Follow the Money not the Cash: Methods for Identifying Consumption and Investment Responses to a Liquidity Shock

Dean Karlan, Adam Osman and Jonathan Zinman*

July 2014

Abstract

Measuring the impacts of liquidity shocks on spending is difficult methodologically but important for theory, practice, and policy. We tackle this methodological question by identifying counterfactual spending-- spending that would not have occurred sans loan-- using random assignment of microloan approvals combined with a short-run follow-up survey on major household, loan repayment and business cash outflows. This yields an estimate that about 100% of loan-financed spending is on business inventory. We also examine whether several other, purely survey-based methods for measuring the impacts of liquidity shocks deliver the same result, and find that borrowers answer by following the cash; i.e., borrowers likely report what they physically did with cash proceeds, rather than counterfactual spending.

Keywords: loan use; consumption; investment; liquidity constraint; liquidity shock; fungibility; microcredit; microenterprise

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

* Contact information: Dean Karlan, dean.karlan@yale.edu, Yale University, IPA, J-PAL, and NBER; Adam Osman, aosman@illinois.edu, University Illinois at Urbana-Champaign; Jonathan Zinman, jzinman@dartmouth.edu, Dartmouth College, IPA, J-PAL, and NBER, Approval from the Yale University Human Subjects Committee, IRB0510000752 and from the Innovations for Poverty Action Human Subjects Committee, IRB #07October-002. The authors thank financial support from the Bill and Melinda Gates Foundation, the Consultative Group for Assistance to the Poor (CGAP) and AusAID. The authors thank Kareem Haggag, Romina Kazadjian, Megan McGuire, Faith McCollister, Mark Miller, and Sarah Oberst at Innovations for Poverty Action for project management and field support throughout the project, and the senior management and staff at First Macro Bank and FICO Bank for their support and collaboration throughout this project. The authors retained full intellectual freedom to report and interpret the results throughout the study. All errors and opinions are those of the authors.

I. Introduction

What are the impacts of liquidity shocks on the consumption and investment decisions of households and small businesses? Answers to this question have implications for the theory, practice, and regulation of credit, as well as for modeling intertemporal consumer choice. They shed light on perceived returns to investment, and on the extent to which constraints bind more for some types of household spending than others. Estimating impacts of liquidity shocks matters in many domains, for example in understanding household leveraging and deleveraging decisions in the wake of credit supply shocks,[1] as well as evaluating interventions such as business grants,[2] unconditional cash transfers,[3] and microcredit expansions.[4]

Papers that track responses to liquidity shocks often focus on estimating medium- and long-term effects by measuring spending patterns, balance sheets, or summary statistics of financial conditions several months or years post-shock[5]. This reduced-form evidence has proven quite useful, but it often leaves the mechanism underlying any change unidentified. For each possible state of the world many months post-liquidity shock -- high enterprise growth relative to baseline, low enterprise growth, consumption growth, etc. -- there are many paths from the liquidity change to that outcome. Identifying mechanisms is important because different paths can have different welfare implications.

To take an example closest to the setting we examine in this paper, many microcredit impact evaluations do not find significant effects of microcredit on enterprise scale or profitability one or two years post-intervention, even when the loans are targeted to those who are microentrepreneurs at baseline.[6] There are at least three possible explanations for these findings: (1) impacts only materialize over longer horizons due to compounded benefits, adjustment, etc. This hypothesis often motivates researchers and program advocates to highlight the value of longer-term outcome data; (2) microentrepreneurs do *not* actually invest marginal liquidity in their businesses, perhaps because they are credit constrained on the margin and have household investment or consumption smoothing with a higher expected return on investment (in utility terms) than business investment; (3) microentrepreneurs *do* invest microloan proceeds in their businesses, but these investments do not end up earning a positive net return.

---

[1] See e.g. Hall (2011), Eggertsson and Krugman (2012), and Mian and Sufi (2012).

[2] See e.g. Fafchamps et al (2013), Karlan, Knight and Udry (2013), and de Mel, McKenzie and Woodruff (2008).

[3] See e.g. Benhassine et al (2013), Blattman, Fiala and Martinez (2012), Haushofer and Shapiro (2013), Karlan et al. (2013).

[4] See e.g. Angelucci, Karlan and Zinman (2013), Attanasio et al (2011) , Augsburg et al (2012), Banerjee et al (2013), Crepon et al (2011), Karlan and Zinman (2010), Karlan and Zinman (2011), and Tarozzi, Desai and Johnson (2013).

[5] See e.g. Agarwal, Liu, and Souleles (2007), Johnson, Parker, and Souleles (2006), Parker et al. (2013), Souleles (1999), Souleles (2002)

[6] See the studies cited in the previous footnote, with the exception of Karlan and Zinman (2010), which examines untargeted consumer loans.

The second and third explanations highlight the potential value of "following the money" from liquidity to spending decisions to reveal mechanisms underlying the paths from shock to outcomes. If the second explanation is accurate that motivates further attempts to identify causes, consequences, and cures for credit constraints. If the third explanation is accurate that motivates further attempts to understand why entrepreneurs make investments that, ex-post at least, do not yield a positive net return on average (Moskowitz and Vissing-Jorgensen 2002; Anagol, Etang, and Karlan 2013; Karlan, Knight, and Udry 2013).[7]

To take another example, Mian and Sufi (2011) find that borrowing against rising home values by existing homeowners drove a significant fraction of both the rise in U.S. household leverage from 2002 to 2006 and the increase in mortgage defaults from 2006 to 2008. How did homeowners deploy the borrowed funds? As the paper explains (p.2134):

> The real effects of the home equity–based borrowing channel depend on what households do with the borrowed money. We find no evidence that borrowing in response to increased house prices is used to purchase new homes or investment properties. In fact, home equity–based borrowing is not used to pay down expensive credit card balances, even for households with a heavy dependence on credit card borrowing. Given the high cost of keeping credit card balances, this result suggests a high marginal private return to borrowed funds.

Knowing what sort of spending generates this high marginal private return would inform how economists specify consumer preferences, expectations, and other inputs into consumer choice models. For example, spend data would help distinguish liquidity constraints from self-control problems as drivers of leveraging, which Mian and Sufi highlight as a fruitful avenue for future research (p.2155).[8]

As both examples suggest, unpacking the mechanisms underlying the long-run effects of a liquidity shock may require data on consumption and investment choices immediately after the shock. If one can follow the money from liquidity shock to spending, it may help identify how households use liquidity to try to improve their lots.

But how exactly one might go about measuring spending in the immediate aftermath of a liquidity shock is not immediately obvious, methodologically speaking. There are several challenges.

Administrative data is rarely available for the right sample, timeframe, or spending frequency, and even more rarely sufficiently comprehensive in its coverage of different types of consumption and investment. This makes survey design very important. Yet money is fungible, and household and (micro)enterprise balance sheets are often complex, so it may be cognitively

[7] Now consider the opposite state of the world: say an evaluation of 12-month impacts does find that a microcredit expansion produces larger, more profitable businesses. The mechanism need not be investment in business assets per se (inventory, physical capital, etc.) Rather, it could be investments in human capital (training, health, child care, etc.) that enable the entrepreneur or business "helpers" from her family to be more productive.

[8] For related inquiries see Bauer et al (2012), and Bhutta and Keys (2013).

difficult for survey respondents to identify the effects of the liquidity shock on their spending, relative to the counterfactual of no shock. Similarly, surveys that simply ask about past purchases produce noisy data, and measurement error increases with the length of the recall period (Nicola, Francesca, and Giné 2012). Moreover, surveys can produce biased rather than merely noisy data if respondents have justification bias,[9] worry about surveyors sharing information with tax authorities or a lender that "requires" loans be used for particular purpose, or feel stigma about using debt for consumption purposes (Karlan and Zinman 2008). In short, data constraints, strategic reporting, and respondent (mis)perceptions may all make it difficult to follow the money.

We address these challenges by comparing results from three different methods for following the money obtained by borrowers subjected to a randomized supply shock from one of two microlenders in Metro Manila or northern Luzon, Philippines. The majority of marginal borrowers (90%) —those close to the banks' credit score cutoffs—were randomly assigned to be offered a loan, while the remaining potential borrowers (10%) were randomly rejected. As is typical in microlending, the loans are targeted to microentrepreneurial investment, and underwritten accordingly, but are not secured by collateral or restricted in their disbursement.

