

Dangers of a Double-Bottom Line: A Poverty Targeting Experiment Misses Both Targets

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March 2018

Abstract

Two for-profit Philippine social enterprises, aiming to increase microlending to the poor, tested a widely-used poverty measurement tool. The banks incorporated 10 questions into their application and underwriting systems, and then randomly assigned a subset of account officers to receive: an *explanation* of the questions' purpose; *exhortation* tying the tool to the organizations' social missions; and *reassurance* that these data, conditional on other characteristics, do not predict default and thus should not jeopardize incentive pay based on portfolio performance. Training for the control group account officers presented these questions as merely “additional household information”. This intervention backfired, leading to no additional poor applicants and lower-performing loans. Descriptive evidence suggests the intervention exacerbated loan officer misperceptions and multitasking problems, highlighting the difficulty of incentivizing personnel tasked with juggling profit and social objectives. Altogether our results help explain why corporate social responsibility efforts are often siloed from core operations.

Keywords: Corporate social responsibility, double-bottom line, multi-tasking, social business, poverty targeting, discrimination, microfinance, microcredit

JEL: D12; D22; D92; G21; O12; O16

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I. Introduction

For-profit social enterprises seek to “do well by doing good”, maximizing a double-bottom line where both profits and social impacts are important objectives (e.g., Yunus 2008). Microcredit in particular “has become the leading example of a broader push for social investment in the health, education, and energy sectors” (Conning and Morduch 2011). Beyond social enterprises *per se*, businesses in most industries have made substantial investments in corporate social responsibility (“CSR”) that introduce elements of a second bottom-line (e.g., Servaes and Tamayo 2013; Flammer 2015).²

Can enterprises pursue twin objectives without comprising one or both them? In particular, can firms in microcredit—a “leading example” among social enterprises-- succeed in maximizing profits while expanding access to credit for the poor? Work on multi-tasking, beginning with the seminal theory of Holmstrom and Milgrom (1991, 1994), highlights the challenge from a mechanism design perspective: if employees (or managers) face relatively strong incentives for one of the objectives, they may neglect the other.³ Another challenge is that poverty targeting is nontrivial (Alatas et al. 2012; Banerjee et al. 2015; Alatas et al. forthcoming; Hanna and Karlan 2017; Karlan and Thuysbaert forthcoming). So is finding the efficient frontier of the *traditional* bottom line (e.g., Karlan and Zinman 2017). In short, maximizing *either* objective for a social enterprise like a microlender seems challenging enough; trying to maximize twin objectives may backfire.

We examine the social enterprise balancing act, using a poverty targeting experiment implemented by two banks in the Philippines. The banks sought to increase their lending to poorer microentrepreneurs-- without sacrificing their own profits. To this end we worked with the banks to integrate a widely-used poverty targeting tool into loan officers’ standard application workflow. Specifically, each bank included the tool’s 10 questions, along with standard questions used for underwriting, in its new loan application management and credit scoring software. The research team trained all 27 loan officers that worked in microlending in 20 branches to use the new software. Of the 20 branches, 10 were randomly assigned to treatment and 10 to control. Loan officers in treatment branches were informed that the new questions should be used to improve identification of poor households, reassured that the poverty indicators were not linked to credit risk or the assessment thereof, and reminded of the bank’s social mission to reach poor households. Loan officers in control branches were informed simply that the 10 new questions are “additional household information”.

We implemented the poverty targeting tool with the intention of making it easier for loan officers to identify and encourage poor households to apply. For the treatment group this objective

² The CSR literature has only recently begun to use within-firm experiments to examine effects of CSR on employees. The employees in these experiments have been freelancers, with their tasks and customers assigned exogenously, and their responsibilities focused on maximizing profits (V. C. Burbano 2016; V. Burbano 2017; Hedblom, Hickman, and List 2016; List and Momeni 2017). Our experiment complements this work by taking place in long-established firms, with longer-tenured employees who are responsible for bringing in customers and juggling both sides of the bottom line.

³ See e.g., Palacios (2018) for a review of the empirical literature on multi-tasking problems in firms.

was made prominent and tied explicitly to the banks' social missions. Beyond the tool and senior management's exhortation and expectations, loan officers faced no additional inducements to bring in poor households. Nonetheless, the exhortations and expectations by senior management were sincere: the plan was for the training to increase lending to the poor and then to use that exogenous increase to study the impact of the banks' microlending on poverty alleviation.

