Delivering Financial Services to the Poor: Constraints on Access Take-Up, and Usage

Delivering Financial Services to the Poor: Constraints on Access, Take-up, and Usage

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Abstract

A majority of the world's poor lack access to basic financial services, leaving them with limited economic capabilities. Improving financial inclusion brings many challenges due to friction-driven constraints on service delivery and social and behavioral constraints on usage. This dissertation examines three innovations designed to improve the efficiency of delivering financial services to the poor. The first chapter investigates the effects of digitizing group microfinance transactions using a field experiment in rural Philippines. The intervention reduced savings by 20% among existing microfinance members. Much of these effects are driven by weakened group cohesion and sensitivity to transaction fees, highlighting the potential importance of interactions between technology and the social environment. The second chapter evaluates the impact of a partial credit subsidy program—a policy widely used to improve credit access among small enterprises—in Indonesia using a difference-in-difference framework. While the program expanded the usage of formal credit to 1.9% of the households nationwide that would otherwise not have received formal credit, I find no short-term effect on enterprise activities. The third chapter uses a field experiment to examine take-up and usage of a social health insurance scheme among Filipino microcredit borrowers. I find strong evidence for the co-presence of adverse and advantageous selection. Despite higher insurance coverage two years later, the intervention did not increase insurance utilization.

Contents

List of Tables

List of Figures

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To my family

Introduction

Financial services and products facilitate resource transfers across time and space. We save and borrow to smoothen consumption over time, or to take on lumpy investment opportunities. We mitigate the risks of financial loss due to possible adverse events through insurance schemes. This ability to transfer resources affects decision-making in every domain of our lives. Improving access to financial services, therefore, has been one of the pillars of global development agenda. The microfinance movement gained increasing popularity throughout the 1990s and 2000s, reaching over 100 million people in 2008 (Microfinance Information Exchange). In 2006, in the height of global enthusiasm for microfinance, Muhammad Yunus, the founder of the Grameen Bank in Bangladesh, received the Noble Peace Prize.

Empirical data and research, however, provide a nuanced picture. A majority of the world's poor still lack access to basic financial services. According to Findex in 2014, more than half of adults in developing countries had no formal account, relying on local moneylenders and informal social networks to manage their financial lives. More importantly, a series of experimental studies on the impacts of financial access shows that access to savings and credit products benefit the poor but do not bring transformative changes (Banerjee et al., 2015a). With the increasing recognition that microfinance is not a silver bullet for poverty reduction, the policy discussion now focuses around what design features could augment potential impacts and reduce specific constraints in improving access, take-up, and usage of financial services.

Economic theories traditionally point to friction-driven constraints as main barriers to the efficient delivery of financial services. With an irregular and uncertain cashflow, the poor make small and frequent transactions, leading to high transaction costs. Information barriers due to lack of documentation (or credit history) further increases the cost of determining their risks. In addition, a low level of assets, and therefore limited collateral, gives providers limited enforcement power. These factors all contribute to market failures in the financial services for the poor. More recent literature, however, underscores the importance of "scarcity-driven" (Ghatak, 2015), social and behavioral barriers in financial decision-making (Dupas and Robinson, 2013b; Karlan et al., 2013).

This dissertation examines three innovations that aim at reducing friction-driven constraints—specifically transaction costs, limited liability, and information asymmetry—in delivering financial services to the poor. With this in mind, two of my chapters pay particular attention to how these innovations might interact with social and behavioral constraints the poor face. The goal of my research is to contribute to the evolving discussion on how, in what conditions, and for whom financial access could bring benefits and provide insights on the product designs.

Chapter One examines the use of digital technology in banking services. I examine the effects of digitizing microfinance transactions among group savers in rural Philippines. Rapid growth of digital financial services fuels an anticipation among policymakers that the new technology could dramatically reduce transaction cost barriers and improve financial inclusion among the poor. Using a matched-pair cluster randomized controlled trial, I show that digitization increased convenience but *reduced* savings by 20% among existing group microfinance clients over two years. These effects are driven by the weakened peer effects of group banking and increased sensitivity to transaction fees (despite the decline in the overall transaction costs). My results on peer effects highlight the potential tension between new technology and the existing social architecture.

Chapter Two examines the effects of a partial credit guarantee scheme in Indonesia, called *Kredit Usaha Rakyat* (KUR). Credit guarantees are a popular policy tool for expanding credit access among credit-constrained entrepreneurs. These schemes provide banks a partial insurance for defaults on the loans given to a target population—often micro, small, and medium enterprises that may not have formal credit access due to lack of collateral. My analysis shows that KUR resulted in the usage of formal business credit among additional 1.9 percent of the population, or over 1 million entrepreneurs, between 2008 and 2009. While this implies a 54% increase in the usage of formal business credit relative to the sample mean immediately prior to the program implementation, these loans account only for 49% of all guaranteed loans. This finding raises a question about the cost-effectiveness of the guarantee scheme.

Chapter Three explores the delivery of a social health insurance scheme through a rural bank in the Philippines. Even though many governments promote universal coverage, providing insurance coverage among low-income households in the informal sector faces many challenges due to high transaction costs and information asymmetry. A microfinance institution (MFI), a trusted local financial institution, could potentially deliver insurance policies more effectively at a lower cost than the traditional insurance provider. This study evaluates the relative effects of compulsory and voluntary health insurance policies among existing individual-liability borrowers. While the intervention significantly increased insurance coverage over 2.5 years, the insurance utilization remained low even among those who received the care covered by PhilHealth. I provide tentative evidence that mandating health insurance enrollment through a third-party entity may reduce motivation for utilizing the insurance policy.

Chapter 1

Effects of Digitization on Financial Behaviors: Experimental Evidence from the Philippines

1.1 Introduction

Digital technologies provide fast and cheap means of exchanging goods and services, and they are rapidly changing the global payments landscape. This effort has been particularly pronounced in financial services, where a large fraction of the poor face costly access. Digitization could dramatically reduce transaction costs of delivering financial services for both users and providers, potentially accelerating access and usage around the world. With the success of the mobile money industry in a handful of countries,[1] the optimism around digital financial services is growing. Donors and governments increasingly direct resources toward digitization as a promising path to improving the financial capabilities of the poor.

Digitization may, however, affect the social contexts of financial behaviors within traditional financial services. For instance, microfinance institutions typically leverage social

[1] Seven Sub-Saharan African countries have a mobile money account ownership above 20% (Findex in 2014), and twelve out of 271 mobile money services providers have reached 1 million active users (GSMA, 2015).

capital among community members to overcome the lack of information in client selection, monitoring, and enforcement. Many programs offer a communal venue where members regularly meet and engage in banking transactions as a group. Studies show that existing social connections among clients facilitate effective monitoring and enforcement (Karlan, 2007), and growing social capital through repeated interactions at regular meetings improves loan payments, even without joint-liability, by fostering cooperation (Feigenberge et al., 2010). If digitization replaces the communal transaction process with more convenient but non-communal transactions, it may disrupt the existing social architecture of group banking that reinforces positive financial behaviors. Therefore, the net effects on financial behaviors, and the cost-efficiency for the provider, are ambiguous.

This paper uses an experimental evaluation to investigate the effects of digitizing group microfinance transactions on financial behaviors. In 2013, a rural bank ("the Bank") in the Philippines introduced mobile banking to 299 members in 7 existing microfinance centers. The Bank selected the pilot centers using a matched-pair randomization, allowing an internally valid assessment of the program with 575 members in 14 centers, all of whom were individual-liability borrowers or savers who were not borrowing. Under the status quo, members deposited through regular meetings in their villages and withdrew at bank offices in town centers. In treatment areas, mobile banking was introduced, and account officers from the Bank no longer accepted cash payments during regular meetings. Instead, members individually made mobile loan payments, deposits, and withdrawals through corner stores in village centers for small fees. This universally increased the convenience of transactions. In addition to increased flexibility of transactions through the stores, conservative estimates based on survey data suggest a time saving of 30% for a deposit and 70% for a withdrawal transaction. This new process also allowed members to make savings and loan payments without the presence of peers.

Three notable findings emerge from my analysis. First, the introduction of mobile banking, on average, resulted in a 20% decline in the average daily balance and a 25% decline in the likelihood of weekly deposits over two years. This large decline in savings has important implications for the service provider, which relies on savings as a cheap source of financing. Second, I observe heterogeneous responses by the proximity to transaction points prior to digitization (i.e., center meeting and bank offices). Among the members who lived far from transaction points, the intervention did not have a significant effect on savings accumulation. In contrast, among the members who lived close to transaction points, mobile banking lowered the usage of financial products at the Bank, reducing deposit and withdrawal frequencies by over 15% and loan usage by 5%. As a result, their average daily balance declined by 28%. Third, the follow-up survey provides suggestive evidence that the heterogeneous decline in savings was driven by weakened peer effects of group banking and increased fee sensitivity.

Specifically, among members near transaction points, mobile banking significantly lowered group cohesion, which I measure using an index of self-reported center meeting attendance, interactions with members and bank staff, and the perceived importance of center performance. Given that this effect emerged before account usage differentially declined for members near transaction points, I can rule out the possibility that group defection was mediated by declined account usage. Rather, the observed heterogeneous effects appear to reflect the effects of differential connection to microfinance centers at baseline. In the control group, members near transaction points presented stronger group cohesion and also saved more than those living farther away, implying that these members are potentially more susceptible to peer effects (and weakened peer effects). These findings suggest that mobile banking, at least in part, resulted in lower account usage via reduced group cohesion.

With regards to fee sensitivity, treated members near transaction points were 34% more likely than their counterparts in control centers to report avoiding frequent deposits due to costs in the follow-up survey. Mobile transaction fees are small in value. An average member in the treatment group paid P4 in transaction fees for a deposit; an average member in the control group paid P5 to cover the travel expense of one member assigned to bring the collected payments to the bank office after each meeting. These fees did not vary across members within the same center. The sharp differential decline in deposit frequency, therefore, is unlikely due to an increase in the actual financial cost of the transaction. Explicit transaction fees, however, may have increased the *salience* of deposit costs. Existing evidence demonstrates that increasing the convenience of deposits could lead to higher savings accumulation, suggesting that the opportunity cost of deposits influences one's savings decisions (Ashraf et al., 2006a; Callen et al., 2014; De Mel et al., 2013).[2] Little is known, however, about whether and how much individuals are willing to pay for a marginal increase in convenience. In this study, savers who had easy access to transaction points at baseline showed strong price sensitivity to increased convenience. This adds to the existing evidence on price sensitivity among the poor, shwing that a small price could significantly dampen the take-up of a range of health and education products among low-income households in developing countries (see Holla and Kremer (2009) for a review).

Beyond the changes in account usage at the Bank, I also examine the potential implications of mobile banking for household financial outcomes.[3] Two and a half years later, treated members who lived far from transaction points continued to save at the Bank, and reported somewhat higher use of savings and receipt of assistance from friends during shocks than their counterparts in the control group. At the same time, they were somewhat more likely to be a net giver—giving more loans and transfers to friends than receiving them from friends on a day-to-day basis—suggesting that easier savings access may have improved the coping capacities of both the treated households and their social networks. These results provide a different example of digital financial services facilitating risk-sharing than documented by Jack and Suri (2011), who show that reducing transaction costs of remittances through M-PESA increases informal transfers during negative shocks. I find that simply reducing the cost of accessing their own savings potentially increases informal

[2]Ashraf et al. (2006a) and Callen et al. (2014) find large effects of deposit collection services on savings in two different settings in the Philippines and Sri Lanka. De Mel et al. (2013) show that simply providing deposit convenience through a community deposit safe box is as effective as providing weekly home visits among savers who already have a habit of regular deposits.

[3]The decline in Bank savings and household financial decisions are likely endogenous to each other. Therefore, this analysis should not be interpreted as examining the causal effects of declines in formal savings.

risk-sharing.

In contrast, treated members near transaction points did not increase non-Bank savings and saw the total household financial assets decline by nearly 30% while reporting no change in economic activities. Consequently, they increased reliance on informal loans. Existing evidence supports that formal savings help individuals cope with shocks (Prina, 2015; Dupas and Robinson, 2013b) and reduce borrowing (Kast and Pomeranz, 2014). An important finding of my study is that even long-time savers broke a savings habit and reduced overall household savings when an exogenous change in product features discouraged the usage of an existing bank account.

To address concerns arising from the small number of microfinance centers in my sample, I conduct my analysis using inference methods whose properties are independent of the number of clusters. Throughout the paper I use a wild cluster bootstrap, which yields valid inference for a cluster size as small as five (Cameron et al., 2008). I test the robustness of the results using randomization inference, a method of hypothesis testing that generates a reference distribution through repeated randomizations of the sample clusters, and the t-statistic-based inference (Ibragimov and Müller, 2010), which exploits the long panel nature of the transaction data and relies on the asymptotics of time dimensions. My results are robust to these three methods.