We posit that the most relevant counterfactual—spending that would not have happened in the absence of the marginal loan-- can be identified by comparing a listing of recent expenditures, with no reference to recent borrowing, across the treatment and control groups.[10]

We then compare results from the counterfactual to those obtained from two other, lower-cost methods for following the money that rely on questions about loan usage *per se*. The first set of questions asks directly about (intended) loan usage with questions along the lines of: "Will you/did you spend at least $X of your loan on Y?" We ask these questions using four different enumerator*timing combinations: by bank staff at application and shortly after the disbursal of the loan, and by independent surveyors two weeks and two months after loan disbursal. The second set of questions asks less directly about loan uses, using a "list randomization" technique that makes it feasible for respondents to respond truthfully to sensitive questions without actually revealing details about their behavior (Karlan and Zinman 2012). We include list randomization in the two-week follow-up that is administered by an independent surveyor.

Comparing results across these methods will shed light on several questions. We infer how borrowers believe they should report their loan usage by comparing results across the four direct elicitation enumerator*timing combinations, and across direct elicitation vs. list randomization. We infer how borrowers actually perceive the impact of the loan on their spending decisions

[9] E.g., my business did not grow from last year to this year, so I won't report (to the surveyor, or even perhaps to myself) that I actually did try to grow my business by investing in new assets earlier this year.

[10] The randomization does actually produce a powerful "first-stage": a substantial increase in borrowing for the treatment (loan approved) group relative to the control (loan rejected) group. This result is not surprising, given that Karlan and Zinman (2011) found a similar result with marginal borrowers from one of the same banks considered here.

(versus how they should report it) using the list randomization. And we infer how these perceptions, at least as elicited, differ from the marginal spending of interest to researchers by comparing the results of the list randomization on loan uses to the counterfactual identified using the randomization and spending questions.

Before summarizing the results, we emphasize that our paper is more about comparing different methodological approaches to identifying spending responses than about extrapolating substantive implications from our particular setting. Nonetheless, the results in our setting serve as a useful example of how inferences can be drawn from comparing methodologies.

The pattern of results suggests three key findings in our setting.

First, respondents report strategically. They report very few non-business uses of loan proceeds to the bank when asked directly, significantly more to independent surveyors when asked directly, and yet significantly more to independent surveyors when presented with lists of statements that allow them to report what they perceive to be the truth without directly revealing what they spent.

Second, even when responding (more) truthfully, answers to questions about "did you spend X or more of your loan on…" are different than the counterfactual of greatest interest to economists and policymakers. For example, although 12% of our treatment group implicitly (via list randomization) reports spending 5,000 pesos (US$1 = 45 Philippine Pesos) or more of their most recent loan on a household expense in the independent survey two weeks post-randomization, the treatment group is no more likely than the control group to say yes to any of a long list of questions regarding household expenditures greater than 1,000 pesos during the past 2 weeks .The proportion is 13% in both groups, for an estimated treatment effect of zero, although we caution that this estimate in noisy and does not rule out substantial differences in either direction.

Third, we estimate that the treatment effect is actually entirely on business investment, specifically inventory. This treatment effect can account for the entire loan amount 2-weeks post-randomization, with even larger but more noisily estimated effects at 2-months post-randomization. This result highlights how our preferred method of identifying counterfactual spending can complement longer-run follow-up data; e.g., in our setting, it will be interesting to see whether the short-run increases in inventory translate into long-run increases, and into higher profits.

Comparing these three results, under the assumption that list randomization elicits *perceived* truthful responses, suggests one surprising inference about how people respond to survey questions that attempt to directly elicit loan-financed spending. Namely, people do *not* respond with the counterfactual of greatest interest to economists: spending that would not have occurred in the absence of the loan. Rather, we suspect that they respond in our more proximate sense by the following the cash; e.g., taking the loan proceeds and going to buy groceries, even if the truly marginal purchase is one that happens a few days later for business inventory.

We emphasize however that our key contribution is methodological. Our paper is the first to bring together several methods--each used separately in different contexts by researchers, firms and financial institutions-- and compare the answers they yield to the question of how additional liquidity affects spending.

II. Market Overview

We collected data with the cooperation of two different banks in the Philippines, one in Metro Manila (covering mostly peri-urban areas) and another in northern Luzon. Both banks are for-profit institutions that offer individual liability microloans at about 60% APR. Loan sizes range from 5,000 pesos to 50,000 pesos, with a mean (median) of 13,996 (10,000) in our sample. Loan maturities range from three to six months, with weekly repayments of principal and interest. Both banks require that applicants have an existing business, and be between 18 and 65 years old.

The Metro Manila bank has operated in the region since the 1960s. It had microloans outstanding to about 2,700 borrowers as of July 2013. This portfolio represents a small fraction of its overall lending, which also includes larger business and consumer loans, and home mortgages. Until the end of 2012 the bank's microlending activities received subsidized technical assistance from a USAID-funded program.[11]

The second bank has operated in mostly rural areas of northern Luzon since the 1980s. It had microloans outstanding to 26,000 borrowers in 2011 and offers other financial products as well.

The microloan market in the Philippines is somewhat competitive, as described in Karlan and Zinman (2011). There are informal options as well, including moneylenders. For our purposes the key fact is that that rejected borrowers do not simply obtain credit elsewhere: our banks' random assignments to credit actually do produce a substantial change in the total/net borrowing of applicants (see Section III-F below).

Our sample is comprised of 1661 marginal loan applicants who were randomized into loan approval or denial (see Section III-B for details on the randomization). Table 1 Column 1 provides baseline descriptive statistics gleaned from loan applications. 81.7% of the sample are women, 73.5% are married, and 32.9% are college educated.[12]. The average applicant is 40.9 years old and has owned her business for 6.7 years. Nearly half of the businesses are "sari-sari" (corner/convenience) stores. 35.8% have regular employees/helpers (i.e., workers besides the owner), and average weekly cash flow in the businesses is 4,901 pesos (a bit more than $100).

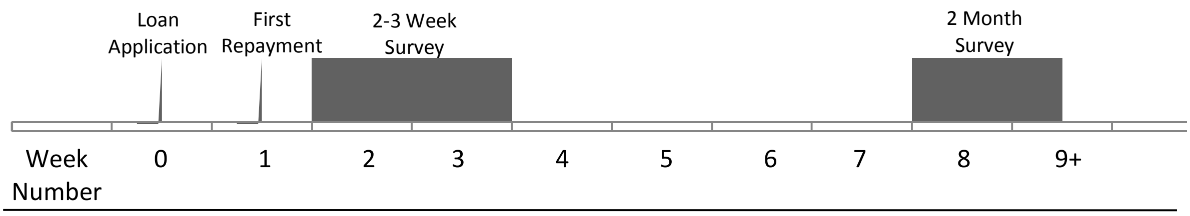
[11]The program was administered by Chemonics, Microenterprise Access to Banking Services (MABS).
[12]Females were not directly targeted by the bank. Enterprises of this size in the Philippines have greater female ownership; larger loans are serviced by a different part of the bank.

III. Methods and Results

A. Overview

To better understand how borrowers deploy loan proceeds, and report thereon, we follow individuals from when they first apply for a loan until two months later. By that endpoint, we suspect that most of any proceeds will have been spent; this seems like a reasonable assumption given the high interest rates and short maturities. Along the way we use a variety of different methods to try to get at the same underlying question: how did the loan change the client's spending relative to a counterfactual in which the loan was not available?

