	6.78	87.84	3.28 (0.001)	4.15 (0.002)
Log Emp_{t-1} -Log Emp_{t-2}	0.163	0.188	-5.36	82.12	-1.98 (0.047)	5.57 (0.000)
Log Emp_{t-2} -Log Emp_{t-3}	0.168	0.171	-0.85	97.31	-0.31 (0.754)	5.46 (0.000)
Log Emp_{t-3} -Log Emp_{t-4}	0.128	0.121	2.12	92.27	0.75 (0.456)	2.68 (0.030)
Log Total $Assets_{t-1}$	11.956	11.806	3.50	92.29	1.84 (0.066)	3.39 (0.009)
$Profit_{t-1}$/ $Sales_{t-1}$	0.063	0.053	0.00	98.76	2.60 (0.009)	3.63 (0.006)
Log $Debt_{t-1}$	11.749	11.636	2.50	96.61	1.48 (0.138)	4.09 (0.003)
Log $Wage_{t-1}$	10.042	10.014	0.83	98.88	0.45 (0.653)	5.20 (0.000)

Employment, Matches in Same Counties, with Pre-Loan Employment Restriction

	Mean				t-test	Regression-based tests
	Treated	Control	% bias	% bias reduction	t-stat (p-value)	F-stat (p-value)
Log Emp_{t-1}	1.713	1.691	1.98	97.24	0.65 (0.518)	0.29 (0.885)
Log Emp_{t-1} sq.	3.968	3.822	3.22	94.23	1.19 (0.233)	0.97 (0.422)
Log Emp_{t-1} -Log Emp_{t-2}	0.164	0.184	-4.19	86.04	-1.15 (0.248)	2.41 (0.047)
Log Emp_{t-2} -Log Emp_{t-3}	0.166	0.182	-4.20	86.64	-1.12 (0.261)	5.06 (0.001)
Log Emp_{t-3} -Log Emp_{t-4}	0.133	0.126	2.04	92.58	0.53 (0.594)	1.56 (0.183)
Log Total $Assets_{t-1}$	12.005	11.870	3.13	93.11	1.27 (0.203)	0.54 (0.704)
$Profit_{t-1}$/ $Sales_{t-1}$	0.064	0.050	0.01	98.31	2.77 (0.006)	4.14 (0.002)
Log $Debt_{t-1}$	11.761	11.630	2.90	96.08	1.29 (0.198)	0.40 (0.811)
Log $Wage_{t-1}$	10.038	10.044	-0.18	99.53	-0.07 (0.940)	3.02 (0.017)

Employment, Matches in Non-USAID Counties, with no Pre-Loan Employment Restriction

	Mean				t-test	Regression-based tests
	Treated	Control	% bias	% bias reduction	t-stat (p-value)	F-stat (p-value)
Log Emp_{t-1}	1.762	1.717	4.32	95.09	6.09 (0.000)	82.70 (0.000)
Log Emp_{t-1} sq.	4.193	3.961	5.64	92.47	8.54 (0.000)	94.56 (0.000)
Log Emp_{t-1} -Log Emp_{t-2}	0.139	0.223	-18.00	42.23	-22.47 (0.000)	256.48 (0.000)
Log Emp_{t-2} -Log Emp_{t-3}	0.177	0.149	6.87	74.14	9.13 (0.000)	70.70 (0.000)
Log Emp_{t-3} -Log Emp_{t-4}	0.146	0.132	3.44	85.96	4.22 (0.000)	47.87 (0.000)
Log Total $Assets_{t-1}$	12.068	12.166	-2.87	95.52	-4.64 (0.000)	67.67 (0.000)
$Profit_{t-1}$/ $Sales_{t-1}$	0.057	0.044	1.94	83.17	11.93 (0.000)	115.00 (0.000)
Log $Debt_{t-1}$	11.939	11.918	0.67	99.09	1.13 (0.260)	50.43 (0.000)
Log $Wage_{t-1}$	10.053	10.220	-6.36	61.78	-11.39 (0.000)	143.52 (0.000)