Loan officers also faced incentives to maximize the traditional bottom line, in the form of performance pay based on number of loans originated and the timeliness of loan repayments. In this sense we expected the poverty targeting tool to alleviate a classic multi-tasking problem where loan officers faced relatively strong incentives to maximize profits, potentially at the expense of bringing in more poor clients.

Treatment effects of the poverty targeting tool, estimated on outcomes measured over a two-year horizon, suggest that it backfired. On the social side of the bottom line, the tool failed to increase lending to the poor: treated loan officers brought in weakly more applicants (12, SE=13), but no more poor applicants (0, SE=3), *decreasing* the proportion of poor applicants and loan approvals. On the traditional side of the bottom line, the tool failed to hold profits constant: loans brought in by treatment group officers have significantly *higher* default rates (3.4 pp, SE=1.7 pp), while loan size and other terms do not change. An increase in default rates is bad not just for the banks but for the individual loan officers as well, in the sense that higher default rates can lead to reprimands and missed incentive bonuses.

Surprised by these results, two years after the experimental period we worked with the banks to administer a survey of all bank loan officers (not just those who cover microcredit; N=68) to explore their beliefs and attitudes. Subject to the caveats that these data are from after the experimental period, on a broader sample of loan officers, and purely descriptive, they nevertheless enable us to explore mechanisms. We infer two key patterns. First, loan officers view the profit-making side of their job as more important than the social welfare side. Second, loan officers perceive poorer borrowers to be less profitable, despite bank management exhortations and empirical evidence to the contrary.⁴

Taking the experimental and descriptive results together, we speculate that treatment group loan officers were trying to act like canonical multi-taskers: maximize profits without making any additional effort to bring in more poor borrowers. But—still speculating—treated loan officers mistakenly thought that the new poverty targeting tool was helping them do a better job of bringing in profitable borrowers by screening out poor credit risks. Perhaps this was a salience-driven overreaction (Bordalo, Gennaioli, and Shleifer 2017); after all, *all* loan officers in the experiment had access to the tool, with the treatment merely drawing *attention* to it and the banks' social objective. Another possibility is that treatment group default rates rose because of a monitoring

⁴ In this sense our study adds to the literatures on biases in expert judgment (e.g., Soll, Milkman, and Payne 2015) and on the economics of discrimination in product markets and credit markets in particular (e.g., Blanchflower, Levine, and Zimmerman 2003; Hanson et al. 2016).

failure: perhaps loan officers failed to anticipate the challenges of managing more borrowers (12 more in T vs C, SE=11).

All told, this study contributes to the above-cited literatures on multi-tasking, poverty targeting, social enterprise, and CSR. Our main innovation is bringing together these related strands of work by considering a policy that asks front-line employees to pursue both profit and social welfare objectives.

II. Setting

We developed and conducted the poverty targeting experiment in close cooperation with the senior management and microlending operations of two longstanding, family-owned-and-operated, for-profit banks. First Macro Bank (FMB) has eight branches in Metro Manila (serving mostly peri-urban areas) and FICO Bank has twelve branches in Northern Luzon (a more rural region). Both banks offer a range of products and services, with microlending comprising only a small fraction of the portfolio.⁵

Each bank touts improving social welfare as a key objective. Taking two examples from our study period, FICO's website stated: "[we] believe in the noble cause of community banking... [the] bank is supportive of the economic ascendancy of the greatest number". Similarly, FMB's website stated that it was founded to help improve the quality of the lives of the poor and "commit[ted] to the development of clients". During our study FMB received subsidized technical assistance from a USAID-funded program to streamline its microlending processes with the objective of bringing in more low-income borrowers (this is commonplace: Cull and Morduch (2017) find that for-profit microlenders obtain more subsidies than do non-profits).

Microcredit's small loan sizes make it a natural focal point for a poverty targeting effort. FICO and FMB offer individual liability loans with terms and targeting that are in line with their many competitors. Loan amounts range from 5,000 pesos to 50,000 pesos (45 pesos \approx 1 USD during our experiment). Repayments are amortized over a 3- to 6-month maturity and due weekly. Annual percentage rates are around 60% (and, given low inflation, approximate real rates). Applicants must have an existing business, be between the ages of 18 and 65, and demonstrate sufficient cash flow to service a new loan.

Senior management at both banks view microlending as a fulcrum of their social missions and the natural starting point for expanding financial inclusion (social mission) and expanding the bank's customer base (traditional bottom line). Yet loan officers face no quantitative directives or incentives on how to implement the social mission; indeed, the poverty targeting tool the banks implemented for the study was the most tangible effort to-date to translate exhortation into a concrete action: bringing in more poor clients. Loan officers face clearer incentives on the traditional side of the bottom line, with an incentive bonus based on meeting quantitative monthly targets for portfolio at risk (PAR) and new originations.