Figure 1: Study Timeline



Loan Activity & Data Collection

Week 0:	*Week 1:*	*Weeks 2&3:*	*Week 8:*	*Weeks 9-24:*
Loan Activity: Applies for Loan Credit Score Calculated Loan Randomization Implemented *Data Collection (Bank Employee):* Loan Use Questions Intended Use Listing	*Loan Activity:* First Repayment (For Treatment) *Data Collection (Bank Employee):* Loan Use Questions Main Use of Funds	*Loan Activity:* Continued Repayment (For Treatment) *Data Collection (Independent Surveyor):* Loan Use Questions List Randomization Spending Outflows	*Loan Activity:* Continued Repayment (For Treatment) *Data Collection (Independent Surveyor):* Loan Use Questions Spending Outflows	*Loan Activity:* Continued Repayment (For Treatment)

Our methods include various attempts to measure the counterfactual through direct elicitation (survey questions). They also include a method that combines less-direct elicitation of loan uses—by attempting to measure all recent large outflows from the household and business—with the random assignment of access to credit. The data come from four different interactions, with the same individual, over the course of about two months. Figure 1 summarizes the timeline and the data collected in four distinct steps: (1) an application for a bank loan by the individual; (2) a short survey of approved applicants at their first repayment, administered by a loan officer; (3) a questionnaire by an independent surveyor two to three weeks after the loan application and (4) a questionnaire by an independent surveyor about two months after the loan application.

In principle, testing different elicitation methods would be better done across-subjects than within-subjects, to avoid the possibility that subjects prefer to give consistent responses. If this preference holds it pushes against our core finding that responses differ in interesting ways across elicitation methods. In this senseone can view our results as lower bounds on the true extent to which elicitation methods influence inferences about the uses of marginal liquidity, at least in our setting.

B. *Sample Creation and Randomization*

Our sample is comprised of 1,661 marginally creditworthy microloan applicants to the two banks described in Section II. Individuals applied from one of 16 bank branches at the Northern Luzon lender, or 8 branches at the Metro Manila lender, between July 2010 and March 2012. Each loan application is digitized by bank staff and credit-scored by underwriting software. For the purposes of this study, relatively small numbers of applicants with the highest (lowest) scores were automatically approved (rejected). The remaining applicants (about 85% of the pool) were randomly assigned to approval (with 90% probability) or rejection (with 10% probability). [13]

This random allocation of loans to marginal clients serves as the identifying instrument for our analysis of the expenditure data described in Sections III-E and III-F below. Table 1 Column 2 confirms that the treatment and control groups are observably identical, in a statistical sense: regressing treatment assignment on treatment strata and the complete set of baseline characteristics in Table 1, we do not reject the hypothesis that the characteristics are jointly uncorrelated with treatment assignment (p-value = 0.488).

C. *Data Collection Step 1: At Application, by Loan Officer*

The first pieces of data on loan uses come from loan applications. Applications are extensive, and take the form of loan officers interviewing applicants, reviewing their documents, and entering data into a small netbook computer. This process typically takes at least an hour to complete, and includes questions on income, household composition, assets and liabilities, and business cash flows.

The banks added three questions on loan uses to their applications at our behest. The applicant was first asked: (1) Do you plan to spend 5,000 pesos or more of your loan on any one household item? [14] (2) Do you plan to spend 2,500 pesos or more of your loan on servicing any other debt? Later the applicant was asked to provide a full listing of intended usage of the loan. The former two questions are designed to identify non-trivial non-business uses of loan proceeds, keeping in mind that the median loan size is 10,000 pesos and that borrowers may split loan proceeds among several different types of expenditures.

[13] The proportions that were allocated to treatment and control were determined through negotiations with the bank. Although a 50/50 split would have provided more statistical power the bank was interested in aggressively expanding their pool of borrowers.

[14] Exchange rate at time of surveys was US$1 = 43 Philippine Pesos.

This first step allows us to see how the applicants *report* their intended loan usage *to the banks*. These data will not be very informative about true intentions if applicants believe that their responses may affect the lender's decision. For example, applicants might reasonably infer that banks prefer to lend exclusively for business purposes, and answer no to the questions about household and refinancing uses, regardless of their true intentions.

Table 2 Column 1 shows that very few applicants report non-businesses loan uses on their loan applications. Only 1.8% report planning to use their loan on a household transaction of 5,000 pesos or more (Panel A), and only 2.3% report planning to use their loan to pay down debt of 2,500 or more (Panel B).[15] Column 1 shows results for the treatment group only, for comparability with subsequent analysis. Results do not change if we include the control group.

Is the low reported prevalence of non-business uses on loan applications driven by strategic underreporting? Results below from steps 3 and 4 suggest yes, although only to a point. Before detailing those results we examine whether borrowers change their reporting behavior to the bank after they obtain a loan.

D. Data Collection Step 2: At First Loan Repayment, by Bank Credit Officer

The second pieces of data on loan uses come from a very short survey, administered by loan officers to a subset of borrowers, at the time of first repayment (about one week after loan disbursal). The loan officers asked two questions designed to parallel the key questions from the application: (1) Did you spend, or do you plan to spend, 5,000 pesos or more of your loan on any one household item? (2) Did you spend, or do you plan to spend, 2,500 pesos or more of your loan on servicing any other debt?

This step allows us to check for differences between what applicants and borrowers tell the bank. We might see such differences if applicants misreported strategically in the first step and the main driver of that behavior was concern about getting approved for the first loan[16]. On the other hand, several factors push against finding differences, including repeat contracting, and any desire among borrowers to appear consistent in their reporting behavior.

Table 2 Column 2 shows that reported prevalence of non-business uses post-loan is essentially unchanged from the loan application. Here we find less than one percent reporting using their loan on a large household transaction, while 2.9% report using it to pay down other

[15] As we show in section 2 of the paper our randomization was successful and so comparing the reported loan use intentions of the treatment and control group will not be informative at this point. The only place where comparing the responses is useful is in columns 5 and 7, reported spending.

[16] It is worth considering how our inferences will be affected if borrowers change their mind over time about how to use loan proceeds. First, note that each of the independent surveys (two weeks and two months) does not suffer from this potential problem—each survey uses multiple elicitation methods, administered at the same time. Second, it is unclear why mind-changing would be asymmetric; e.g., for every person that changes their mind from a business use to a household use, we might well expect someone else to make the opposite change. .

debt.[17] Sample size is lower in Column 2 because this step was implemented only at one bank and only for a short period of time. The data collection proved onerous for the bank, and the bank discontinued it after we observed the strong similarity in reporting behavior between this step (post-loan) and step one (application).

E. Data Collection Steps 3 and 4: 2-Week and 2-Month Surveys, by Independent Surveyor

The third and fourth pieces of data on loan uses come from two surveys, administered by an independent surveyor about two weeks and two months after loan application, of both treatment and control group individuals. Surveyors located individuals at their place of business or home and invited them to take a survey on behalf of Innovations for Poverty Action (IPA), a research organization. Surveyors were not aware of any connection to the banks. Surveyors informed people in the sample frame that IPA obtained a list of potential survey respondents from a database of local businesses.[18]

Both surveys focus on direct elicitation of loan uses and the measurement of all recent substantial outflows, although the second survey is a bit shorter. Both were administered by the same surveyor. The scripts for key questions are reproduced in Appendix 1. Relative to the two-week survey, measuring outflows at two months has the potential advantage of allowing more time for all loan proceeds to be spent. It also has several potential disadvantages: more time for the control group to find alternative sources of financing (weakening power), a longer recall period (increasing measurement error), and/or more time for any short-run returns on investment to effect spending decisions (confounding inferences about the *direct* effect of borrowing on spending).

84% of our initial sample of 1,661 completed the first (two-week) survey. Table 1 Column 3 shows that treatment assignment does not significantly affect two-week survey completion. Column 4 shows, unsurprisingly, that baseline characteristics do predict survey completion. But Column 5 shows that these characteristics do not interact significantly with treatment assignment (p-value on the joint test = 0.239), offering reassurance that the treatment leaves the composition as well as proportion of survey respondents unchanged.

65.9% of our initial sample completed the second (two-month) survey. Table 1 Column 6 shows that treatment assignment does not significantly affect two-month survey completion. Column 7 shows, unsurprisingly, that baseline characteristics do predict survey completion. Column 8 shows that the interactions between baseline characteristics and treatment assignment

---

[17] The loan officers also asked the borrowers what they primarily spent their loans on and *every* borrower replied that they spent it on their business.

[18] The goal was to be truthful yet also mask the relationship with the specific partnering bank. The surveyors themselves had no knowledge of the bank connection.

are jointly significant; raising the possibility that treatment affects the composition of two-month survey respondents (Column 8) if not the response rate (Column 6).