Employment, Matches in Non-USAID Counties, with Pre-Loan Employment Restriction

	Mean				t-test	Regression-based tests
	Treated	Control	% bias	% bias reduction	t-stat (p-value)	F-stat (p-value)
Log Emp_{t-1}	1.744	1.774	-2.90	96.71	-2.56 (0.011)	9.19 (0.000)
Log Emp_{t-1} sq.	4.082	4.022	1.46	98.05	1.38 (0.168)	5.93 (0.000)
Log Emp_{t-1} -Log Emp_{t-2}	0.129	0.189	-13.01	58.24	-9.45 (0.000)	62.80 (0.000)
Log Emp_{t-2} -Log Emp_{t-3}	0.181	0.206	-6.03	77.30	-4.44 (0.000)	10.67 (0.000)
Log Emp_{t-3} -Log Emp_{t-4}	0.154	0.157	-0.83	96.60	-0.60 (0.548)	61.24 (0.000)
Log Total $Assets_{t-1}$	12.116	12.524	-11.87	81.42	-12.71 (0.000)	96.22 (0.000)
$Profit_{t-1}$/ $Sales_{t-1}$	0.052	0.043	1.35	88.29	5.12 (0.000)	54.58 (0.000)
Log $Debt_{t-1}$	11.983	12.226	-7.56	89.75	-8.23 (0.000)	51.98 (0.000)
Log $Wage_{t-1}$	10.031	10.354	-12.28	26.18	-13.90 (0.000)	79.91 (0.000)

Sales, Matches in Same Counties, with no Pre-Loan Sales Restriction

	Mean				t-test	Regression-based tests
	Treated	Control	% bias	% bias reduction	t-stat (p-value)	F-stat (p-value)
Log $Sales_{t-1}$	15.238	15.042	11.08	85.75	4.11 (0.000)	8.67 (0.000)
Log $Sales_{t-1}$ sq.	234.638	228.868	11.18	85.24	3.98 (0.000)	8.25 (0.000)
Log $Sales_{t-1}$ -Log $Sales_{t-2}$	0.137	0.092	6.33	76.86	2.11 (0.035)	6.42 (0.000)
Log $Sales_{t-2}$ -Log $Sales_{t-3}$	0.042	0.051	-1.25	81.94	-0.42 (0.671)	0.94 (0.439)
Log $Sales_{t-3}$ -Log $Sales_{t-4}$	0.108	0.078	5.47	75.84	1.66 (0.098)	2.97 (0.018)
Log Total $Assets_{t-1}$	11.955	11.877	1.82	96.56	0.81 (0.418)	0.88 (0.477)
$Profit_{t-1}$/ $Sales_{t-1}$	0.068	0.060	0.00	99.16	1.75 (0.080)	1.71 (0.145)
Log $Debt_{t-1}$	11.739	11.654	1.89	97.57	0.93 (0.352)	0.61 (0.655)
Log $Wage_{t-1}$	10.058	10.068	-0.23	99.63	-0.14 (0.887)	0.41 (0.804)

Sales, Matches in Same Counties, with Pre-Loan Sales Restriction

	Mean				t-test	Regression-based tests
	Treated	Control	% bias	% bias reduction	t-stat (p-value)	F-stat (p-value)
Log $Sales_{t-1}$	15.188	15.184	0.26	99.67	0.06 (0.952)	0.03 (0.998)
Log $Sales_{t-1}$ sq.	232.851	232.632	0.42	99.44	0.09 (0.926)	0.03 (0.998)
Log $Sales_{t-1}$ -Log $Sales_{t-2}$	0.105	0.069	4.96	81.86	1.06 (0.290)	1.23 (0.296)
Log $Sales_{t-2}$ -Log $Sales_{t-3}$	0.034	0.037	-0.39	94.39	-0.08 (0.940)	0.29 (0.882)
Log $Sales_{t-3}$ -Log $Sales_{t-4}$	0.083	0.100	-3.09	86.36	-0.55 (0.583)	1.73 (0.141)
Log Total $Assets_{t-1}$	11.894	11.895	-0.02	99.96	-0.01 (0.996)	0.40 (0.808)
$Profit_{t-1}$/ $Sales_{t-1}$	0.066	0.053	0.00	98.57	1.88 (0.061)	1.93 (0.104)
Log $Debt_{t-1}$	11.660	11.733	-1.61	97.94	-0.46 (0.647)	0.36 (0.836)
Log $Wage_{t-1}$	10.044	10.105	-1.38	97.83	-0.50 (0.621)	0.20 (0.939)