⁵ At the time of the experiment FICO had approximately 26,000 microcredit clients and FMB 2,700.

Our loan officers thus face a difficult problem: they have front-line responsibility for both sides of the bottom line but face quantitative incentives and directives only on the traditional side. There may well be tension between profit maximization and bringing in more poor clients. If poor clients are worse credit risks, or perceived to be worse by officers, they could jeopardize loan officers' incentive pay. And if poor clients are difficult to identify—if it is difficult to measure whether a given borrower actually contributes to the bank's objective of expanding lending the poor—then finding poor borrowers could leave less time for other screening and monitoring activities that are key inputs to the traditional bottom line.

The intervention was designed to alleviate tension in the loan officers' juggling act by making it easier to identify poor applicants, signaling the importance of bringing in more clients, and clarifying the bank management's belief that credit risk and poverty status are uncorrelated, conditional on the other applicant characteristics considered in underwriting.

III. Experimental Design

Management at both banks sought to make it easier and more salient for loan officers to bring in more poor applicants. To this end the banks worked with us to design and implement a simple experiment on poverty targeting training. In March 2010 we randomized the population of loan officers from the two banks ($N_L=27$), pairwise at the branch level ($N_B=20$), to one of two groups: Treatment and Control.⁶ The randomization produces twelve treated and fifteen control loan officers, from ten treatment and ten control branches. Table 1 Panel A shows that we cannot reject equality of means for treatment and control branches across the few branch characteristics for which we have data at baseline: poverty headcount in the branch's catchment area, total number of loan officers (including those not included in the experiment because they are not responsible for microloans), and year opened. Panel B shows no evidence of differential loan officer attrition across the two arms. The null effects in Table 1—and below when we estimate treatment effects—are subject to the important caveat that they are imprecisely estimated.

Both treatment and control groups used the same loan application process; specifically, the start of our experiment coincided with the banks changing from paper applications and manual underwriting to electronic and more-automated underwriting. We embedded a widely-used targeting tool, the Poverty Probability Index (PPI), into the new electronic application.⁷ The PPI is comprised of ten simple, country-specific questions used to calculate a poverty likelihood (the Appendix details the questions and scoring for the Philippines).

⁶ We use pairwise randomization due to the small number of branches ($N_B=20$), matching each branch with another branch from the same bank based on the poverty headcount ratio of each branch's catchment area, and then randomizing within each branch pair. We calculate the poverty headcount ratio using the Annual Poverty Indicators Survey produced by the Philippines' National Statistics Office.

⁷ The PPI was developed by the Grameen Foundation in 2006 and is now used by organizations in 45 countries. In 2016, Grameen transferred management of the index to Innovations for Poverty Action (IPA), which then changed the PPI name from the Progress out of Poverty Index to the Poverty Probability Index. The new name better reflects what the index measures, which is poverty likelihood at a point in time.

The treatment was simple and threefold: as part of the training on the new system, treated loan officers received: 1) *Explanation* that the purpose of the ten questions was to make it easier to identify and service poor applicants (training for the control loan officers simply referred to these questions as “additional household information); 2) *Exhortation* tying the PPI to the organization’s social mission to help the poor by providing them access to microfinance (the control loan officers received no such exhortation); 3) *Reassurance* that, taking into account the other information required of applicants, poverty status should not impact credit risk and hence that bringing in more poor borrowers should not affect loan officers’ ability to meet their incentive targets for loan performance (the control loan officers received no such reassurance).

IV. Results

We estimate treatment effects of the poverty targeting training on loan officer behavior by regressing an outcome y_i , pertinent to the traditional or social side of the banks’ bottom lines and measured over the 24-months post-random assignment, on a treatment group indicator T_i and our randomization strata δ_k (i.e., our branch-pair fixed effects):

$$y_i = \beta * T_i + \sum_k \delta_k * Pair_k + \varepsilon$$

i indexes loan officers, loan applicants, or loans, depending on the outcome. We always cluster standard errors at the level of randomization: the branch. Because we have a small number of branches (20), we bootstrap our standard errors with 5,000 repetitions (Cameron, Gelbach, and Miller 2008). Table 2 reports treatment effect estimates for various outcomes in column 3, with regression-adjusted means for each outcome in columns 1 and 2 (for control and treatment observations, respectively).