This study contributes to the growing literature on financial access in developing countries. Specifically, it brings new insights into three areas. First, there is scarce evidence on how digital technologies affect the financial lives of the poor and financial services providers. The use of technology in large-scale social payments programs has been shown to improve the efficiency and effectiveness of service delivery in Niger and India (Aker et al., 2011; Muralidharan et al., 2014). In the context of traditional financial services, Jack and Suri (2011) examine the impact of expanded access to mobile money services on informal transfers. My study draws on a sample of existing users of microfinance services and investigates how digitization affects their existing financial habits. Contrary to the widely accepted premise, the negative effect of digitization on the usage of services leaves open the question of whether digitization is cost-efficient for the Bank. Second, my findings on increased group defection contribute to the literature on peer effects on savings behaviors. The role of peer effects in group savings schemes has long been documented (Basley et al., 1993; Dupas and Robinson, 2013b), and recent field experiments tease out specific channels of the peer effects (Kast et al., 2012; Breza and Chandrasekhar, 2015).[4] While I am unable to distinguish different types of peer effects, my results suggest that they remain powerful in reinforcing long-term savings habits. Third, my findings on fee sensitivity highlight the potential salience of transaction costs in financial services. Removing the fixed cost of opening an account has been shown to have a modest effect on take-up and usage (see Dupas et al. (2016) for the most recent review), but the role of transaction costs is largely understudied.

The reminder of the paper is organized as follows. In the next section, I describe the study context, intervention, and data. I outline the empirical strategy in Section 3. Sections 4 and 5 discuss the results on Bank savings and household outcomes, respectively. I discuss the cost-benefit implications in Section 6 and conclude in Section 7.

1.2 Context and intervention

1.2.1 Formal financial access in the Philippines

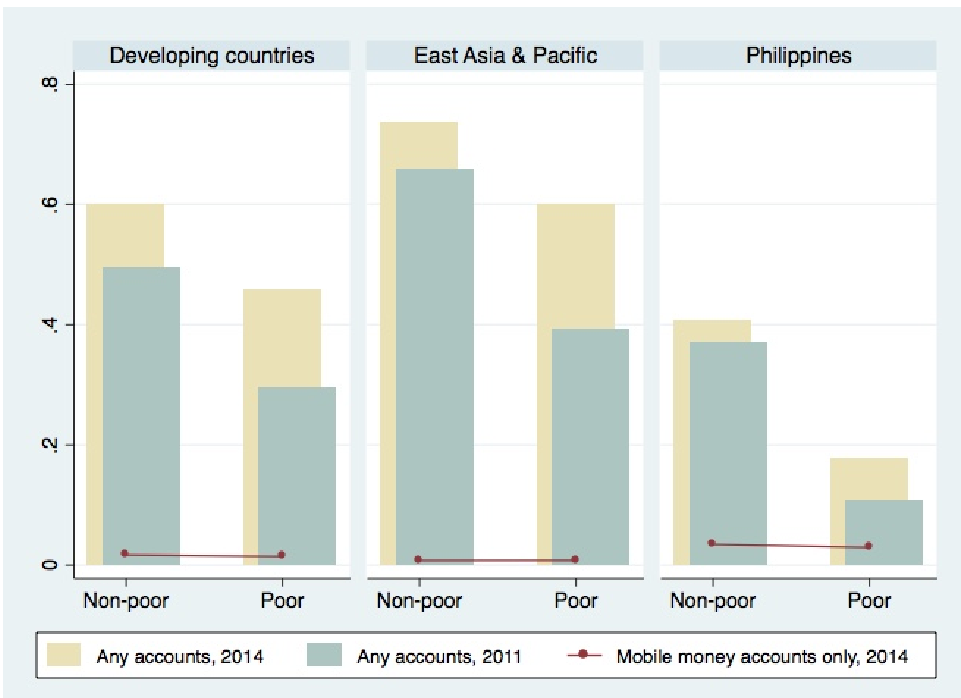
Even though the microfinance sector in the Philippines is one of the more mature markets in Asia, usage of formal financial services remains low among the poor. Findex data shows a modest 4% growth of account ownership between 2011 and 2014, compared to the 13% growth in the region and the 32% growth in developing countries (Figure 1.1). In 2014, the wealthiest 60% of Filipino adults were more than twice as likely to own an account as the poorest 40%.

[4]Kast et al. (2012) find that the information on peer performance without exerting peer pressure could generate a substantial effect on savings accumulation. Using extensive social network data, Breza and Chandrasekhar (2015) show that individuals save more when a peer who monitors her savings performance has a stronger connection with her and when the peer is more socially connected.

There are also large differences in the reported barriers to opening an account across income groups. In particular, the bottom 40% of Filipino adults are significantly more likely than others to cite high transaction costs (i.e., too costly or too far). In fact, according to the Central Bank, 37% of rural municipalities lacked banking offices with a physical presence and only 10% of rural banks offered mobile transactions in 2015 (Central Bank of the Philippines). These figures illustrate difficulties faced by the rural poor in accessing formal financial services.

Despite the high rate of mobile phone penetration in the Philippines, digital financial services have so far played a limited role in closing the gap in financial access. Individuals with mobile money without any bank account constitute 2.5% of adults, and this figure does not vary substantially by income levels (Findex 2014).

Figure 1.1: Changes in account ownership: 2011-2014 (Findex 2014)



Notes: Country averages are weighted by population size. The proportion of individuals with mobile money accounts only is calculated for a subsample of countries with the presence of GSMA.

1.2.2 Research Context

This study took place in the vicinities of two municipal towns in Laguna province, south of metro Manila. The implementing Bank offers credit, savings, and insurance products to low-income households across the country. Its flagship microfinance program offers individual-liability loans ranging from $40-$1,100 and basic savings accounts to the rural poor. Clients are organized into groups of 20-40 from the same village to form a microfinance center. Historically, the Bank focused on providing productive credit to female microentrepreneurs. Now, it lends to low-income households for a wide range of purposes and no longer monitors loan usage. Members are also allowed to stay in the program as savers after three successful loan cycles. In 2012, nearly 50% of microfinance members were savers without loans.

The Bank adopted mobile technology to increase operational efficiency with two specific goals in mind. First, by reducing the cost of providing services, the Bank planned to increase the caseload and profits per account officer and expand outreach in remote, underserved areas. Second, the Bank saw an opportunity to increase its competitiveness in the crowded microfinance market by sharing operational cost-saving with clients through reduced prices of credit and other products in the future.

1.2.3 Intervention

Overview

Table 1.1 summarizes the changes in transaction processes under mobile banking. The program allows members to access their own savings accounts using mobile phones. A member can deposit and withdraw through a storeowner (cash point agent),[5] who operates the store in a village center every day. In exchange for increased convenience, mobile

[5]To initiate a deposit, a member hands over cash to an agent. The agent then sends a text message to the mobile platform to facilitate a fund transfer from her savings account to the member's account. To initiate a withdrawal, a member sends a text message (P1) to facilitate a fund transfer from her account to the agent's account. The agent releases cash upon receiving a confirmation text message. No mobile transfers between accounts are allowed.

transactions incur small transaction fees which increase stepwise with the transaction amount.

A member is required to open an ATM account at the Bank and register her mobile phone. This account has no special feature besides flexible access through ATMs in town centers near bank offices and a lower interest rate of 1.5% per annum, instead of the 2% offered on the microfinance savings account. The ATM account was available for microfinance members before the intervention, but few had previously opened one.

The introduction of mobile banking also affected loan policies. First, in the treatment areas, the Bank disburses loan proceeds directly into the member's savings account instead of releasing in cash at a bank office. This saves the member a trip to the bank office and the need to physically transport a large amount of cash, but she now has to pay a withdrawal fee to receive the proceeds through an agent. Second, the member makes loan payments either out of a savings account using a registered mobile phone for a regular texting fee of P1, or over the counter through an agent for P4.

Finally, the Bank eliminated cash handling at center meetings upon the mobile banking implementation, cutting the average meeting time from an hour to a half hour. In the control group, one member is assigned to bring all cash payments to the bank office after each meeting, and members who make deposits and loan payments pitch in to cover her travel expenses (i.e., transportation cost and snack allowance). In the treatment group, account officers no longer accept cash payments, and all members are required to use mobile banking.

Changes in Transaction Costs for Members

Mobile banking significantly reduced transaction time for both deposits and withdrawals.[6] An average mobile transaction at the store takes 10 minutes. This implies a 30% decline in transaction time for deposits (due to shorter meeting time) and a 70% decline for

[6] The figures are based on the data on time allocation of account officers and time spent on transactions among members after the intervention.

Table 1.1: Summary of changes in transaction processes under mobile banking

	Control	Treatment
1. Savings policies		
Deposit process	Cash deposit at center meetings or at bank branch	Through village agent
Deposit fees	No direct fee, but contribution to remitter's travel expenses (avg. P5, or 11 cents)	Transaction fee of P4 (10 cents) for a deposit < P500 ($11).
Withdrawal process	At bank branch during banking hours	Through village agent or ATM
Withdrawal fees	No direct fee	Transaction fee of P11 (24 cents) for a withdrawal < P1000 ($22) and SMS fee of P1 (2 cents)
Balance inquiry	Passbook regularly updated	Mobile inquiry for P1
Account type	Regular account with 2% interest per annum	ATM account with 1.5% interest per annum
2. Loan policies		
Disbursement	Cash disbursement in bank branch	Disbursement into savings account
Repayment process	Cash payment at center meetings	Mobile payment out of savings account or through village agent
Repayment fees	No direct fee, but contribution to remitter's travel expenses (avg. P5, or 11 cents)	SMS fee of P1 (for mobile payment) or transaction fee of P4 (for payment through village agent)
3. Other changes		
Center meetings	1 hour on average (Payment/deposit collection, new bank services/products, business management, etc.)	30 minutes on average (New bank services/products, business management, etc.)

withdrawals.[7] These changes correspond to the opportunity cost saving of P17 and P43, respectively, using the provincial minimum wage of P350/day(≈$7.78).

Financial cost also declined for most clients under mobile banking. During the study period, 66% of deposit transactions were under P500 (≈$11) and charged P4 (≈8 cents) in fees, slightly lower than the average contribution toward the remitter's travel expenses before the intervention. Roughly 60% of withdrawal transactions were under P1,000 and charged P11 in fees, substantially lower than the average one-way travel cost to the nearest

[7] The Bank only processes in-branch transactions in bulk in the afternoons. A control member on average spends more than an hour for a withdrawal transaction.

bank office (P21). However, it is difficult to estimate the precise financial cost-saving because most members withdraw at the bank office when they have other reasons to be in town centers.

In the qualitative interviews, 15 out of 29 members with active accounts explicitly mentioned that mobile banking was time-saving and more convenient. Some members talked about spending less time in center meetings, and other members mentioned the option value of mobile banking in cases of emergency—they can access savings without having to travel to the town during banking hours. On the other hand, 5 members indicated that they preferred the manual system because of mobile banking fees. Overall, these interviews suggest that there was heterogeneity in members' views on the value of mobile banking, but the majority of members found it to be time- and cost-saving.

1.2.4 Conceptual Framework

Traditional economic theories predict that reducing transaction costs would increase efficiency and usage of financial services.[8] In the presence of social and behavioral constraints, however, transaction costs may not always act as inefficient frictions, or they may not be accurately internalized.

• Costly withdrawals as a commitment feature: The literature on savings in developing countries has shown that individuals face various control problems—self, other, and spousal—over savings (Ashraf et al., 2006b; Schaner, 2013a). In such an environment, costly withdrawals may help individuals overcome immediate constraints and achieve long-term savings goals. Making savings more accessible through digitization may therefore increase overspending, leading to higher withdrawals and lower balances.

• Rigid schedule of deposits and payments: Many microfinance programs offer a rigid schedule of deposits and payments. This system is designed to lower the cost of payment

[8]For example, the Baumol-Tobin model of transaction costs shows that lower withdrawal costs for an agent consuming a fixed amount of savings over his lifetime would lead to smaller and more frequent withdrawals and higher average daily balances because of larger cumulative interest earnings (Baumol, 1952; Tobin, 1956). In practice, however, the latter effect is likely small in my study setting where the average savings level is relatively low.

collection for the provider, but insights from behavioral economics suggest that it also benefits users by reducing the cognitive burden of saving. Without a pre-determined day and time of transaction, flexible deposit opportunities require an active decision about when to make a deposit, potentially increasing the cognitive cost of saving and lowering deposit frequencies and savings balances.[9]

- Peer effects of group banking: The communal banking system could encourage positive financial behaviors through many forms of peer effects. Being observed, one may feel pressured to save in order to maintain reputation (peer pressure). Observing the decisions of others, one may learn the behavioral norm of the group and conform to it (peer information). Even in the absence of peer pressure and information, simply the presence of others could stimulate consciousness and attention, facilitating the co-action effects (Zajonc, 1965). By removing cash handling from meetings, mobile banking makes savings and payment decisions less visible to peers and lowers the motivation to attend center meetings. These changes could disrupt the social architecture of group banking and weaken the peer effects, reducing deposit frequencies and savings balances.

- Salience of transaction fees: In the control centers, the financial costs of transactions were in the form of transportation fees. In the treatment centers, members were explicitly charged for processing a transaction. Even though such fees are small in value, their explicit nature may increase the salience of deposit costs, creating a new psychological barrier to making deposits.[10] This effect would also lower savings accumulation through reduced deposit frequencies.