The two-week survey begins with questions about basic demographics, health and savings. These introductory questions are designed to mitigate the likelihood that respondents infer any connection or association between the survey and their recent loan (application). The surveyor then asked the respondent for details on any outstanding loans, starting with the most recent one. Respondents reporting a loan were then asked about their deployment of loan proceeds using three different methods.

First, the surveyor explicitly asked the two key loan use questions: (1) Did you spend 5,000 pesos or more of your loan on any one household item? (2) Did you spend 2,500 pesos or more of your loan on servicing any other debt? We expect the proportion of "yeses" here to be higher than those reported to the bank, since incentives for strategic misreporting to an independent surveyor should be lower. Table 2 Column 3 shows that this is indeed the case, to some extent. 5.5% of individuals report using a loan for a large household expense; compared to 1.8% on the loan application (the 3.7 percentage point difference has a p-value less than 0.001). 7.7% report using the loan to pay down other debt, compared to 2.3% on the loan application (the 5.4 percentage point difference has a p-value less than 0.001). Of course, borrowers may still underreport non-business uses if such uses are stigmatized, or if borrowers suspect a connection between the surveyor and their bank. Such concerns motivate our second elicitation method.

Second, the surveyor administered a list randomization exercise to elicit estimates of group-level proportions of respondents using loan proceeds to pay down debt or buy household goods. List randomization is used across various disciplines to mitigate the underreporting of socially or financially sensitive information (Karlan and Zinman 2012). The procedure asks a randomly-selected half of the respondents to report the total number of "yes" answers to four innocuous binary questions (Appendix 1), and the other half to report the total number of "yes" answers to the same four innocuous binary questions plus a fifth sensitive one. We did this separately for the two different loan use questions: (1) I spent over 5,000 pesos of my loan of a single household transaction" and (2) "I spent more than 2,500 pesos of my loan to pay down other debt." We then estimate the proportion responding "yes" to the sensitive (loan use) question by subtracting the mean count of "yeses" for those who had only had the four innocuous questions from the mean count for those who had all five questions (including a loan use question).[19] As expected, list randomization produces substantially higher estimates of non-business uses (Table 2 Column 4). We infer that 11.5% of respondents report spending at least 5,000 pesos of their loan proceeds on

[19] Those who do not report an outstanding loan instead are assigned the mean count of the short-list (innocuous, non-loan use questions only) group. Results are nearly identical if we instead drop these non-borrowers.

a single household transaction (p-value = 0.285), with 19.1% spending at least 2,500 of their loan proceeds on paying down other debt (p-value = 0.021).[20]

All told, the results in Columns 1-4 suggest that elicitation method can have substantial effects on how borrowers report loan uses. Borrowers report more non-business uses when asked by an independent surveyor rather than a bank, and still more when they can report anonymously. The results suggest that list randomization, administered by an independent surveyor, produces relatively accurate estimates of how borrowers *perceive* their loan uses.

These results thus far do not address the question of how borrower perceptions accord with the reality that is most interesting to many researchers, practitioners, and funders: what did the respondent buy that they would not have in the absence of the marginal loan? Fungibility may make it difficult to construct survey questions that elicit that counterfactual. For example, loan proceeds may be used to purchase inventory in the *proximate* sense of cash from bank being handed over to a supplier. But if the business owner would have purchased that inventory anyway, the marginal (counterfactual) purchase could be something else entirely; e.g., perhaps the cash flow that would have been used to purchase inventory is now used to purchase health care for an ailing family member.

The difficulty of identifying the counterfactual of interest motivates our third type of survey question: we ask each respondent to list each household and business outflow greater than 1,000 pesos from the past two weeks (type and amount).[21] (Note the lack of any reference to loans or loan proceeds: this question asks about spending more broadly.) Together with the random assignment of loan approvals, we use responses to this question to identify the counterfactual: the impacts of the marginal loan on consumption and investment.[22] Table 2 Column 5 reports the results, which show a striking *lack* of impact on non-business spending. The treatment (loan approved) and control (loan rejected) groups have identical proportions (0.133) of respondents reporting one or more household expenses >= 5000 pesos, for a treatment effect of zero (SE =

[20] Those that respond positively to all items in the larger list of 5 questions may be less worried about anonymity as they are identifying themselves as having used the funds in the sensitive manner. Nonetheless, we continue to see under-reporting by this group: only 1 of 7 individuals who answered "5" on the debt question directly reported they used their loan on refinancing debt when asked, while only 3 of 10 who answered "5" on the household question directly reported they used their loan on a large household expenditure.
[21] Without any prompts for specific expense types.
[22] We treat the results from this method of elicitation as the "truth" regarding where the funds went. Although this is an assumption, the finding that the average increase in spending between treatment and control lines up with the average increase in credit availability lends credence to the idea that this is in fact where the money was spent.

0.30).[23] For debt pay down, the treatment group has a slightly higher proportion (0.142 vs. 0.126), but the 1.6 percentage point difference is not statistically significant (p-value = 0.580).[24]

We find similar results, on a much higher base, in the two-month survey.[25] Regarding the base, many more respondents directly report non-business uses, whether directly (Column 6) or on the outflow list (Column 7).[26] Regarding the counterfactual of interest, when we compare the treatment group to the control group we find that the control group has an equally high base, statistically speaking. 22.7% of the treatment group report spending at least 5,000 pesos on any one household transaction while 18.0% of the control group does so. This difference of 4.7 percentage points is not statistically significant (p-value = 0.210). Similarly, 23.7% of the treatment group reports spending more than 2,500 pesos on other debt[27] while 19.7% of the control group does so. This difference of 4.1 percentage points is not statistically significant (p-value = 0.291).

Taken together, the results in Table 2 highlight several key findings. Substantively, there is little evidence of substantial non-business uses of microenterprise loans in this particular setting. This is surprising, given low impact on business growth in general from microcredit (Angelucci, Karlan, and Zinman 2013; Attanasio et al. 2011; Augsburg et al. 2012; Banerjee et al. 2013; Crepon et al. 2011; Karlan and Zinman 2010), findings from a prior study with one of the lenders here that marginal borrowers *decrease* investment in their microenterprises (Karlan and Zinman 2011), and mounting concerns that people "over-borrow" to finance consumption (Zinman forthcoming).

Methodologically, we find that borrower reporting responds strongly to the elicitation method, and that direct elicitation of loan uses does not produce evidence on a key counterfactual—what borrowers purchase that they would not have purchased in the absence of a loan. Rather, we identify the counterfactual using random assignment of credit access coupled with short-term follow-up measurement of substantial outflows.

F. So Where Does the Money Go?

[23] If we instead use a 1,000 peso cut-off we get an increase of 0.026 in treatment, (p-value=0.560). The cut-off at 5,000 pesos allows us to check for large household expenditures and lines up with the direct questions that are asked of the borrowers.

[24] We are implicitly using the random assignment as an instrument for borrowing over the subsequent two weeks. The top rows of Table 3 confirm that the instrument is a powerful one; e.g., a treatment group member is 16 percentage points more likely to have a formal sector loan than a control group member.

[25] The higher base could be due to respondents taking > 2 weeks to fully spend their loan proceeds, and/or to respondents' increased comfort with the survey or surveyor.

[26] We did not include list randomization on the two-month survey.

[27] It may seem peculiar that the proportion of respondents who report spending more than 2,500 on debt pay down in the explicit question asked by the surveyor (column 6) is higher than the proportion that report this when listing out their spending over the past 2 months (column 7). This may be due to the fact that the outflow list has a 1,000 peso threshold, so if someone pays off debt in increments < 1,000 pesos but a total amount >= 2,500 pesos, the outflow list would miss this, whereas that direct question might capture it.

If the marginal expenditure financed by a loan is *not* on a household item or other debt service (Table 2), it presumably *is* on some sort of business investment. Can we actually detect an increase in business investment, or do measurement error or reporting biases make it futile to attempt to follow the money with survey data?