Starting with the social side of the bottom lines, Table 2 Panel A considers application characteristics, measured at the loan officer level. Treated loan officers bring in weakly more loan applicants over the 24 months post-treatment (12, SE=13), but no more poor applicants (-0.04, SE=2.67), leading to a lower proportion of poor applicants (-6pp on a base of 22pp in the control group, SE=2pp). Panel B provides additional support for the inference that the treatment makes the average applicant *less* likely to be poor, estimating negative effects on the raw PPI score and the inferred poverty likelihood at the application level.⁸ Panel C provides further evidence that loan officers use the PPI training to select richer not poor applicants, with variables collected for underwriting purposes but not included in the PPI index indicating higher-income and higher-wealth applicants. Appendix Table 2 shows a similar pattern of results if we consider only approved applications in Panels A-C instead of all applications.

Turning to the traditional side of the bottom line, we note that while treated loan officers bring in weakly more applicants (Panel A) and approved loans (Appendix Table 2 Panel A), an increase in loan volume will increase profitability only if there are fixed costs (which is a fair assumption),

⁸ Appendix Table 1 reports treatment effects on each of the 10 components of the PPI. As expected-- given that the point of the index is to aggregate noisy correlated signals to produce a better estimate than any single component can provide—we do not find any treatment effects with p-values < 0.05.

loan terms do not become less favorable to bank, and loan performance does not deteriorate. Table 2 Panel D examines the latter two assumptions, and shows that loan terms are essentially unchanged: the effect on loan amount is 619 pesos (SE=969) on a base of 17,000. (Other loan terms are essentially fixed per bank policy; e.g., loan officers have little if any discretion over interest rates, repayment frequency, repayment period, collateral requirements, etc.) But the key result in Table 2 Panel D suggests that loan performance *does* deteriorate: the most important measure of portfolio-at-risk (based on the actual performance incentives of loan officers), loan default, increases by 3.4pp (SE=1.7pp) on loans originated by treated loan officers. A reduction in loan performance of this magnitude would almost certainly prevent a loan officer from earning a performance bonus, as bonuses are forfeited if their portfolio at risk is above 5%.

All told, the results in Table 2 suggest that the poverty targeting treatment caused loan officers to miss both social impact and profitability targets.

V. Exploring Mechanisms through an Ex-Post Loan Officer Survey

Bank management and we were surprised by the results in Table 2. To explore how loan officer attitudes and beliefs might moderate and/or drive the results, we fielded a survey approximately two years after the conclusion of our study period (i.e., approximately four years after starting the experiment). Neither bank had made changes to its loan application or scoring system in the interim, with new loan officers using the PPI questions as “additional household information” with no additional training, *a la* our Control Group. Research team staff interviewed the 17 of the 27 loan officers who were part of our experiment and still with their original bank, as well as 41 other loan officers from other parts of the banks. Below we report results for all 68 loan officers, and do not focus on the subsample of loan officers remaining from our experiment, given the high attrition rate and small remaining sample size for that sub-sample.

Table 3 explores how loan officers view their jobs (Column 1), with an eye on the relative importance of the two bottom lines. Tellingly, when asked for their “Most important reason for choosing to work at this company,” only 4 of the 68 selected “Best Opportunity to Work on Local Development/Welfare,” while 93% selected a reason related to their private returns such as “Best Paid,” or “Job Security.” Similarly, when asked to “Name 3 things that you like most about working at this company,” 65% choose “Salary”, and 100% named at least one reason related to private returns, while only 21% choose “I feel I can really help people”. In the same vein, when asked “Do you think your job is more like...?”, 75% choose “Bank work”, while only 9% choose “NGO work” and 16% choose “Both”. The response patterns in this table suggest that loan officers see themselves as bankers first and foremost, valuing job attributes that are hardly unique to social enterprise. Among the 17 loan officers remaining from our experiment, the slant towards the traditional bottom line seems more pronounced if anything (compare Columns 2 and 3 to Column 1).

Table 4 sheds some light on how loan officers map an applicant’s poverty status onto each side of the double bottom line. The survey asks, for each of the ten PPI component questions, “If you learn the following about a borrower how will it change your opinion of the impact a loan would

have on...” (1) “Profitability for the bank”, and (2) “Social welfare for the borrower’s family.” We code “More” responses as 1, “Equal” as zero, and “Less” as -1.