Sales, Matches in Non-USAID Counties, with no Pre-Loan Sales Restriction

	Mean				t-test	Regression-based tests
	Treated	Control	% bias	% bias reduction	t-stat (p-value)	F-stat (p-value)
Log $Sales_{t-1}$	15.244	15.120	7.35	92.82	11.12 (0.000)	107.61 (0.000)
Log $Sales_{t-1}$ sq.	234.797	230.658	8.54	91.56	12.24 (0.000)	112.03 (0.000)
Log $Sales_{t-1}$ -Log $Sales_{t-2}$	0.164	0.105	8.02	81.60	10.58 (0.000)	141.79 (0.000)
Log $Sales_{t-2}$ -Log $Sales_{t-3}$	0.067	0.127	-8.14	49.40	-9.19 (0.000)	21.60 (0.000)
Log $Sales_{t-3}$ -Log $Sales_{t-4}$	0.128	0.134	-0.94	96.72	-1.09 (0.277)	101.55 (0.000)
Log Total $Assets_{t-1}$	12.233	12.370	-3.96	94.60	-6.99 (0.000)	123.60 (0.000)
$Profit_{t-1}$/ $Sales_{t-1}$	0.064	0.049	0.58	82.91	14.21 (0.000)	190.31 (0.000)
Log $Debt_{t-1}$	12.033	12.081	-1.43	98.26	-2.64 (0.008)	134.30 (0.000)
Log $Wage_{t-1}$	10.192	10.357	-3.89	93.56	-12.70 (0.000)	300.78 (0.000)

Sales, Matches in Non-USAID Counties, with Pre-Loan Sales Restriction

	Mean				t-test	Regression-based tests
	Treated	Control	% bias	% bias reduction	t-stat (p-value)	F-stat (p-value)
Log $Sales_{t-1}$	15.104	15.110	-0.40	99.49	-0.30 (0.764)	0.81 (0.517)
Log $Sales_{t-1}$ sq.	230.152	230.129	0.05	99.94	0.03 (0.973)	0.78 (0.538)
Log $Sales_{t-1}$ -Log $Sales_{t-2}$	0.128	0.079	6.74	75.35	4.30 (0.000)	18.63 (0.000)
Log $Sales_{t-2}$ -Log $Sales_{t-3}$	0.111	0.109	0.32	95.40	0.17 (0.865)	10.08 (0.000)
Log $Sales_{t-3}$ -Log $Sales_{t-4}$	0.116	0.103	2.14	90.53	1.18 (0.238)	11.97 (0.000)
Log Total $Assets_{t-1}$	12.199	12.497	-8.61	83.74	-7.72 (0.000)	33.79 (0.000)
$Profit_{t-1}$/ $Sales_{t-1}$	0.048	0.060	-0.48	-71.75	-5.29 (0.000)	85.24 (0.000)
Log $Debt_{t-1}$	12.085	12.191	-3.17	95.92	-3.06 (0.002)	17.59 (0.000)
Log $Wage_{t-1}$	10.333	10.206	2.98	95.33	5.03 (0.000)	31.60 (0.000)

Appendix Table 4: Hotelling T^2 Tests by Propensity Score Quintile
Employment, Matches in Same Counties,
with no Pre-Loan Employment Restriction

Quintile	T^2 statistics	F-test statistics	p-value
First	294.16	8.29	0.000
Second	339.82	7.20	0.000
Third	242.12	4.69	0.000
Fourth	331.92	4.57	0.000
Fifth	139.57	1.43	0.006
All	139.32	1.44	0.003

Employment, Matches in Same Counties, with Pre-Loan Employment Restriction

Quintile	T^2 statistics	F-test statistics	p-value
First	210.26	6.60	0.000
Second	173.59	3.88	0.000
Third	132.12	2.76	0.000
Fourth	169.82	2.27	0.000
Fifth	82.16	0.83	0.856
All	88.75	0.95	0.616

Employment, Matches in Non-USAID Counties, with no Pre-Loan Employment Restriction

Quintile	T^2 statistics	F-test statistics	p-value
First	5,720.46	172.67	0.000
Second	4,436.79	130.31	0.000
Third	4,886.88	125.15	0.000
Fourth	2,481.81	49.34	0.000
Fifth	1,541.07	19.04	0.000
All	6,599.61	79.43	0.000

Employment, Matches in Non-USAID Counties, with Pre-Loan Employment Restriction

Quintile	T^2 statistics	F-test statistics	p-value
First	1,701.28	62.55	0.000
Second	3,969.33	116.47	0.000
Third	2,425.61	65.18	0.000
Fourth	1,178.31	24.43	0.000
Fifth	792.72	11.09	0.000
All	5,136.01	70.17	0.000

Sales, Matches in Same Counties with no Pre-Loan Sales Restriction

Quintile	T^2 statistics	F-test statistics	p-value
First	179.32	4.89	0.000
Second	310.88	6.77	0.000
Third	220.33	4.48	0.000
Fourth	170.27	2.72	0.000
Fifth	121.62	1.20	0.103
All	110.66	1.14	0.169