These different effect mechanisms do not provide cleanly distinguishable predictions for changes in deposit and withdrawal behaviors. I will first assess the overall effects on savings at the Bank using the administrative data, and then examine potential channels

[9]The power of planning in task completion has been empirically demonstrated in many contexts (**?**Choi et al., 2012; Rogers et al., 2015).

[10]The empirical evidence on sensitivity to the salience of fees also exists in various contexts, including value-added taxes for daily consumption goods, toll rates for drivers, and bank overdraft fees (Finkelstein, 2009; Chetty et al., 2009; Stango and Zinman, 2014).

using the follow-up survey.

1.3 Experimental Design

1.3.1 Randomization and Timeline

The intervention took place in communities served by two bank offices located near the Bank's head office. The Bank first matched centers in pairs by account officer, travel time from bank office, and loan performance, and then randomly selected one pair of centers for each of the seven account officers in the study sample (Appendix B provides a detailed description of the sample selection). The mobile banking treatment was randomly assigned within each center pair. The final sample of this study consists of 575 active microfinance members in 14 centers as of September 2012.

Banking agents in 7 treatment centers were recruited and trained in October-November 2012, and the mobile banking system was launched in January 2013. The Bank adhered to the original treatment assignment for the first 15 months. In April 2014, it introduced mobile banking in one of the bank offices, which affected three out of seven control centers in the study sample. This was part of the larger roll-out—the Bank started introducing mobile banking in other areas before the pilot evaluation was completed and mistakenly included the three control centers in this roll-out.

1.3.2 Data

I use three sets of data to examine the effects of mobile banking. First, I use the administrative data from the Bank, including the basic membership information and all savings transaction and loan disbursement records between January 2012 and December 2014. I construct a balanced panel of weekly savings and loan outcomes for all members in the study.[11] Second, I use the survey data collected by the Bank three months after the mobile banking

[11] If a member closed the account and dropped out of the program, all savings outcomes for the remaining weeks are coded as zeros. Treating these observations as missing does not affect the results.

implementation (Bank survey). This survey gathered information on interactions among members, meeting attendance, and attitudes toward center performance. Finally, I use the household survey data collected in July-August 2015 (follow-up survey). In this survey, I collected retrospective data on travel time, cost, and distance to the center meeting and bank office locations to construct a measure of proximity to transaction points. I also gathered information on current financial attitudes and conditions to analyze the long-term effects of mobile banking. Out of the original sample of 575 members, I identified 521 who still lived in the two municipalities of the study area and conducted the survey with 448 members (a reach rate of 86%). There is no statistically significant difference between the treatment and control centers in either the survey inclusion rate (90.9% in the control and 90.3% in the treatment centers) or the survey completion rate (86.5% in the control and 85.2% in the treatment centers). Even though women are somewhat more likely to complete the follow-up survey, I show in Appendix Table A1 that there is no differential attrition by observable demographic characteristics.

1.3.3 Randomization Verification and Sample

Table 1.2 presents differences in baseline characteristics between the treatment and control groups. I report the results for the full sample in Panel A and for the subsample of members who completed the follow-up survey in Panel B. Columns 1-4 show the average differences in weekly savings and loan outcomes over 1 year prior to the intervention using a time-series model with week-year and center-pair fixed effects. Columns 5-7 show the treatment-control differences in baseline demographic characteristics using a cross-sectional OLS model with center-pair fixed effects. The coefficients are neither quantitatively nor qualitatively distinguishable from zero, suggesting that the experimental groups are well-balanced.

Control means reported at the bottom of each panel illustrate the sample characteristics. A majority of the sample members are women, and forty percent are savers who had no active loan for at least 6 months prior to the intervention. The average savings balance of P3,023 ($\approx$ \$67) equals 1-2 weeks' worth of microenterprise sales among typical Bank

Table 1.2: Randomization Verification

	Weekly savings and loan outcomes						Demographic characteristics		
	Average daily balance								
	Winsorized	Log value	Deposit likelihood	Withdrawal likelihood	Active loan	Active savings account	Age	Female	Regular borrower
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Full sample									
Treatment	68.539	0.030	-0.006	-0.003	0.003	-0.009	-0.087	0.004	0.007
	(238.548)	(0.062)	(0.024)	(0.005)	(0.054)	(0.006)	(0.692)	(0.027)	(0.068)
	[0.907]	[0.790]	[0.924]	[0.703]	[0.981]	[0.452]	[0.928]	[0.932]	[0.960]
Control mean	2945.213	8.315	0.862	0.069	0.449	0.989	41.699	0.895	0.598
(standard deviation)	(3200.55)	(0.863)	(0.345)	(0.253)	(0.497)	(0.103)	(12.092)	(0.307)	(0.491)
Number of observations	29,899	29,899	29,899	29,899	29,899	29,899	575	575	575
Panel B. Members who completed the follow-up survey									
Treatment	74.421	0.033	-0.003	-0.003	0.008	-0.007	-0.104	0.011	0.006
	(309.753)	(0.085)	(0.023)	(0.003)	(0.052)	(0.006)	(0.818)	(0.019)	(0.073)
	[0.907]	[0.828]	[0.950]	[0.601]	[0.930]	[0.492]	[0.938]	[0.746]	[0.971]
Control mean	2900.501	8.301	0.867	0.072	0.489	0.992	42.885	0.904	0.642
(standard deviation)	(3156.95)	(0.861)	(0.339)	(0.259)	(0.500)	(0.091)	(12.103)	(0.296)	(0.480)
Number of observations	23,359	23,359	23,359	23,359	23,359	23,359	448	448	448
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No

This table reports the pre-intervention differences in savings outcomes and demographic characteristics between the experimental groups. Columns 1-5 report the average treatment-control differences in Bank savings outcomes over one year prior to the intervention, estimated using Equation 1 where *Post* = 0 for all observations. Columns 6-8 report the cross-sectional differences in member characteristics at baseline. *Average daily balance* in Column 1 is winsorized at the 99th percentile within each week; the log value of daily average balance in Column 2 uses the natural log transformation. *Active loan* indicates any oustanding loan. Active savings account indicates at least one deposit or withdrawal within the previous 90 days. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-values in braket under each coefficient. Regular borrower is defined as those who had any active loan over 6 months prior to the introduction of mobile banking.

members in this area. Members generally had a regular deposit habit at baseline—in an average week, 86% of members made a deposit and 7% withdrew from the account.

Appendix Table A2 reports additional baseline characteristics of the study sample, gathered retrospectively in the follow-up survey. Most members have relatively easy access to the center meeting location: an average member lives within 1km of the center and it takes 11 minutes to travel. Bank offices are farther away: The average one-way trip takes 24 minutes and costs P21, implying that a trip to the bank office involves multiple jeepney and tricycle rides. Even though the mean distance to the bank office is lower for the treated members, the proximity index—index of time, distance, and cost to the transaction points—is not significantly different between the experimental groups.

1.3.4 Empirical Specifications

To assess the impact of mobile banking on savings outcomes at the Bank, I estimate the following difference-in-difference model:

$$Y_{icmt} = \alpha + \beta(T_c \cdot Post) + \gamma T_c + \delta_t + \theta_m + \epsilon_{icmt} \tag{1.1}$$

where i denotes the individual, c the center, m the center pair, and t the week-year. T_c is the center-level treatment indicator, $Post$ is the indicator for post-intervention weeks, δ_t are the time fixed effects, and θ_m are the center-pair fixed effects. The standard errors are clustered at the center level. Since the random assignment ensures that T_c is uncorrelated with the error term, β measures the unbiased intent-to-treat (ITT) effect of mobile banking, the average difference in the post-intervention outcome between the treatment and control centers compared to the average difference before the intervention.

The Bank introduced mobile banking in three out of seven control centers in April 2014. To take this into account and examine the effects of exposure to mobile banking, I also estimate the following 2SLS model:

$$Y_{icmt} = \alpha_2 + \beta_2 M_{ct} + \gamma_2 T_c + \delta_{t2} + \theta_{m2} + \epsilon_{icmt} \tag{1.2}$$

where M_{ct} indicates centers with mobile banking at time t. This variable takes the value of 1 for all post-intervention weeks in treated centers and for post-April 2014 weeks in the three control centers that received mobile banking. In the first stage, I regress M_{ct} on the interaction between the original treatment assignment and the indicator for post-intervention weeks:

$$M_{ct} = \alpha_1 + \beta_1(T_c \cdot Post) + \gamma_1 T_c + \delta_{t1} + \theta_{m1} + v_{icmt} \tag{1.3}$$

$T_c \cdot Post$ is the excluded instrument. The identifying assumption here is that the average change in the outcome I observe operates only through the adoption of mobile banking. The treatment-on-the-treated coefficient β_2 identifies the local average treatment effect (LATE), or the causal effect of mobile banking among complying centers. I report the estimates on β_2 in Appendix Table A3.

Equation 1 estimates the average treatment effect over two years. I also examine the changes in savings outcomes over time by modifying Equation 1 and estimating the quarterly treatment effects in the following model:

$$Y_{icmt} = \alpha_3 + \sum_{q=1}^{8} \beta_q(T_c \cdot Post_q) + \gamma_3 T_c + \eta_3 T_{ct} + \delta_{t3} + \theta_{m3} + \epsilon_{icmt} \tag{1.4}$$

where $Post_q$ denotes the post-intervention quarter (1 = 1st quarter of 2013, 2 = 2nd quarter of 2013, etc.). Changes in β_q could provide some insights on potential effect mechanisms.

Finally, to measure the impact of mobile banking on household behaviors, I compare the post-intervention outcomes of interest in the following cross-sectional OLS model:

$$Y_{icm} = \alpha + \beta T_c + \theta_m + \epsilon_{icm} \tag{1.5}$$

where Y_{icm} is the survey outcome for individual i.

1.3.5 Small Cluster Tests

With only seven pairs of centers in the sample, clustered standard errors may be subject to the few-cluster bias. To address this concern, I use three methods of inference. First, I calculate wild cluster bootstrap p-values. Wild cluster bootstrap allows inferences for small cluster samples by applying cluster-specific weights to the sample residual vectors in each bootstrapping iteration, most commonly using the Rademacher distribution.[12] Second, I use randomization inference and test the null hypotheses using the distribution of the estimates obtained from $2^7 = 128$ permutations of random assignment within 7 matched pairs of centers (Rosenbaum, 1996; Greevy et al., 2004). Note that randomization inference produces somewhat conservative p-values when the total number of permutation is relatively small: with 128 permutations, p-values will be in increments of 1/128 = 0.0078. Third, I take advantage of the long time-series transaction data in the analysis of savings outcomes at the Bank and use the t-statistic approach developed by Ibragimov and Müller (2010). For this inference, I first estimate the change in the outcome after the intervention for each cluster and then obtain p-values using the t-test for two-paired sample mean comparisons with 6 degree of freedom.[13] I present wild bootstrap p-values using 5,000 bootstrap repetitions in the main tables and report all the test results in Appendix Table A9, confirming that three methods yield similar p-values.

1.4 Effects on Savings and Loan Outcomes at the Bank

1.4.1 Average impact on Bank savings and loan usage

Figure 1.2 provides the visual comparison of the trends in savings between the treatment and control centers. Before January 2013, the trends of the two groups closely follow each other. The daily average balance in the treatment centers starts falling behind immediately after the mobile banking implementation. Then, the trend in the control group breaks in April 2014 when three out of seven control centers received mobile banking. In July 2014, a typhoon affected the study area. Even though 50% of the treated members and 44% of control members reported this event as an economic shock to the household in the follow-up

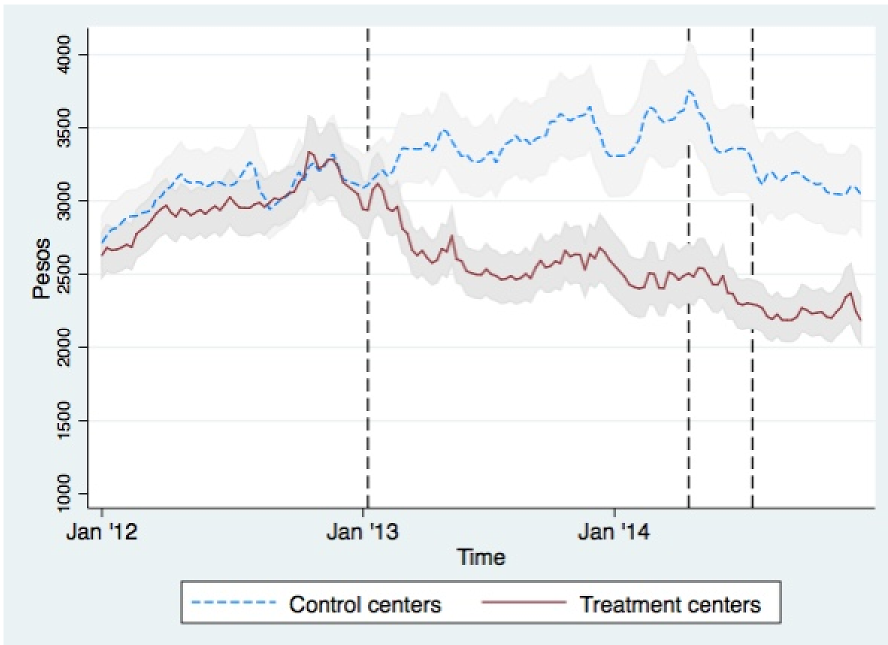
[12] Rademacher weights use $+1$ at probability $1/2$ and -1 at probability $1/2$. Since the weights are applied at the cluster-level, there are $2^{14} = 16,834$ resampling variations in my sample.