Column (1) suggests that loan officers perceive the three poverty indicators (many children, light wall and roof materials) as being negatively correlated with profitability. Conversely, most of the seven wealth indicators are thought to be positively correlated with profitability. Combining the ten responses per loan officer into a single index by multiplying wealth indicator responses by -1, we infer that on the whole loan officers perceive a strong negative correlation between profitability and poverty: -1.1 (SE=0.29). Table 5 takes this hypothesis to actual data on loan performance and borrower characteristics and finds no evidence to support it, either unconditionally or conditional on credit score. It seems that loan officers have incorrect perceptions.

Table 4 Column (2) suggests that loan officers perceive poorer borrowers as benefiting no more from loans than richer borrowers, and perhaps relatively *less*. The three poverty indicators are thought by the loan officers to be weakly correlated with social welfare (with mean responses indicating basically no correlation), while the seven wealth indicators have a small and positive perceived correlation with social welfare on average. Aggregating the ten responses into a single index, the perceived correlation between poverty and impacts on the borrower is -0.54 (SE=0.29).

In sum, Table 4 suggests that the banks’ loan officers tend to think that bringing in more poor borrowers hurts profitability and does not improve social welfare. This suggests an explanation for the failure of our targeting intervention: 1) treated loan officers shared these perceptions during our study—despite management reassurances and exhortations to the contrary; 2) treated loan officers tried to use the PPI as a credit risk screening tool instead of a poverty targeting tool; 3) this (mis)use of the PPI backfired, because it led loan officers to bring in applicants that actually had greater ex-ante risk (screening failure), and/or because it led loan officers to take on larger portfolios that proved unexpectedly difficult to manage (monitoring failure).

VI. Conclusion

We worked with two for-profit microfinance institutions in the Philippines to implement and test a widely-used poverty targeting tool (the Poverty Probability Index), with the objective of providing more loans to poor households. The PPI consists of ten simple questions and was integrated into the standard loan application at each institution. Loan officer training at control group branches (N=10) simply referred to the tool as “additional household information”. Training at treatment group branches (N=10) featured *explanation* of the questions; *exhortation* to use them to meet the banks’ social missions by bringing in more poor borrowers; and *reassurance* from management that poverty status and loan performance are uncorrelated, conditional on other applicant characteristics.⁹

The treatment group training backfired: it produced no improvement on the social side of the bottom line (bringing in no more poor applicants or borrowers), while harming the traditional side

⁹ This reassurance is empirically validated (Table 5), but the training did not provide any quantitative evidence.

of the bottom line (loan performance deteriorated substantially). Descriptive evidence suggests that the additional training exacerbated loan officer misperceptions and multitasking problems, with loan officers trying and failing to use the poverty measurement tool in pursuit of profit rather than social objectives.

Some important caveats are worth emphasizing. These are surprising findings given the intent of the changed policy, and so from a Bayesian perspective replication is especially important. Even taking our findings at face value, we consider briefly two important dimensions of external validity. First, these results do not imply that PPI is an ineffective targeting tool in general. The PPI may well be effective in the context of a program whose main purpose is reaching and helping the poor. Second, our results do not imply that double-bottom line efforts will always backfire; it is important to keep in mind that the banks in our study provided stronger incentives (and perhaps selected personnel) for the traditional bottom line.

Nonetheless our findings suggest that caution is warranted when entrusting employees to balance two bottom lines. Our results also provide an explanation for why many firms take the balancing act out of front-line employees' hands, by segregating corporate social responsibility (CSR) functions from core activities. But separating CSR from core functions may not be practical, or optimal, in many companies—and perhaps in social enterprises especially.

Hence future work would do well to unpack whether and how front-line employees can successfully juggle both sides of a double-bottom line (or multiple margins of a multi-tasking problem more broadly). Complementary approaches to the methods used in this study include testing different incentive mechanisms, training content, employee recruitment strategies, and/or feedback and workflow management tools; better-timed surveys on employee attitudes and perceptions; and more granular measurement of employee activities. There is much more to learn about the challenges and opportunities of implementing a double-bottom line.

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Table 1: Orthogonality of Branch Characteristics and Account Officer Attrition

	Control	Treatment	Difference
<i>Panel A: Branch Baseline Characteristics</i>			
Average Poverty Headcount Ratio	0.195 N=8	0.229 N=8	0.035 (0.033)
Year Opened	2003.4 N=10	2002.5 N=10	-0.900 (2.240)
Total # of Loan Officers	2.20 N=10	2.10 N=10	-0.100 (0.233)
<i>Panel B: Microloan Officer Attrition</i>			
Average # Months with outcome data (Max=24)	15.73 N=10	17.26 N=10	1.53 (5.79)

* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$. We use pairwise randomization due to the small number of branches (12 at FICO Bank, 8 at First Macro Bank), matching each branch with another branch from the same bank based on the poverty headcount ratio, and then randomizing within each branch pair. Standard errors, in parentheses, are clustered at the branch level and adjusted for our small number of clusters by bootstrapping with 5,000 repetitions per Cameron et al (2008). Poverty ratio is the proportion of households in the branch's catchment area below the 30th percentile of per capita household income, measured using the Annual Poverty Indicators Survey (2004) from the Philippines' National Statistics Office. That survey did not cover the areas served by four FMB branches, and so we matched those four into two pairs based on geographic proximity.