Sales, Matches in Same Counties with Pre-Loan Sales Restriction

Quintile	T^2 statistics	F-test statistics	p-value
First	53.70	1.67	0.031
Second	146.91	3.27	0.000
Third	82.70	1.82	0.004
Fourth	91.86	1.25	0.137
Fifth	56.36	0.52	1.000
All	57.21	0.62	0.998

Sales, Matches in Non-USAID Counties with no Pre-Loan Sales Restriction

Quintile	T^2 statistics	F-test statistics	p-value
First	5,158.68	155.23	0.000
Second	4,799.00	133.08	0.000
Third	5,379.35	137.72	0.000
Fourth	4,756.87	92.94	0.000
Fifth	1,704.52	21.37	0.000
All	6,799.86	83.86	0.000

Sales, Matches in Non-USAID Counties with Pre-Loan Sales Restriction

Quintile	T^2 statistics	F-test statistics	p-value
First	1,907.17	89.34	0.000
Second	1436.44	47.74	0.000
Third	733.32	23.37	0.000
Fourth	534.02	14.02	0.000
Fifth	438.86	6.48	0.000
All	2,560.70	38.06	0.000

Appendix Table 5: Dynamic Estimates of the International Loan Impact on Employment and Sales

	Matches in Same Counties		Matches in Non-USAID Counties	
	Employment	Sales	Employment	Sales
$Loan_{t-3}$	0.008	-0.006	-0.076	0.044
	(0.077)	(0.124)	(0.084)	(0.152)
$Loan_{t-2}$	0.021	0.014	-0.017	-0.016
	(0.070)	(0.114)	(0.081)	(0.130)
$Loan_{t-1}$	-0.019	0.004	-0.126*	-0.062
	(0.069)	(0.109)	(0.074)	(0.118)
$Loan_{t}$	0.088	0.239**	-0.012	0.163
	(0.068)	(0.112)	(0.074)	(0.110)
$Loan_{t+1}$	0.188***	0.318***	0.035	0.372***
	(0.069)	(0.107)	(0.077)	(0.121)
$Loan_{t+2}$	0.200***	0.322***	0.052	0.361***
	(0.076)	(0.117)	(0.077)	(0.124)
$Loan_{t+3}$	0.179**	0.340***	0.101	0.362***
	(0.075)	(0.122)	(0.080)	(0.131)
$Loan_{t+4}$	0.306***	0.426***	0.247***	0.482***
	(0.079)	(0.117)	(0.089)	(0.138)
$Loan_{t+5+}$	0.293***	0.348***	0.193***	0.421***
	(0.070)	(0.108)	(0.077)	(0.123)
F-Statistic	0.37	0.03	3.94	0.70
	(0.946)	(0.999)	(0.268)	(0.873)
Firms	6,026	4,209	52,373	48,182
Obs.	57,592	46,980	504,794	521,993

Notes: "Matches in same counties" refers to the matched comparison group where the matched pair is in the same county (non-USAID counties are excluded); "matches in non-USAID counties" refers to the case where matches are drawn only from counties where USAID loans were unavailable. Firm fixed effects, treatment timing effects, and year effects are included in all regressions. F-Statistics (P–Values) are shown for the test of the estimated pre-loan impact of loans: $Loan_{t-3} = Loan_{t-2} = Loan_{t-1} = 0$. Standard errors, bootstrapped with 500 repetitions, are in parentheses. * = significant at 10-percent level. ** = significant at 5-percent level. *** = significant at 1-percent level.

Appendix Table 6: Dynamic Estimates of the International Loan Impact on Employment and Sales, Imposing Pre-Loan Employment or Sales Restriction

	Matches in Same Counties		Matches in Non-USAID Counties	
	Employment	Sales	Employment	Sales
$Loan_{t-3}$	-0.020	-0.031	-0.143	-0.081
	(0.085)	(0.146)	(0.091)	(0.137)
$Loan_{t-2}$	-0.016	-0.060	-0.102	-0.032
	(0.076)	(0.137)	(0.082)	(0.128)
$Loan_{t-1}$	-0.050	-0.122	-0.168**	0.020
	(0.077)	(0.129)	(0.082)	(0.119)
$Loan_{t}$	0.084	0.143	-0.098	0.240**
	(0.073)	(0.126)	(0.078)	(0.121)
$Loan_{t+1}$	0.185***	0.268**	-0.043	0.342***
	(0.074)	(0.124)	(0.078)	(0.121)
$Loan_{t+2}$	0.189**	0.299**	-0.043	0.309**
	(0.082)	(0.132)	(0.090)	(0.132)
$Loan_{t+3}$	0.206***	0.337***	-0.010	0.414***
	(0.080)	(0.132)	(0.088)	(0.120)
$Loan_{t+4}$	0.318***	0.317**	0.148	0.415***
	(0.089)	(0.136)	(0.097)	(0.131)
$Loan_{t+5+}$	0.325***	0.221*	0.126	0.328***
	(0.074)	(0.127)	(0.085)	(0.127)
F-Statistic	0.47	1.01	4.55	0.67
	(0.925)	(0.799)	(0.208)	(0.879)
Firms	3,368	1,469	23,250	13,433
Obs.	32,500	16,374	241,409	160,086

