

A Methodological Note on Using Loan Application and Survey Data  
to Measure Poverty and Loan Uses of Microcredit Clients\*

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“[We provide] financial services to the world's lowest-income entrepreneurs so they can create jobs, build assets and improve their standard of living. We target the poorest of the working poor...”

*-Quote from website of leading microcredit organization*

## **Introduction**

Policymakers place increasing emphasis on expanding outreach to poorer (potential) entrepreneurs, and microfinance institutions (MFIs) often claim to target poor entrepreneurs who then invest loan proceeds in growing their businesses. Typically these claims are evaluated using readily available but unverified self-reports from client loan applications. We examine whether MFIs and third-parties can rely on client self-reports to make inferences about poverty status and loan usage.

If there is any incentive to lie, self-reports from clients to MFIs are likely to be biased in whichever direction serves the interests of the clients. Even if clients are assured that their answers will not affect their loan eligibility, respondents may lie if they do not trust the surveyors' (or loan officers') guarantees or if they wish to project a socially desirable image.

We report on two mini-studies that are both part of larger ongoing studies. The first study, from Peru, examines the “use of funds” question. Here we conduct a simple test, employing a technique called “list randomization” (explained below), to assess whether individuals feel compelled to underreport using loan proceeds for consumption, rather than investment. The presumption is that if individuals underreport using funds for consumption *to an independent surveyor*, then they will likely also underreport the same if asked by a lender, when the lender is emphasizing using loans for entrepreneurial purposes. The second study is simpler: we assess whether individuals report higher economic wellbeing to a Mexican microlender than they do to an independent surveyor.

## **Design, Data, and Results**

### *List Randomization - Araiwa MFI, Peru*

Acknowledging concerns about the validity of responses to sensitive survey questions, researchers in the social sciences have developed a variety of techniques that attempt to elicit truthful responses. One approach includes direct methods such as matching the gender of surveyors and respondents, using forgiving language, using unfolding financial brackets, and collecting data in private. A second approach involves using indirect methods such as the bogus pipeline, the randomized response technique, and the list randomization technique.<sup>1</sup>

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<sup>1</sup> The randomized response technique was first developed in 1965 by Stanley Warner as a process in which a randomizing device such as a spinner would select one of two statements about a sensitive topic. The spinner would select one statement with known probability  $p$  and the other statement with probability  $1-p$ . The respondent would then inform the surveyor whether or not she agreed with the selected statement, without disclosing which statement

List randomization provides a simple way for respondents to report on sensitive behavior without allowing the researcher or surveyors to identify individual responses. To employ this technique, half of the survey respondents are randomly selected to receive a short list of statements (in our case a list of business investments) and asked to report how many, but not which, statements are true. The other half of the survey respondents are presented with the same list of statements and one key additional statement (in our case non-business investment or type of consumption). By subtracting the mean number of true statements in the first group from the mean number of true statements in the second group, researchers can identify the proportion of the sample that engages in the key behavior.

Several studies suggest that the randomized list technique can yield more accurate responses to sensitive survey questions. Across 48 comparisons of direct report and list randomization, one meta-analysis found that 63% of the estimates for socially undesirable behavior were significantly larger when elicited through list randomization (Holbrook and Krosnick 2009). A more limited meta-analysis found that while the list randomization estimates of socially undesirable behavior were generally larger, the overall difference was not significant (Tourangeau 2007). Some studies have more precisely estimated the effectiveness of the technique by comparing direct report to list randomization for both sensitive and non-sensitive questions. Tsuchiya et al. (2007) finds that the technique results in a significantly higher proportion of a sample admitting to shoplifting, whereas the difference between methods in estimates of blood donation is insignificant. Similarly, LaBrie and Earleywine (2000) finds that list randomization results in a higher proportion of undergraduate students admitting to having unprotected sex, whereas there was no significant difference for drinking alcohol, which presumably has less stigma.

Our first mini-study uses a comparison of list randomization to direct reports to estimate underreporting of the use of microloans for non-business purposes. Prior to evaluating the use of video and radio as a means for financial education, 1650 MFI clients were surveyed in Cuzco, Peru. The lending institution, Arariwa, provides microcredit to approximately 20,000 low-income households in southeastern Peru. As part of the baseline survey, Arariwa clients were asked questions related to their personal finances and education. Surveyors were not affiliated with any MFI and informed survey respondents that their responses would not be shared with anyone other than researchers studying how entrepreneurs that are Arariwa clients manage their household finances.

