

## The Miracle of Microfinance? Evidence from a Randomized Evaluation<sup>†</sup>

By ABHIJIT BANERJEE, ESTHER DUFLO, RACHEL GLENNERSTER,  
AND CYNTHIA KINNAN\*

*This paper reports results from the randomized evaluation of a group-lending microcredit program in Hyderabad, India. A lender worked in 52 randomly selected neighborhoods, leading to an 8.4 percentage point increase in takeup of microcredit. Small business investment and profits of preexisting businesses increased, but consumption did not significantly increase. Durable goods expenditure increased, while “temptation goods” expenditure declined. We found no significant changes in health, education, or women’s empowerment. Two years later, after control areas had gained access to microcredit but households in treatment area had borrowed for longer and in larger amounts, very few significant differences persist. (JEL G21, G31, O16, O12, L25, I38)*

Microfinance institutions (MFIs) have expanded rapidly over the last 10 to 15 years: according to the Microcredit Summit (Microcredit Summit Campaign 2012), the number of very poor families with a microloan has grown more than 18-fold from 7.6 million in 1997 to 137.5 million in 2010. Microcredit has generated considerable enthusiasm and hope for fast poverty alleviation, culminating in the Nobel Prize for Peace, awarded in 2006 to Mohammed Yunus and the Grameen Bank for their contribution to the reduction in world poverty. In the last several years, however, the enthusiasm for microcredit has been matched by

\*Banerjee: MIT Department of Economics, 40 Ames Street E17-201A, Cambridge, MA 02142 and National Bureau of Economic Research (NBER) and J-PAL (e-mail: [banerjee@mit.edu](mailto:banerjee@mit.edu)); Duflo: MIT Department of Economics, 40 Ames Street E17-201B, Cambridge, MA 02142 and NBER and J-PAL (e-mail: [eduflo@mit.edu](mailto:eduflo@mit.edu)); Glennerster: J-PAL, 30 Wadsworth Street E53-320, Cambridge, MA 02142 (e-mail: [rglenner@mit.edu](mailto:rglenner@mit.edu)); Kinnan: Northwestern University Department of Economics, 2001 Sheridan Road 3222, Evanston, IL 60208 and NBER and J-PAL (e-mail: [c-kinnan@northwestern.edu](mailto:c-kinnan@northwestern.edu)). This paper updates and supersedes the 2010 version, which reported results using one wave of end-line surveys. Funding for the first wave of the survey was provided by The Vanguard Charitable Endowment Program and ICICI bank. Funding for the second wave was provided by Spandana and J-PAL. This draft was not reviewed by The Vanguard Charitable Endowment Program, ICICI Bank, or Spandana. The Centre for Micro Finance at the Institute for Financial Management Research (IFMR) (Chennai, India) set up and organized the experiment and the data collection, and made the anonymized data available first to the research team, and then publicly. At the time, IFMR did not have an IRB. Data analysis and on-going data collection have received IRB approval from MIT COUHES (1203004973) and Northwestern University (STU00063636). Aparna Dasika and Angela Ambroz provided excellent assistance in Hyderabad. Adie Angrist, Leonardo Elias, Harris Eppsteiner, Shehla Imran, Seema Kacker, Tracy Li, Aditi Nagaraj, and Cecilia Peluffo provided excellent research assistance. Datasets for both waves of data used in this paper are available at <http://www.centre-for-microfinance.org/publications/data/>. The authors wish to extend thanks to CMF and Spandana for organizing the experiment, to Padmaja Reddy CEO of Spandana whose commitment to understanding the impact of microfinance made this project possible, to Annie Duflo (the executive director of CMF at the time of the study) for setting up this project, and to numerous seminar audiences and colleagues for insightful suggestions.

<sup>†</sup>Go to <http://dx.doi.org/10.1257/app.20130533> to visit the article page for additional materials and author disclosure statements or to comment in the online discussion forum.

an equally strong backlash. For instance, a November 2010 article in the *New York Times*, appearing in the wake of a rash of reported suicides linked to over-indebtedness, quotes Reddy Subrahmanyam, an official in Andhra Pradesh (the setting of this study), accusing MFIs of making “hyperprofits off the poor.” He argues that “the industry [has] become no better than the widely despised village loan sharks it was intended to replace.... The money lender lives in the community. At least you can burn down his house. With these companies, it is loot and scoot” (Polgreen and Bajaj 2010).

What is striking about this debate is the relative paucity of evidence to inform it. Anecdotes about highly successful entrepreneurs or deeply indebted borrowers tell us nothing about the effect of microfinance on the average borrower, much less the effect of having access to it on the average household. Even representative data about microfinance clients and nonclients cannot identify the causal effect of microfinance access, because clients are self-selected and therefore not comparable to nonclients. Microfinance organizations also purposely choose some villages and not others in which to operate. These issues make the evaluation of microcredit particularly difficult, and until recently there was little rigorous evidence on the impact of microfinance.

This has changed in the last few years, as several studies evaluating microfinance have been conducted by different research teams with different partners in different settings: Morocco (Crépon et al. 2015), Bosnia-Herzegovina (Augsburg et al. 2015), Mexico (Angelucci, Karlan and Zinman 2015), Mongolia (Attanasio et al. 2015), and Ethiopia (Tarozzi, Desai and Johnson 2015). In this paper we report on the oldest of these, the first randomized evaluation of the effect of the canonical group-lending microcredit model, which targets women who may not necessarily be entrepreneurs. This study also follows the households over the longest period of any evaluation (3 to 3.5 years after the introduction of the program in their areas), which is necessary since many impacts may be expected to surface only over the medium run.

The experiment, a collaborative project between the Centre for Micro Finance (CMF) at the Institute for Financial Management Research (IFMR) in Chennai and Spandana, one of India’s fastest growing MFIs at the time, was conducted as follows. In 2005, 52 of 104 poor neighborhoods in Hyderabad were randomly selected for the opening of a Spandana branch, while the remainder were not.<sup>1</sup> Hyderabad is the fifth largest city in India, and the capital of Andhra Pradesh, the Indian state where microcredit has expanded the fastest and where it has been most controversial in recent years. Fifteen to 18 months after the introduction of microfinance in each area, a comprehensive household survey was conducted with an average of 65 households in each neighborhood, for a total of about 6,850 households. In the meantime, other MFIs had also started their operations in both treatment and comparison areas, but the probability of receiving an MFI loan was still 8.4 percentage

<sup>1</sup> An alternative way to measure the impact of borrowing is to randomize microcredit offers among applicants. This approach was pioneered by Karlan and Zinman (2010), who use individual randomization of the “marginal” clients in a credit scoring model to evaluate the impact of consumer lending in South Africa, finding that access to microcredit increases the probability of employment. The authors use the same approach to measure impact of microcredit among small businesses in Manila (Karlan and Zinman 2011). It should be noted, however, that these two studies evaluate slightly different programs: consumer lending in the South Africa study, and “second generation” individual-liability loans to existing entrepreneurs in Manila.

points (46 percent) higher in treatment areas than in comparison areas (26.7 percent borrowers in treated areas versus 18.3 percent borrowers in comparison areas). Two years after this first endline survey, the same households were surveyed once more. By that time, both Spandana and other organizations had started lending in the treatment and control groups, so the fraction of households borrowing from microcredit organizations was not dramatically different (38.5 percent in treatment and 33 percent in control). But households in treatment groups had larger loans and had been borrowing for a longer time period. This second survey thus gives us an opportunity to examine some of the longer term impacts of microcredit access on households and businesses, although the setting is not perfect since we are comparing those who borrow for longer versus those who borrow for a shorter time, rather than those who borrow and those who do not borrow at all.

Since it is entirely possible that there are spillover or general equilibrium effects (as analyzed by Buera, Kaboski, and Shin 2011), and effects that operate through the *expectation* of being able to borrow when needed (such as reductions in precautionary savings, as documented in Thailand by Kaboski and Townsend (2011) and in India by Fulford (2011)), or through general equilibrium effects on prices or wages (Giné and Townsend 2004), we focus here on reduced-form/intent-to-treat estimates.

We examine the effect on borrowing from various sources, consumption, new business creation, business income, etc., as well as on measures of other human development outcomes, such as education, health, and women's empowerment. At the first endline, while households do borrow more from microcredit institutions, the overall take up is reasonably low (only 26.7 percent of the eligible households borrow, not the 80 percent that Spandana expected) and some microloans are substitutes for informal loans. Informal borrowing declines, and we see no significant difference in overall borrowed amount (though the point estimate is positive). This in itself was a surprising result at the time, though it has been replicated in other studies: the demand for microcredit is less than expected, and may not correspond to an important demand for additional credit. We see no significant difference in monthly per capita consumption or monthly nondurable consumption. We do see significant positive impacts on the purchase of durables. There is evidence that this is financed partly by an increase in labor supply and partly by cutting unnecessary consumption: households have reduced expenditures on what they themselves describe as "temptation goods."

Thus, in our context, microfinance plays a role in helping some households make different intertemporal choices in consumption. This is not the only impact that is traditionally expected from microfinance, however. The primary engine of growth that it is supposed to fuel is business creation.<sup>2</sup> This is typically true even for lenders that do not insist that households have a business to take a first loan (Spandana is one of them), but still hope and expect that the ability to borrow will eventually help households start or expand small businesses. (The description of Spandana's group-loan product is careful not to mention an automatic link between

<sup>2</sup>To give a sense of the prevalence of the purported link between microfinance and business creation, of the roughly 3.1 million Google search results for "microfinance," 1.35 million (44 percent) also contain the phrase "business creation" or "entrepreneurship" (retrieved November 2013).

credit and self-employment activity, but does state that “Loans are used for cash flow smoothening [*sic*], predominantly for productive purposes.”) Fifteen to 18 months after gaining access, households are no more likely to be entrepreneurs (that is, have at least one business), but they invest more in the businesses they do have (or the ones they start). There is an increase in the average profits of the businesses that were already in existence before microcredit, which is entirely due to very large increases in the upper tail of profitability. At every quantile between the fifth and the ninety-fifth percentile, there is no difference in the profits of the businesses. The median marginal new business is both less profitable and less likely to have even one employee in treatment than in control areas.

After three years, when microcredit is available both in treatment and control groups but treatment group households have had the opportunity to borrow for a longer time, businesses in the treatment groups have significantly more assets, and business profits are now larger for businesses above the eighty-fifth percentile of profitability. However, the average business is still small and not very profitable. In other words, perhaps contrary to most people’s belief, to the extent microcredit helps businesses, it may help the most profitable businesses the most. There is still no difference in average consumption.

We do not find any effect on any of the women’s empowerment or human development outcomes we examine, either after 18 or 36 months. Furthermore, almost 70 percent of eligible households do not have an MFI loan, preferring instead to borrow from other sources, if they borrow (and most do).

A number of caveats must be kept in mind when interpreting and generalizing these results. First, the difference in microfinance take-up between treatment and control areas is low, even at the first endline, which raises two issues: it lowers power and precision (though we have a number of significant effects), and it means that the impact of microcredit we detect is driven by marginal borrowers—those who do not borrow when the cost of doing so is high (because they have fewer MFIs to choose from or do not want to change neighborhoods), but do borrow when that cost is lower.

Second, the evaluation was run in a context of very high economic growth, which could have either decreased or increased the impact of microfinance. Third, this is the evaluation of a for-profit microfinance model; not-for-profit microfinance lenders may have larger positive effects if their interest rates are kept low. Fourth, as the MFI we study does not provide any complementary services, such as business training or sensitivity education, we are studying the pure impact of providing loans to women who may or may not use them for their own businesses (though Spandana does believe that this is what the money will be used for eventually, and we do find an expansion in women-owned businesses). Fifth, the study took place in “marginal” neighborhoods—those Spandana was indifferent about working with at the outset—and the impacts may have been different in the neighborhoods they chose to exclude from the randomization (Heckman 1992).

Thus, it is an important reassurance that our results find a strong echo in five other studies that look at similar programs in different contexts (discussed below). This gives us confidence in the robustness and external validity of our findings. In short, microcredit is not for every household, or even most households, and it does not lead to the miraculous social transformation some proponents have claimed. Its principal

impact seems to be, perhaps unsurprisingly, that it allows some households to sacrifice some instantaneous utility (temptation goods or leisure) in order to finance lumpy purchases, either for their home or in order to establish or expand a business. *Prima facie*, these marginal businesses do not appear to be highly productive or profitable, but more data and more time may be needed to fully establish their impacts on individuals, markets, and communities.