Tables 3 and 4 suggest that our methods can in fact identify the marginal spending: business inventory, in this case. We switch from the mean comparisons in Table 2 (Columns 5 and 7) to regressions to improve precision, and estimate OLS intention-to-treat (ITT) models, with Huber-White standard errors, of the form:

$$Y_{it} = \alpha + \beta * treatment_i + \delta * FE_{it} + \varepsilon$$

Where i indexes individuals and t time, *treatment* = 1 if i was randomly *assigned* to loan approval, and FE is a vector of randomization strata (a bank indicator, credit score category, application month-year, and the survey month-year). Y is an outcome measuring borrowing (to show the magnitude of the first-stage) or spending, measured at either t = 2 weeks or t = 2 months post-random assignment. Because inferences about these outcomes may be influenced by outliers, we present results from three different functional forms: Column 1 estimates effects on the level of spending (in pesos); Column 2 "winsorizes" the data, recoding the top 1% of Y's to the 99th percentile; and Column 3 "trims" the data, dropping observations in the top percentile of Y. We do not use log(Y) because most of our borrowing and spending variables have many zeros.

Table 3 shows treatment effects on different measures of Y over the two weeks after random assignment. Table 4 shows treatment effects on the spending measures over the two months after random assignment.

The first panel of Table 3 shows that we have a strong first stage, similar to that found in Karlan and Zinman (2011) with the Metro Manila lender participating in this study. The treatment effect on the likelihood of having a loan from one of our partner banks is 0.33 (p-value < 0.001). This is measured using administrative data from the bank. The effect is < 1 due to approved applicants in the treatment group deciding to not actually go ahead with the loan, and to control group applicants who managed to avail a loan anyway. The remaining Y's are measured using the follow-up surveys. Treatment effects on measures of total formal sector borrowing are still statistically significant but about one-half the size on borrowing from our partner lenders, due in part to some control group individuals obtaining credit from comparable lenders, and in part to substantial underreporting of debt that is line with what we have found in other studies (Karlan and Zinman 2008; Zinman 2009; Zinman 2010; Karlan and Zinman 2011).[28]

The next panel of Table 3 estimates the treatment effect on total spending, as measured using our question asking respondents to list all outflows >= 1,000 pesos during the past two weeks.

[28] 34% of those we know, from administrative data, to have a loan with one of our lenders do not report *any* outstanding formal sector loans at the two-week follow-up survey.

Depending on our treatment of outliers, the estimate ranges from 4,996 to 5,696 pesos (with p-values of 0.059, 0.038, and 0.028). Scaling up these estimates by the difference in borrowing rates from the administrative data (since that data is not subject to underreporting of debt), we get estimated treatment-on-the-treated effects of about 15,000-16,000 pesos. The average loan size is 14,601 pesos, suggesting that our two-week outflow questions do successfully follow the money. They also suggest that borrowers spend all loan proceeds within the first two weeks, which seems plausible given the high interest rate and short maturity.

The rest of Table 3 disaggregates spending into several categories of interest. We confirm that lack of significant effects on household spending and debt pay down found in the earlier means comparisons (Table 2). Most notably, we find increases in business expenditures, in magnitudes commensurate with the treatment effect on overall spending. Disaggregating business expenses into fixed assets, inventory, renovations, utilities, salaries, and other, we find evidence suggesting that the entirety of the (business) spending increase is due to inventory. The ITT estimates on inventory range from 3,738 to 6,045 depending on how we treat outliers, with p-values of 0.005, 0.008, and 0.049. The focus on inventory may be due to the 3-6 month loan amortization, which may be too short for other types of investments to produce the returns needed to service the debt.

Table 4 repeats the spending analysis using data from the two-month follow-up survey. The results are qualitatively consistent with the two-week results. Point estimates are again more than large enough to offer a complete accounting of the loan proceeds. The pattern of results on spending (sub-)categories again suggests that about 100% of marginal spending is on business inventory. There are two noteworthy differences between the two-month and two-week results. One is that the two-month results are less precise. This is most likely due to the relative difficulty of recalling spending over a two month period. The second is that the two-month point estimates on total business expenditure, and inventory specifically, are much larger. This could be an artifact of wide confidence intervals or respondent reporting. Or it could be capturing a true multiplier whereby treated individuals reinvest increased profits from the initial inventory increase, or obtain additional financing from other sources, to further increase inventory.

In any case, the suggestion that quantitative effect sizes may differ substantially over as short a period as six weeks—two weeks vs. two months—highlights the utility of short-run and high-frequency follow-ups for capturing and interpreting spending dynamics in the aftermath of a liquidity shock.

IV. Conclusion

Discussions of outcome measurement following liquidity shocks often focus on how *longer*-run data may be needed to measure key impacts (e.g., of investments that require longer gestation periods, or learning). We take a different tack, and test three different methods for measuring the short-run responses.

The first method uses direct questions about intended loan usage on the banks' loan applications, shortly after loan disbursal, and nearly identical direct questions asked of borrowers, by independent surveyors, with no link to the bank, two weeks and two months after loan disbursal. The second method uses indirect questions through two "list randomization" questions, asked by independent surveyors two weeks after disbursal, that make it feasible for respondents to respond truthfully to sensitive questions without actually revealing details about their behavior. The third method uses the lenders' randomizations and the two-week and two-month independent follow-up surveys, by comparing a listing of recent expenditures (with no reference to recent borrowing) across the treatment and control groups.

The results suggest three key findings in our setting. First, respondents report strategically. They report very little non-business uses of loan proceeds to the bank, significantly more to independent surveyors when asked direct questions, and yet significantly more to independent surveyors when presented with lists of statements that allow them to report what they believe to be the truth without directly revealing what they spent. Second, even when borrowers are more likely to respond with what they perceive to be the truth, their answers to questions about "did you spend X or more of your loan on…" are different than the counterfactual of greatest interest to economists and policymakers. For example, although 12% of our treatment group implicitly (via list randomization) reports spending 5,000 pesos or more of their most recent loan on a household expense in the independent survey two weeks post-randomization, the treatment group is no more likely than the control group to say yes to any of a long list of questions regarding household expenditures greater than 1,000 pesos during the past two weeks (the proportion is 13% in both groups, for an estimated treatment effect of zero). Third, we estimate that the treatment effect is actually entirely on business investment, specifically inventory. This treatment effect can account for the entire loan amount 2-weeks post-randomization, with even larger but more noisily estimated effects at 2-months post-randomization.

We believe the main implication of our results is methodological: researchers should consider collecting spending data on both treatment and control subjects very shortly after an exogenous liquidity shock. In particular, our study highlights the value of *shorter*-run, *high-frequency* data collection on substantial outflows following a liquidity shock. To take just two examples, if we are interested in the possibility of over-borrowing, the methods used in this paper can be used to address the question of "over-borrowing on what"? In the settings studied here, the answer appears to be "not on consumption". If we are interested in why expanding access to microcredit does not reliably lead to business growth and increased profits, the methods here can be used to address the question "is this because borrowers invest in something else, or because they invest and fail?" In the settings studied in this paper it appears that any downstream lack of business growth is not for lack of trying.

Future non-methodological work can build on this, and use our estimates of loan usage with longer-term follow-up data on business and household outcomes. This will enable us to measure whether the short-run investments in inventory produce long-run increases in profits and/or improvements in household outcomes. We are also interested in testing whether alternative direct elicitation methods might help borrowers and researchers zero in on the key counterfactual. Perhaps asking “what did you spend your loan on that you would not have bought if you had not gotten a loan?” would produce the same inferences, at less expense, than a randomized experiment followed by elicitation of all major household and business outflows.

References

Agarwal, Sumit, Chunlin Liu, and Nicholas S. Souleles. 2007. "The Reaction of Consumer Spending and Debt to Tax Rebates—Evidence from Consumer Credit Data." *Journal of Political Economy* 115 (6): 986–1019. doi:10.1086/528721.

Anagol, Santosh, Alvin Etang, and Dean Karlan. 2013. "Continued Existence of Cows Disproves Central Tenets of Capitalism." *Yale University Working Paper*.

Angelucci, Manuela, Dean Karlan, and Jonathan Zinman. 2013. "Win Some Lose Some? Evidence from a Randomized Microcredit Program Placement Experiment by Compartamos Banco." *Working Paper*.

Attanasio, Augsburg, Britta Augsburg, Ralph de Haas, Fitz Fitzsimons, and Heike Harmgart. 2011. "Group Lending or Individual Lending? Evidence from a Randomised Field Experiment in Mongolia." *EBRD Working Paper* 136 (December).