Notes: "Matches in same counties" refers to the matched comparison group where the matched pair is in the same county (non-USAID counties are excluded); "matches in non-USAID counties" refers to the case where matches are drawn only from counties where USAID loans were unavailable. Firm fixed effects, treatment timing effects, and year effects are included in all regressions. F-Statistics (P–Values) are shown for the test of the estimated pre-loan impact of loans: $Loan_{t-3}$ = $Loan_{t-2}$ = $Loan_{t-1}$ = 0. Standard errors, bootstrapped with 500 repetitions, are in parentheses. * = significant at 10-percent level. ** = significant at 5-percent level. *** = significant at 1-percent level.

Figure 1: Dynamics of the Loan Effect on Employment
(Controls from Same County, No Pre-Loan Employment Restriction)

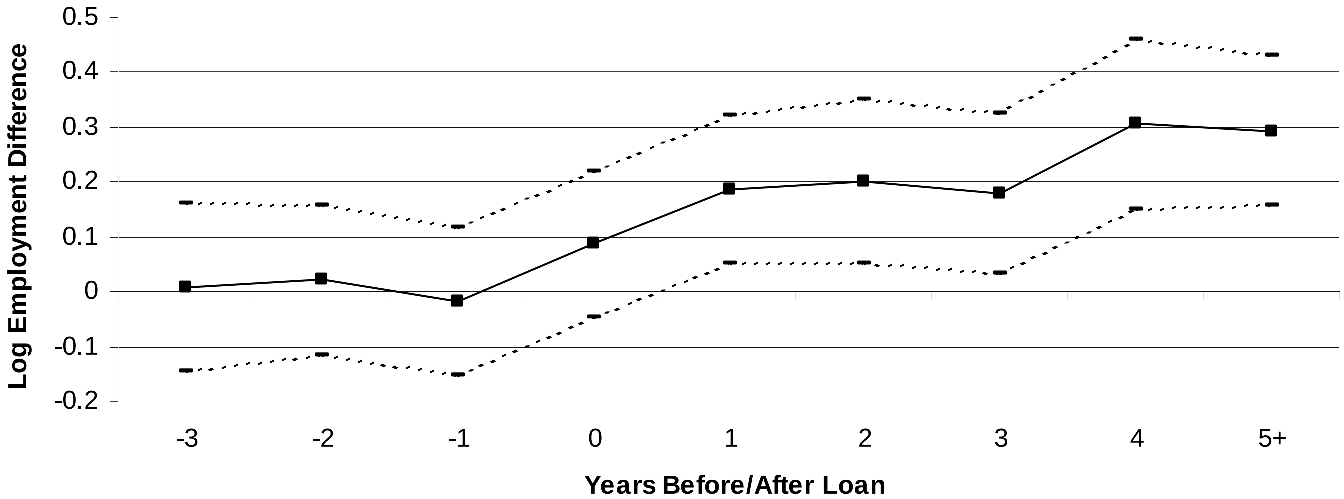

Figure 2: Dynamics of the Loan Effect on Employment
(Controls from non-USAID Counties, No Pre-Loan Employment Restriction)