## I. The Spandana Microcredit Product and the Context

### A. *Spandana and Its Microcredit Product*

Until the major crisis in Indian microfinance in 2010, Spandana was one of the largest and fastest growing microfinance organizations in India, with 1.2 million active borrowers in March 2008, up from 520 borrowers in 1998, its first year of operation (MIX Market 2009). It had expanded from its birthplace in Guntur, a dynamic city in Andhra Pradesh, across the state and into several others.

The basic Spandana product is the canonical group-loan product, first introduced by the Grameen Bank. A group is comprised of 6 to 10 women, and 25–45 groups form a “center.” Women are jointly responsible for the loans of their group. The first loan is Rs. 10,000, about \$200 at market exchange rates, or \$1,000 at 2007 purchasing power parity (PPP)-adjusted exchange rates (World Bank 2007).<sup>3</sup> It takes 50 weeks to reimburse principal and interest; the interest rate is 12 percent (nondeclining balance; equivalent to a 24 percent APR). If all members of a group repay their loans, they are eligible for second loans of Rs. 10,000–12,000. Loan amounts increase up to Rs. 20,000. During the course of the study, Spandana also introduced an individual product, for clients who had been successful with one or two group-loan cycles. The individual product was available in the treatment areas. Very few people in our sample ended up taking this loan, however, so the study is mainly an evaluation of a group-lending product.

Eligibility is determined using the following criteria. Clients must (i) be female, (ii) be aged 18 to 59, (iii) have resided in the same area for at least one year, (iv) have valid identification and residential proof (ration card, voter card, or electricity bill), and (v) at least 80 percent of women in a group must own their home.<sup>4</sup> Groups are formed by women themselves, not by Spandana.

Unlike some other microfinance organizations, Spandana does not require its clients to start a business (or pretend to) in order to borrow: the organization recognizes that money is fungible, and clients are left entirely free to choose the best use of the money, as long as they repay their loan. Spandana does not determine loan eligibility by the expected productivity of the investment, although selection into groups may screen out women who cannot convince fellow group members that they are likely to repay. Also, unlike other microlenders (most notably Grameen) Spandana

<sup>3</sup>In 2007 the PPP exchange rate was \$1 = Rs. 9.2, while the market exchange rate was \$1  $\simeq$  Rs. 50. All following references to dollar amounts are in PPP terms unless noted otherwise.

<sup>4</sup>The home ownership requirement is not because the house is used as collateral, but because home owners are more stable and less likely to migrate. Spandana does not require a formal property title, just a general agreement that this house belongs to this household (something that tends to be clear even in informal settlements).

does not explicitly insist on “transformation” in the household. There is no chanting of resolutions at group meetings, which are very short and focused on the repayment transaction. Spandana is primarily a lending organization, not directly involved in business training, financial literacy promotion, etc. It is, however, the belief of the management that the very fact of borrowing will lead to such transformation and to business creation. Spandana is also a for-profit operator, charging interest rates sufficient to make profits, though all the profits were re-invested in the organization in the period we study. The organization obtained private capital and would probably have launched an IPO if it had not been caught in the middle of the Andhra Pradesh microfinance crisis in 2010. This makes it different from Grameen Bank (Mohammed Yunus has explicitly and vigorously criticized for-profit MFIs after the IPO of Compartamos, a large Mexican MFI). All these features are important to keep in mind when interpreting the results of this study; it is possible that a Grameen-type organization would have different impacts. However, from an evaluation point of view, there are clear advantages to this product: in particular, any impact on business expansion, etc. can be attributed to credit alone, rather than to other services. Moreover, to the extent we find “positive” results in the study, they are unlikely to be attributable to social desirability bias. It is also worth noting that, in the period we study, the interest rates charged by Spandana were low by typical microfinance standards, even when compared to rates charged by Grameen.

### B. *The Context*

Table 1A uses the baseline data to show a snapshot of households from the study area in 2005, before the Spandana product was launched. As we describe below, these numbers need to be viewed with some caution, as the households sampled at baseline were not necessarily representative of the area as a whole, and were not purposely resurveyed at endline. At baseline, the average household was a family of five, with monthly expenditure of just under Rs. 5,000, or \$540 at PPP-adjusted exchange rates (\$108 per capita) (World Bank 2005).<sup>5</sup> There was almost no MFI borrowing in the sample areas at baseline. However, 68 percent of the households had at least one outstanding loan. The average amount outstanding was Rs. 38,000. Sixty-three percent of households had a loan from an informal source (moneylenders, friends or neighbors, family members, or shopkeepers). Commercial bank loans were very rare (3.6 percent).

Although business investment was not commonly named as a motive for borrowing, businesses were common, with 32 businesses per 100 households, compared to an OECD-country average of 12 percent who say that they are self-employed. Less than half of all businesses were operated by women (14.5 woman-run businesses per 100 households.) Business owners and their families spent on average 76 hours per week working in the business.

*Growth between 2005 and 2010.*—Table 1B shows some of the same key statistics for the endline 1 and endline 2 (EL1 and EL2) samples in the control group.

<sup>5</sup> Column 2 reports the control mean and column 4 reports the treatment-control difference. Only one difference out of 33 is significant at the 10 percent level (column 5).



TABLE 1A—BASELINE SUMMARY STATISTICS

	Control group			Treatment – control	
	Obs. (1)	Mean (2)	SD (3)	Coeff. (4)	p-value (5)
<i>Household composition</i>					
Number members	1,220	5.038	(1.666)	0.095	0.303
Number adults (>=16 years old)	1,220	3.439	(1.466)	−0.011	0.873
Number children (<16 years old)	1,220	1.599	(1.228)	0.104	0.098
Male head	1,216	0.907	(0.290)	−0.012	0.381
Head's age	1,216	41.150	(10.839)	−0.243	0.676
Head with no education	1,216	0.370	(0.483)	−0.008	0.787
<i>Access to credit</i>					
Loan from Spandana	1,213	0.000	(0.000)	0.007	0.195
Loan from other MFI	1,213	0.011	(0.103)	0.007	0.453
Loan from a bank	1,213	0.036	(0.187)	0.001	0.859
Informal loan	1,213	0.632	(0.482)	0.002	0.958
Any type of loan	1,213	0.680	(0.467)	0.002	0.942
<i>Amount borrowed from (in Rs)</i>					
Spandana	1,213	0	(0.000)	69	0.192
Other MFI	1,213	201	(2,742)	170	0.568
Bank	1,213	7,438	(173,268)	−5,420	0.279
Informal loan	1,213	28,460	(65,312)	−570	0.856
Total	1,213	37,892	(191,292)	−5,879	0.343
<i>Self-employment activities</i>					
Number of activities	1,220	0.320	(0.682)	−0.019	0.579
Number of activities managed by women	1,220	0.145	(0.400)	−0.007	0.750
Share of HH activities managed by women	295	0.488	(0.482)	−0.006	0.904
<i>Businesses</i>					
Revenue/month (Rs)	295	15,991	(53,489)	4,501	0.539
Expenses/month (Rs)	295	3,617	(26,144)	641	0.751
Investment/month (Rs)	295	385	(3,157)	14	0.959
Employment (employees)	295	0.169	(0.828)	0.255	0.148
Self-employment (hours per week)	295	76.315	(66.054)	−4.587	0.414
<i>Businesses (all households)</i>					
Revenue/month (Rs)	1,220	3,867	(27,147)	904	0.626
Expenses/month (Rs)	1,220	875	(12,933)	116	0.812
Investment/month (Rs)	1,220	93	(1,559)	−0.098	0.999
Employment (employees)	1,220	0.041	(0.413)	0.057	0.166
Self-employment (hours per week)	1,220	18.453	(46.054)	−1.801	0.400
<i>Consumption (per household per month)</i>					
Total consumption (Rs)	1,220	4,888	(4,074)	270	0.232
Nondurables consumption (Rs)	1,220	4,735	(3,840)	252	0.235
Durables consumption (Rs)	1,220	154	(585)	18	0.531
Asset index	1,220	1.941	(0.829)	0.027	0.669

*Notes:* Unit of observation: household. Standard errors of differences, clustered at the area level, in parentheses. Sample includes all households surveyed at baseline. Informal lender includes moneylenders, loans from friends/family, and buying goods/services on credit from seller. Asset index is calculated on a list of 40 home durable goods. Each asset is given a weight using the coefficients of the first factor of a principal component analysis. The index, for a household  $i$ , is calculated as the weighted sum of standardized dummies equal to 1 if the household owns the durable good.

*Source:* Baseline household survey

TABLE 1B—ENDLINE 1 AND 2 SUMMARY STATISTICS (*Control group*)

	EL1 control group			EL2 control group			EL2-EL1	
	Obs. (1)	Mean (2)	SD (3)	Obs. (4)	Mean (5)	SD (6)	Coeff. (7)	p-value (8)
<i>Household composition</i>								
Number members	3,264	5.645	(2.152)	2,943	6.269	(2.548)	0.624	0.000
Number adults (≥16 years old)	3,264	3.887	(1.754)	2,943	4.039	(1.848)	0.152	0.000
Number children (<16 years old)	3,264	1.738	(1.310)	2,943	1.764	(1.321)	0.026	0.247
Male head	3,261	0.895	(0.307)	2,938	0.811	(0.391)	−0.083	0.000
Head's age	3,257	41.149	(10.222)	2,940	42.258	(10.154)	1.109	0.000
Head with no education	3,256	0.311	(0.463)	2,940	0.292	(0.455)	−0.020	0.021
<i>Access to credit</i>								
Loan from Spandana	3,247	0.051	(0.219)	2,943	0.111	(0.315)	0.061	0.000
Loan from other MFI	3,183	0.149	(0.356)	2,943	0.268	(0.443)	0.120	0.000
Loan from a bank	3,247	0.079	(0.270)	2,943	0.073	(0.260)	−0.006	0.480
Informal loan	3,247	0.761	(0.427)	2,943	0.603	(0.489)	−0.158	0.000
Any type of loan	3,264	0.867	(0.339)	2,943	0.904	(0.294)	0.037	0.000
<i>Amount borrowed from (in Rs)</i>								
Spandana	3,247	597	(2,907)	2,943	1,567	(5,618)	969	0.000
Other MFI	3,200	1,806	(5,918)	2,943	4,775	(10,736)	2,969	0.000
Bank	3,247	8,422	(101,953)	2,943	6,127	(40,308)	−2,296	0.221
Informal loan	3,247	41,045	(78,033)	2,943	32,356	(76,704)	−8,689	0.000
Total	3,264	59,836	(133,693)	2,943	88,631	(144,634)	28,795	0.000
<i>Self-employment activities</i>								
Number of activities	3,236	0.503	(0.854)	2,943	0.561	(0.787)	0.058	0.003
Number activities managed by women	3,209	0.185	(0.487)	2,943	0.234	(0.520)	0.050	0.000
Share activities managed by women	1,104	0.377	(0.453)	1,231	0.403	(0.454)	0.026	0.113
<i>Businesses</i>								
Revenue/month (Rs)	1,039	14,700	(56,350)	1,218	14,066	(23,713)	−634	0.724
Expenses/month (Rs)	1,071	12,030	(51,531)	1,218	12,568	(30,483)	538	0.769
Investment/month (Rs)	1,127	785	(6,806)	1,231	2,331	(14,645)	1,546	0.001
Employment (employees)	1,103	0.380	(1.644)	1,231	0.565	(2.938)	0.185	0.062
Self-employment (hrs/wk)	1,103	100.03	(69.87)	1,231	88.47	(60.16)	−11.56	0.000
<i>Businesses (all households)</i>								
Revenue/month (Rs)	3,145	4,856	(33,108)	2,930	5,847	(16,784)	991	0.105
Expenses/month (Rs)	3,177	4,055	(30,446)	2,930	5,225	(20,603)	1,169	0.088
Investment/month (Rs)	3,231	280	(4,038)	2,943	1,007	(9,623)	727	0.001
Employment (employees)	3,209	0.131	(0.980)	2,943	0.236	(1.920)	0.106	0.011
Self-employment (hrs/wk)	3,209	34.13	(62.59)	2,943	37.00	(58.46)	2.88	0.037
<i>Consumption (per household per month)</i>								
Consumption	3,248	6,375	(4,906)	2,943	8,787	(6,547)	2,412	0.000
Nondurables consumption	3,230	5,831	(4,212)	2,943	8,050	(5,780)	2,219	0.000
Durables consumption	3,230	551	(1,623)	2,941	720	(1,536)	169	0.000
Asset index	3,254	2.371	(0.861)	2,943	2.662	(0.828)	0.291	0.000

*Notes:* Summary statistics for comparison areas only. Standard errors of differences, clustered at the area level, in parentheses (column 3). All monetary amounts in 2007 Rs. Asset index is calculated on a list of 40 home durable goods. Each asset is given a weight using the coefficients of the first factor of a principal component analysis. The index, for a household  $i$ , is calculated as the weighted sum of standardized dummies equal to 1 if the household owns the durable good.