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was selected by the spinner. Other methods include the unrelated question technique, the forced alternative technique, and the bogus pipeline technique. In the unrelated question technique, respondents are asked to answer "yes" or "no" to one of two randomly selected questions: the sensitive question or a question with a known probability of a "yes" answer. In the forced alternative technique, the respondent is presented with a sensitive question and then uses a randomizing device to determine whether to respond "yes", "no", or to present her true response. The bogus pipeline technique tells respondents they are being monitored by a lie detector.

All respondents were asked to report their loan uses through direct report and list randomization techniques. For the direct report, respondents were asked to list up to five loans that they had taken out in the past 12 months, by loan source and amount. They were then asked, “Which need or which needs did you cover with this loan?” and allowed to list up to three uses for each loan. Though respondents were not prompted with categories, surveyors matched uses against one of 18 possibilities (Table 1). After eight more questions related to personal finances, respondents were presented with the list randomization module.

Prior to beginning the list randomization, surveyors were instructed to demonstrate the technique using an example. Surveyors were provided with five innocuous statements printed on a piece of paper with a clear clipboard placed over the sheet. Respondents were handed the clipboards and asked to use a white board marker to put check marks next to statements that are true for them. Next, respondents were instructed to count the number of true statements before erasing their check marks, returning the clipboard, and reporting the total count. After confirming that the clients understood the anonymity ensured by the process, surveyors began the list randomization module.

Clients were randomly selected to be presented with one of four possible groups of three to six statements. All clients received the following three statements: “I used part of my Arariwa loan to buy *merchandise* for my economic activity”, “I used part of my Arariwa loan to buy *equipment* for my economic activity” and “I *shared* my loan with another person”. Clients in group A (n=408) only received these statements. Clients in group B (n=414) additionally received the following statement: “I used at least a quarter of my Arariwa loan on *household items*, such as food, a TV, a radio, etc.” Group C (n=388) received the four previous statements in addition to, “I used at least a quarter of my Arariwa loan to pay for my family’s *medical expenses*.” Group D (n=401) received the previous five statements and the statement, “I used at least a quarter of my Arariwa loan to pay for my family’s *educational expenses*.” By subtracting the mean number of true statements for group A from the mean number of true statements for group B, we get the proportion of clients that used a quarter of their loan for household items. We similarly subtract B from C and C from D to get the proportions of clients using their loans on education or medical expenses.

In order to compare estimates, we match the loan uses from direct report to those from list randomization. Since the direct report question allows clients to list up to five loans from any source, we limit the sample to only include Arariwa loans or communal loans facilitated by Arariwa.<sup>2</sup> Due to cultural norms and surveyor training, “household items” is best approximated by the direct report responses that are classified as “consumption good”, “purchase clothing or shoes”, and “other consumption need.”

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<sup>2</sup> In piloting the survey, clients did not seem to differentiate between loans directly from Arariwa and loans from the savings accumulated by peers in village banks organized by Arariwa.

Table 1:  
Loan Uses from Direct Response Question  
from ICT Financial Literacy Project in Peru

Use	Mean	Standard Error
<b>Use, by Category</b>		
Any Production [Responses (1) - (8) or (-666)]	0.758	0.011
Any Consumption [Responses (9) - (17) or (-667)]	0.300	0.011
Household Item [Responses (13), (15), or (-667)]	0.077	0.007
<b>Use, by Specific Response</b>		
(1) Purchase land	0.022	0.004
(2) Purchase equipment	0.068	0.006
(3) Agricultural inputs (fertilizer, pesticide, etc)	0.051	0.005
(4) Purchase animals	0.179	0.009
(5) Animal husbandry inputs (fodder, medicines, etc)	0.021	0.004
(6) Raw materials	0.090	0.007
(7) Purchase merchandise	0.411	0.012
(8) Purchase of assets to enable a shop or office	0.021	0.004
(9) Education	0.072	0.006
(10) Health	0.022	0.004
(11) Ceremonies(weddings, funerals, etc)	0.004	0.002
(12) Purchase of vehicles	0.020	0.003
(13) Consumption goods	0.052	0.005
(14) To pay off another loan	0.042	0.005
(15) Purchase clothing and shoes	0.008	0.002
(16) Travel	0.008	0.002
(17) Home improvement	0.067	0.006
(-666) Other productive need	0.061	0.006
(-667) Other consumption need	0.020	0.003

N = 1650. An individual use = 1 if it is listed as any of three possible uses across any of five possible loans. Only loans identified as "Loan from Arariwa" or "Loan from Communal Bank (facilitated by Arariwa)" are included.