Table 2: Impacts of Poverty Targeting Training

	Control	Treatment	Difference
Outcomes measured over the first 24 months post-random assignment	(1)	(2)	(3)
<i>Panel A: Application Characteristics, Measured at Loan Officer Level</i>			
Total Number of Applications Processed over the 24-month study period	53.24 (6.88)	64.76 (10.30)	11.52 (13.49)
Total Number of Applicants with Poverty Likelihood >49%	11.60 (1.91)	11.56 (2.38)	-0.04 (2.67)
Proportion of Applicants with Poverty Likelihood >49%	0.22 (0.04)	0.16 (0.02)	-0.06** (0.02)
Number of Loan Officers	15	12	27
<i>Panel B: Poverty Indices, Measured at Applicant Level Using the 10 PPI Questions (Appendix Table 1 has question-by-question breakdown)</i>			
Poverty Likelihood	0.30 (0.01)	0.26 (0.01)	-0.05*** (0.01)
PPI Score	47.10 (0.58)	49.77 (0.52)	2.67*** (1.08)
Number of microloan applicants over the 24-month study period	679	754	1433
<i>Panel C: Wealth Variables Not Used in Poverty Indices, Measured at Applicant Level</i>			
Monthly Income (Inverse Hyperbolic Sine)	8.97 (0.05)	9.32 (0.04)	0.36** (0.10)
Total Assets (Inverse Hyperbolic Sine)	10.74 (0.09)	11.28 (0.05)	0.54** (0.15)
Number of Businesses	0.92 (0.01)	1.00 (0.02)	0.08 (0.05)
Owns Home	0.65 (0.02)	0.69 (0.01)	0.05 (0.04)
Number of microloan applicants over the 24-month study period	679	754	1433
<i>Panel D: Microloan Characteristics, Measured at Loan Level</i>			
Loan Amount	17400 (550)	18019 (536)	619 (969)
Loan in Default at end of our study period	0.122 (0.015)	0.156 (0.016)	0.034** (0.017)
Number of microloans originated over the 24-month study period	452	527	979

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each row reports regression-adjusted means (columns 1 and 2), and the treatment effect (column 3) estimated by regressing the outcome on the treatment indicator and strata (branch-pair) fixed effects. Standard errors, in parentheses, are clustered at the branch level and adjusted for our small number of clusters by bootstrapping with 5,000 repetitions per Cameron et al (2008). Panel C variables are collected routinely on loan applications, just like the PPI variables used to construct the poverty indices in Panel B (see the Appendix for details on index construction). Sample size drops from Panels B and C to Panel D because some approved applicants do not avail a loan. Besides loan amount, we do not report other loan terms (e.g., interest rate, maturity) because bank policies allow for little to no variation in those terms. Exchange rate during study period was 45 pesos \approx 1 USD.

**Table 3. Loan Officer Attitudes,
Elicited from All Loan Officers Employed by Partner Banks Four Years Post-Experiment**

	<i>All loan officers</i>		<i>Loan officers in</i>	
	<i>at the two banks</i>		<i>our experiment</i>	
	<i>Full Sample</i>	<i>Control</i>	<i>Treatment</i>	
	(1)	(2)	(3)	
Most important reason for choosing to work at this company				
Compensation/Private Return	93%	100%	89%	
Local Development/Social Welfare	4%	0%	0%	
Other*	3%	0%	11%	
Name 3 things that you like most about working at this company				
Compensation/Private Return Items				
Good salary	65%	75%	67%	
Reasonable working hours	29%	13%	11%	
My superiors are very accommodating	12%	13%	11%	
Significant chances for promotion	22%	25%	56%	
It will help me find a better job	4%	13%	0%	
Job tenure security	12%	0%	11%	
Building own human capital	22%	25%	11%	
Convenience of job	22%	25%	11%	
Enjoy their co-workers	40%	38%	56%	
Local Development/Social Welfare Items				
I feel I can really help people	21%	13%	11%	
Do you think your job is more like... ?				
Bank work	75%	100%	78%	
NGO work	9%	0%	0%	
It is like both	16%	0%	22%	
Observations: Number of Loan Officers Surveyed	68	8	9	