Comparing the control baseline sample (2005) with the control households in the EL1 (2008) and EL2 (2010) samples reveal very rapid secular growth in Hyderabad over 2005–2010.<sup>6</sup> Average household consumption rose from Rs.4,888 (2005) to

<sup>6</sup>While the comparison may not be perfect since the baseline survey was not conducted on the same sample as the endline, the growth between EL1 and EL2 is for the same set of households, using the same survey instruments, and thus gives us a good sense of the dynamism of this economy.



Rs. 6,375 in 2007 and Rs. 8,787 in 2010 (all expressed in 2007 rupees). The fraction of households with at least one outstanding loan rose from 68 percent at baseline to 87 percent in EL1 and 90 percent in EL2.

The prevalence of businesses increased from 32 per hundred households at baseline to 50 at EL1 and 56 at EL2. At endline 1, 37.7 percent, and at endline 2, 40.3 percent of the businesses were operated by women. However, the businesses remained very small, with, on average, 0.38 employees in EL1 and 0.57 in EL2. As well as remaining very small in terms of employment, average sales remained fairly steady: Rs. 14,700 at EL1 and 14,100 at EL2. However, looking across all households (not just those with businesses), business revenues increased from around Rs. 4,900 to Rs. 5,800 (in constant 2007 rupees). At EL2, business owners reported business expenses (working capital) plus investment in assets of almost Rs. 15,000, up from about Rs. 13,000 at EL1. (These expense estimates do not account for the cost of the proprietors' time.)

This context of rapid growth in urban Andhra Pradesh is another important feature to keep in mind, and may color the results of this study; of all the randomized evaluations on microfinance, ours has probably the most dynamic context. The setting of this study is clearly an important one, since microfinance clients in India represent roughly 30 percent of all microfinance clients worldwide,<sup>7</sup> and since microfinance has developed in many other rapidly growing environments (Bangladesh being probably the prime example). Nonetheless, the results of other evaluations of microfinance may be different in contexts either with much slower growth or in recession. Fortunately, the other five RCT studies of microfinance in this issue cover a wide variety of settings, which will help to understand the extent to which results depend on context.

## II. Experimental Design

### A. Experimental Design

At the time this study was started, microfinance had already taken hold in several districts in Andhra Pradesh, but most microfinance organizations had not yet started working in the capital, Hyderabad. Spandana initially selected 120 areas (identifiable neighborhoods, or *bastis*) in Hyderabad as places in which they were interested in opening branches but also willing not to do so. These areas were selected based on having no preexisting microfinance presence and on having residents who were desirable potential borrowers: poor, but not "the poorest of the poor." Areas with high concentrations of construction workers were avoided because they move frequently, which makes them undesirable as microfinance clients. While the selected areas are commonly referred to as "slums," these are permanent settlements with concrete houses and some public amenities (electricity, water, etc.). Conversely, the largest such areas in Hyderabad were not selected for the study, since Spandana was keen to start operations there: the large population in these slums allowed them to benefit from economies of scale and quickly reach a number of clients that justified expansion in the city. The population in the neighborhoods selected for the study ranges from 46 to

<sup>7</sup>MIX Market reported 94 million borrowers worldwide in 2011, of whom 28 million were located in India (<http://www.mixmarket.org/mfi/country/India>).

555 households. The slums chosen to be part of the study were typically not continuous to avoid spillovers across treatment and control slums.

In each area, CMF first hired a market research company to conduct a small baseline neighborhood survey in 2005, collecting information on household composition, education, employment, asset ownership, expenditure, borrowing, saving, and any businesses currently operated by the household or stopped within the last year. They surveyed a total of 2,800 households in order to obtain a rapid assessment of the baseline conditions of the neighborhoods. However, since there was no existing census, and the baseline survey had to be conducted very rapidly to gather some information necessary for stratification before Spandana began their operations, the households were not selected randomly from a household list: instead, field officers were asked to map the area and select every  $n$ th house, with  $n$  chosen to select 20 households per area. Unfortunately, this procedure was not followed very rigorously by the market research company, and we are not confident that the baseline is representative of the slum as a whole. Thus, the baseline survey was used solely as a basis for stratification, the descriptive analysis above, and to collect area-level characteristics that are used as control variables.<sup>8</sup> Beyond this, we do not use the baseline survey in the analysis that follows.

After the baseline survey, but prior to randomization, 16 areas were dropped from the study because they were found to contain large numbers of migrant-worker households. Spandana (like other MFIs) has a rule that loans should only be made to households who have lived in the same community for at least one year because the organization believes that dynamic incentives (the promise of more credit in the future) are more important in motivating repayment for these households.<sup>9</sup> The remaining 104 areas were grouped into pairs of similar neighborhoods, based on average per capita consumption and per-household debt, and one of each pair was randomly assigned to the treatment group.<sup>10</sup> Figure 1 shows a timeline of data collection and randomization.

Table 1A uses the baseline sample to show that treatment and comparison areas did not differ in their baseline levels of demographic, financial, or entrepreneurship characteristics in the baseline survey. This is not surprising, since the sample was stratified according to per capita consumption and fraction of households with debt.

Spandana then progressively began operating in the 52 treatment areas between 2006 and 2007. The rollout happened at different dates in different slums. Note that in the intervening periods, other MFIs also started their operations, both in treatment and in comparison areas. We will show that there is still a significant difference between MFI borrowing in treatment and comparison groups. Spandana credit officers also started lending in very few of the control slums, although this

<sup>8</sup> However, omitting these controls makes no difference to the results.

<sup>9</sup> We can compare baseline characteristics in the 16 areas dropped to those in the 104 areas included in the randomization. The differences are consistent with Spandana's rationale for dropping the omitted areas: household size is smaller in these areas (due to migrant workers there without families or children); there is less business creation (presumably because migrants are unlikely to start a business); and there is less credit outstanding (likely because informal lenders are also reluctant to lend to these very mobile households). (Results available upon request.)

<sup>10</sup> Pairs were formed to minimize the sum across pairs  $A, B$  (area  $A$  avg. loan balance area  $B$  avg. loan balance)<sup>2</sup> + (area  $A$  per capita consumption area  $B$  per capita consumption)<sup>2</sup>. Within each pair one neighborhood was randomly allocated into treatment.

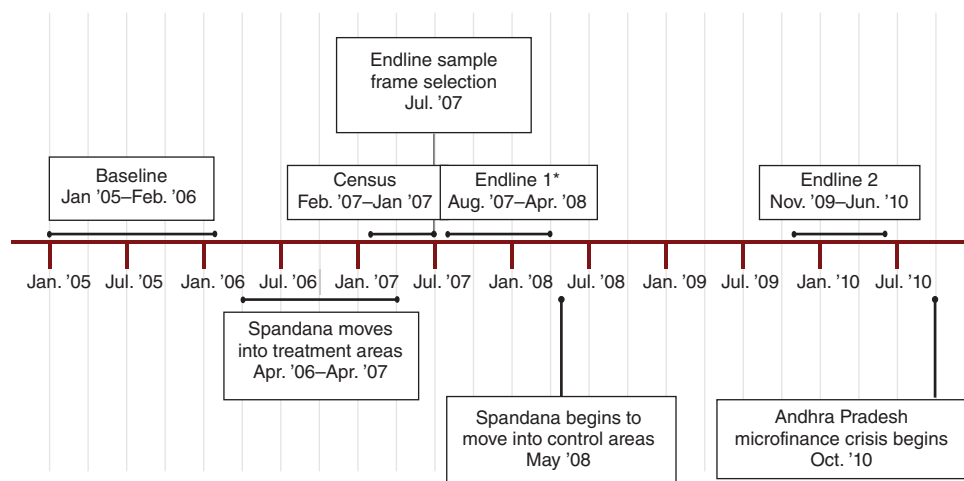


FIGURE 1. TIMELINE OF INTERVENTION AND DATA COLLECTION

*Note:* No treatment area was surveyed for endline 1 until at least one year had elapsed from the start of Spandana lending in that area.

was stopped relatively rapidly. Furthermore, there was no rule against borrowing in another slum (if one could find a group to join), and some people did do so. Overall, 5 percent of households in control slums were borrowing from Spandana at the time of the first endline.

To create a proper sampling frame for the endline, CMF staff undertook a comprehensive census of each area in early 2007, and included a question on borrowing. The census revealed low rates of MFI borrowing even in treatment areas, so the endline sampling frame consisted of households whose characteristics suggested high likelihood of having borrowed: those that had resided in the area for at least 3 years and contained at least 1 woman aged 18 to 55. Spandana borrowers identified in the census were oversampled because we believed that heterogeneity in treatment effects would introduce more variance in outcomes among Spandana borrowers than among nonborrowers, and that oversampling borrowers would therefore give higher power. The results presented below weight the observation to account for this oversampling so that the results are representative of the population as a whole. Since the sampling frame at baseline was not sufficiently rigorous, baseline households were not purposely resurveyed in the follow-up. The first endline survey began in August 2007 and ended in April 2008, and the rollout of the endline followed the rollout of the program. This first endline survey was conducted at least 12 months after Spandana began disbursing loans within a given area, and generally 15 to 18 months after (the survey followed the same calendar in the control slums, in order to ensure comparability between treatment and control). The overall sample size was 6,863 households.

Two years later, in 2009–2010, a second endline survey, following up on the same households, was undertaken. It included the same set of questions as in 2007–2008 to ensure comparability. The re-contact rate was very high (90 percent). We discuss this attrition in more detail below.

### B. *Potential Threats to Identification and Caveats on Interpretation*

*Attrition and Selective Migration.*—Since we lack a rigorous baseline sample that was systematically followed, a potential worry is that the sample that was surveyed at endline may not be strictly comparable in treatment and control areas, if there was differential attrition in treatment and in control groups. For example, people could have moved into the area, or avoided moving out of the area, because Spandana had started their operations there. This does not seem highly likely, given that if someone really wanted to borrow, they had options to do so either from another MFI (we will see that a fair number of people did) or even from Spandana, by going to another neighborhood. The treatment only made it marginally easier to borrow (as we will see in the next section). Nevertheless, in retrospect, it was a clear mistake not to attempt to systematically re-survey at least a fraction of the baseline sample, even though the baseline sampling frame was weak.

That said, we have a number of ways to assess the extent to which attrition is a problem. First of all, in Appendix Table A1,<sup>11</sup> we verify that the households surveyed at endlines 1 and 2 are similar in treatment and control groups, in terms of a number of characteristics that are fixed over time (the  $p$ -value on the joint difference of these characteristics across treatment arms is 0.983 at EL1 and 0.567 at EL2). This is a first indication that we have a comparable sample at baseline and at endline, even allowing for attrition.

Second, the sample at EL1 was drawn from a census that was conducted fairly soon after the introduction of microcredit (on average less than a year). Moreover, the sampling frame for EL1 was restricted to people who had lived in the area for at least three years before the census. This means that no one in the survey had migrated into the area because of Spandana: they were all residents of the area well before Spandana moved into the area (the vast majority had been there for years). This removes the most plausible channel for differential selection into the sample in treatment and control groups. There remains the possibility that fewer people (or different people) left the treatment areas between the launch of the product and the census due to the option to borrow more easily, but in less than a year, the migration rate out of Hyderabad is low, and given the ability to borrow if someone wants to, it seems far-fetched that people would have been differentially likely to migrate out of the slums based on the ability to become a Spandana client.

We next examine attrition between the census and the first endline, and between the first and second endlines. There was some attrition between the census and EL1, especially since, as is customary in these types of surveys, census surveyors were given replacement lists in case they did not find the exact person they were looking for. However, this attrition (roughly 25 percent) is almost exactly the same in treatment and in control areas: 27.6 percent in treatment and 25.2 percent in control ( $p$ -value of difference: 0.332; see online Appendix Table A2, panel A). Moreover, the attrition is totally uncorrelated with the months elapsed since Spandana entered the slum (Table A2, panel B), which is not what we would

<sup>11</sup> All Appendix tables are available in the online Appendix.

expect if it were somehow related to the program (it would have had more time to play out if Spandana had entered a longer time before). The only characteristics that predict that someone is more likely to be found is that they are a Spandana borrower (4.2 percentage points lower attrition; SE of 1.97 percentage points), and living in a “non-pucca” (lower-quality) house (2.7 percentage points lower attrition; SE of 1.4 percentage points). The most likely reason for the former is that the Spandana officers helped the CMF field team to locate their clients. For example, surveyors could attend weekly meetings to collect addresses and find directions to people’s homes. The latter likely reflects greater mobility among wealthier households. In all of the analysis that follows, we correct for this by adjusting the sampling weights for the ratio between the probability to find a non-Spandana borrower and the probability to find a Spandana borrower (0.948 in endline 1, 0.914 in endline 2).