[13] Ibragimov and Müller (2010) show that this method produces correct inferences even in the presence of serial correlations across time periods.

survey, there is no visual indication of a change in the savings trends.

I formally estimate the causal effects of mobile banking using Equation 1. Table 1.3

Figure 1.2: Trends in average daily balance
Treatment vs. control centers



Notes: The solid red line follows the changes in weekly savings balance for the treatment centers and the blue dashed line follows the control centers. Gray shaded areas represent standard errors. Mobile banking was implemented in January 2013 (first vertical line), three control centers received mobile banking in April 2014 (second vertical line), and a typhoon in July 2014 affected a larger proportion of treated than control members (third vertical line).

Panel A presents the estimates of β for the first fifteen months before mobile banking was introduced to three control centers. The results confirm the visual trends in Figure 1.2. The intervention resulted in a 20% decline in average daily balances—the estimates are consistent between Columns 1 and 2, the winsorized value at the 99th percentile within each week and the natural log value, respectively. The decline in average daily balances is accompanied by a 25% decline in the likelihood of deposits.[14] Panel B presents the treatment effects over 24

[14]Note that the changes in the loan disbursement and payment policies under mobile banking mechanically affect the deposit and withdrawal outcomes in the treatment centers. To account for this, I construct adjusted measures comparable between the treatment and control centers. Deposit likelihood indicates the weeks in which a member makes excess deposits beyond loan and insurance payments, and withdrawal likelihood indicates the weeks in which a member withdraws beyond loan proceeds disbursed within the previous four

months. The estimates on average daily balances and the likelihood of deposits change little but are likely attenuated due to the mobile banking expansion into three control centers in later months. In Appendix Table A3, I show that the LATE estimates over 24 months are 20% larger than the 15-month estimates.

Table 1.3: Impact on Bank savings and loan outcomes
Sample: Full sample

	Average daily balance		Deposit likelihood	Withdrawal likelihood	Active loan	Active savings account
	Winsorized	Log value				
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. ITT estimates on the effect over 15 months						
Treatment x Post	-622.878**	-0.220	-0.188***	0.006	0.036*	-0.022
	(221.976)	(0.119)	(0.033)	(0.006)	(0.017)	(0.026)
	[0.025]	[0.103]	[<0.001]	[0.335]	[0.050]	[0.431]
Number of observations	66,124	66,124	66,124	66,124	66,124	66,124
Control mean (post-intervention)	3220.136	8.214	0.803	0.073	0.390	0.932
Panel B. ITT estimates on the effect over 24 months						
Treatment x Post	-681.164**	-0.206	-0.198***	-0.004	0.013	-0.040
	(267.389)	(0.133)	(0.037)	(0.005)	(0.022)	(0.031)
	[0.028]	[0.169]	[<0.001]	[0.555]	[0.585]	[0.232]
Number of observations	88,549	88,549	88,549	88,549	88,549	88,549
Control mean (post-intervention)	3170.87	8.126	0.768	0.075	0.396	0.908
Week-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table presents the treatment effects on savings and loan outcomes at the Bank (ß in Equation 1). *Average daily balance* in Column 1 is winsorized at the 99th percentile within each week; the log value of daily average balance in Column 2 uses the natural log transformation. *Active loan* indicates members with any oustanding loan. *Active savings account* indicates at least one deposit or withdrawal within the previous 90 days. The sample for Panel A excludes the data after April 2014 when mobile banking was introduced to three out of seven control centers. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values.

The cumulative distribution of average daily balances shows that the average treatment effects are not driven by a small number of high savers. Appendix Figures A.1 plot the cumulative distribution functions by treatment assignment (a) one year prior to the

weeks.

intervention, (b) 15 months after the intervention, and (c) 24 months after the intervention. The gap between the treatment and control groups is particularly visible in Figure 3b and becomes somewhat smaller after the contamination.

1.4.2 Quarterly Effects on Bank Savings and Loan Usage

I next examine the treatment effects over time. Appendix Table A4 reports the estimates on quarterly treatment effects, β_q from Equation 4. I highlight four noteworthy points. First, the changes in the average daily balance and the likelihood of deposits gradually grow over the first year, confirming that the persistent decline in savings coincides with declining deposit frequency. By the end of the first fifteen months, deposit frequency fell by 23 percentage points, or 33% of the control mean, and the average daily balance declined by P806, or 28%.

Second, the gradual decline in deposit frequency rules out the possibility that the declining savings was simply driven by members who dropped out immediately upon mobile banking implementation and stopped using the account at once. In fact, Column 6 shows no immediate effect on the likelihood of having an active savings account, which is defined by any deposit or withdrawal transaction over the previous 90 days. The dropout rate during the study period was relatively low—40 out of 575 clients (7%) closed the account over 2 years. The rate in the treatment centers is somewhat higher (8.4% as opposed to 5.4% in the control group), but this difference is not statistically significant, nor can it explain the observed treatment effects over time.

Third, deductions of transaction fees only partially explain the observed savings decline. An average member paid P272 in transaction fees over the first five quarters, including the fees for deposits, withdrawals, and balance inquiries. This only accounts for one third of the savings decline during the same time period.

Finally, there was a large but brief positive treatment effect on withdrawals. In the first post-intervention quarter, the likelihood of withdrawals increased by 3.7 percentage points, 50% of the control mean. This is unlikely to be an optimal adjustment in account usage under reduced withdrawal costs, given the brevity of the effect. The observed effects are, however, consistent with the hypothesis that mobile banking removes the commitment feature of a microfinance savings account, resulting in overspending in the short-term and eventually lower account usage and savings accumulation.

1.4.3 Heterogeneous Impact by Proximity to Baseline Transaction Points

The declines in account usage and savings balances over time suggest that the potential benefits of increased convenience under mobile banking were not large enough to encourage account usage. To further investigate this surprising result, I next examine heterogeneity in impact by differential change in increased convenience. Even though the intervention reduced the transaction time equally across all members, the value of a marginal increase in convenience may have been relatively small for members who lived close to, and thus had easier access to, transaction points at baseline (i.e., center meeting and bank office locations). These members may respond differently to the introduction of mobile banking.

I construct the measure of proximity to baseline transaction points using the retrospective data collected in the follow-up survey on the time, distance, and financial cost of traveling to the nearest bank office and center meeting location in 2012.[15] I take the first principal components of the quartile indicators of these measures[16] and identify "nearby" members as individuals with the below-median score within each center pair.

A major caveat of this analysis is that center meeting and bank locations are endogenous decisions of the Bank. Members who live near transaction points may be different in important ways from those who live far away. I regress measures of proximity to transaction points on baseline characteristics to gain insights on this point. Appendix Table A5 shows that there are no systematic correlations between the proximity index and observable characteristics, even though members near transaction points are somewhat more likely to

[15]The data shows that only 6.5% of respondents moved after 2012 (6.5% in the treatment and 6.4% in the control centers). The recall bias therefore is likely small.

[16]I use quartile indicators instead of standardized values to construct the PCI because many respondents had a difficulty estimating the exact distance and time. Using quartile indicators significantly improves the Kaiser-Meyer-Olkin (KMO) measure of sampling adequacy.

have secondary school education. Higher education may be correlated with higher economic capacity and opportunity costs, but I find no correlation with the member's economic and financial characteristics.

To estimate the heterogeneous impact on Bank outcomes, I modify Equation 1 and interact T_c with the indicator for members close to baseline transaction points:

$$\begin{aligned} Y_{icmt} = \alpha + \beta(T_c \cdot Post) + \beta^n(T_c \cdot Near \cdot Post) + \phi(Near \cdot Post) \\ + \gamma T_c + \gamma^n(T_c \cdot Near) + \eta Near + \theta_m + \delta_t + v_{icmt} \end{aligned} \tag{1.6}$$

where *Near* denotes a member with the proximity index below median.

Table 1.4 presents the estimates on β, β^n, ϕ, and η. There are large and significant heterogeneous effects by proximity to transaction points. Columns 1 and 2 show that an average member near transaction points in the treatment group saved nearly 30% less than her counterpart in the control group. The monetary value of the total effect (305 + 748 = P1,053) equals several days' worth of sales for a typical microentrepreneur. Note that the negative coefficient for the likelihood of withdrawals on the interaction term suggests that mobile banking did not reduce savings accumulation by making withdrawals too easy. In fact, Columns 3-6 show that mobile banking generally reduced usage of financial services at the Bank for this subgroup of members. The likelihoods of deposits, withdrawals, active loans, and savings accounts fell by 30%, 17%, 5% and 9%, respectively. These effects are quantitatively and qualitatively significant.

I test the persistence of the heterogeneous effects by plotting the quarterly treatment effects separately for members close to and far from baseline transaction points. As shown in Appendix Figures A.2(a)-(f), the treatment effects for members close to transaction points (red solid line) are consistently more negative than the effects for members far from transaction points (blue dashed line). Figures A.2(c)-(d) show a steady and increasingly larger decline in deposit frequencies among members near transaction points, but no large increase in the likelihood of withdrawals, even initially, for those members. Furthermore, Figures A.2(e) and (f) indicate that the intervention did not immediately decrease active loan and savings accounts. Taken together, the decline in savings among members near transaction

Table 1.4: Heterogeneous impact by proximity to transaction points
Sample: Members who completed the follow-up survey

	Average daily balance		Deposit likelihood	Withdrawal likelihood	Active loan	Active savings account
	Winsorized	Log value				
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x Post	-305.123*	0.072	-0.124***	0.007	0.045	0.021
	(157.494)	(0.188)	(0.033)	(0.009)	(0.038)	(0.039)
	[0.091]	[0.714]	[0.004]	[0.474]	[0.269]	[0.627]
Treatment x Near x Post	-747.830**	-0.589**	-0.140**	-0.026**	-0.074*	-0.105**
	(241.874)	(0.219)	(0.043)	(0.011)	(0.034)	(0.045)
	[0.014]	[0.026]	[0.010]	[0.030]	[0.055]	[0.037]
Near x Post	201.730	0.280	0.048	0.016*	0.021	0.048
	(182.563)	(0.169)	(0.028)	(0.006)	(0.019)	(0.040)
	[0.436]	[0.181]	[0.152]	[0.050]	[0.298]	[0.263]
Near	396.025	0.139	0.043	0.014**	0.156*	0.015
	(406.316)	(0.121)	(0.031)	(0.006)	(0.068)	(0.014)
	[0.381]	[0.296]	[0.256]	[0.047]	[0.085]	[0.367]
Total effect for Near						
Wild-bootstrap p-value	0.013	0.003	<0.001	0.024	0.193	0.034
Number of observations	69,055	69,055	69,055	69,055	69,055	69,055
Control mean (post-intervention)	3116.78	8.145	0.796	0.082	0.454	0.927
Week-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table presents the heterogeneous treatment effects on savings and loan outcomes at the Bank by proximity to transaction points. *Near* indicates below-median index of self-reported travel cost, time, and distance to center meeting and bank office locations within each center pair. *Average daily balance* in Column 1 is winsorized at the 99th percentile within each week; the log value of daily average balance in Column 2 uses the natural log transformation. *Active loan* indicates any oustanding loan. *Active savings account* indicates at least one deposit or withdrawal within the previous 90 days. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values.

points is triggered by a steady decline in deposit frequency.

1.4.4 Impact on Loan Performance

I next examine the treatment effects on the loan payment behavior. It is difficult to obtain robust estimates on the changes in loan performance because only 40% of members had an active loan in any given week, and incidences of late payments and pastdues are low. Thus, for this analysis I generate aggregate loan performance figures over 155 post-intervention weeks for each member and estimate the treatment effects on the proportions of weeks with arrears and average daily value of non-performing loans (NPLs) in a cross-sectional OLS model.[17] Table 1.5 Columns 1-2 show that the intervention almost tripled late payments. The effects are equally large for members close to and far from transaction points. A marginally significant but qualitatively large increase in the likelihood of NPLs suggests that members are not simply taking advantage of flexible payment schedules, but that some late payments accumulate and turn into NPLs, or the even riskier arrearage. In addition, the statistically insignificant but large coefficient on the value of NPLs (Column 5) implies that the volume of NPLs in treatment centers more than doubled over two years.[18] Even though the rates of pastdue loans remained low at $< 1\%$ in both treatment and control centers, these results may have implications for the cost of loan management and the overall cost efficiency of the Bank's operation.

1.4.5 Effect Mechanisms

The findings so far show that mobile banking lowered savings accumulation among members near transaction points through a persistent decline in deposit frequency. Based on the key program features and the conceptual framework outlined in Section 2.4, I investigate three mechanisms for declined deposit frequency.