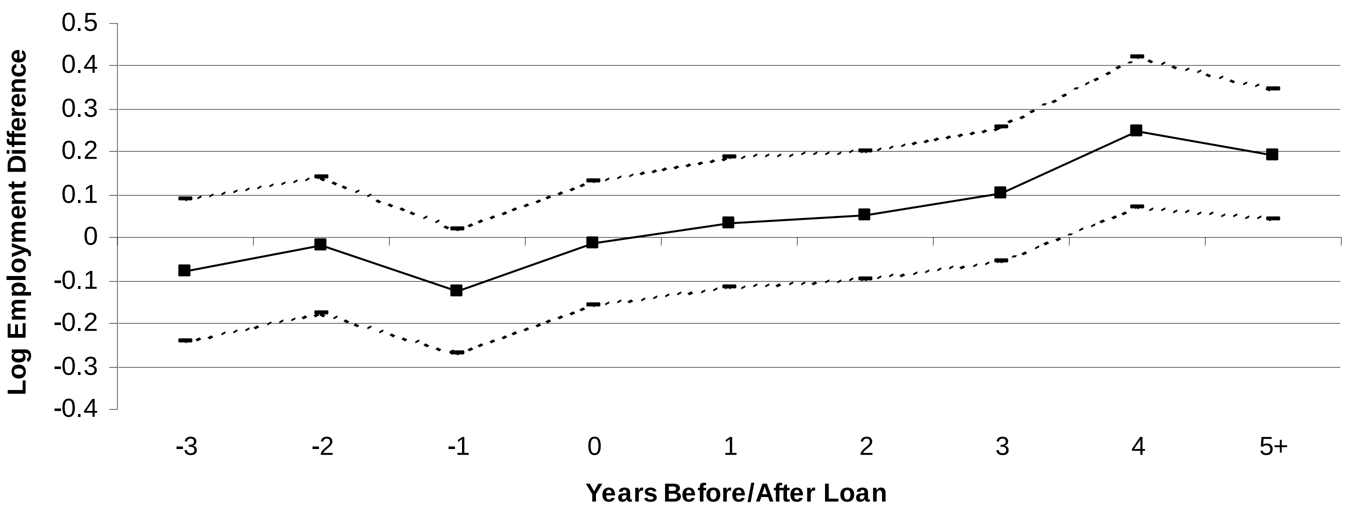

Figure 3: Dynamics of the Loan Effect on Sales
(Controls from Same County, No Pre-Loan Sales Restriction)

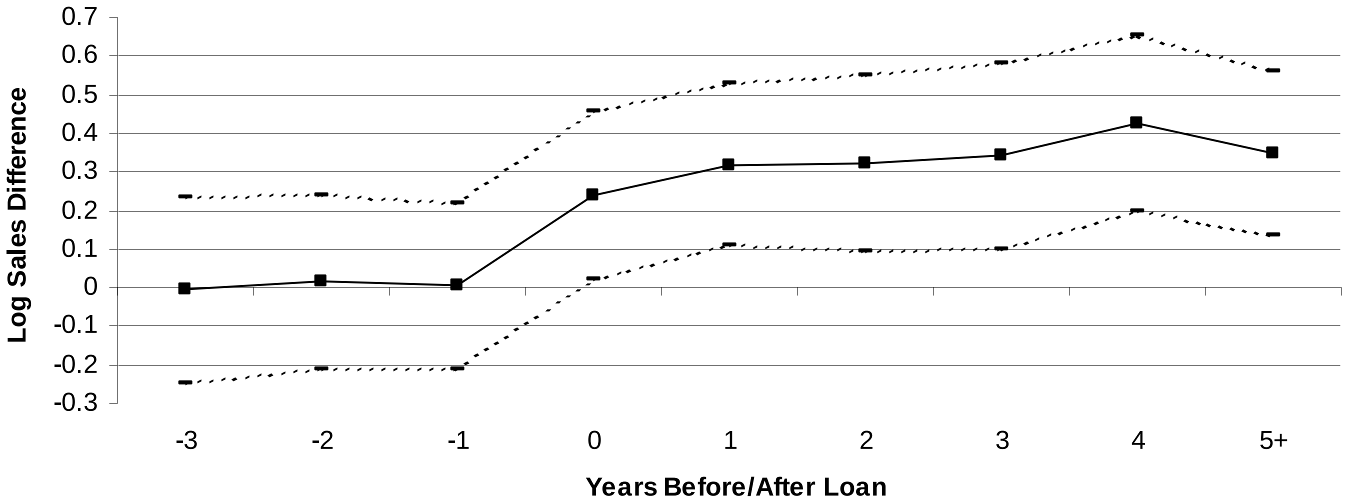

Figure 4: Dynamics of the Loan Effect on Sales
(Controls from non-USAID Counties, No Pre-Loan Sales Restriction)