Online Appendix Table A3, panel A shows that the re-contact rate at endline 2 for households initially interviewed at endline 1 was very high (much higher than in most randomized controlled trials in either the United States or developing countries). It was also similar in the treatment and the control group, at 89.9 percent and 90.2 percent, respectively (the  $p$ -value of the difference is 0.248). Panel B shows average characteristics of the re-contacted versus attrited households. The samples do not differ significantly along most dimensions. However, those who attrited had slightly higher per capita expenditure at endline 1, with a Rs. 1,000 increase in expenditure associated with a 0.0099 increase in likelihood of attrition (column 1: the standard error is 0.0032). Having a Spandana loan at endline 1 was associated with 3.4 percentage points lower attrition (column 5: the standard error is 1pp); having any MFI loan is associated with 2.8 percentage points lower attrition (column 6: the standard error is 0.8pp), driven by the effect of Spandana loans. Again, the explanation for this is that the credit officers helped the field team find the clients, if they had moved within their slum. Panel C of Table A3 shows that between treatment and control, attrition was not differentially correlated with characteristics, with the exception of having an MFI loan (column 6), an effect likely driven by loan officers assisting in re-contacting survey respondents.

This data suggests that there is no evidence that migration or attrition patterns were driven by the treatment, except through the mechanical effect that Spandana credit officers helped surveyors locate their clients, which we correct for.

Nevertheless, to systematically address the concern that attrition may affect the results, we have re-estimated all the regressions below with a correction for sample selection inspired by DiNardo, Fortin, and Lemieux (1996), where we re-weight the data using the inverse of the propensity to be observed at endline 2, so that the distribution of observable characteristics (at endline 1) among households observed at endline 2 resembles that in the entire endline 1 sample. We then apply the same weights to endline 1 data (implicitly assuming a similar selection process between the onset of microfinance and endline 1). The results, presented for key outcomes in online Appendix Table A5, are very similar to what we present here. (Full results available on request.) Note that this procedure only corrects for differential attrition by observables, not by unobservable variables.

*Interpreting the Results.*—The experimental design and the implementation raise a number of issues worth keeping in mind in interpreting the results that follow.

First, given the sampling frame, ours will be an intent-to-treat (ITT) analysis on a sample of “likely borrowers.” This is thus neither the effect on those who borrow nor the average effect on the neighborhood. Rather, it is the average effect of easier access to microfinance on those who are its primary targets. Second, microfinance was available in both treatment and control areas, though access was easier in treatment areas. Microfinance take-up is indeed higher in treatment areas, which generates experimental variation, but the marginal clients may be different from the first clients to borrow in an area. This also affects power: the initial power calculations were performed when Spandana thought that 80 percent of eligible households would become clients very rapidly after the launch. In fact, the data shows that the proportion reached only 18 percent in 18 months (and stayed at just below 18 percent after two and a half years). This is low, and also gave other MFIs, which were behind Spandana in terms of penetration in Hyderabad, time to catch up. Overall, take-up of microfinance from any organization was only 33 percent by EL2. This is an important result in its own right, and very surprising at the time, but it implies that, with the benefits of hindsight, more areas would have been needed. This is not something that could be addressed *ex post*. Fortunately, subsequent evaluations of microfinance programs were able to do so, and find a very similar set of results (and nonresults), suggesting that these outcomes are not the artifact of samples that are too small or of a very nonrepresentative set of clients.

### III. Results

To estimate the impact of microfinance becoming available in an area on likely clients, we focus on intent-to-treat (ITT) estimates; that is, simple comparisons of averages in treatment and comparison areas, averaged over borrowers and non-borrowers. We present ITT estimates of the effect of microfinance on businesses operated by the household; for those who own businesses, we examine business profits, revenue, business inputs, and the number of workers employed by the business. (The construction of these variables is described in online Appendix A.) Each column of each table reports the results of a regression of the form

$$y_{ia} = \alpha + \beta \times Treat_{ia} + \mathbf{X}'_a \gamma + \varepsilon_{ia},$$

where  $y_{ia}$  is an outcome for household  $i$  in area  $a$ ,  $Treat_{ia}$  is an indicator for living in a treated area, and  $\beta$  is the intent-to-treat effect.  $\mathbf{X}'_a$  is a vector of control variables, calculated as area-level baseline values: area population, total businesses, average per capita expenditure, fraction of household heads who are literate, and fraction of all adults who are literate. Standard errors are adjusted for clustering at the area level and all regressions are weighted to correct for oversampling of Spandana borrowers and for higher probability of tracking them. We also estimated two sets of regressions with different specifications: one with no controls whatsoever, and one controlling for strata used in randomization rather than for the average characteristics in the control slums. The results (available on request) are qualitatively unchanged.



Controlling for strata somewhat increases the precision in this case, so some results that are almost significant here become significant with strata controls (this is particularly true for the grouped outcomes).

In any study of this kind, where there are many possible outcomes and multiple possible causal pathways, there is a danger of overinterpreting any single significant result (or even of discerning a pattern of results when there is none). We take a number of steps to avoid this problem. First, we report outcomes following the template that all papers in this issue follow, ensuring no selection of outcomes based on what is significant or not. Second, for each table (which corresponds to a “family” of outcomes) we report an index (*à la* Kling, Liebman, and Katz 2007) of all the outcomes in the family taken together.<sup>12</sup> Finally, for each of these index outcomes, we report both the standard *p*-value and the *p*-value adjusted for multiple hypotheses testing across all the indices. The adjusted *p*-values are calculated using the step-down procedure of Hochberg (1988), which controls the family-wise error rate for all the indices.<sup>13</sup>

### A. Borrowing from Spandana and other MFIs

Treatment communities were randomly selected to receive Spandana branches, but other MFIs also started operating both in treatment and comparison areas. We are interested in testing the impact of access to *microcredit*, not only of borrowing from Spandana. Table 2, panel A shows that, by the first endline, MFI borrowing was indeed higher in treatment than in control slums, although borrowing from other MFIs offset part of the difference in Spandana borrowing. Households in treatment areas are 12.7 percentage points more likely to report being Spandana borrowers: 17.8 percent versus 5.1 percent (Table 2 panel A, column 1). The difference in the percentage of households saying that they borrow from any MFI is 8.4 points (Table 2 panel A, column 3), so some households who ended up borrowing from Spandana in treatment areas would have borrowed from another MFI in the absence of the intervention. While the absolute level of total MFI borrowing is not very high, it is about 50 percent higher in treatment than in comparison areas. Columns 1 and 3 show that treatment households also report significantly higher loan amounts from MFIs (and from Spandana in particular) than comparison households. Averaged over borrowers and nonborrowers, treatment households report Rs. 1,334 more borrowing from Spandana than do control households, and Rs. 1,286 more from all MFIs (both significant at the 1 percent level).

While both the absolute take-up rate and the implicit “first stage” are relatively small, this result appears similar to what was found in most other evaluations of the impact of access to microfinance, despite the different contexts. In rural Morocco, Crépon et al. (2015) find that the probability of having any loan from the MFI Al Amana in areas that received access to it is 10 percentage points, whereas it is essentially 0 in control; moreover, since no other MFI operated in their study area,

<sup>12</sup> The variables are signed such that a positive treatment effect is a “good” outcome. They are then normalized by subtracting the mean in the control group and dividing by the standard deviation in the control group. The index is the simple average of the normalized variables.

<sup>13</sup> See online Appendix A4 for details.

TABLE 2—CREDIT

	Spandana (1)	Other MFI (2)	Any MFI (3)	Other bank (4)	Informal (5)	Total (6)	Ever late on payment? (7)	Number of cycles borrowed from an MFI (8)	Index of dependent variables (9)
<i>Panel A. Endline 1</i>									
<i>Credit access</i>									
Treated area	0.127*** (0.020)	−0.012 (0.024)	0.084*** (0.027)	0.003 (0.012)	−0.052** (0.021)	−0.023 (0.014)	−0.060** (0.026)	0.084** (0.041)	0.106*** (0.0291)
Observations	6,811	6,657	6,811	6,811	6,811	6,862	6,475	6,811	6,862
Control mean	0.051	0.149	0.183	0.079	0.761	0.867	0.616	0.330	0.000
Hochberg-corrected p-value									0.000
<i>Loan amounts (in Rupees)</i>									
Treated area	1,334*** (230)	−94 (336)	1,286*** (439)	75 (2,163)	−1,069 (2,520)	2,856 (4,548)			
Observations	6,811	6,708	6,811	6,811	6,811	6,862			
Control mean	597	1,806	2374	8,422	41,045	59,836			
<i>Panel B. Endline 2</i>									
<i>Credit access</i>									
Treated area	0.063*** (0.019)	−0.039 (0.026)	0.002 (0.029)	0.001 (0.009)	0.002 (0.018)	0.000 (0.010)	0.007 (0.021)	0.085 (0.067)	0.0288 (0.0253)
Observations	6,142	6,142	6,142	6,142	6,142	6,142	6,142	5,926	6,142
Control mean	0.111	0.268	0.331	0.073	0.603	0.904	0.598	0.724	0.000
Hochberg-corrected p-value									0.256
<i>Loan amounts (in Rupees)</i>									
Treated area	979*** (287)	−217 (628)	799 (669)	−1,181 (1,086)	158 (2,940)	2,554 (6,156)			
Observations	6,142	6,142	6,142	6,142	6,142	6,142			
Control mean	1,567	4,775	5,544	6,127	32,356	88,632			

*Notes:* The table presents the coefficient of a “treatment” dummy in a regression of each variable on treatment (with control variables listed in the text). Cluster-robust standard errors in parentheses. Results are weighted to account for oversampling of Spandana borrowers. Columns 1–6 under “Credit access” report the probability of having at least one loan from the source listed. The corresponding columns under “Loan amounts” report the loan amount (zero for nonborrowers). “Informal lender” includes moneylenders, loans from friends/family, and buying goods/services on credit. Number of loan cycles from an MFI is the maximum number of loan cycles borrowed with a single MFI, including the current loan (if any); number of cycles is zero for MFI never-borrowers. All monetary amounts in 2007 Rs. Column 9 presents the coefficient of a “treatment” dummy in a regression on treatment of an index of z-scores of the outcome variables in columns 1–8 (including both credit access and loan amounts) for each round following Kling, Liebman, and Katz (2007). p-values for this regression are reported using Hochberg’s step-up method to control the FWER across all index outcomes. See text for details.

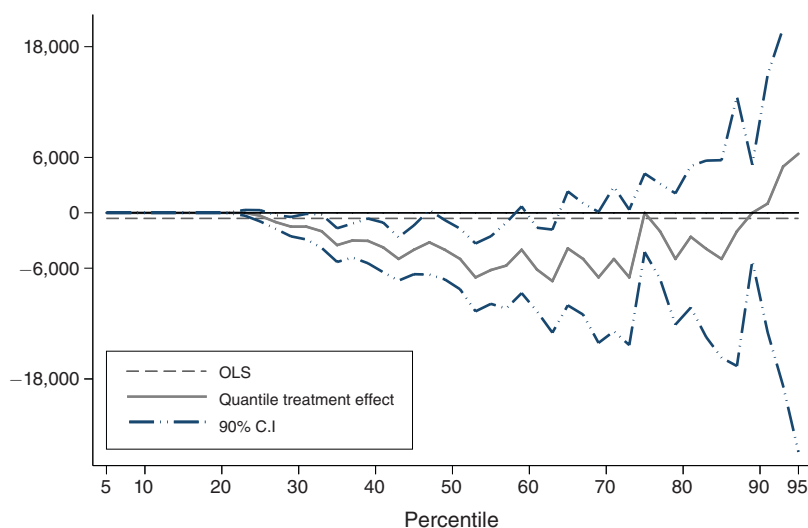
\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

this represents the total increase in microfinance borrowing. In Mexico, Angelucci, Karlan, and Zinman (2015) find an increase of 10 percentage points in the probability of borrowing from the MFI Compartamos in areas that got access to the lender, relative to a base of 5 percentage points in control. In Ethiopia, Tarozzi, Desai, and Johnson (2015) find a larger impact of microcredit introduction: 36 percent.

The fairly low take-up rate in these different contexts is in itself a striking result, given the high levels of informal borrowing in these communities and the purported benefits of microcredit over these alternative forms of borrowing. In all cases, except when the randomization was among those who had already expressed explicit interest in microcredit (Attanasio et al. 2015 and Augsburg et al. 2015), only a minority of “likely borrowers” end up borrowing.