1. *Procrastination channel*: Flexibility of deposits increases the cognitive burden of making regular deposits, and thus procrastination tendency in depositing. I measure the awareness of procrastination using an indicator for members who agreed to the following statement in the follow-up survey: I tend to procrastinate on financial obligations, for example, saying 'I will save or pay tomorrow'.[19]

[17] Non-performing loans are defined by the Central Bank of the Philippines as loans with arrearage of at least 10% of receivable balance.

[18] An increase in NPLs does not appear to drive the observed savings decline. I show in Section 1.4.6 that the treatment effect on savings is no larger for borrowers than for savers.

[19] I'm agnostic about whether behavioral characteristics are stable or changeable over time. I am simply testing for the change in awareness (or salience) of one's procrastination problems conditional on one's innate characteristics.

Table 1.5: Impact on loan performance
Sample: Full sample/Members who completed the follow-up survey

	Proportion of weeks with any arrears		Proportion of weeks with non-performing loans		Average daily value of non-performing loans	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.041***	0.052***	0.015*	0.017**	18.987	15.669
	(0.010)	(0.011)	(0.005)	(0.005)	(9.040)	(12.909)
	[0.005]	[0.001]	[0.090]	[0.015]	[0.202]	[0.379]
Treatment x Near		-0.018		-0.002		8.348
		(0.016)		(0.007)		(17.998)
		[0.316]		[0.792]		[0.693]
Near		-0.000		0.003		-6.244
		(0.007)		(0.006)		(13.378)
		[0.991]		[0.610]		[0.887]
Total effect for Near						
Wild-bootstrap p-value		0.036		0.097		0.079
Number of observations	575	448	575	448	575	448
Control mean	0.021	0.022	0.011	0.011	16.623	17.616
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the OLS estimates for the treatment effects on post-intervention loan outcomes at the Bank. *Non-performing loans* are defined as loans with arrearage of at least 10% of receivable balance. *Near* indicates below-median index of self-reported travel cost, time, and distance to center meeting and bank office locations within each center pair. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. All regressions use fixed effects for center-pair.

2. *Fee sensitivity channel*: The introduction of transaction fees discourages deposits because of increased perceived deposit costs, measured by the likelihood of agreeing to the following statement in the follow-up survey: I avoid making frequent bank deposits because it's costly to travel to the bank and to transact.

3. *Group defection channel*: Removing cash handling undermines the role of center meetings, which weakens group cohesion and the peer effects of group banking. To measure group defection, I take the principal component of the following indicators collected in the 3-month Bank survey:[20]

[20] I first recode the responses so that each variable indicates a higher level of group defection.

[leftmargin=0.5in]

i. I sometimes attend to my business and/or chores instead of attending center meetings.
ii. Even if I have no plan of taking out a loan, weekly payment status of other members in my center is important to me.
iii. It is important that a new member who joins the center has good recommendations from my friends.
iv. Any interaction with the center members in the last 7 days
v. Any interaction with the bank staff in the last 7 days

These mechanisms are not mutually exclusive. It is important to note that the goal of this analysis is not to isolate the causal effect of each channel, but to assess whether the data provides consistent support for any or some of the channels.

In Table 1.6, I present the treatment effects on procrastination tendency, fee sensitivity, and group defection. The estimates on individual components of the group defection index are reported in Appendix Table A7.

The results support the fee sensitivity and group defection channels, but not the procrastination channel. Columns 1-2 show that mobile banking had no effect on the awareness of procrastination in financial behaviors, either on average or differentially by proximity to transaction points.

In contrast, mobile banking increased the likelihood of avoiding frequent deposits due to costs. Column 4 shows that the increase in fee sensitivity is only present for members near transaction points. The magnitudes of the coefficients imply a 40% increase in the likelihood that these members avoid deposits due to transaction costs under mobile banking. Given that the fees were small and not significantly different from the contribution to the remitter's travel expenses in the control group, I speculate that the increased fee sensitivity reflects a psychological effect of introducing explicit transaction fees.[21] The strong heterogeneity in

[21] The estimates remain equally large and significant when excluding one center pair near the bank office where there was no contribution toward weekly center payment remittance before mobile banking, supporting

Table 1.6: Effect channels: procrastination, fee sensitivity, and group defection channels
Sample: Members who completed the follow-up survey

Dependent variable:	Aware of procrastination tendency		Avoid deposits due to costs		Group defection index	
Data:	(2.5-year follow-up)		(2.5-year follow-up)		(3-month follo-up)	
	(1)	(2)	(3)	(4)	(3)	(6)
Treatment	-0.043	-0.047	0.081	-0.008	0.237	0.063
	(0.037)	(0.031)	(0.039)	(0.059)	(0.101)	(0.128)
	[0.506]	[0.246]	[0.199]	[0.917]	[0.151]	[0.702]
Treatment x Near		0.014		0.190*		0.379**
		(0.045)		(0.099)		(0.153)
		[0.767]		[0.090]		[0.031]
Near		-0.048		-0.126		-0.307*
		(0.031)		(0.064)		(0.124)
		[0.183]		[0.109]		[0.052]
Total effect for Near						
Wild-bootstrap p-value		0.627		0.045		0.006
Number of observations	448	448	448	448	448	448
Control mean	0.381	0.381	0.578	0.578	0.000	0.000
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the effects of mobile banking on self-reported procrastination tendency, fee sensitivity, and group defection. *Aware of procrastination tendency* indicates individuals who agreed to the statement: I tend to procrastinate on financial obligations, for example saying `I will save/repay tomorrow'. *Group defection index* is the first principal component score of self-reported meeting attendance delinquency, low perceived importance of center performance, and lack of interactions with other members and bank staff. *Near* indicates below-median index of self-reported travel cost, time, and distance to center meeting and bank locations within each center pair. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values.

the treatment effect on fee sensitivity between members near and far from transaction points is still surprising because there was no variation among members in the same center in terms of mobile deposit fees or contributions for the remitter's travel expenses. In addition, the intervention did not substantially change physical access to deposit transaction points: the middle 60th percentile of the difference in the travel times to center meeting and agent's store is within 3 minutes. One plausible explanation here is that members who had easy deposit access at baseline had particularly low perceived cost of deposits before mobile

that the observed effect is not driven by the actual price change.

banking and thus responded more strongly to the introduction of fees.

Turning to Columns 5-6, the results indicate a significant increase in group defection under mobile banking. This increase is, again, largely driven by members near transaction points. In Appendix Table A7 Panel A, I show that the effects are particularly strong for the index components that directly indicate attitudes toward center performance (Columns 1-3). Mobile banking increased the likelihood of members reporting that they skip center meetings and that the reputation and the payment status of other members very important. Panel B shows that this pattern holds for members near transaction points. All components except the likelihood of interaction with bank staff contribute to the significant heterogeneous increase in group defection, and the total effects are more significant for Columns 1-3.

It is unclear *ex-ante* why the effect on group defection would vary by proximity to transaction points. In my data, members near transaction points in the control group presented stronger connection to their microfinance groups (Table 1.6 Column 6 and Appendix Table A7). Intuitively, center cohesion may grow stronger when members live physically closer to other members and bank locations. The Bank may also strategically place center meetings in areas with more socially connected households.[22] Regardless, it is likely that the members with stronger center connections benefited more from the peer effects of group banking, and therefore were also adversely affected by the weakened role of center meetings. The positive correlation between proximity to transaction points and account usage in the control group, shown in Table 1.4, corroborates this narrative. Of course, without a random variation in center connection, I cannot formally test whether declined account usage was mediated by group defection. However, the Bank survey took place only three months after the introduction of mobile banking when the treatment effect on deposit frequency was similarly small for members near and far from transaction points (illustrated in Figure A.2c). Taken together, my findings suggest that digitization weakened the role of center meetings and resulted in group defection, leading to declines in deposit frequency and

[22] It is common to hold center meetings in the house of a center official, often a trusted and well-connected member of the community. And members near transaction points often come to meetings early to set up the meeting space and sometimes even to fetch members who are late or delinquent.

savings accumulation. Heterogeneity by proximity to transaction points in part captures the differential effects by group connection at baseline.

Finally, it is worth noting that the decline in deposit frequency among members far from transaction points is not driven by any of the three channels explored here. Instead, mobile banking appears to have changed the norm of expected deposit behavior. The 3-month Bank survey asked members how important it was to make a deposit every week. Nearly 98% of all respondents agreed that it was important, but treated members were 11 percentage points less likely to *strongly* agree (Appendix Table A7, Column 6). This effect is large and significant regardless of the proximity to transaction points, suggesting that mobile banking generally loosened the discipline to deposit weekly. This, however, affected neither the overall deposit amount (Appendix Table A6, Columns 1 and 2) nor savings balances for members living far. Thus, treated members far from transaction points made less frequent but larger deposits to maintain their savings. The general wisdom is that the poor with frequent income streams would benefit from frequent deposit opportunities. Here, I find that when given more flexibility, members far from transaction points maintained Bank savings with significantly lower deposit frequency.

1.4.6 Alternative Explanations for the Decline in Bank Savings

There are several other changes under mobile banking that could have triggered the decline in deposits and savings. First, I revisit the changes in the loan policies. It is plausible that loan disbursement into a savings account reduced deposit frequency because members maintained savings by keeping loan proceeds in the account instead of saving cash income. I show in Appendix Table A8 that this was not the case. Columns 1-3 report the heterogeneous treatment effects on savings balances and deposit likelihood by borrowing status at baseline.[23] There are no significant differences in the treatment effects on the average daily balance and weekly deposit likelihood among borrowers and non-borrowers. These results

[23] The loan status is relatively stable before and after the intervention: 85.8% of borrowers during the intervention are borrowers at baseline.

support that the decline in deposit frequency was not driven by changes in loan policies.

Second, members had to adopt a new technology to continue using the savings account. Despite high mobile phone penetration in the Philippines, digital financial services, such as mobile money and internet banking, are not prevalent among the rural poor. Anecdotally, many members had expressed concerns about having to use a mobile phone for transactions. However, my findings provide no evidence that the technological barrier contributed to declining savings accumulation. Mobile banking initially resulted in an *increase*, not a decrease, in withdrawal transactions, which members were required to initiate by sending an SMS. Furthermore, mobile phone ownership is balanced between members near and far from transaction points (72.7% and 74.2%, respectively). Technological barriers, therefore, cannot explain the large differential effects by access to transaction points. In fact, the members with low mobile literacy, defined by no ownership of mobile phone and lack of knowledge on how to send an SMS in 2012, are no less likely to deposit and maintain savings balances under mobile banking than those with high mobile literacy, as reported in Appendix Table A8 Columns 4-6. The lack of personal mobile phone ownership and unfamiliarity did not prevent the adoption of mobile banking in my setting, where mobile phone literacy in the general population is high.[24]

Lastly, mobile banking members were required to open an ATM account with a lower interest rate. In theory, it is possible that the lower interest rate reduced the motivation to save. It is unlikely, however, that members reacted to a small change in the interest rates between the two types of savings accounts. For a mean balance of P3213, the difference in a half percentage point in the per annum interest rate implies a difference in the annual interest earning of P16. For such a small difference in the interest earning to generate 20% decline in savings, they would have to have had an extremely long time horizon for financial decision-making. Furthermore, in the open feedback gathered at the end of the follow-up survey, not a single respondent brought up the interest rate of the ATM account as an

[24]Qualitative accounts suggest that members without mobile phones relied on their family members and mobile banking agents to make the transactions for them.

issue,[25] while a number of respondents complained about transaction fees.

1.5 Implications for Household Financial Behaviors

In this section, I examine the implications of mobile banking for household financial conditions. Even though this analysis is exploratory due to the low statistical power I have in cross-sectional comparisons, it could provide useful insights on the potential effects on household financial behaviors. I focus my investigation around three questions. First, how does mobile banking affect household savings portfolios? This question is particularly important for treated members close to transaction points who reduced account usage at the Bank. Second, does mobile banking affect economic activities either through changes in savings accumulation or through easier savings access? Recent studies suggest a positive link between access to a liquid savings account and household economic capacity (Callen et al., 2014; Schaner, 2013b; Dupas and Robinson, 2013a). It is thus important to view my findings on Bank savings together with the changes in household financial and economic portfolios. Third, does mobile banking affect the capacity to cope with shocks and risk-sharing arrangements? Easier access to savings may improve one's ability to use savings when the household faces immediate financial needs. The lack of s significant treatment effect on account usage in the earlier analysis doesn't negate the possibility that the intervention affected coping methods during shocks that occur at low probabilities.

1.5.1 Household Savings and Economic Portfolios

Table 1.7 Columns 1-6 present the treatment effects on self-reported household savings amounts. I report the average effects in Panel A and heterogeneous effects by proximity to transaction points in Panel B. First, I note that the treatment effect on self-reported savings

[25]This is consistent with the findings of Karlan and Zinman (2013), who studied the savings price sensitivity in a similar context in the rural Philippines. They found that a variation in savings interest rates within 1-2 percentage points of the prevailing rate affects neither the take-up nor the usage of the savings account.