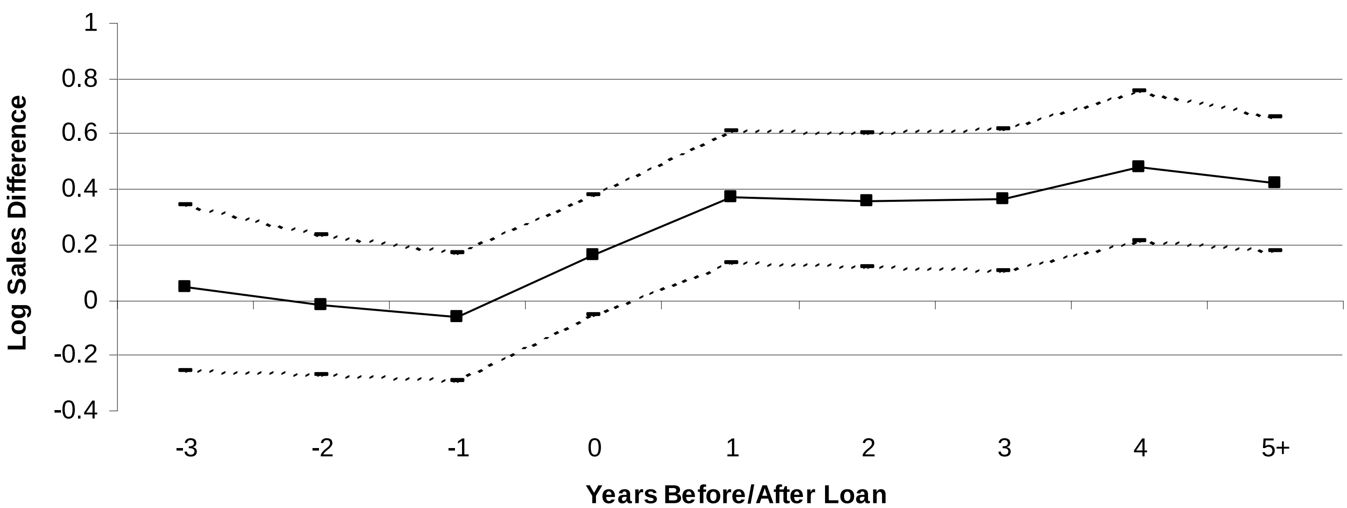

Figure 5: Dynamics of the Loan Effect on Employment
(Controls from Same County, Pre-Loan Employment Restriction)

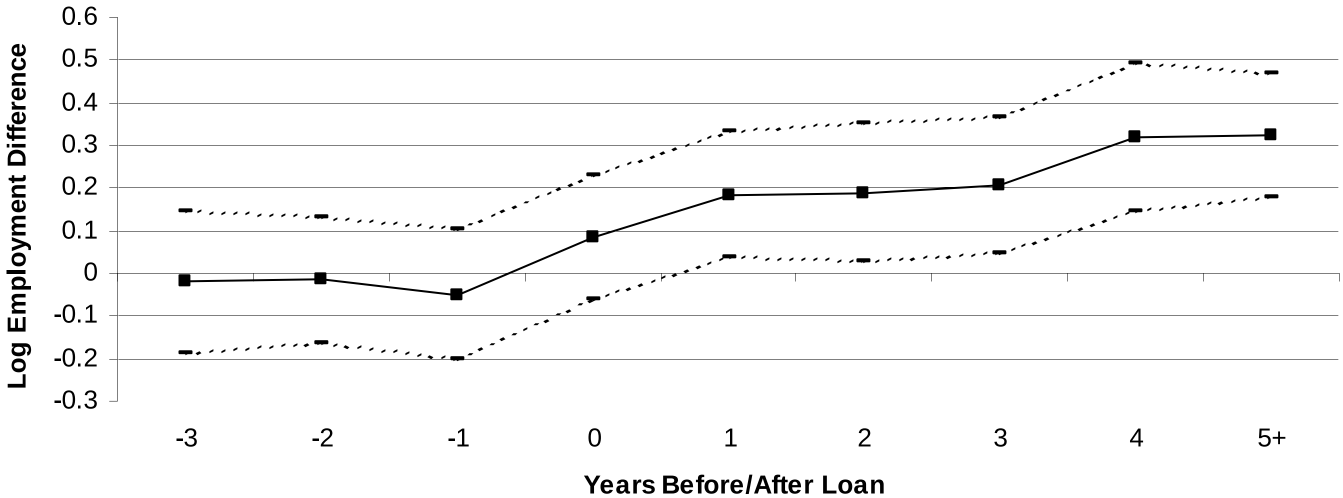

Figure 6: Dynamics of the Loan Effect on Employment
(Controls from non-USAID Counties, Pre-Loan Employment Restriction)