Data is from survey administered 4 years post-random assignment, to all loan officers working for the two banks at the time, not just microloan officers. 17 of the 27 loan officers included in our experiment (see Tables 1 and 2) were still working for the banks at the time of this survey and hence are included in this sample. Aside from the observation count, each cell reports the proportion of all 68 respondents giving the response described in the row label. Private *One individual responded that the bank was their first choice but did not provide a reason why, another said they chose their job because it is "respected work".

Table 4. Perceived Correlations between Loan Outcomes and Poverty, Elicited in a Loan Officer Survey Taken Four Years Post-Random Assignment

<i>If you learn the following about a borrower how will it change your opinion of impact the loan on...</i>	<i>Profitability for Bank</i>	<i>Social welfare for borrower's family</i>
	<i>More = 1; Equal = 0; Less = -1</i>	<i>More = 1; Equal = 0; Less = -1</i>
	(1)	(2)
<i>Poverty Related Components</i>		
There are >=3 children in the family that are aged 0-14	-0.176	0.044
House outer walls are made of light materials	-0.176	-0.015
House roof is made of light materials	-0.265	-0.074
<i>Wealth Related Components</i>		
All children in the family ages 6-14 go to school	-0.309	-0.147
Female head/spouse is a high school graduate	-0.235	-0.176
Other family members have salaried employment	0.632	0.485
Toilet Facility is water sealed	0.074	0.044
Family owns refrigerator	0.118	0.147
Family owns television set	0.088	0.088
Family owns washing machine	0.118	0.059
Index of above components (Negative weight on wealth components)	-1.100*** (0.287)	-0.544* (0.285)
Observations	68	68

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data is from survey administered 4 years post-random assignment, to *all* loan officers working for the two banks at the time, not just microloan officers. 17 of the 27 loan officers included in our experiment (see Tables 1 and 2) were still working for the banks at the time of this survey and hence are included in this sample. Aside from the observation count, each cell reports the mean response across all 68 respondents. Standard errors are in parentheses and clustered at the branch level.

Table 5. Empirical correlation between loan profitability & poverty scores

Outcome Variable: Loan Default	(1)	(2)	(3)	(4)
PPI Score	-0.0002 (0.0009)	-0.0002 (0.0009)		
Poverty Likelihood			-0.0114 (0.0496)	-0.0093 (0.0498)
Controlling for Credit Score	N	Y	N	Y
Number of Loans	979	979	979	979

Each column presents results from an OLS regression of an indicator (1= loan is in default at the end of our study period) on the variable(s) described in the rows. Standard errors, in parentheses, are clustered at the branch level and adjusted for our small number of clusters by bootstrapping with 5,000 repetitions per Cameron et al (2008).

Appendix Table 1. Impacts of Poverty Targeting Training on Poverty Index Components of Applicants Brought in by Loan Officers (see Table 2 Panel B for analogous result on poverty indices)

	Control	Treatment	Difference
Characteristics of microloan applicants during the first 24 months post-random assignment	(1)	(2)	(3)
How many in the family are aged 0-14?	3.15 (0.08)	3.43 (0.08)	0.28 (0.10)
Do all children in the family of ages 6-14 go to school?	0.39 (0.02)	0.28 (0.02)	-0.11 (0.07)
What is the education level of the female head/spouse?	3.51 (0.03)	3.45 (0.03)	-0.06 (0.07)
Do any family members have salaried employment?	0.48 (0.02)	0.57 (0.02)	0.08 (0.04)
Are the house's outer walls made of strong materials?	0.96 (0.01)	0.96 (0.01)	0.00 (0.02)
Are the houses roof made of strong materials?	0.97 (0.01)	0.98 (0.00)	0.01 (0.02)
Does the family own a closed toilet?	-0.98 (-0.01)	-0.94 (-0.01)	0.03 (0.04)
Does the family own a refrigerator?	0.91 (0.01)	0.87 (0.01)	-0.04* (0.01)
How many television sets does the family own?	1.37 (0.02)	1.38 (0.02)	0.02 (0.06)
Does the family own a washing machine?	1.37 (0.02)	1.39 (0.01)	0.02 (0.03)
Number of clients	679	754	1433

* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$. Each row reports regression-adjusted means (columns 1 and 2) and the treatment effect (column 3) estimated by regressing the outcome on the treatment indicator and strata (branch-pair) fixed effects. Standard errors, in parentheses, are clustered at the branch level and adjusted for our small number of clusters by bootstrapping with 5,000 repetitions per Cameron et al (2008). Variables here are used to construct the poverty indices in Table 2 Panel B (see the Appendix for details on index construction).