Table 1.7: Impact on household financial and economic portfolios
Sample: Members who completed the follow-up survey

	Household savings						Economic profile			
	Savings at the Bank		Non-Bank savings		Total household savings		Main occupation in the last 12 months			Any enterprise in the household
Winsorized at:	99th percentile (1)	95th percentile (2)	99th percentile (3)	95th percentile (4)	99th percentile (5)	95th percentile (6)	Self-employed (7)	Salaried work (8)	Casual work (9)	(10)
Panel A. Average effects										
Treatment	-924.666	-532.493	-662.732	312.286	-1374.164	-282.535	-0.027	0.021	0.014	-0.026
	(524.563)	(327.319)	(974.443)	(367.840)	(1375.654)	(608.548)	(0.024)	(0.025)	(0.027)	(0.040)
	[0.265]	[0.338]	[0.833]	[0.707]	[0.666]	[0.826]	[0.509]	[0.628]	[0.759]	[0.686]
Panel B. Heterogeneous effects by proximity to transaction points										
Treatment	-110.492	-84.526	72.527	545.777	-74.840	657.102	0.016	-0.002	0.031	0.055
	(618.341)	(407.572)	(1135.885)	(520.425)	(1594.549)	(810.348)	(0.038)	(0.028)	(0.027)	(0.063)
	[0.893]	[0.866]	[0.959]	[0.404]	[0.973]	[0.507]	[0.716]	[0.934]	[0.386]	[0.501]
Treatment x Near	-1799.984**	-986.880*	-1710.174	-497.531	-2991.454	-2058.210*	-0.090	0.044	-0.032	-0.168
	(802.483)	(538.795)	(1668.150)	(788.441)	(2063.411)	(1139.293)	(0.071)	(0.057)	(0.041)	(0.106)
	[0.032]	[0.089]	[0.331]	[0.539]	[0.183]	[0.090]	[0.262]	[0.489]	[0.457]	[0.155]
Near	1643.00**	874.68*	2190.81*	330.36	3611.50***	1760.55**	0.038	0.013	-0.009	0.082
	(589.078)	(396.297)	(1089.452)	(323.375)	(1401.504)	(659.255)	(0.061)	(0.042)	(0.036)	(0.081)
	[0.047]	[0.073]	[0.083]	[0.379]	[0.004]	[0.039]	[0.651]	[0.832]	[0.805]	[0.340]
Total effect for Near										
Wild-bootstrap p-value	0.007	0.020	0.376	0.949	0.165	0.196	0.218	0.492	0.981	0.207
Number of observations	448	448	448	448	448	448	448	448	448	448
Control mean	3219.86	2632.75	4318.72	2701.1	7495.89	5706.71	0.385	0.151	0.133	0.610
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the impact of mobile banking on household savings and economic activities reported in the 2.5-year follow-up survey. *Near* indicates below-median index of self-reported travel cost, time, and distance to center meeting and bank office locations within each center pair. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. All regressions use fixed effects for center-pair.

at the implementing Bank (Column 1) is quantitatively consistent with the earlier analysis of the administrative data. The point estimate of -P925 is comparable to the average quarterly effects in the second year of intervention (Appendix Table A4 Column 1).

Columns 3-4 provide no evidence for savings substitution among members near transaction points. The point estimates on the interaction term between *Treatment* and *Near* are negative and insignificant, suggesting that they did not increase other forms of savings. As a result, the total household savings significantly declined for the treated members. Even though they are only marginally significant, the coefficients on the interaction term in Columns 5-6 suggest a nearly 30% decline in household financial assets.

It is unlikely that the declines in the Bank and household savings are driven by increased investment in income-generating activities. In Columns 7-10, I report the treatment effects on main occupation and the likelihood of operating a microenterprise over 12 months. The coefficients are generally small and insignificant. If anything, the likelihood of operating an enterprise fell for members near transaction points: the coefficient on the interaction term implies a 16% decline. This could be a consequence of declined savings and lack of working capital. The estimates are imprecise, however, and this effect is suggestive at best.

1.5.2 Coping Strategies and Informal Risk-sharing

I now turn to the question on risk-sharing arrangements and the capacity to cope with shocks. I report the treatment effects on coping methods during shocks in Table 1.8 Columns 1-4[26] and informal loans and transfers over 30 days in Columns 5-9.[27] There are three sets of findings to highlight. First, Panel A Column 2 indicates that easier savings access on average increased the use of savings during negative shocks. The coefficient of 0.053 with

[26]The survey asked respondents to recall all events that "had a significant negative effect on household financial situation since January 2013" and to identify all methods used to cope with each shock. I use the total number of times the household cited each coping method as an outcome. The estimates I report in the main table exclude the typhoon incident in 2014 as a significantly larger proportion of the treated members report this event as an economic shock to the household.

[27]The respondent was asked to report the total number of times in the last 30 days anyone in her household received from friends or gave friends 1) in-kind or cash transfers which the receiver was not expected to be paid back and 2) in-kind or cash loans which the receiver was expected to pay back, and 3) goods on credit.

the control mean of 0.083 times implies an increase of over 60%, and the effect is larger for those who lived far from transaction points. This was not detected in the earlier analysis on withdrawal frequency at the Bank because the incidence of negative shocks is very low: the average number of shocks reported over 2.5 years was 0.518 in the control group.

Second, the treatment effects on informal risk-sharing outcomes underscore the potential change in the pattern of risk-sharing among treated members. Though statistically insignificant, Panel B Columns 5-8 suggested that treated members near transaction points tend to give more *and* receive less under mobile banking. They are thus more likely to be a net giver on a day-to-day basis. The same group of members reports increased use of

Table 1.8: Impact on coping methods and informal risk-sharing
Sample: Members who completed the follow-up survey

	Methods of coping with shocks, Total number in the last 2.5 years				Informal risk-sharing in the last 30 days (loans and transfers to/from friends)				
	Total # of shocks (1)	Withdrew savings (2)	Received gifts from friends (3)	Borrowed/ Sold assets/ Reduced consumption (4)	# of transfers given (5)	# of loans given (6)	# of transfers received (7)	# of loans received (8)	Net giver (9)
Panel A. Average effects									
Treatment	0.013	0.053	0.026	0.019	0.491	0.947	0.594	-0.160	0.064
	(0.044)	(0.021)	(0.014)	(0.027)	(0.740)	(0.742)	(0.643)	(0.649)	(0.024)
	[0.884]	[0.109]	[0.293]	[0.709]	[0.730]	[0.491]	[0.577]	[0.914]	[0.105]
Panel B. Heterogeneous effects by proximity to transaction locations									
Treatment	0.019	0.077*	0.063*	-0.027	-0.490	2.264	0.143	-1.575	0.152*
	(0.054)	(0.034)	(0.029)	(0.036)	(1.239)	(1.770)	(1.435)	(1.116)	(0.062)
	[0.753]	[0.076]	[0.070]	[0.425]	[0.748]	[0.272]	[0.928]	[0.309]	[0.072]
Treatment x Near	-0.020	-0.049	-0.082	0.101	2.134	-2.744	1.169	3.049**	-0.195
	(0.122)	(0.068)	(0.062)	(0.097)	(1.670)	(3.132)	(2.338)	(1.235)	(0.111)
	[0.873]	[0.498]	[0.217]	[0.340]	[0.239]	[0.405]	[0.641]	[0.027]	[0.137]
Near	0.074	0.020	0.069	-0.086	-1.734	1.314	-2.330	-2.248**	0.193***
	(0.097)	(0.060)	(0.032)	(0.085)	(1.038)	(1.451)	(1.230)	(1.001)	(0.051)
	[0.449]	[0.755]	[0.114]	[0.380]	[0.168]	[0.466]	[0.160]	[0.044]	[<0.001]
Total effect for Near									
Wild-bootstrap p-value	0.990	0.607	0.651	0.392	0.097	0.803	0.319	0.002	0.460
Number of observations	448	448	448	448	448	448	448	448	448
Control mean	0.518	0.083	0.142	0.326	6.780	6.294	5.760	3.761	3.642
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the impact of mobile banking on coping methods during negative shocks and informal risk-sharing patterns reported in the 2.5-year follow-up survey. *Near* indicates below-median index of self-reported travel cost, time, and distance to center meeting and bank office locations within each center pair. *Net giver* indicates members who give more than receive loans and transfers from friends (i.e., Cols (5) + (6) > Cols (7) + (8)). Columns 1-5 report the coping methods for reported negative shocks that had a significant impact on household economic conditions, but excludes a typhoon in July 2014 which disproportionately affected treated centers. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. All regressions use fixed effects for center-pair.

gifts from friends as a coping method during shocks (Column 3). It is plausible that easier access to savings not only improved a member's own capacity to cope with shocks, but also strengthened informal risk-sharing with her social network. Finally, a different story emerges for members near transaction points. Negative coefficients on the interaction term in Columns 2, 3, and 9 suggest that none of the treatment effects discussed above is present for this subgroup of members. Instead, they increased their reliance on informal loans (Columns 8), which may be a direct consequence of declined savings in the household.

1.6 Cost-benefits of Mobile Banking

1.6.1 Implications for the Bank

The financial report in 2013 suggests that the annual net profit per client at baseline was roughly P270 ($6). In this section, I calculate how this figure would change under mobile banking. First, consider the implications of the changes in account usage among existing members. The observed, 20% decline in savings mobilization increases the cost of financing loans. Twenty percent of total savings deposits at the Bank in 2013 roughly equal P613 million ($13.6 million). The interest rate paid on savings is 1.5% per annum, whereas the Bank pays 6.5% on external borrowing on average. If the Bank increases the external borrowing to offset the decline in savings, the interest expense would increase by 5% of P613 million, or P30.7 million ($681,484). In addition, a 114% increase in NPLs lowers net income due to larger provision for credit losses (a portion of income the Bank puts aside to cover expected losses), increased staff time on NPL management, and smaller net interest income. Assuming a 1:1 change (i.e., 114% changes in provision for credit losses, staff time on NPL management,and net interest income), I estimate the annual net profit per client under mobile banking to go down to P190 ($4.20) per client, a 29% decline from the baseline.

This profit loss would be at least partially offset by improved operational efficiency. After all, the Bank's goal in digitization was to improve profits per staff through increased caseload. The post-intervention data on time allocation among account officers shows that mobile banking reduced the average time spent on center meetings by half, from an hour to a half hour. For an average account officer handling 730 clients in 18 centers, this implies a total time-saving of 6-7 hours per week. A simple extrapolation then implies that the account officer's caseload could increase by up to 16% (6.5/40 hours). In other words, the Bank could cut 16% of field staff to serve the same number of existing members. Readjusting the operating expenses, I reach the annual net profit per client of P241 ($5.35).

The net profit per client after adjusting for the potential increase in operational efficiency still falls short of the baseline figure by 10.7%, suggesting that digitization did not improve the cost-efficiency for the Bank in the short-term. It is plausible that the Bank has not realized the maximum potential gain in efficiency in the initial phase of technology adoption, and thus this calculation may change over time. Furthermore, if mobile banking increases credit officer's capacity to manage a higher caseload over time, the net profit per client could shift upward.[28] This exercise, however, underscores the complexity of the process of technology adoption. Despite the widespread notion that digital technology benefits both the service provider and the users through reduced transaction costs, the overall cost-efficiency depends on how well the provider adjusts its operational processes and how digitization affects financial behaviors of the users, both of which are largely understudied.

1.6.2 Implications for Users

Finally, I consider the implications for users. I use the observed change in transaction costs and transaction frequencies to assess the aggregate change in transaction cost per existing member. As discussed in Section 2.3.2, an average member saves P17 per deposit (from P63 to P46) and P43 per withdrawal (from P71 to P28) under mobile banking.[29] Multiplying the average per-transaction cost by observed transaction frequencies, I estimate the total annual cost of transactions to be P2,391 ($53) in the control group and P1,177 ($26) in the treatment

[28] This depends on savings and loan payment behaviors of the newly recruited clients under mobile banking.

[29] The total cost saving of P43 for a withdrawal transaction is a lower bound and does not take into account the travel cost. The time saving inclusive of travel cost to the bank office is significantly larger at P81 per transaction.

group. A minimum wage earner makes $2,373 for working 6 days a week throughout the year. This implies that the lower bound of the transaction cost-saving under mobile banking equals to roughly 1% of minimum wage income: Digitization could bring a significant transaction cost-saving for users.

1.7 Conclusion

This study examined the effect of mobile banking among existing group microfinance clients in the Philippines using a matched-pair randomized experiment. Treated members who lived relatively far from transaction points at baseline adopted the new technology and maintained savings balances with less frequent deposits. However, the intervention resulted in a 30% decline in deposit frequency and a 28% decline in the average daily savings balance among members living near transaction points at baseline.

My analysis suggests that the disruption of the social architecture of group banking triggered this decline in savings, at least in part. Maintaining a regular savings habit is difficult. Self-help groups like ROSCAs and microfinance groups leverage social connections among members to create motivations to save. We have little knowledge of whether these habits persist after years of participation in group savings schemes. While my findings suggest that they do not, this study was not designed to isolate the causal effect of peer effects. As more MFIs turn to mobile technology to improve efficiency, further research is warranted to understand conditions under which a financial habit formed in group banking might persist with a weaker social architecture, and how technology could help replicate pre-existing social effects.