Augsburg, Britta, Ralph de Haas, Heike Harmgart, and Costas Meghir. 2012. "Microfinance at the Margin: Experimental Evidence from Bosnia and Herzegovina." *Working Paper*, September.

Banerjee, Abhijit, Esther Duflo, Rachel Glennerster, and Cynthia Kinnan. 2013. "The Miracle of Microfinance? Evidence from a Randomized Evaluation". Working paper.

Bauer, Michal, Julie Chytilová, and Jonathan Morduch. 2012. "Behavioral Foundations of Microcredit: Experimental and Survey Evidence from Rural India." *American Economic Review* 102 (2): 1118–39. doi:10.1257/aer.102.2.1118.

Benhassine, Najy, Florencia Devoto, Esther Duflo, Pascaline Dupas, and Victor Pouliquen. 2013. *Turning a Shove into a Nudge? A "Labeled Cash Transfer" for Education*. Working Paper 19227. National Bureau of Economic Research. http://www.nber.org/papers/w19227.

Bhutta, Neil, and Benjamin Keys. 2013. "Interest Rates and Equity Extraction During the Housing Boom."

Blattman, Christopher, Nathan Fiala, and Sebastian Martinez. 2012. *Employment Generation in Rural Africa: Mid-Term Results from an Experimental Evaluation of the Youth Opportunities Program in Northern Uganda*. SSRN Scholarly Paper ID 2030866.

Crepon, Bruno, Florencia Devoto, Esther Duflo, and William Pariente. 2011. "Impact of Microcredit in Rural Areas of Morocco: Evidence from a Randomized Evaluation." *M.I.T. Working Paper*, March.

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.

Eggertsson, Gauti B., and Paul Krugman. 2012. "Debt, Deleveraging, and the Liquidity Trap: A Fisher-Minsky-Koo Approach*." *The Quarterly Journal of Economics* 127 (3): 1469–1513. doi:10.1093/qje/qjs023.

Fafchamps, Marcel, David McKenzie, Simon Quinn, and Christopher Woodruff. 2013. "Female Microenterprises and the Flypaper Effect: Evidence from a Randomized Experiment in Ghana."

Hall, Robert E. 2011. "The Long Slump." *American Economic Review* 101 (2): 431–69. doi:10.1257/aer.101.2.431.

Haushofer, Johannes, and Jeremy Shapiro. 2013. “Welfare Effects of Unconditional Cash Transfers: Evidence from a Randomized Controlled Trial in Kenya.” *M.I.T. Working Paper*.

Johnson, David S, Jonathan A Parker, and Nicholas S Souleles. 2006. “Household Expenditure and the Income Tax Rebates of 2001.” *American Economic Review* 96 (5): 1589–1610. doi:10.1257/aer.96.5.1589.

Karlan, Dean, Ryan Knight, and Christopher Udry. 2013. “Consulting and Capital Experiments with Micro and Small Tailoring Enterprises in Ghana.” *Yale University Working Paper*.

Karlan, Dean, Isaac Osei-Akoto, Robert Darko Osei, and Christopher R. Udry. 2013. “Agricultural Decisions after Relaxing Credit and Risk Constraints.” *Yale University Economic Growth Center Working Paper*. doi:10.2139/ssrn.2169548.

Karlan, Dean, and Jonathan Zinman. 2008. “Lying About Borrowing.” *Journal of the European Economic Association*, Journal of the European Economic Association, 6 (2-3): 510–21.

———. 2010. “Expanding Credit Access: Using Randomized Supply Decisions to Estimate the Impacts.” *Review of Financial Studies* 23 (1): 433–64.

———. 2011. “Microcredit in Theory and Practice: Using Randomized Credit Scoring for Impact Evaluation.” *Science* 332 (6035): 1278–84.

———. 2012. “List Randomization for Sensitive Behavior: An Application for Measuring Use of Loan Proceeds.” *Journal of Development Economics* 98 (1): 71–75. doi:10.1016/j.jdeveco.2011.08.006.

Mian, Atif, and Amir Sufi. 2011. “House Prices, Home Equity-Based Borrowing, and the US Household Leverage Crisis.” *American Economic Review* 101 (5): 2132–56.

———. 2012. “What Explains High Unemployment? The Aggregate Demand Channel.”

Moskowitz, T.J., and A. Vissing-Jorgensen. 2002. “The Returns to Entrepreneurial Investment: A Private Equity Premium Puzzle.” *American Economic Review* 92 (4): 745–78.

Nicola, De, Francesca, and Xavier Giné. 2012. *How Accurate Are Recall Data? Evidence from Coastal India*. SSRN Scholarly Paper ID 2027805. Rochester, NY: Social Science Research Network. http://papers.ssrn.com/abstract=2027805.

Parker, Jonathan A, Nicholas S Souleles, David S Johnson, and Robert McClelland. 2013. “Consumer Spending and the Economic Stimulus Payments of 2008.” *American Economic Review* 103 (6): 2530–53. doi:10.1257/aer.103.6.2530.

Souleles, Nicholas S. 1999. “The Response of Household Consumption to Income Tax Refunds.” *American Economic Review* 89 (4): 947–58. doi:10.1257/aer.89.4.947.

Souleles, Nicholas S. 2002. “Consumer Response to the Reagan Tax Cuts.” *Journal of Public Economics* 85 (1): 99–120.

Tarozzi, Alessandro, Jaikishan Desai, and Kristin Johnson. 2013. “On the Impact of Microcredit: Evidence from a Randomized Intervention in Rural Ethiopia.” *UPF Working Paper*.

Zinman, Jonathan. forthcoming. “Consumer Credit: Too Much or Too Little (or Just Right)?” *Journal of Legal Studies*, no. Special Issue on Benefit-Cost Analysis of Financial Regulation.

———. 2009. “Where Is the Missing Credit Card Debt? Clues and Implications.” *Review of Income and Wealth* 55 (2): 249–65.

———. 2010. “Restricting Consumer Credit Access: Household Survey Evidence on Effects Around the Oregon Rate Cap.” *Journal of Banking & Finance* 34 (3): 546–56.

Table 1: Orthogonality of Treatment to Applicant Characteristics and Attrition

Purpose of specification:	Means	Pre-Attrition Orthogonality Test	Orthogonality of Attrition Test	Orthogonality of Attrition Test, with Controls	Orthogonality of Attrition Test, including Compositional Effects	Orthogonality of Attrition Test	Orthogonality of Attrition Test, with Controls	Orthogonality of Attrition Test, including Compositional Effects
Dependent Variable:		Loan Assigned = 1	Completed Two-Week Follow-up Survey = 1	Completed Two-Week Follow-up Survey = 1	Completed Two-Week Follow-up Survey = 1	Completed Two-Month Follow-up Survey = 1	Completed Two-Month Follow-up Survey = 1	Completed Two-Month Follow-up Survey = 1
Sample:	All	All	All	All	All	All	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male	0.183	0.006		-0.048*	-0.077		-0.020	-0.035
	(0.387)	(0.020)		(0.026)	(0.080)		(0.028)	(0.092)
Marital Status -- Married	0.735	0.036		0.017	-0.025		0.050	-0.041
	(0.441)	(0.023)		(0.027)	(0.075)		(0.032)	(0.090)
Marital Status -- Widowed/Separated	0.110	0.034		-0.015	-0.095		0.035	-0.023
	(0.312)	(0.031)		(0.041)	(0.131)		(0.046)	(0.133)
Education -- Some College	0.255	0.000		0.086***	-0.026		0.100***	0.061
	(0.436)	(0.021)		(0.024)	(0.078)		(0.028)	(0.084)
Education -- Graduated High School	0.319	0.027		0.086***	0.000		0.091***	0.047
	(0.466)	(0.018)		(0.023)	(0.069)		(0.027)	(0.090)
Education -- Some High School or Less	0.097	-0.003		0.129***	0.148***		0.113***	0.217***
	(0.296)	(0.031)		(0.030)	(0.057)		(0.038)	(0.083)
Primary Business Location -- Residential	0.612	-0.037**		-0.030	-0.023		-0.025	0.002
	(0.487)	(0.018)		(0.022)	(0.070)		(0.025)	(0.086)
Primary Business Arrangment -- Rent	0.309	-0.008		-0.026	-0.016		-0.052**	-0.062
	(0.462)	(0.018)		(0.022)	(0.073)		(0.027)	(0.092)
Primary Business Type - Small Grocery/Convenience Store	0.492	0.000		0.034	-0.078		0.054*	-0.005
	(0.500)	(0.023)		(0.027)	(0.075)		(0.031)	(0.090)
Primary Business Type - Wholesale	0.026	0.026		0.001	0.116		-0.031	0.364***
	(0.161)	(0.050)		(0.065)	(0.105)		(0.074)	(0.111)
Primary Business Type - Service	0.138	0.039		0.042	0.083		0.046	0.048
	(0.345)	(0.026)		(0.034)	(0.088)		(0.040)	(0.135)
Primary Business Type - Manufacturing	0.020	0.059		0.133***	0.099		-0.003	-0.222
	(0.140)	(0.046)		(0.045)	(0.120)		(0.083)	(0.358)
Primary Business Type - Vending	0.116	0.010		0.035	0.071		0.045	0.199*