Appendix Table 2: Impacts of Poverty Targeting Training on Approved Applicants
(Compare to Table 2 Panels A-C, which include *all* applicants)

	Control	Treatment	Difference
Outcomes measured over the first 24 months post-random assignment	(1)	(2)	(3)
<i>Panel A: Application Characteristics, Measured at Loan Officer Level</i>			
Total Number of Applications Processed over the 24-month study period	38.80 (5.69)	50.51 (9.88)	11.71 (11.39)
Total Number of Applicants with Poverty Likelihood >49%	10.00 (1.66)	10.05 (2.29)	0.05 (2.11)
Proportion of Applicants with Poverty Likelihood >49%	0.25 (0.04)	0.21 (0.03)	-0.04** (0.01)
Number of Loan Officers	15	12	27
<i>Panel B: Poverty Indices, Measured at Applicant Level Using the 10 PPI Questions</i>			
Poverty Likelihood	0.29 (0.01)	0.25 (0.01)	-0.04*** (0.01)
PPI Score	47.94 (0.62)	50.59 (0.55)	2.65*** (0.86)
Number of microloan approvals over the 24-month study period	582	669	1251
<i>Panel C: Wealth Variables Not Used in Poverty Indices, Measured at Applicant Level</i>			
Monthly Income (Inverse Hyperbolic Sine)	8.94 (0.58)	9.23 (0.04)	0.29 (0.10)
Total Assets (Inverse Hyperbolic Sine)	10.76 (0.10)	11.31 (0.05)	0.55** (0.15)
Number of Businesses	0.92 (0.01)	1.00 (0.02)	0.08 (0.05)
Owns Home	0.62 (0.02)	0.68 (0.01)	0.07 (0.03)
Number of microloan approvals over the 24-month study period	582	669	1251

* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$. Each row reports regression-adjusted means (columns 1 and 2), and the treatment effect (column 3) estimated by regressing the outcome on the treatment indicator and strata (branch-pair) fixed effects. Standard errors, in parentheses, are clustered at the branch level and adjusted for our small number of clusters by bootstrapping with 5,000 repetitions per Cameron et al (2008).

Appendix: Poverty Probability Index Construction

(1) How many in the family are aged 0-14?

- a. 1 (0 Points)
- b. 2 (4 Points)
- c. 3 (9 Points)
- d. 4 (15 Points)
- e. 5+ (20 Points)
- f. 0 (26 Points)

(2) Do all children in the family of ages 6 to 14 go to school?

- a. No (0 Points)
- b. Yes (2 Points)
- c. No one aged 6 to 14 (4 Points)

(3) What is the education level of the female head/spouse?

- a. Elementary or less (0 Points)
- b. First to fourth year secondary (3 Points)
- c. Graduate Secondary (6 Points)
- d. First year college or higher (11 Points)
- e. No female head (-9 Points)

(4) Do any family members have salaried employment?

- a. Yes (0 Points)
- b. No (5 Points)

(5) What are the house's outer walls made of?;

- a. Light Materials (0 Points)
- b. Strong Materials (4 Points)

(6) What is the house's rood made of?;

- a. Light Materials (0 Points)
- b. Strong Materials (2 Points)

(7) What kind of toilet facility does the family own?;

- a. None, pit, other (0 Points)
- b. Water Sealed (7 Points)

(8) Does the family own a refrigerator?;

- a. Yes (10 Points)
- b. No (0 Points)

(9) How many television sets does the family own?;

- a. None (0 Points)
- b. One (6 Points)
- c. Two or More (21 Points)

(10) Does the family own a washing machine?

- a. Yes (10 Points)
- b. No (0 Points)

PPI Score	Below Poverty Line	PPI Score	Below Poverty Line
0-5	96.6%	50-54	14.8%
5-9	93.7%	55-59	7.2%
10-14	91.5%	60-64	5.0%
15-19	87.8%	65-69	3.2%
20-24	80.9%	70-74	1.4%
25-29	68.5%	75-79	1.4%
30-34	59.6%	80-84	0.0%
35-39	48.9%	85-89	0.0%
40-44	36.8%	90-94	1.5%
45-49	21.1%	95-100	0.0%