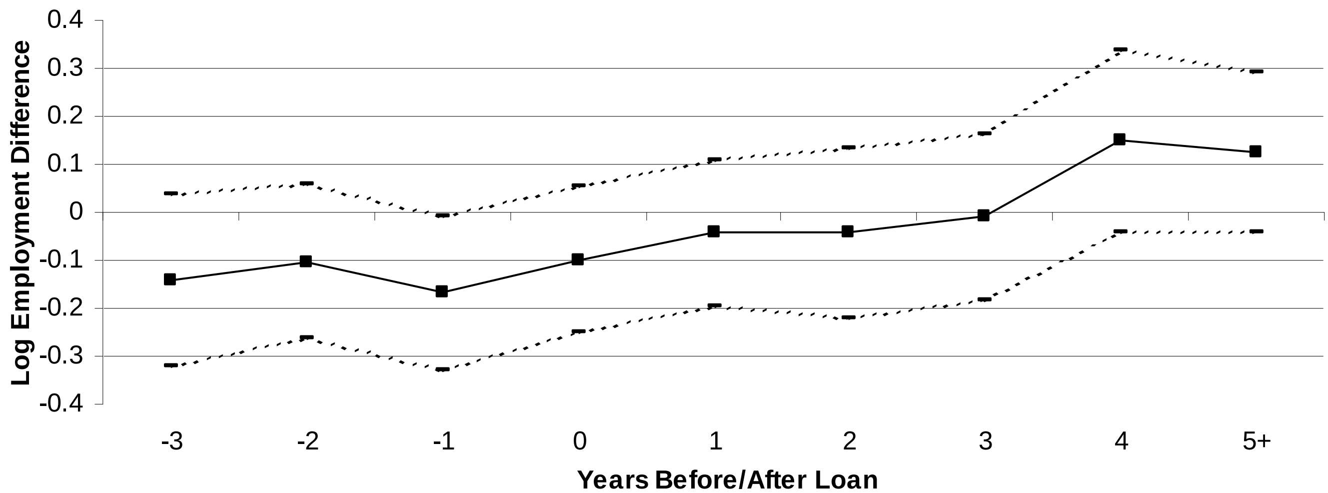

Figure 7: Dynamics of the Loan Effect on Sales
(Controls from Same County, Pre-Loan Sales Restriction)

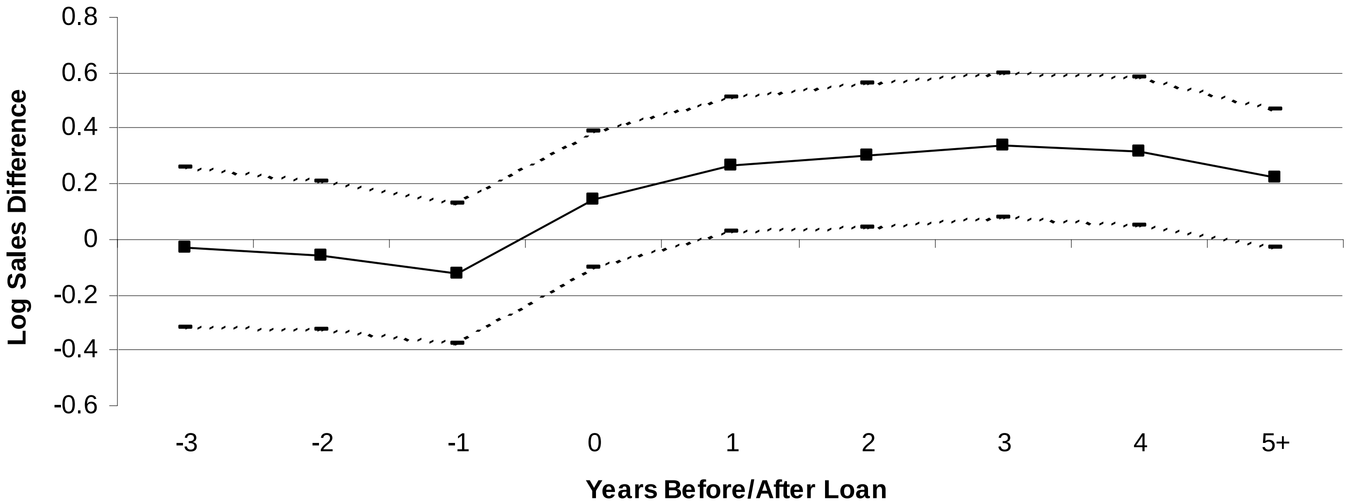

Figure 8: Dynamics of the Loan Effect on Sales
(Controls from non-USAID Counties, Pre-Loan Sales Restriction)