Table 2 demonstrates a striking contrast in results yielded through direct questioning and those yielded through list randomization. Direct questioning resulted in only 7.7% of the sample volunteering household items as a use for any of their Arariwa or Arariwa-facilitated loans. In comparison, the list randomization technique suggests that 31.3% of the sample used at least a quarter of their Arariwa loans on household items. Similarly, 2.2% of the sample volunteered a health related loan use through direct report, whereas list randomization resulted in an estimate of 23.1% of the sample using at least a quarter of their loan amounts on medical expenses. Finally, the proportion for clients using loans for educational expenses is 7.1% through direct questioning, but 33.2% through list randomization. Z-tests of proportions indicate that each of these three differences is statistically significant.

There are several reasons why list randomization might produce such different, and higher, estimates of loan uses than direct report. Another issue is whether asking clients to do direct report first and list randomization second biases the results. Future research could test this by randomizing the order of direct report and lists. Another issue is whether list randomization reduces lying, and/or facilitates recall. Future research could test this by comparing direct reports versus list randomization on topics not likely to be sensitive (e.g., asking about using microloan proceeds for business expenses), and/or by testing how prompting specific categories changes responses in direct elicitation.

Table 2:  
Comparison of Direct Report and List Randomization Estimates  
from ICT Financial Literacy Project in Peru

	Loan Use:	Household Items (1)	Health (2)	Education (3)
<b>Direct Report</b>				
Proportion reporting this use		0.077	0.022	0.072
	SE	(0.007)	(0.004)	(0.006)
	N	1650	1650	1650
<b>List Randomization</b>				
Mean of "Yes" Responses for Short List		1.213	1.527	1.758
	SE	(0.031)	(0.038)	(0.049)
	N	408	414	388
Mean of "Yes" Responses for Long List		1.527	1.758	2.090
	SE	(0.038)	(0.049)	(0.055)
	N	414	388	401
Difference (Proportion reporting this use)		0.313	0.231	0.332
	SE of Difference	(0.049)	(0.062)	(0.074)
	p-value from ttest	0.000	0.000	0.000
	N	822	802	789
<b>Comparison of Direct Report &amp; List Randomization</b>				
List Randomization minus Direct Report		0.236***	0.209***	0.261***
Z-test statistic for difference in proportions		4.752	3.386	3.512

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors in parentheses. Direct report question allows up to three uses to be reported for each of five loans. Only loans directly from Arariwa or facilitated by Arariwa are included. "Household items" question from list randomization is matched to the following direct report options: "consumption goods", "purchase clothing or shoes", and "other consumption need". List randomization questions required that over 1/4 of the loan was used for the specified purpose, whereas the direct report question did not have a lower bound on proportion of loan used.

### *Poverty Scoring – Mexico MFI, Compartamos*

Our second mini-study explores whether microlender clients report their economic status differently to a surveyor than to a lender. The analysis compares the responses of 89 households in Nogales, Agua Prieta and Caborca, Mexico who completed both loan applications with Compartamos Banco (a for-profit Mexican MFI), and surveys with a survey firm.

The survey covered a wide range of questions on demographics, business, and household finances. Surveyors were not affiliated with Compartamos and were instructed to inform survey respondents that their responses would not be shared with anyone other than researchers studying the socioeconomic status of women in Sonora, Mexico. However, we discovered ex-post that at least one surveyor told respondents that the survey was related to a bank. We believe this should bias the results towards convergence between the administrative and survey sources; nevertheless we find some significant differences between the two sources.

Compartamos collects socioeconomic data to report targeting and outreach data to the global microfinance community. The data are collected in borrowing group meetings prior to authorizing the disbursement of joint liability loans. Loan officers ask each borrower a short series of questions on household income sources, the number of children and dependents, homeownership, and housing quality. Responses to these questions are not validated by the loan officer with a house visit and officially have no bearing on the applicant's eligibility for a loan. Indeed, casual observation and conversations with bank staff suggest that loan officers often fill out the application with more attention paid to filling out the form completely, as the bank has strict rules regarding its documentation, rather than on the content of the responses.