FIGURE 2. TREATMENT EFFECT ON INFORMAL BORROWING (*Endline 1*)

*Notes:* Informal borrowing: borrowing from moneylenders, friends and family, and buying goods on credit. Confidence intervals are cluster-bootstrapped at the neighborhood level. For quantiles 0.05 to 0.20, confidence intervals are not reported because the quantile does not vary sufficiently across neighborhoods to bootstrap standard errors. The point estimates are zero for these quantiles.

Table 2 also displays the impact of microfinance access on other forms of borrowing. A sizable fraction of the clients report repaying a more expensive debt as a reason to borrow from Spandana, and we do indeed see some action on this margin. The share of households who have some informal borrowing—defined as borrowing from family, friends, or moneylenders or purchasing goods on credit extended by the seller—goes down by 5.2 percentage points in treatment areas (column 5), but bank borrowing is unaffected (column 4). The point estimate of the amount borrowed from informal sources is also negative, suggesting substitution of expensive borrowing with cheaper MFI borrowing (an explicit objective of Spandana), and the point estimate, though insignificant, is quite similar in absolute value to the increase in MFI borrowing (column 3). However, given the high level of informal borrowing, this corresponds to a decline of only 2.6 percent. When we examine the distribution of endline 1 informal borrowing, in Figure 2, informal borrowing is significantly lower in treatment areas from the thirtieth to sixtieth percentiles. Overall, treatment affects the index of borrowing outcomes, and the  $p$ -value is small even when accounting for multiple hypothesis testing across families (column 9).

After the end of the first endline, following our initial agreement with Spandana, Spandana started to expand into control areas. Other MFIs also continued their expansion. However, two years later, a significant difference still remained between treatment and control slums: Table 2, panel B shows that 17 percent of the households in the treatment slums borrowed from Spandana, against 11 percent in the control slums. Other MFIs continued to expand both in the former treatment and control slums, and MFI lending overall was almost the same in the treatment and the control group. By the second endline survey, 33.1 percent of households had borrowed from an MFI in the former control slums, and 33.3 percent in the treatment slums. Since lending started

TABLE 3—SELF-EMPLOYMENT ACTIVITIES: REVENUES, ASSETS, AND PROFITS (*All households*)

	Assets (stock) (1)	Investment in last 12 months (2)	Expenses (3)	Profit (4)	Has a self- employment activity (5)	Number of self- employment activities (6)	Has started a business in the last 12 months (7)	Has closed a business in the last 12 months (8)	Index of dependent variables (9)
<i>Panel A. Endline 1</i>									
Treated area	598 (384)	391* (213)	255 (1,056)	354 (314)	0.0083 (0.0215)	0.018 (0.0380)	0.009 (0.006)	0.002 (0.008)	0.0357 (0.0188)
Observations	6,800	6,800	6,685	6,239	6,810	6,810	6,757	2,352	6,810
Control mean	2,498	280	4,055	745	0.349	0.503	0.047	0.037	0.000
Hochberg-corrected <i>p</i> -value									0.175
<i>Panel B. Endline 2</i>									
Treated area	1,261** (530)	−134 (207)	−530 (547)	542 (372)	0.023 (0.023)	0.045 (0.040)	−0.000 (0.010)	−0.000 (0.006)	0.0151 (0.0186)
Observations	6,142	6,142	6,116	6,090	6,142	6,142	6,142	6,142	6,142
Control mean	5,003	1,007	5,225	953	0.418	0.561	0.083	0.053	0.000
Hochberg-corrected <i>p</i> -value									>0.999

*Notes:* The table presents the coefficient of a “treatment” dummy in a regression of each variable on treatment (with control variables listed in the text). Cluster-robust standard errors in parentheses. Results are weighted to account for oversampling of Spandana borrowers. The outcome variables are set to zero when the household does not have a business. Business outcomes are aggregated at the household level when the households have more than one business. Information on closing a business in the year prior to the endline 1 survey was only collected for those who had a business as of endline 1. Observations with missing or inconsistent itemized sales or revenues are dropped in columns 3 and 4. See online Appendix 1 for description of the construction of the profits, sales, and inputs variables. All monetary amounts in 2007 Rs. Column 9 presents the coefficient of a “treatment” dummy in a regression on treatment of an index of *z*-scores of the outcome variables in columns 1–8, plus revenues, number of new businesses, and number of new female-run businesses (see online Appendix Table A6, columns 1–3) for each round following Kling, Liebman, and Katz (2007). *p*-values for this regression are reported using Hochberg’s step-up method to control the FWER across all index outcomes. See text for details.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

later in the control group, however, households in the treatment group had, on average, been borrowing for longer than those in the control group, which is reflected in the fact that they had completed more loan cycles. On average, there was a difference of 0.085 loan cycles between the treatment and the control households at endline 2 (column 8), which is almost unchanged from endline 1.<sup>14</sup> The primary difference between treatment and control group at endline 2 is thus the length of access to microfinance. Since microfinance loans grow with each cycle, treatment households also had larger loans. Among those who borrow, there was by endline 2 a significant difference of about Rs. 2,400 (or 14 percent) in the size of the loans (not reported). Since about one third of households borrow, this translates into an (insignificant) difference of about Rs. 800 in average borrowing (column 3).

### B. New Businesses and Business Outcomes

Panel A in Table 3 presents the results from the first endline on business outcomes. Column 7 indicates that the probability that a household starts a business is in fact not significantly different in treatment and control areas. In comparison

<sup>14</sup> This difference is no longer significant at EL2, possibly owing to recall error and to the fact that we only collected information on the maximum number of cycles borrowed from any MFI, so this figure does not distinguish, e.g., a household that borrowed three cycles each from two lenders versus three cycles from one lender.

areas, 4.7 percent of households opened at least one business in the year prior to the survey, compared to 5.6 percent in treated areas. However, treatment households were *somewhat more likely to have opened more than one business* in the past year, and more new businesses were created in treatment areas overall: 6.8 per 100 households, versus 5.3 per 100 households in control areas.<sup>15</sup> The 90 percent confidence interval on new business creation ranges from an additional 0.3pp to 2.6pp additional new businesses. Overall, treatment households are no more likely to have a business and do not have significantly more businesses (columns 5 and 6).

Consistent with the fact that Spandana lends only to women, and with the stated goals of microfinance institutions, the marginal businesses tend to be female-operated. When we look at creation of businesses that are owned by women,<sup>16</sup> we find that almost all of the differential business creation in treatment areas is in female-operated businesses—there are 0.014 more female-owned businesses in treatment households than in control households, an increase of 55 percent (see Table 7, column 8). Households in treated areas were no more likely to report closing a business, an event reported by 3.9 percent of households in treatment areas and 3.7 percent of the households in comparison areas (column 8).<sup>17</sup>

Treatment households invest more in durables for their businesses. Since only a third of households have a business, and most businesses use no assets whatsoever, the point estimate is small in absolute value (Rs. 391 over the last year, or a bit less than a third of the increase in average MFI borrowing in treatment households), but the increment in treatment is more than the total value of business durables purchased in the last year by comparison households (Rs. 280), and is statistically significant (column 2).

The rest of the columns in the panel A of Table 3 report on current business status and last month's input expenses and profits (exclusive of interest payments). In these regressions, we assign a zero to those households that do not have a business, so these results give us the overall impact of credit on business activities, including both the extensive and intensive margins. Treatment households have more business assets (although the t-statistic on the asset stock is only 1.56). The treatment effect on expenses is positive but insignificant (column 3).<sup>18</sup>

Finally, there is an insignificant increase in business profits (column 4). Since this data includes zeros for households that do not have a business, this answers the question of whether microcredit, as it is often believed, increases poor households' income by expanding their business opportunities. The point estimate, at Rs. 354 per month, corresponds to a roughly 50 percent increase relative to the profits received by the average comparison household. This is thus large in proportion to profits, but it represents only a very small increase in disposable income for an average household—recall that the average total consumption of these households is about Rs. 6,500

<sup>15</sup> See online Appendix Table A6, column 2

<sup>16</sup> A business is classified as owned by a woman if the first person named in response to the question "Who is the owner of this business?" is female. Only 74 out of 3,188 businesses have more than 1 owner. Classifying a business as owned by a woman if any person named as the owner is female does not change the result.

<sup>17</sup> It is possible that households not represented in our sample, such as households that had not lived in the area for three years, may have been differentially likely to close businesses in treated areas. However, the relatively small amount of new business creation makes general-equilibrium effects on existing businesses rather unlikely.

<sup>18</sup> There is also a positive but insignificant effect on business revenues; see online Appendix Table A6, column 2.

TABLE 3B—SELF-EMPLOYMENT ACTIVITIES: REVENUES, ASSETS AND PROFITS (*Households with old businesses*)

	Assets (stock) (1)	Investment in last 12 months (2)	Revenue (3)	Expenses (4)	Profit (5)	Employees (6)	Index of dependent variables (7)
<i>Panel A. Endline 1</i>							
Treated area	898 (1,063)	1,119 (698)	5,266 (3,720)	1,620 (3,257)	2,105* (1,100)	−0.05 (0.0824)	0.09 (0.0406)
Observations	2,083	2,083	1,955	2,020	1,624	2,088	2,088
Control mean	6,757	678	14,505	12,325	2,038	0.41	0.00
Hochberg-corrected <i>p</i> -value							0.057
<i>Panel B. Endline 2</i>							
Treated area	1,682 (1,412)	−948 (588)	343 (1,263)	−2,644* (1,491)	839 (945)	−0.12 (0.099)	−0.007 −0.0263
Observations	1,878	1,878	1,859	1,862	1,844	1,878	1,878
Control mean	10,301	2,292	12,564	12,418	1,948	0.46	0.00
Hochberg-corrected <i>p</i> -value							>0.999

*Notes:* The table presents the coefficient of a “treatment” dummy in a regression of each variable on treatment (with control variables listed in the text). Cluster-robust standard errors in parentheses. Results are weighted to account for oversampling of Spandana borrowers. The outcome variables are set to missing when the household does not have an old business (i.e., one started more than a year prior to the survey). Business outcomes are aggregated at the household level when households have more than one business. Observations with missing or inconsistent itemized sales or revenues are dropped in columns 3 to 5. See online Appendix 1 for description of the construction of the profits, sales, and inputs variables. All monetary amounts in 2007 Rs. Column 7 presents the coefficient of a “treatment” dummy in a regression on treatment of an index of *z*-scores of the outcome variables in columns 1–6 for each round following Kling, Liebman, and Katz (2007). *p*-values for this regression are reported using Hochberg’s step-up method to control the FWER across all index outcomes. See text for details.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

per month, and an increase of Rs. 354 per month in business revenues is certainly not going to change the life of the average person who gets access to microcredit.

Looking at all businesses outcomes taken together, we find a 0.036 standard deviation increase in the standardized index of business outcomes, which is significant with conventional standard errors but not once the multiple hypothesis testing across different families of outcomes is taken into account (*p*-value of 0.18).<sup>19</sup>

This is the ITT estimate, and part of the reason it is low is that few households took advantage of microcredit in the treatment groups (and some did in the control as well). The marginal borrower in the treatment group may also have fewer opportunities than someone who was interested enough to borrow in the control group. This does not rule out that the businesses of some specific groups could have benefited from the loan. To look at this in more detail, we focus on businesses that were already in existence before microcredit started. We do this in Table 3B.<sup>20</sup> For businesses that existed before Spandana expanded, we find an expansion in businesses (revenue, inputs, and investment). While most individual indicators are imprecise, the overall business index is significant and positive, even after correcting for multiple inference (0.09 standard

<sup>19</sup> It is significant even with this correction when we control for strata dummies.

<sup>20</sup> In Table 3, we show that households are no more or less likely to close a business in the last year; there is thus no sample selection induced by microfinance.