	(0.320)	(0.026)		(0.034)	(0.080)		(0.040)	(0.103)
Number of Employees	0.813	-0.006		0.001	-0.011		0.002	0.001
	(1.960)	(0.005)		(0.004)	(0.017)		(0.005)	(0.018)
Age	40.9	0.006		-0.014	-0.057**		-0.000	-0.016
	(9.2)	(0.009)		(0.010)	(0.029)		(0.012)	(0.032)
Years Primary Business in Business	6.7	0.001		0.001	-0.009		0.010	-0.089**
	(6.0)	(0.007)		(0.010)	(0.032)		(0.011)	(0.040)
Primary Business Weekly Cashflow	4901	0.009		-0.024**	-0.044		-0.023*	-0.035
	(6115)	(0.007)		(0.012)	(0.042)		(0.012)	(0.041)
Number of Dependents	1.880	0.000		0.018**	0.023		0.023**	0.049
	(1.460)	(0.008)		(0.009)	(0.023)		(0.011)	(0.031)
Assigned to Treatment Group	0.899		-0.016	-0.018	-0.158	-0.039	-0.044	-0.134
	(0.301)		(0.029)	(0.029)	(0.128)	(0.034)	(0.035)	(0.154)
Interaction of all Covariates with Treatment Assignment		No	No	No	Yes	No	No	Yes
Mean of dependent variable		0.899	0.839	0.839	0.839	0.657	0.657	0.657
P-Value on joint F-test: all RHS covariates listed=0?		0.534		0.000			0.000	
P-Value on joint F-test: all RHS covariates interaction term=0?					0.209			0.001
Observations	1,661	1,661	1,661	1,661	1,661	1,661	1,661	1,661

Notes: Column 1 reports the means and standard deviation of each variable. All other columns are OLS regressions with Huber-White standard errors in parentheses -- * significant at 10%; ** significant at 5%; *** significant at 1%. Sample frame contains 1,661 marginal applicants eligible for the treatment (i.e., for loan approval). Other regressors (not shown) are the randomization conditions (credit score cut-offs), bank, application year/month, survey year/month. 'Single' is the omitted marital status category. 'College graduate' is the omitted educational attainment variable. Commercial is the omitted primary business location variable. 'Own' is the omitted primary business property arrangement. 'Other retail' is the omitted primary business type variable. The five non-binary variables (number of employees, age, years in business, weekly cashflow, number of dependents) are standardized to have mean equal to zero and standard deviation equal to one.

Table 2: Loan Use Elicitation Methods

Data Source:	Reported to Bank		Reported to Surveyor at 2-Week Follow-up			Reported to Surveyor at 2-Month Follow-up	
	Proportion reporting "yes" on loan application	Proportion reporting "yes" at first repayment	Proportion reporting "yes" in direct self-report to independent surveyor	Implicit proportion reporting "yes" from list randomization	Proportion reporting "yes" in list of all large household or enterprise outflows	Proportion reporting "yes" in direct self-report to independent surveyor	Proportion reporting "yes" in list of all large household or enterprise outflows
Survey wording found in:	Appendix 1A	Appendix 1B	Appendix 1B	Appendix 1C	Appendix 1D	Appendix 1E	Appendix 1F
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Household Expenditures: Will/Did you use 5,000 pesos or more of your loan on any single transaction for your household?							
Treatment Group Mean	0.018	0.008	0.055	0.115	0.133	0.216	0.227
	(0.003)	(0.006)	(0.006)	(0.056)	(0.009)	(0.013)	(0.013)
Control Group Mean					0.133		0.180
					(0.028)		(0.035)
Treatment - Control					**0.000**		**0.046**
					(0.030)		**(0.037)**
Observations	1,493	238	1,245	1,245	1,388	973	1,095
Panel B: Payoff Other Debt: Will/Did you use 2,500 pesos or more of your loan to pay down other debt?							
Treatment Group Mean	0.023	0.029	0.077	0.191	0.142	0.325	0.237
	(0.004)	(0.011)	(0.008)	(0.049)	(0.010)	(0.015)	(0.014)
Control Group Mean					0.126		0.197
					(0.028)		(0.036)
Treatment - Control					**0.016**		**0.041**
					(0.029)		**(0.039)**
Observations from Treatment	1493	238	1245	1245	1245	973	973
Observations from Control	0	0	0	0	143	0	122

Notes: Means and means comparisons, with standard errors in parentheses. Column 1 includes our entire sample assigned to treatment, whether they were reached for the follow up survey or not. Column 2 includes only the small subset of clients who were asked this question at first loan repayment. This was logistically difficult for the bank, and was thus stopped after finding few respondents reporting answers different than what they reported on their loan application (i.e., Column 1). Columns 3 through 5 include those found for the first follow-up survey (for columns 3 and 4, if the respondent did not report a loan, they were coded as saying "no" to using a loan for that panel's purpose). Columns6 and 7 include those found for the second follow-up survey (for column 6, if the respondent did not report a loan, they were coded as saying "no" to using a loan for that panel's purpose). Sample size declines from application (Column 1) to the first survey (Columns 3-5) and then to the second survey (Columns 6-7) because of attrition. Table 2 shows that attrition is uncorrelated with treatment assignment.

Table 3: First Stage, and OLS Treatment Effects on Expenditures During the First Two Weeks After Loan Application

Dependent variables	(1)	(2)	(3)
Borrowing Activity in Past Two Weeks			
Has Loan from Experimenting Lender (Admin Data)	0.329***	0.329***	0.329***
	(0.042)	(0.042)	(0.042)
Any Outstanding Formal Loan (Survey Data)	0.159***	0.159***	0.159***
	(0.045)	(0.045)	(0.045)
Number of Outstanding Formal Loans (Survey Data)	0.181***	0.166***	0.166***
	(0.061)	(0.061)	(0.061)
Total Outstanding Formal Loans, Pesos (Survey Data)	1,535	1,725	2,644***
	(1,919)	(1,119)	(788)
Total Spending in Past Two Weeks	5,696*	5,374**	4,996**
	(3,010)	(2,588)	(2,136)
Business Expenditures in Past Two Weeks	7,031***	6,280***	4,523**
	(2,268)	(2,104)	(1,985)
Assets for Business	356*	137	-93
	(187)	(121)	(94)
Merchandise for Business	6,045***	5,328***	3,738*
	(2,173)	(2,013)	(1,914)
Business Renovations	120	-3	2
	(203)	(30)	(2)
Utilities for Business	303	92	63
	(252)	(119)	(98)
Salaries for Employees	159	102	0
	(135)	(126)	(111)
Other Business Expenses	48	-16	109
	(271)	(228)	(146)
Household Expenditures in Past Two Weeks	-1,676	-3	320
	(1,934)	(413)	(317)
Household Items	-150	-38	27
	(248)	(142)	(98)
Utilities for Home	7	23	169**
	(114)	(103)	(81)
Home Renovation	-1,815	-79	-77
	(1,887)	(103)	(71)
Education Expenditure	60	6	-112
	(174)	(165)	(153)
Health Expenditure	123	33	-42
	(88)	(64)	(54)
Other Personal Expenses	163	32	85
	(151)	(106)	(75)
Debt Repayment in Past Two Weeks	371	98	-59
	(284)	(223)	(206)
Winsorized (top 1%)	N	Y	N
Trimmed (top 1%)	N	N	Y
Observations	1,388	1,388	1,374