Members who had easy access to transaction points at baseline also became more sensitive to transaction costs under mobile banking and avoided frequent deposits due to costs. The fees were small in value, suggesting that the intervention increased the *perceived* cost of deposit. This finding points to the importance of understanding the willingness to pay for increased convenience of financial services. It is a standard practice of mobile money services providers to charge upfront transaction fees. If small fees adversely affect long-term financial behaviors and economic wellbeing, the per-transaction fee structure may not be optimal from either the business or social perspective.

Finally, the back-of-the-envelope calculations suggest that digitization significantly reduced the average transaction cost for service users. For the provider, in contrast, reduced transaction costs through digitization does not automatically imply higher cost-efficiency or profitability. The decline in savings mobilization could be particularly costly for financial institutions whose alternative source of loan finance is external borrowing.

Ultimately, the cost-benefits of digitization for the provider need to be weighed against the welfare change among the potential users. The growing literature on the impact of financial access provides some evidence that improved access to bank accounts could benefit the poor,[30] but a recent review by Dupas et al. (2016) shows that the breadth and depth of impacts vary widely across studies. More importantly, we do not have a clear understanding of the long-term impact on welfare. Digitization is likely to bring in new types of clients who were previously unbanked. As digital financial services spread rapidly around the world, it is critical to take a systematic approach to gathering data to understand the impact on cost-efficiency for the service provider as well as the impact on the financial decisions and overall welfare of underbanked households.

[30] For example, access to a formal bank account has ben shown to increase household financial assets (Prina, 2015), investments in microenterprises (Dupas and Robinson, 2013a), and income (Schaner, 2013b; Callen et al., 2014).

Chapter 2

Improving Credit Access Through Guarantees: A Case of Indonesia

2.1 Introduction

Micro, small, and medium enterprises (MSMEs) contribute significantly to Gross Domestic Product (GDP) in developing countries.[31] The literature has widely documented, however, that they face high barriers to accessing finance. Recent cross-country analyses, in particular, highlight borrowing constraints among MSMEs in developing countries (Beck et al., 2011; Ardic et al., 2011). Many governments actively adopt policies designed to reduce these barriers, and partial credit guarantees are one such policy. In a recent review, Gozzi and Schmukler (2015) report that roughly 2,000 credit guarantee schemes were in place in over 70 countries by the early 2000s. Despite this popularity, the empirical literature on their impact is relatively small.

Guaranteed loans aim to expand financial access among credit-constrained enterprises by reducing the risks the lenders bear. Governments offer partial guarantees to alleviate market inefficiencies borne of limited liability and reduce collateral requirements for MSMEs

[31] According to the Indonesian Ministry of Finance, for instance, MSMEs accounted for 60% of GDP and 97% of workforce in 2016.

with growth potential. Whether guarantee schemes are effective in achieving this objective is an empirical question. First, the lender may or may not have the ability to distinguish productive from unproductive borrowers. Second, even if the lender has such ability, guarantees may distort the incentives for both the lender and borrowers to maintain good repayment performance. Third, without effective monitoring and enforcement mechanisms, the lender may allocate guaranteed loans to existing risky borrowers or relabel existing bad loans to increase its profit margin.

This study investigates the impact of a large partial credit program—Kredit Usaha Rakyat (KUR)—introduced by the Indonesian government in 2007. KUR guaranteed 70% of the value of loans disbursed to "feasible but unbanked" enterprises by selected program banks. In the first two years, these banks disbursed $1.2 billion to over 2.3 million entrepreneurs under KUR. I take advantage of the fact that over 98% of KUR micro loans and 95% of all KUR loans[32] were disbursed through village units of Bank Rakyat Indonesia (BRI) in the first two years and use the district-level variation in the density of BRI units prior to the program to estimate the impacts of KUR. Specifically, I use repeated cross-sections of nationally representative data between 2005 and 2010 to compare the pre-post changes in business credit usage between the districts with high vs. low BRI densities in a difference-in-difference model.

Rigorous assessment of credit guarantee schemes is scarce in part due to the challenge of constructing an appropriate counterfactual to estimate causal impacts. Existing empirical analyses show that credit guarantee schemes contribute to credit additionality—increased credit usage that would not have come about without the guarantee scheme—but that the effects among the target firms are relatively small, for instance, in Italy (Zecchini and Ventura, 2009; D'Ignazio and Menon, 2013), Chile (Cowan et al., 2015), Canada (Riding et al., 2007), and South Korea (Kang and Heshmati, 2008) (see Samujh et al. (2012) and Gozzi and Schmukler (2015) for recent reviews.). However, most studies compare beneficiary firms or

[32] These figures are based on the number of guaratneed loans reported by the government guarantee companies.

banks to similar non-beneficiary firms or banks to obtain impact estimates, raising concerns about endogeneity in the analysis. Even with an exogenous variation in access to credit guarantees, the firm- and bank-level analyses could be problematic because they do not account for the potential substitution of loans within and across banks. In fact, Zia (2008), who uses the exogenous shock in the supply of credit guarantees in Pakistan to estimate the program impact, shows that nearly half of guaranteed loans were misallocated to financially unconstrained firms. This study takes an approach different from those of previous studies in that I exploit the geographic variation in the presence of program banks and use national household surveys to measure the impact on the aggregate level of business credit usage in a population. While this approach lacks the granularity of the firm- or bank-level analyses, it captures the net program effect on credit additionality regardless of the presence of loan substitution.

My findings provide strong evidence for credit additionality under KUR. At the extensive margin, KUR significantly expanded the usage of formal business credit in the first two years and the usage of any business credit in the second year. More importantly, these effects are concentrated among households without wage earners. Because non-wage earners' access to formal credit is largely limited to productive loans, the observed increase in formal business credit implies that KUR expanded the usage of *any* formal credit. Comparing the observed program impacts to the reported number of KUR micro loans, I estimate that roughly 49% of KUR micro loans contributed to credit additionality.

Furthermore, expanded usage of formal credit likely resulted in an increase in the overall volume of credit supply. Using the impact estimates and BRI's data on loan portfolios, I calculate the average BRI loan size among those who would have received formal credit in the absence of KUR. This figure followed the prior trend and increased after 2007. This suggests that BRI did not cut back on the volume of loans under the regular microfinance program in order to expand outreach under KUR, providing qualitative evidence that KUR increased the overall volume of credit.

The remainder of the paper is organized as follows. In the next section, I provide background on the financial sector in Indonesia and a description of KUR. I lay out the research design and the empirical strategy in Section 3 and discuss the results in the following section. I conclude in Section 5.

2.2 Background and Intervention

2.2.1 Development of the financial sector in Indonesia

The banking sector in Indonesia is composed of a diverse range of formal and semi-formal institutions. While commercial banks have a low presence in rural areas, government-owned banks and rural banks (BPRs) have historically played an important role in providing credit and savings services for rural households and channeling public funds through government-subsidized programs (Robinson, 2002). In addition, a large number of village banks and cooperatives provide traditional microfinance services at the subdistrict and village levels.

Despite the diversity of the financial institutions serving different target markets, the usage of formal financial services remains generally low. The gains from the financial liberalization and deepening of the financial system throughout the 1990s were concentrated on the island of Java (World Bank, 2005). Rosengard and Prasetyantoko (2011) report that the financial reforms following the Asian financial crisis in 1997 encouraged consolidation of smaller village banks and strengthened larger banks, reducing financial access for the rural poor and increasing geographic disparity in financial access. Figure B.1(a) I illustrates the geographic variation in the presence of financial institutions using the village census data in 2005. The proportions of sub-districts with the presence of commercial banks, rural banks, and even microfinance institutions are significantly higher in the island of Java than elsewhere, and microfinance coverage tends to be positively correlated with the penetration of formal banks.

2.2.2 Bank Rakyat Indonesia's Village Units

Bank Rakyat Indonesia (BRI) is regarded as one of the most successful state-owned financial institutions in the microfinance industry. BRI created its village unit system in the 1970s to channel the government's subsidized credit program for rice farmers. In an effort to establish financing outlets close to beneficiaries, BRI primarily focused on rural subdistricts in these early years. Only in the late 1980s did BRI start to expand the unit outreach in urban areas. Over the following two decades, the village unit system continued to grow, and it has become the largest network of financial services provider in the country. Even though the restructuring of the BRI system in the early 2000s resulted in a contraction of service coverage (Rosengard et al., 2007), BRI is by far the most common financial services providers in rural areas. According to the Indonesian Family Life Survey (IFLS) in 2007,[33] over 95% of villages in the sample reported that people in the community use services from BRI, as opposed to 83% for cooperatives, 70% for rural banks, and 24% for private banks.

Figure B.1(b) shows, however, that there is still a large variation in BRI's penetration across provinces. High-density provinces have two- to threefold higher number of BRIs per 10,000 households, although the penetration is less concentrated in Java compared to other types of financial institutions. Appendix Table B.1 shows that the BRI density is positively correlated with the presence of other financial institutions, road accessibility, and the absence of large employers. The predictive power of these characteristics becomes significantly weaker when including province fixed effects in Column (3), highlighting the strong across-province variation.[34]

BRI village units offer productive credit and micro-savings products for low-income households, both of which are considered some of the most successful microfinance programs in the world. BRI's savings mobilization in the 1980s and 1990s was motivated by the

[33]The Indonesian Family Life Survey is a panel household survey in 321 communities from 13 provinces that started in 1993. The survey sample representes 83% of the population.

[34]Futhermore, the joint significance of individual characteristics in Column (3) becomes insignificant when the measures of financial access (i.e., proportions of villages with commercial banks, rural banks, microfinance insitutions, and cooperatives) are excluded.

goal to achieving financial sustainability and meeting the demand for credit. Following the successful savings mobilization, the flagship microcredit program called "KUPEDES" grew steadily throughout the 1990s and 2000s.[35]

2.2.3 Intervention

The Indonesian government has taken an interventionist approach to developing the rural financial sector and improving credit access with the goal of enhancing productivity in agriculture and industry. The range of subsidized credit programs implemented since the 1970s include subsidized in-kind credit for rice farmers, interest subsidies, and directed credit targeting small enterprises. Many of these programs, however, have performed poorly, lacking appropriate incentive structures for both lenders and borrowers (Machmud and Huda, 2011).

The government initiated *Kredit Usaha Rakyat* (KUR) in 2007 as a way to improve and consolidate these programs. The main objective was to expand credit access among risky but potentially productive MSMEs. Under KUR, the Indonesian government guaranteed through two state-owned credit guarantee agencies 70% of the value of loans given to first-time borrowers. Unlike the earlier credit subsidy programs, KUR was implemented fully through selected banks, and the government had no direct contact with beneficiaries.

KUR provided two types of credit: micro loans of between IDR 1-5 million (≈$74 - 370)[36] at 22% interest per annum and retail loans of between IDR 20-500 million (≈$1,500 - 37,000) at 13% interest per annum. These rates are substantially lower than the market rates: Helms and Reille (2004) report the average interest rates to be 28-63% among MFIs and 18% among commercial banks in the early 2000s. To encourage outreach among the unbanked, collateral was not required for KUR micro loans. KUR micro had a far greater outreach in terms

[35]KUPEDES offers business capital ranging from IDR 3-50 million (≈ US$222 - 3700) at an effective annual interest rate of $< 30\%$.

[36]An average household spent $55 on food consumption and $44 on non-food consumption in 2007, implying that the average KUR micro loan of IDR 3.9 million at BRI is roughly equivalent to 25% of the annual household expenditure.

of borrowers than KUR retail. BRI's annual reports show nearly 1.5 million outstanding borrowers under KUR micro at the end of 2008, as opposed to 26 thousand borrowers under KUR retail.

When the program was first launched, program credit was channeled through six large banks. However, most of these commercial or state-owned banks were not equipped to reach unbanked MSMEs in rural areas. This led BRI village units to become the dominant provider of KUR micro loans.

A number of program features changed in 2010. First, the government added 13 regional development banks as KUR implementing banks in the first quarter of 2010, accelerating disbursements of guaranteed loans. Second, the KUR micro loan ceiling was increased to IDR 20 million from IDR 5 million to meet the needs of MSMEs in making large investments. Third, the guarantee amount was increased from 70% to 80% of the outstanding loan amount, providing greater incentives for program banks to reach risky MSMEs. The volume of guaranteed loans continued to expand after these changes.

2.3 Research Design

2.3.1 Data

I construct the outcome measures using repeated cross-sections of the National Socioeconomic Survey data (SUSENAS) between 2005 and 2010.[37] The SUSENAS core survey is carried out in July-August every year with a nationally representative sample of households across all districts. The sample households are selected using two-stage sampling at the census block level and stratification by rural/urban classification.[38] Between 2005 and 2010, SUSENAS gathered information on whether the household received any business credit

[37] I limit my data analysis to these five years because SUSENAS did not gather information on household business credit usage before 2005, and my identification strategy is not robust to the changes in program features that took place in 2010.