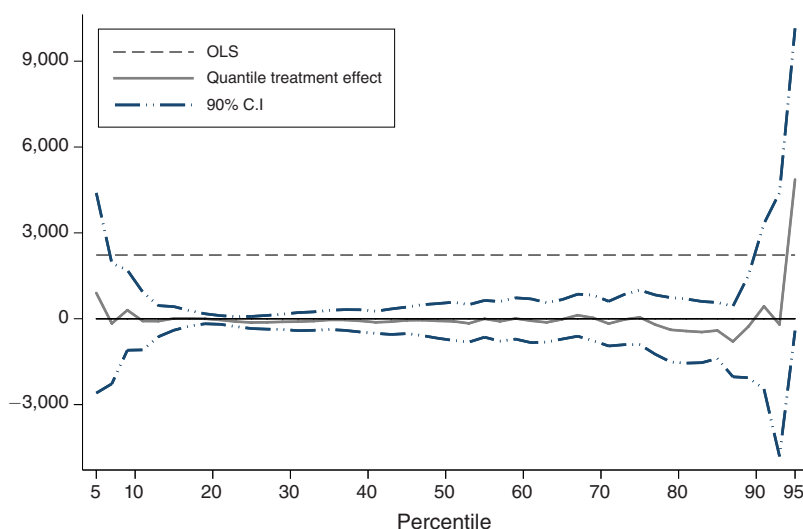


FIGURE 3. TREATMENT EFFECT ON BUSINESS PROFITS  
(HHs who have an old business, endline 1)

Notes: Old businesses are businesses started at least one year before the survey. Confidence intervals are cluster-bootstrapped at the neighborhood level.

deviations, with a  $p$ -value of 0.057 after the correction). We find an average increase in profits of Rs. 2,105 in treatment areas, which is statistically significant and represents more than doubling, relative to the control mean of Rs. 2,038. This increase is not due to a few outliers; however, it is worth nothing it is concentrated in the upper tail (quantiles 95 and above), as shown in Figure 3. At every other quantile, there is very little difference between the profits of existing businesses in treatment and control areas. There are 81 businesses above the ninety-fifth percentiles, far more than a handful, but the ninety-fifth percentile of monthly profit of existing businesses is Rs. 15,050 (or \$1,640 at PPP), which makes them quite large and profitable businesses for this setting. The vast majority of the small businesses make very little profit to start with, and microcredit does nothing to help them. This finding, that microcredit is most effective in helping already profitable businesses, is contrary both to much of the rhetoric of microcredit and to the view of microcredit skeptics.

Finally, we have seen that the treatment led to some more business creation, particularly the creation of female-owned businesses. In Figure 4, Table 3C and online Appendix Table A4, we show more data on the characteristics of these new businesses. The quantile regressions in Figure 4 (profits for businesses that did not exist at baseline) show that all new businesses between the thirty-fifth and sixty-fifth percentiles have significantly lower profits in treatment areas. Table 3C, column 5 shows that the mean profit is not significantly different across treatment and control due to the noisy data, but the median new business in treatment areas has Rs. 1,250 lower profits, significant at the 5 percent level (not reported in tables, but shown in the figure). The average new business is also significantly less likely to have employees in the treatment areas: the number of employees per new business is 0.29 in control and only 0.11 in treatment (column 6). For new businesses, the index across all outcomes

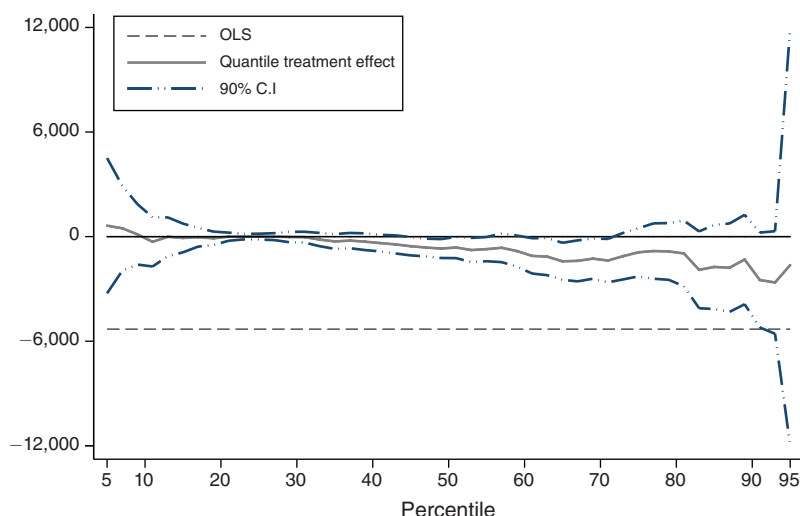


FIGURE 4. TREATMENT EFFECT ON BUSINESS PROFITS  
(HHs who have new business, *endline 1*)

*Notes:* New businesses are businesses started less than one year before the survey. Confidence intervals are cluster-bootstrapped at the neighborhood level.

TABLE 3C—SELF-EMPLOYMENT ACTIVITIES: REVENUES, ASSETS, AND PROFITS  
(Households with new businesses, *EL1 only*)

	Assets (stock) (1)	Investment in last 12 months (2)	Revenue (3)	Expenses (4)	Profit (5)	Employees (6)	Index of dependent variables (7)
Treated area	-873 (2,201)	-706 (1,324)	-8,167 (7,314)	-5,013 (4,049)	-3,548 (3,813)	-0.195* (0.112)	-0.0815 (0.0445)
Observations	356	356	332	339	270	356	356
Control mean	8,411	2,418	17,423	12,114	6,081	0.29	0.00
Hochberg-corrected <i>p</i> -value							0.280

*Notes:* The table presents the coefficient of a “treatment” dummy in a regression of each variable on treatment (with control variables listed in the text). Cluster-robust standard errors in parentheses. Results are weighted to account for oversampling of Spandana borrowers. The outcome variables are set to missing when the household does not have a new business (i.e., one started less than a year prior to the EL1 survey). Business outcomes are aggregated at the household level when the households have more than one business. Observations with missing or inconsistent itemized sales or revenues are dropped in columns 3 to 5. See online Appendix 1 for description of the construction of the profits, sales, and inputs variables. All monetary amounts in 2007 Rs. Column 7 presents the coefficient of a “treatment” dummy in a regression on treatment of an index of *z*-scores of the outcome variables in columns 1–6 following Kling, Liebman, and Katz (2007). *p*-values for this regression are reported using Hochberg’s step-up method to control the FWER across all index outcomes. See text for details.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

is negative (0.082 standard deviations) and significant with conventional levels but not after correcting for multiple inference (corrected *p*-value: 0.28).

These results could in principle be a combination of a treatment effect and a selection effect, but since the effect on existing businesses suggests a treatment effect that is close to zero for most businesses (and the point estimate is positive), the effect for new businesses is likely due to selection—the marginal business that gets started in

treatment areas is less profitable than the marginal business in the control areas. The hypothesis that the marginal business that gets started is different in the treatment group gains some additional support in online Appendix Table 4, which shows a comparison of the industries of old businesses and new businesses, across treatment and comparison areas.<sup>21</sup> Industry is a proxy for the average scale and capital intensity of a business, which is likely to be measured with less error than actual scale or asset use. The industry composition of new businesses do differ. In particular, the fraction of food businesses (tea/coffee stands, food vendors, kirana/small grocery stores, and agriculture) is 8.5 percentage points (about 45 percent) higher among new businesses in treatment areas than among new businesses in comparison areas, and the fraction of rickshaw/driving businesses among new businesses in treatment areas is 5.4 (more than 50 percent) percentage points lower. Both these differences are significant at the 10 percent level. Food businesses are the least capital-intensive businesses in these areas, with assets worth an average of just Rs. 930 (mainly dosa tawas, pots and pans, etc.). Rickshaw/driving businesses, which require renting or owning a vehicle, are the most capital-intensive businesses, with assets worth an average of Rs. 12,697 (the bulk of which is the cost of the vehicle).

Microcredit would be expected to lower the profitability threshold to start a business if interest rates are lower than those of other sources of lending available to the households. Another explanation for both results could be that, due to the fact that Spandana lends to women, the marginal businesses are more likely to be female-owned, and are thus started in sectors in which women are active. Furthermore, businesses operated by women generally tend to be less profitable, perhaps because of social constraints on what women can do and how much effort they can devote to an enterprise.<sup>22</sup>

Panel B of Table 3 shows the results for the business performance variables at the time of the second endline. As noted above, by this time treatment and control households are equally likely to have a microcredit loan, but loan amounts in treatment areas are larger and borrowers have been borrowing for a longer time. The results follow a clear pattern, consistent with the idea that control households now borrow at the same rate. We find no significant difference in business creation in treatment and control areas: the point estimate is virtually zero (the 90 percent confidence interval ranges from 2pp fewer new businesses, to 2.5pp more). The new businesses are in the same industries in treatment and control areas, and the negative effects for new businesses at the median have disappeared (results omitted). For the contemporaneous flow investment outcomes such as new business creation, business assets acquired in the previous year, etc. (columns 8 through 11) the point estimate is very close to zero (however the standard errors are large). On the other hand, businesses in treatment areas have significantly larger asset stocks (column 1), which reflects the cumulative effect of the past years during which they had a chance to borrow and expand. Despite this, their profits are still not significantly larger, though the point estimate is around 60 percent of the sample mean (with a *t*-statistic of around 1.5). As shown in

<sup>21</sup> Respondents could classify their businesses into 22 different types, which we grouped into the following: food, clothing/sewing, rickshaw/driving, repair/construction, crafts vendor, and "other."

<sup>22</sup> This is true in this data, and also found, for example, in Sri Lanka by de Mel, McKenzie, and Woodruff (2009).

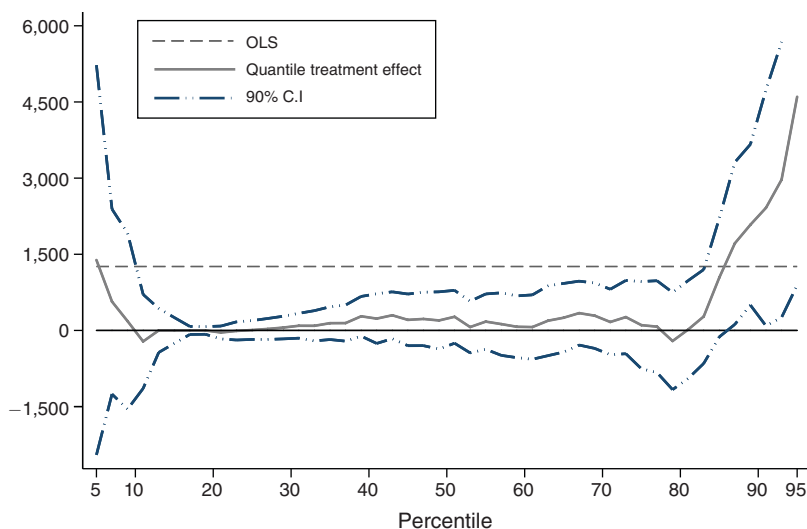


FIGURE 5. TREATMENT EFFECT ON BUSINESS PROFITS  
(Full sample of business owners, endline 2)

Figure 5, the positive increase is once again concentrated in the right tail, although it starts being positive a little earlier, at the eighty-fifth percentile.

Overall, microfinance is indeed associated with (some) business creation: in the first year, it does lead to an increase in the number of new businesses created, particularly by women (though not in the number of households that start a business). However, these marginal businesses are even smaller and less profitable than the average business in the area, the vast majority of which are already small and unprofitable. Microfinance does also lead to greater investment in existing businesses, and an improvement in the profits for the most profitable of those businesses. For everyone else, business profits do not increase, and, on average, microfinance does not help the businesses to grow in any significant way. Even after three years, there is no increase in the number of employees of businesses that existed before Spandana started its operation (Table 3B, column 6). Table 4 shows that total self-employment income is unaffected by treatment.

### C. Labor Supply

Access to credit can lead to an increase in labor supply to finance investment or the purchase of durable goods which were out of reach before due to savings and borrowing constraints. This is an area where different evaluations of micro-credit have very different results, ranging from a worrying increase in labor supply for teenagers in Augsburg et al. (2015) to steep decreases for everyone in Crépon et al. (2015). Table 5 shows the impact of the program on labor supply. In endline 1, the household head and spouse in treatment households increase their overall labor supply by an average of 3.18 hours (column 6; 90 percent CI: 0.84, 5.5). The increase occurs entirely in the households' own businesses (column 7), and there is no increase in number of hours worked for wages (column 8): those hours may

TABLE 4—INCOME

	Self employment (profit) (1)	Daily labor/salaried (2)	Index of dependent variables (3)
<i>Panel A. Endline 1</i>			
Treated area	354 (314)	−526 (358)	−0.0501 (0.0459)
Observations	6,239	6,827	6,832
Control mean	745	2,988	0.000
Hochberg-corrected <i>p</i> -value			>0.999
<i>Panel B. Endline 2</i>			
Treated area	542 (372)	−141 (212)	0.0114 (0.0261)
Observations	6,090	6,142	6,142
Control mean	953	5,514	0.000
Hochberg-corrected <i>p</i> -value			>0.999

*Notes:* The table presents the coefficient of a “treatment” dummy in a regression of each variable on treatment (with control variables listed in the text). Cluster-robust standard errors in parentheses. Results are weighted to account for oversampling of Spandana borrowers. Self-employment income equals profit of a self-employment activity (summed across activities if multiple in the household). Equal to zero for households with no self-employment activity. Daily labor/salaried income is income from employment other than self employment, summed across working household members. See online Appendix 1 for description of the construction of the profit variable. Column 3 presents the coefficient of a “treatment” dummy in a regression on treatment of an index of *z*-scores of the outcome variables in columns 1–2 for each round following Kling, Liebman, and Katz (2007). *p*-values for this regression are reported using Hochberg’s step-up method to control the FWER across all index outcomes. See text for details.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

be much less elastic, if the households do not fully choose them. However, we do not find the increase in teenagers’ labor supply that is sometimes feared to be a potential downside of microfinance and that Augsburg et al. (2015) find in Bosnia (as adolescents are drawn into the business by their parents); indeed, teenage girls work about two hours less per week in treatment than control areas (column 4), and this difference is significant at the 5 percent level. There is no effect on teenage boys’ hours (column 5). Given that there is an increase in work hours among adults and a decrease among teens, the overall index is, not surprisingly, close to zero and insignificant. By endline 2, as control households have started borrowing, the difference between treatment and control disappears.