We compare four key variables that were collected using identical questions and elicitation methods (respondent self-reports rather than enumerator observations) from both data sources. These variables are often used as inputs to a "poverty scorecard" that is used as a targeting metric for social programs and/or MFIs.

Table 3 shows statistically significant differences between survey and MFI data in two of the four poverty score inputs into the poverty score. Survey responses show lower levels of piped water (12% lack piped water versus 5%) and telephones in the home in the survey (31% lack a phone, versus 1%). We believe the telephone response may be biased for both measures: individuals may underreport having a telephone to a surveyor if they fear it will encumber them with being asked to do more surveys in the future, and individuals may overreport having a telephone to the bank if they feel they must have a phone number in order to convince the lender that they are reachable (in which case they may provide their relative, neighbor or friend's cellphone number as if it were their own). We do not find significant differences in the use of low-quality cooking fuel, or in the dwelling's floor material.

An important question for future research is whether the survey or MFI data produces more accurate measures of poverty. The main concern with MFI data is that respondents may exaggerate their resources in order to appear more creditworthy to loan officers (although, as noted above, in practice loan officers have little influence on loan terms or administration). The main concern with survey data on poverty is that, in many countries, surveys are used to determine program eligibility (e.g., Progesa). So surveys may lead respondents to understate their resources. Future research could test this by comparing survey vs. administrative data among populations that are not plausibly eligible for social programs, and/or among populations where eligibility is determined using methods other than household surveys.

## **Conclusion**

Data on the loan uses and poverty status of (potential) microfinance clients are important inputs into business strategy and policy evaluation. We have highlighted some challenges in eliciting accurate measures, presented some evidence suggesting that data collected by different methods produces different inferences, and highlighted several directions for further research.

On a substantive, policy level, we learn two lessons. First, we document potential bias in lender-collected poverty measures. Clearly the bias could go in either direction: if the lender screens on creditworthiness, one is likely to observe upward bias income, assets, cash flow, etc. Likewise, (although not observed in our setting), if a lender specifically targets the poor, one might expect a bias toward finding more poverty in the lender-collected data.

Second, we learn about biases in client self-reports on the use of microcredit loan proceeds. The MFI community often claims and advertises a strict focus on enterprise investment. Here we find evidence of substantial consumption uses. More to the point, we find that microcredit clients overreport enterprise investments and underreport consumption uses *even to an independent surveyor*. If clients mispresent loan uses to an independent surveyor, it stands to reason that client reports to MFIs may be inaccurate as well.

Note that even accurate self-reports on loan uses have their limitations. Money is fungible, and hence observing the mechanical deployment of loan proceeds does not identify answers to what are typically the greater questions of interest: how does credit access change actual expenditures shortly after loan disbursement, whether on investment or consumption goods? Identifying such impacts requires data on a valid comparison group of would-be borrowers that did not get a loan for some exogenous reason.

Table 3: Comparison of Poverty Scores from Household Survey and Loan Application Data Banco Compartamos in Mexico				
	Baseline Survey (1)	Compartamos Loan Application (2)	Difference between (1) and (2) (3)	P-value from t- test for (1) = (2) (4)
<b>Poverty Score Inputs</b>				
Floor material in household				
Earth (mean)	0.022	0.022	0.000	1.000
SE	(0.016)	(0.016)	(0.023)	
Cement (mean)	0.854	0.843	0.011	0.836
SE	(0.038)	(0.039)	(0.054)	
Other (mean)	0.124	0.135	-0.011	0.820
SE	(0.035)	(0.036)	(0.049)	
N	89	89		
Does household have piped water?				
No (mean)	0.124	0.045	0.079	0.034**
SE	(0.045)	(0.022)	(0.037)	
N	89	89		
What type of cooking fuel is used? (Wood or Other)				
Wood (mean)	0.022	0.000	0.022	0.159
SE	(0.016)	(0.000)	(0.016)	
N	89.000	89.000		
Does household have telephone (fixed line or cellular)?				
No (mean)	0.315	0.011	0.303	0.000***
SE	(0.050)	(0.011)		
Yes (mean)	0.674	0.989	-0.315	0.000***
SE	(0.050)	(0.011)	(0.050)	
Doesn't know (mean)	0.011			
SE	(0.011)			
N	89	89		

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors in parentheses.

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