Notes: Each cell presents the intent-to-treat treatment effect on two-week expenditures. The dependent variable is the sum of all expenditures reported in each row's category, from a question which asked respondents to detail every outflow of cash of over 1000 pesos in the past two weeks. Each regression includes controls for the bank and credit scoring band (i.e., the probability of assignment to treatment), and application month and survey month fixed effects. Results are robust to not including the fixed effects. All self-reported borrowing measures are stock measures at the time of the survey. Amounts are in Philippine Pesos (exchange rate is US$1 = 43PHP). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: OLS Treatment Effects on Expenditures During the First Two Months Post-Application

Dependent variables	(1)	(2)	(3)
Total Spending in Past Two Months	23,577	13,849	22,209**
	(17,046)	(13,643)	(8,868)
Business Expenditures in Past Two Months	20,826	11,092	18,774**
	(16,518)	(13,295)	(8,363)
Assets for Business	28	15	-45
	(229)	(154)	(94)
Merchandise for Business	19,726	9,748	17,978**
	(16,075)	(13,094)	(8,018)
Business Renovations	-561	-241	-83
	(828)	(168)	(71)
Utilities for Business	237	26	117
	(382)	(235)	(174)
Salaries for Employees	584	195	-172
	(500)	(374)	(316)
Other Business Expenses	813	46	-160
	(525)	(274)	(252)
Household Expenditure in Past Two Months	699	-63	457
	(1,746)	(1,204)	(901)
Household Items	287	345	273
	(503)	(349)	(275)
Utilities for Home	-32	-47	30
	(225)	(207)	(185)
Home Renovation	1,065	-25	-196
	(1,254)	(284)	(136)
Education Expenditure	386	288	147
	(283)	(268)	(247)
Health Expenditure	-767	-43	-3
	(874)	(213)	(132)
Other Personal Expenses	164	2	17
	(432)	(264)	(198)
Debt Repayment in Past Two Months	1,719	622	387
	(1,618)	(1,087)	(775)
Winsorized (1%)	N	Y	N
Trimmed (1%)	N	N	Y
Observations	1,095	1,095	1,084

Notes: Each cell presents the intent-to-treat treatment effect on two-month expenditures. The dependent variable is the sum of all expenditures reported in each row's category, from a question which asked respondents to detail every outflow of cash of over 1,000 pesos in the past two months. Each regression includes controls for the bank and credit scoring band (i.e., the probability of assignment to treatment), and application month and survey month fixed effects. Results are robust to not including the fixed effects. All self-reported borrowing measures are stock measures at the time of the survey. The two-month survey did not ask about borrowing, administrative data about borrowing is the same data that is used in Table 3 and so not reported here but results are substantively equivalent. Amounts are in Philippine Pesos (exchange rate is US$1 = 43PHP). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix 1: Survey Questions

1A – Bank Interaction

Panel A: Will you use 5,000 pesos or more of your loan on any single transaction for your household?
Panel B: Will you use 2,500 pesos or more of your loan to pay down other debt?

1B – 1st Loan Payment & 2 Week Survey

Panel A: Did you use 5,000 pesos or more of your loan on any single transaction for your household?
Panel B: Did you use 2,500 pesos or more of your loan to pay down other debt?

1C – List Randomization

Panel A:

Short Version:

As with our example, I will now read five statements. I would like you to tell me how many are true for you, but do not tell me which ones are true.

1. I have a washing machine in my home.
2. I am originally from this city.
3. I have completed one year or more of formal education post-high school.
4. My household owns a computer.

.

Long Version:

As with our example, I will now read five statements. I would like you to tell me how many are true for you, but do not tell me which ones are true.

1. I have a washing machine in my home.
2. I am originally from this city.
3. I have completed one year or more of formal education post-high school.
4. My household owns a computer.
5. I used 5,000 pesos or more of my loan on any single transaction for my household.

Panel B:

Short Version:

As with our example, I will now read five statements. I would like you to tell me how many are true for you, but do not tell me which ones are true

1. I have visited a hospital or clinic in the last six months.
2. I have more than three siblings.
3. I have purchased some type of insurance in the past five years.

4. My household owns an air conditioner.

Long Version:

As with our example, I will now read five statements. I would like you to tell me how many are true for you, but do not tell me which ones are true

1. I have visited a hospital or clinic in the last six months.
2. I have more than three siblings.
3. I have purchased some type of insurance in the past five years.
4. My household owns an air conditioner.
5. I used 2,500 pesos or more of my loan to pay down other debt.

1D- 2 Week Survey

Please list all transactions of 1,000 pesos or more that you have made in the last 14 days. List each item with the amount that you spent.

1E – 2 Month Survey

Panel A: In the past two months, did you spend 5,000 pesos or more on any single transaction for your household?
Panel B: In the past two months, did you spend 2,500 pesos or more to pay down debt?

1F- 2 Month Survey

Please list all transactions of 1,000 pesos or more that you have made in the last two months. List each item with the amount that you spent.

Table A1: Orthogonality of List Randomization Group to Applicant Characteristics

Purpose of specification:	Means	Orthogonality Test for Debt List Randomization	Orthogonality Test for HH Expenditure List Randomization
Dependent Variable:	Means	Assigned to Large List = 1	Assigned to Large List = 1
	(0)	(1)	(2)
Male	0.157	0.070	0.003
	(0.352)	(0.051)	(0.050)
Marital Status -- Married	0.749	-0.078	0.066
	(0.411)	(0.053)	(0.054)
Marital Status -- Widowed/Separated	0.109	-0.121*	0.042
	(0.293)	(0.073)	(0.074)
Education -- Some College	0.259	0.088*	0.037
	(0.440)	(0.048)	(0.048)
Education -- Graduated High School	0.338	0.020	-0.011
	(0.470)	(0.045)	(0.045)
Education -- Some High School or Less	0.106	0.055	-0.006
	(0.293)	(0.065)	(0.065)
Primary Business Location -- Residential	0.609	-0.023	-0.006
	(0.470)	(0.042)	(0.042)
Primary Business Arrangment -- Rent	0.27	0.045	0.051
	(0.440)	(0.044)	(0.045)
Primary Business Type - Small Grocery/Convenience Store	0.507	-0.037	-0.051
	(0.499)	(0.051)	(0.051)
Primary Business Type - Manufacturing	0.019	-0.223*	-0.172
	(0.146)	(0.124)	(0.136)
Primary Business Type - Vending	0.118	-0.078	-0.031
	(0.323)	(0.065)	(0.065)
Number of Employees	-0.019	0.007	0.011
	(1.081)	(0.014)	(0.012)
Age	0.04	0.002	0.005
	(0.940)	(0.020)	(0.020)
Years Primary Business in Business	0.037	-0.009	-0.022
	(0.999)	(0.018)	(0.018)
Primary Business Weekly Cashflow	-0.017	-0.015	-0.024
	(0.911)	(0.022)	(0.020)
Number of Dependents	0.001	0.024	0.021
	(0.969)	(0.019)	(0.018)
Assigned to Treatment Group		0.534	0.509
P-Value on joint F-test: all RHS covariates listed=0?		0.365	0.537
Observations	864	864	864

Notes: Column (0) reports the means and standard deviation of each variable. All other columns are OLS regressions with Huber-White standard errors in parentheses -- * significant at 10%; ** significant at 5%; *** significant at 1%. Sample frame contains 864 marginal applicants in the treatment group that were subjected to the list randomization questions. Other regressors (not shown) are the randomization conditions (credit score cut-offs), bank, appication year/month, survey year/month, as well as indicator variables for wholesale and service primary business types, both being small and insignificant. 'Single' is the omitted marital status category. 'College graduate' is the omitted educational attainment variable. Commercial is the omitted primary business location variable. 'Own' is the omitted primary business property arrangement. 'Other retail' is the omitted primary business type variable. The five non-binary variables (number of employees, age, years in business, weekly cashflow, number of dependents) are standarized to have mean equal to zero and standard deviation equal to one.