#### D. Consumption

Table 6 gives intent-to-treat estimates of the effect of microfinance on household spending. Columns 1 and 3 of panel A show that there is no significant difference between treatment and comparison households in total household expenditures—either total or nondurable—per adult equivalent. The point estimate is essentially zero in both cases and we can reject at the 5 percent level the null hypothesis that there was a Rs.85 per month increase in total consumption per adult equivalent and a Rs.57 per month increase in nondurable consumption (about 6 percent of the

TABLE 5—TIME WORKED BY HOUSEHOLD MEMBERS

	Hours worked over the past seven days, by age group:								Index of dependent variables (9)
	All adults and teens			Teens		Household head and spouse			
	of which:			Girls (4)	Boys (5)	of which:			
	Total (1)	Self employment (2)	Outside activities (3)			Total (6)	Self employment (7)	Outside activities (8)	
<i>Panel A. Endline 1</i>									
Treated area	0.739 (2.245)	2.466 (2.361)	−2.033 (2.741)	−2.076** (1.046)	−0.026 (2.065)	3.176** (1.421)	2.710* (1.474)	0.466 (1.418)	0.00647 (0.0179)
Observations	6,827	6,762	6,762	2,174	1,866	6,827	6,827	6,827	6,849
Control mean	92.38	34.38	58.01	7.94	25.12	57.79	25.83	31.96	0.000
Hochberg-corrected <i>p</i> -value									>0.999
<i>Panel B. Endline 2</i>									
Treated area	−1.238 (1.544)	1.713 (2.162)	−2.951 (2.490)	0.440 (0.948)	−1.387 (1.521)	0.991 (1.176)	1.703 (1.583)	−0.712 (1.488)	−0.00555 (0.0130)
Observations	6,142	6,142	6,142	1,789	1,665	6,142	6,142	6,142	6,142
Control mean	83.34	37.00	46.34	5.83	20.95	51.31	25.38	25.93	0.000
Hochberg-corrected <i>p</i> -value									>0.999

Notes: Teens are household members aged 16 to 20. Adults are household members aged 21 and above. Total hours includes hours worked in self-employment and in outside activities. It does not include time spent in housework. See online Appendix 1 for description of the construction of the self-employment variable. Column 9 presents the coefficient of a “treatment” dummy in a regression on treatment of an index of *z*-scores of the outcome variables in columns 1–8 for each round following Kling, Liebman, and Katz (2007). *p*-values for this regression are reported using Hochberg’s step-up method to control the FWER across all index outcomes. See text for details.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

TABLE 6—CONSUMPTION (*Per capita, per month*)

	Total (1)	Durables (2)	Nondurable (3)	Food (4)	Health (5)	Education (6)	Temptation goods (7)	Festivals and celebrations (8)	Home durable good index (9)
<i>Panel A. Endline 1</i>									
Treated area	10.24 (37.22)	19.73* (11.35)	−6.50 (31.81)	−12.11 (12.06)	−3.7 (11.51)	−2.061 (9.865)	−8.785* (4.92)	−14.16* (8.09)	−0.051 (0.057)
Observations	6,827	6,781	6,781	6,827	6,827	5,415	6,827	6,827	6,841
Control mean	1,419	116	1,305	525	140	168	84	69	2.37
Hochberg-corrected <i>p</i> -value	>0.999								
<i>Panel B. Endline 2</i>									
Treated area	−48.83 (51.53)	0.42 (9.88)	−45.45 (46.92)	−11.20 (17.88)	−22.54 (17.50)	12.16 (15.19)	−10.07 (6.61)	6.17 (4.12)	−0.0127 (0.0426)
Observations	6,142	6,140	6,142	6,142	6,141	4,910	6,142	6,103	6,142
Control mean	1,914	131	1,755	687	187	206	118	90	2.66
Hochberg-corrected <i>p</i> -value	0.691								

Notes: Columns 1–8: Monthly per capita household expenditures. Temptation goods include alcohol, tobacco, betel leaves, gambling, and food consumed outside the home. Column 9 calculated on a list of 40 home durable goods (stock, not flow). Each asset is given a weight using the coefficients of the first factor of a principal component analysis. The index, for a household *i*, is calculated as the weighted sum of standardized dummies equal to 1 if the household owns the durable good, 0 otherwise. See online Appendix 1 for description of the construction of the consumption variables. *p*-values for the regression in column 1 (total consumption) reported using Hochberg’s step-up method to control the FWER across all outcomes. See text for details.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.



average in control for consumption, and 4 percent for nondurable consumption) increase.<sup>23</sup> Hence, enhanced microcredit access does not appear to be associated with any meaningful increase in consumption after 15 to 18 months. Of course, this may partly be due to the fact that relatively few people borrow, and that some in the control group borrow from another MFI.<sup>24</sup>

While there are no significant impacts on average consumption and nondurable consumption, there are shifts in the *composition* of expenditure: column 2 shows that households in treatment areas spent a statistically significant Rs. 19.73 more per capita per month,<sup>25</sup> or Rs. 237 per capita over the last year, on durables than did households in comparison areas. Note that this is probably an underestimate of the total effect of loans on durable purchases, since our measure would miss anyone who borrowed more than a year before the survey (the survey was 15 to 18 months after the centers opened) and immediately bought a durable with the loan. The most commonly purchased durables include gold and silver, motorcycles, sarees (purchased in bulk, presumably mainly for weddings or as stock for a business), color TVs, refrigerators, rickshaws, computers, and cellphones.

Columns 7 and 8 show that while there was no detectable change in nondurable spending otherwise, the increase in durable spending by treatment households was essentially offset by reduced spending on “temptation goods” and festivals. Temptation goods are goods that households in our baseline survey said that they would like spend less on (this is thus the same list of goods for all households). They include alcohol, tobacco, betel leaves, gambling, and food consumed outside the home. Spending on temptation goods is reduced by about Rs. 9 per capita per month (column 7). We also see in column 8 a large fall in festival spending per capita in the previous year (Rs. 14 or 21 percent of the control level), both significant at the 10 percent level). Together, the average drop in consumption in temptation goods and festivals is Rs. 23 per capita per month. The decrease in festival expenditures does not come from large changes in large, very expensive ceremonies such as weddings (we see very few of them in the data) but rather appears to come from declines at all levels of the distribution of spending on festivals.

The absolute magnitude of these changes is relatively small: for instance, the Rs. 19.73 of increased durables spending per capita per month at endline 1 is approximately \$2.14 at 2007 PPP exchange rates. However, this represents an increase of about 17 percent relative to total spending on durable goods in comparison areas. Furthermore, this figure averages over nonborrowers and borrowers, and would be larger if it was attributed to borrowers alone.

Panel B of Table 6 reports on the impact effects at the time of the second endline, when both treatment and control households have access to the microfinance program. The effects on both total per capita spending and total per capita nondurable

<sup>23</sup>The 90 percent CIs are (−52, 72) for total consumption and (−59, 46) for nondurable consumption.

<sup>24</sup>For total consumption, the implied treatment on the treated (TOT) or IV estimate is a Rs. 122 (10.24/0.084), or 9 percent, increase, and for nondurable consumption it is a Rs. 77 (6 percent) decrease. However, the 90 percent confidence interval on the TOT estimate is wide, ranging from an increase of Rs. 857 (or 60 percent) to a decrease of Rs. 613 (or 43 percent) in total consumption per capita. The width of the TOT confidence intervals stems, of course, from the low first stage.

<sup>25</sup>The 90 percent CI is (1, 39).

spending (columns 1 and 3) are negative with t-statistics around 1. Spending on temptation goods is still lower by about Rs. 10 per month (column 7), similar to endline 1, though the effect is now insignificant. The effect on festivals is now positive but insignificant. There is also no difference on average in durable goods spending in endline 2 (column 2). Given that the main difference between treatment and control households at endline 2 is that treatment households have been borrowing for longer, this suggests that, in the second cycle, households in the treatment seem to just repeat the first cycle with another durable (of roughly the same size), while households in the control group also acquire a durable.

#### *E. Microfinance as Social Revolution: Education, Child Labor, and Women's Empowerment?*

The evidence so far suggests a different picture from the standard description of the role of microfinance in the life of the poor: the pent-up demand for it is not overwhelming; many households use their loan to acquire a household durable, reducing avoidable consumption to finance it; some invest in their businesses, but this does not lead to significant growth in the profitability of most businesses. Another staple of the microfinance literature is that because the loans are given to women and give them a chance to start their own businesses, this would lead to a more general empowerment of women in the households, and this empowerment would in turn translate into better outcomes for everyone in the household, including education, health, etc. (e.g., CGAP 2009). Indeed, we see a significant increase in the number of businesses managed by women in endline 1 (Table 7, column 9).<sup>26</sup> To examine whether this increase in women's entrepreneurship translates into increased bargaining power for women, Table 7 examines the effects of access to microfinance on measures of women's decision making and children's education and labor supply.

A finding of many studies of household decision making is that an increase in women's bargaining power leads to an increase in investments in children's human capital (see Thomas 1990 and Duflo 2003). However, we find that there is no change in the probability that children or teenagers are enrolled in school (Table 7, columns 1, 2, 5, and 6), although we do see a reduction in teenage girls' labor supply (Table 5, column 5). There is no difference in spending on private school fees, or in private versus public school enrollment (results omitted). There is also no difference in the number of hours worked by girls or boys aged 5 to 15 (columns 3 and 4).

Because there are many possible proxies for women's empowerment and many "social" outcomes we use the approach of Kling, Liebman, and Katz (2007) to test the null hypothesis of no effect of microcredit on "social outcomes" against the alternative that microcredit improves social outcomes. We construct an equally weighted average of z-scores for 16 social outcomes; this method gives us maximal power to detect an effect on social outcomes, if such an effect is present.<sup>27</sup> Column 7

<sup>26</sup> There is no difference in the number of women-run businesses between treatment and control in endline 2, which is unsurprising since all areas have access to microfinance at that point.

<sup>27</sup> The 16 outcomes we use are: indicators for women making decisions on each of food, clothing, health, home purchase and repair, education, durable goods, gold and silver, investment; levels of spending on school tuition, fees, and other education expenses; medical expenditure; teenage girls' and teenage boys' school enrollment; and

TABLE 7—SOCIAL EFFECTS

	Share of children aged 5–15 in school		Hours worked per child aged 5–15 over the past 7 days:		Share of teenagers (aged 16–20) in school		Index of women’s independence/ empowerment (7)	Number new self-employ. activities managed by women (all HHs) (8)	Index of dependent variables (9)
	Girls (1)	Boys (2)	Girls (3)	Boys (4)	Girls (5)	Boys (6)			
<i>Panel A. Endline 1</i>									
Treated area	−0.016 (0.013)	−0.012 (0.011)	−0.028 (0.202)	0.613 (0.743)	−0.037 (0.024)	−0.007 (0.028)	0.007 (0.023)	0.0143*** (0.005)	−0.008 (0.0097)
Observations	3,035	3,073	3,035	3,073	2,174	1,866	6,862	6,762	6,862
Control mean	0.919	0.918	0.594	0.577	0.338	0.429	−0.001	0.026	0.000
Hochberg-corrected <i>p</i> -value									>0.999
<i>Panel B. Endline 2</i>									
Treated area	0.015 (0.011)	0.007 (0.011)	0.092 (0.133)	−0.531* (0.269)	0.021 (0.024)	−0.021 (0.027)	−0.011 (0.021)	−0.005 (0.006)	0.005 (0.009)
Observations	2,755	2,746	2,755	2,746	1,789	1,665	6,142	6,142	6,142
Control mean	0.923	0.928	0.286	1.379	0.329	0.474	−0.003	0.047	0.000
Hochberg-corrected <i>p</i> -value									>0.999

Notes: In columns 1–4 the sample is restricted to households with children between the age of 5 and 15. In columns 5–6 the sample is restricted to households with teens between the age of 16 and 20. Column 7 is the effect on an equally weighted average of z-scores for the 16 social outcomes: indicators for women making decisions on each of food, clothing, health, home purchase and repair, education, durable goods, gold and silver, investment; levels of spending on school tuition, fees, and other education expenses; medical expenditure; teenage girls' and teenage boys' school enrollment; and counts of female children under one year and one- to two-years-old. Column 9 presents the coefficient of a "treatment" dummy in a regression on treatment of an index of z-scores of the outcome variables in columns 1–9 for each round following Kling, Liebman, and Katz (2007). *p*-values for this regression are reported using Hochberg's step-up method to control the FWER across all index outcomes. See text for details.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

shows that there is no effect on the index of social outcomes (point estimate 0.007 standard deviations) and we can rule out an increase of more than one twentieth of a standard deviation with 95 percent confidence.<sup>28</sup>

This suggests that there is no *prima facie* evidence that microcredit leads to important changes in household decision making or in social outcomes. Furthermore, this null effect is not an artifact of observing households only in the very short run. Nothing major changes by endline 2: the effect of microfinance access on the index of women empowerment is still very small (indeed, slightly negative) and insignificant, and anything but a small effect can still be ruled out. Recall that we are comparing households who, by EL2, are equally likely to borrow: the main difference by EL2 is that households in the treatment group have had greater access to microfinance for the first 18 months; this may limit power to detect differences in the social outcomes at the community level.

counts of female children under one year and one- to two-years-old. We selected these outcomes because they would likely be affected by changes in women's bargaining power within the household.

<sup>28</sup>The 95 percent CI is (−0.04, 0.05). The units are standard deviations.

#### IV. Conclusion

This study—the first and longest running evaluation of the standard group-lending loan product that has made microfinance known worldwide—yields a number of results that may prompt a rethinking of the role of microfinance.

The first result is that, in contrast to the claims sometimes made by MFIs and others (including our partner), demand for microloans is far from universal. By the end of our three-year study period, only 33 percent of households borrow from an MFI,<sup>29</sup> and this is among households selected based on their relatively high propensity to take up microcredit. This does not appear to be an anomaly: two other randomized interventions that have a similar design (in Morocco and in Mexico) also find relatively low take-up, while another study in rural South India that focuses specifically on take-up of microfinance also finds it to be low (Banerjee et al. 2013). Perhaps despite evidence of high marginal rates of return among microbusinesses, e.g., de Mel, McKenzie, and Woodruff (2008), most households either do not have a project with a rate of return of at least 24 percent—the APR on a Spandana loan—or simply prefer to borrow from friends, relatives, or money lenders due to the greater flexibility those sources provide, despite costs such as higher interest (from moneylenders) or embarrassment (when borrowing from friends or relatives) (Collins et al. 2009).

For those who choose to borrow, while microcredit “succeeds” in leading some of them to expand their businesses (or to start a female-owned business), it does not appear to fuel an escape from poverty based on those small businesses. Monthly consumption, a good indicator of overall welfare, does not increase for those who had early access to microfinance, either in the short run (when we may have foreseen that it would not increase, or perhaps even expected it to *decrease*, as borrowers finance the acquisition of household or business durable goods), or in the longer run, after this crop of households have access to microcredit for a while and when those in the former control group should be the ones tightening their belts. Business profits do not increase for the vast majority of businesses, although there are significant increases in the upper tail of profitability. This study took place in a dynamic urban environment, in a context of very high growth. Microcredit seems to have played very little part in this growth, though it may have different impacts in other settings.

Furthermore, in the Hyderabad context, we find that access to microcredit appears to have no discernible effect on education, health, or women’s empowerment in the short run. In the longer run (when borrowing rates are the same, but households in the treatment groups have on average borrowed for longer), there is still no impact on women’s empowerment or other social outcomes. The results differ from study to study on these outcomes, but as a whole they don’t paint a picture of dramatic changes in basic development outcomes for poor families.

Microcredit therefore may not be the “miracle” that it is sometimes claimed to be, although it does allow some households to invest in their small businesses. One reason may be that the average business run by this target group is tiny (almost none of them have an employee), is not particularly profitable, and is difficult to expand

<sup>29</sup>The take-up rate is 42 percent in treatment areas and 33 percent percent in control areas.

even in a high-growth context, given the skill sets of the entrepreneurs and their life situations. And the marginal businesses that get created thanks to microcredit are probably even less profitable and dynamic: we find that the average new business in a microcredit treatment area is less likely to have an employee than the new business in the control areas, and the median new business is even less profitable in treatment versus control areas.

Nevertheless, microcredit does affect the structure of household consumption. We see households invest in home durable goods and restrict their consumption of temptation goods and expenditures on festivals and parties. They continue to do so several years later, and this decrease is not due to a few particularly virtuous households, but seems to be spread across the sample. Similar declines in these types of expenses are also found in all the other studies. Altered consumption thus does not seem to be tied to the ideology of a particular MFI.

Microfinance affects labor supply choices as well: here we find that households that have access to loans seem to work harder on their own businesses; in other settings, they are found to cut arduous labor elsewhere. Thus, microcredit plays its role as a financial product in an environment where access to both credit and saving opportunities is limited. It expands households' abilities to make different intertemporal choices, including business investment. The only mistake that the microcredit enthusiasts may have made is to overestimate the potential of businesses for the poor, both as a source of revenue and as a means of empowerment for their female owners.

## REFERENCES

- Angelucci, Manuela, Dean Karlan, and Jonathan Zinman. 2015. "Microcredit Impacts: Evidence from a Randomized Microcredit Program Placement Experiment by Compartamos Banco." *American Economic Journal: Applied Economics* 7 (1): 151–82.
- Attanasio, Orazio, Britta Augsburg, Ralph De Haas, Emla Fitzsimons, and Heike Harmgart. 2015. "The Impacts of Microfinance: Evidence from Joint-Liability Lending in Mongolia." *American Economic Journal: Applied Economics* 7 (1): 90–122.
- Augsburg, Britta, Ralph De Haas, Heike Harmgart, and Costas Meghir. 2015. "The Impacts of Microcredit: Evidence from Bosnia and Herzegovina." *American Economic Journal: Applied Economics* 7 (1): 183–203.
- Banerjee, Abhijit, Esther Duflo, Rachel Glennerster, and Cynthia Kinnan. 2015. "The Miracle of Microfinance? Evidence from a Randomized Evaluation: Dataset." *American Economic Journal: Applied Economics*. <http://dx.doi.org/10.1257/app.20130533>.
- Banerjee, Abhijit, Arun G. Chandrasekhar, Esther Duflo, and Matthew O. Jackson. 2013. "The Diffusion of Microfinance." *Science* 341 (6144).
- Buena, Francisco J., Joseph P. Kaboski, and Yongseok Shin. 2011. "The Macroeconomics of Microfinance." [http://www.cfsp.org/sites/default/files/macroeconomics\\_of\\_microfinance.pdf](http://www.cfsp.org/sites/default/files/macroeconomics_of_microfinance.pdf).
- Collins, Daryl, Jonathan Morduch, Stuart Rutherford, and Orlanda Ruthven. 2009. *Portfolios of the Poor: How the World's Poor Live on Two Dollars a Day*. Princeton: Princeton University Press.
- Crépon, Bruno, Florencia Devoto, Esther Duflo, and William Parienté. 2015. "Estimating the Impact of Microcredit on Those Who Take It Up: Evidence from a Randomized Experiment in Morocco." *American Economic Journal: Applied Economics* 7 (1): 123–50.
- de Mel, Suresh, David McKenzie, and Christopher Woodruff. 2008. "Returns to Capital in Microenterprises: Evidence from a Field Experiment." *Quarterly Journal of Economics* 123 (4): 1329–72.
- de Mel, Suresh, David McKenzie, and Christopher Woodruff. 2009. "Are Women More Credit Constrained? Experimental Evidence on Gender and Microenterprise Returns." *American Economic Journal: Applied Economics* 1 (3): 1–32.
- DiNardo, John, Nicole M. Fortin, and Thomas Lemieux. 1996. "Labor Market Institutions and the Distribution of Wages, 1973–1992: A Semiparametric Approach." *Econometrica* 64 (5): 1001–44.

- Duflo, Esther.** 2003. "Grandmothers and Granddaughters: Old Age Pensions and Intra-Household Allocation in South Africa." *World Bank Economic Review* 17 (1): 1–25.
- Fulford, Scott.** 2011. "The Effects of Financial Development in the Short and Long Run." <http://fmwww.bc.edu/ec-p/wp741.pdf>.
- Giné, Xavier, and Robert M. Townsend.** 2004. "Evaluation of Financial Liberalization: A General Equilibrium Model with Constrained Occupation Choice." *Journal of Development Economics* 74 (2): 269–307.
- Heckman, James J.** 1992. "Randomization and Social Program Evaluation." In *Evaluating Welfare and Training Programs*, edited by Charles F. Manski and Irwin Garfinkel, 201–30. Cambridge, MA: Harvard University Press.
- Hochberg, Yosef.** 1988. "A Sharper Bonferroni Procedure for Multiple Tests of Significance." *Biometrika* 75 (4): 800–802.
- Kaboski, Joseph P., and Robert M. Townsend.** 2011. "A Structural Evaluation of a Large-Scale Quasi-Experimental Microfinance Initiative." *Econometrica* 79 (5): 1357–1406.
- Karlan, Dean, and Jonathan Zinman.** 2010. "Expanding Credit Access: Using Randomized Supply Decisions To Estimate the Impacts." *Review of Financial Studies* 23 (1): 433–64.
- Karlan, Dean, and Jonathan Zinman.** 2011. "Microcredit in Theory and Practice: Using Randomized Credit Scoring for Impact Evaluation." *Science* 332 (6035): 1278–84.
- Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz.** 2007. "Experimental Analysis of Neighborhood Effects." *Econometrica* 75 (1): 83–119.
- Microcredit Summit Campaign.** 2012. "State of the Campaign Report." Washington, DC: Results Educational Fund.
- MIX Market.** 2009. "Profile for Spandana." <http://www.mixmarket.org/mfi/spandana>.
- Polgreen, Lydia, and Vikas Bajaj.** 2010. "India Microcredit Faces Collapse From Defaults." *New York Times*, November 17. [http://www.nytimes.com/2010/11/18/world/asia/18micro.html?pagewanted=all&\\_r=0](http://www.nytimes.com/2010/11/18/world/asia/18micro.html?pagewanted=all&_r=0).
- Tarozzi, Alessandro, Jaikishan Desai, and Kristin Johnson.** 2015. "The Impacts of Microcredit: Evidence from Ethiopia." *American Economic Journal: Applied Economics* 7(1): 54–89.
- Thomas, Duncan.** 1990. "Intra-Household Resource Allocation: An Inferential Approach." *Journal of Human Resources* 25 (4): 635–64.
- World Bank.** 2005. "World Development Indicators." <http://data.worldbank.org/products/data-books/WDI-2005>.
- World Bank.** 2007. "World Development Indicators, Table 4.14: Exchange rates and prices." <http://siteresources.worldbank.org/DATASTATISTICS/Resources/WDI07frontmatter.pdf>.



**This article has been cited by:**

1. Rukmini Banerji, James Berry, Marc Shotland. 2017. The Impact of Maternal Literacy and Participation Programs: Evidence from a Randomized Evaluation in India. *American Economic Journal: Applied Economics* 9:4, 303-337. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
2. Erica Field, Seema Jayachandran, Rohini Pande, Natalia Rigol. 2016. Friendship at Work: Can Peer Effects Catalyze Female Entrepreneurship?. *American Economic Journal: Economic Policy* 8:2, 125-153. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
3. Alessandro Tarozzi, Jaikishan Desai, Kristin Johnson. 2015. The Impacts of Microcredit: Evidence from Ethiopia. *American Economic Journal: Applied Economics* 7:1, 54-89. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
4. Orazio Attanasio, Britta Augsborg, Ralph De Haas, Emla Fitzsimons, Heike Harmgart. 2015. The Impacts of Microfinance: Evidence from Joint-Liability Lending in Mongolia. *American Economic Journal: Applied Economics* 7:1, 90-122. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
5. Britta Augsborg, Ralph De Haas, Heike Harmgart, Costas Meghir. 2015. The Impacts of Microcredit: Evidence from Bosnia and Herzegovina. *American Economic Journal: Applied Economics* 7:1, 183-203. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]