[38] I use the reported sampling weights in all analysis to account for over-sampling of urban households.

in the previous 12 months and where the largest loan came from.[39] In each year, I also observe the profession (i.e., sector) and the employment type (i.e., self-employed, wage worker, freelance, etc.) of the main work in the past 7 days for all adult household members.

The information on baseline bank densities and financial access comes from two separate datasets. First, I construct the district-level BRI density measure at baseline using the BRI village unit addresses I obtained from the microfinance department of BRI. This listing contains the unit address and the date of the unit establishment. Because a substantial number of unit addresses did not provide sufficient information to identify the sub-district or village, I match the data at the district level. Second, I construct the measures of other bank densities using the village census data from 2005. These data contain the information on the numbers of commercial bank branches and rural bank branches. Again, I aggregate the figures at the district level. All bank density measures reflect the number of bank offices per 10,000 households as of 2005. Third, I take the principal component of the indicators on the presence of commercial banks, rural banks, microfinance institutions, and cooperatives at the village level to construct a baseline index measure of overall financial access.

2.3.2 Sample Frame

KUR was launched in November 2007. Around the same time, the Indonesian government also introduced community and household cash transfer programs in six provinces and Jakarta DKI.[40] The sample frame for the cash transfer programs was selected based on the poverty rate and access to healthcare facilities and schools. These districts also have a somewhat lower BRI unit density. Given the endogeneity of the sample selection and the timing of program implementation, it is nearly impossible to disentangle the effects of KUR from cash transfer programs using repeated cross-sections of SUSENAS. I thus exclude

[39] The survey questions are not identifcal across surveys. For example, the survey in 2005 gather data on the amount of the largest credit source, while the surveys after 2007 gather data on all sources of credit but no data on loan amount.

[40] The six provinces for the two types of cash transfer programs include West Java, East Java, Gorontalo, North Sulawesi, East Nusa, and West Sumatera.

these seven provinces from the study sample. Two other provinces were excluded from the sample. First, district and sub-district boundaries in Papua were largely redefined in the 2000s. Because the BRI density measure is only available at the district level, this introduces a logistical difficulty in creating a district panel. Second, the province of Banda Aceh was not included in the SUSENAS core survey in 2005 in the aftermath of the Indian Ocean earthquake. My final sample consists of 254 districts across 25 provinces.

2.3.3 Identification Strategy

In this section, I describe the empirical strategy for estimating the casual effects of KUR. I take advantage of the initial roll-out design of KUR where over 95% of KUR borrowers received guaranteed loans through BRI village units. Conditional on the overall financial access index at baseline, I first show that the local density of BRI units is positively correlated with increased credit supply through the program. To measure the program impacts, I compare the pre- vs. post-intervention changes in the outcome of interest between the high BRI density ("treatment") and the low BRI density ("comparison") districts using a difference-in-difference model. This treatment measure identifies the districts with an above-median number of BRI units per 10,000 households within each province. Because the BRI density and the presence of financial institutions are strongly correlated across provinces, stratification significantly reduces the correlation between treatment and financial access index at baseline.

In Table 2.1, I present cross-sectional correlations between my treatment indicator "High BRI density" and the rate of KUR disbursements, which I define as the number of KUR loans per 10,000 households. For both the full sample (Panel A) and the restricted analysis sample (Panel B), the treatment indicator strongly predicts the rate of KUR disbursements in 2008 and 2009. The correlations become substantially smaller and no longer significant in 2010 when the government expanded the program through regional banks.

A key assumption in my estimation strategy is that the pre-intervention growth trends are not significantly different between the treatment and comparison areas. For example,

Table 2.1: Cross-sectional correlations between BRI density and KUR disbursements

	2008		2009		2010	
	Micro	Total	Micro	Total	Micro	Total
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Full sample						
High BRI density	109.66***	112.87***	67.04***	68.11***	25.95*	26.50*
	(28.63)	(29.36)	(16.47)	(16.66)	(14.23)	(14.41)
Baseline financial access index	660.77**	700.55**	292.08*	310.39*	345.74	349.23
	(247.90)	(260.60)	(151.82)	(156.96)	(291.31)	(292.09)
Number of districts	372	372	372	372	372	372
Mean of dependent variable	226.214	233.538	120.886	123.281	73.551	75.558
Panel B. Restricted analysis sample						
High BRI density	105.93**	109.85**	71.05***	72.28***	30.97	31.41
	(38.96)	(40.18)	(24.69)	(25.11)	(22.80)	(23.10)
Baseline financial access index	1135.81***	1195.75***	452.83*	481.27*	631.78	637.39
	(346.72)	(365.60)	(240.42)	(248.27)	(473.06)	(474.30)
Number of districts	254	254	254	254	254	254
Mean of dependent variable	234.422	242.877	121.339	124.223	78.253	79.921

This table reports cross-sectional correlations between the densities of banks and the number of SMEs that received KUR loans at the district level. *High BRI density* indicates districts with an above-median number of BRI village units per 10,000 households within each province. *Baseline financial access index* is defined by the average of village-level financial access index I construct using the presence of commercial banks, rural banks, cooperatives, and microfinance reported in the village census in 2005 within each district. Standard errors are clustered at the province level and reported in the parentheses: *10% significance level; **5% significance level; ***1% significance level. All regressions use province fixed effects.

if the baseline BRI density were highly correlated with the local demand for credit, the high BRI density districts would have experienced faster economic growth even in the absence of the intervention. The available data suggest that there was no differential growth between the treatment and comparison districts before 2008. First, I show in Table 2.2 that relevant district-level economic and financial characteristics, including population, number of small businesses, and access to productive credit and banks have no differential pre-intervention trends between the two areas. Furthermore, using the SUSENAS panel data between 2005 and 2007,[41] I show that the households in the treatment and comparison districts do not report differential changes in credit access, participation in self-employment activities, and household expenditures (the results are reported in Appendix Table B.2). The standardized joint coefficients across all outcomes in both tables are indistinguishable from zero, supporting the parallel trend assumption.

[41] The SUSENAS panel survey followed a nationally representative sample of over 65,000 households between 2005 and 2007.

I next verify that KUR did not lead to differential changes in other aid programs

Table 2.2: Pre-intervention trends in district characteristics

Dependent variable:	Number of households (1)	Number of small businesses (2)	Proportion of villages with SME credit access (3)	Number of banks (4)	Proportion of households in farming (5)	Standardized joint coefficient (6)
High BRI density x 2008	-2879.982	85.410	0.029	-1.290	0.006	0.016
	(4733.265)	(245.149)	(0.023)	(2.395)	(0.010)	(0.021)
Baseline financial access x 2008	25031.683	1052.965	-0.025	-74.636***	0.076	
	(21763.039)	(1554.038)	(0.205)	(22.068)	(0.067)	
Number of sub-districts	254	254	254	254	254	
Number of observations	508	508	508	508	508	
Mean dep var, control	124,242.64	4642.912	0.259	35.541	0.670	
District fixed effects	Yes	Yes	Yes	Yes	Yes	
Provincial trend control	Yes	Yes	Yes	Yes	Yes	

The district-panel regression results reported in this table use the village census data collected in March 2005 and March 2008. *High BRI density* indicates districts with an above-median number of BRI village units per 10,000 households within each province. *Baseline financial access index* is defined by the average of village-level financial access index I construct using the presence of commercial banks, rural banks, cooperatives, and microfinance reported in the village census in 2005 within each district. Standard errors are clustered at the district level and reported in the parentheses: *10% significance level; **5% significance level; ***1% significance level. All regressions use district fixed effects and control for provincial time trends. In Column (6), I report the standardized joint coefficient for Columns (1) - (5).

between the treatment and comparison districts. The difference-in-difference approach may not isolate the program impacts if the intervention triggered differential responses in other policy interventions. An increased supply of business credit through KUR may attract complementary interventions, such as skills training programs, or conversely crowd out poverty alleviation programs. Any differential changes in policy interventions may obscure the impacts of KUR. I use the village census data to test whether the introduction of KUR was accompanied by changes in the likelihood of receiving relevant interventions after 2007. Table 2.3 verifies that there was no differential change in the flow of aid revenue or in the presence of skills training and village-based revolving fund programs between the treatment and comparison areas. Neither was there a change in the pace of population growth between the two areas. Even though I cannot rule out the possibility that there were other differential policy changes in the two areas, the absence of effects on some of the most relevant interventions provides reasonable support for the validity of my identification strategy.

Table 2.3: Post-intervention trends in district size, aid revenues and aid programs

Comparison years:	2007 vs 2010			2005-2008 vs. 2008-2011	
Dependent variable:	Number of households (in thousands)	Total aid amount (Million Rp)	Log of total aid amount (Million Rp)	Proportion of villages with skills training	Proportion of villages with revolving fund
	(1)	(2)	(3)	(4)	(5)
High BRI density x Post	-1.7136	-0.0022	-0.0096	0.0181	-0.0193
	(2.6070)	(0.0410)	(0.0235)	(0.0259)	(0.0333)
Baseline financial access x Post	31.9759	-0.5101**	-0.3100**	-0.4021**	-0.3533
	(24.6704)	(0.2329)	(0.1209)	(0.1842)	(0.2219)
Number of districts	254	254	254	254	254
Number of observations	508	508	508	508	508
Mean dependent variable	121.190	0.335	0.225	0.150	0.352
Fixed effects	District	District	District	District	District

This table reports district-level changes in the amount of aid revenues and presence of poverty reduction programs before vs. after the introduction of KUR. Dependent variables are constructed using the village census data collected in the first quarters of 2008 and 2011. *Total aid amount* includes the aid revenues from local and national governments, foreign countries/organizations, and private sources for the previous calendar year. The presence of skills training and revolving fund programs indicates any programs and activities that took place over 3 years prior to the survey. Standard errors are clustered at the district level and reported in parentheses: *10% significance level; **5% significance level; ***1% significance level. All regressions use district fixed effects and control for provincial time trends.

2.3.4 Empirical Specifications

To assess the impact of KUR on the usage of business credit, I estimate the following difference-in-difference model:

$$Y_{idt} = \alpha + \beta(B_d \cdot I_t) + \theta_d + I_t \cdot \eta_p + \epsilon_{idt}, \tag{2.1}$$

where i denotes the individual, d the district, and t the year. B_d identifies the districts with high BRI density at baseline, θ_d are the district fixed effects, I_t are the year fixed effects, and η_p are the province fixed effects. I interact the year and province fixed effects to allow differential time trends across provinces. The vector β captures the differential trend in the outcome over time. The standard errors are clustered at the district level. Throughout the analysis, I define the base year as 2007, immediately prior to the introduction of KUR, and omit I_{2007} from all specifications.

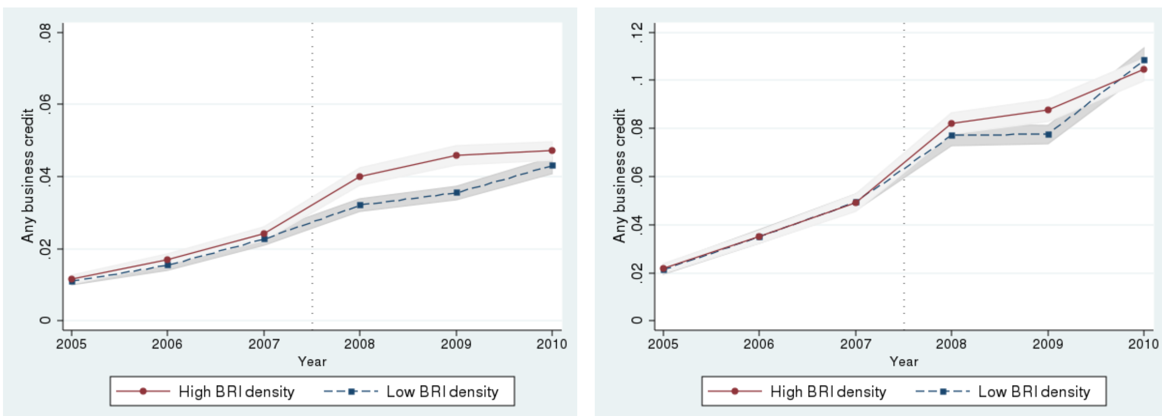
To further account for the correlations between the BRI density and the presence of other financial institutions, I use a specification with an additional control for the differential time trends by financial access at baseline. If my estimation strategy identifies the causal effect of KUR, the impact estimates will remain consistent when including an interaction between the year fixed effects and the district-level financial access index at baseline.

Finally, I also estimate the average program effects for the post-intervention period by replacing $B_d \cdot I_t$ with $B_d \cdot Post$, where $Post$ is an indicator for > 2007.

Figure 2.1: Changes in the usage of business credit: High BRI vs. low BRI density districts

(a) Largest business credit: Bank/unspecified government program (including KUR)

(b) Any business credit over 12 months



The above figures illustrate the changes in the usage of business credit in the treatment (high BRI density) and comparison (low BRI density) districts over time. Gray shaded areas indicate standard errors. The dotted vertical line indicates the introduction of KUR.

2.4 Results

2.4.1 Did KUR Increase the Usage of Formal Credit?

To test the impact of KUR on the usage of formal credit, I first consider two outcomes: whether the household received any business credit over the 12 months prior to the survey and whether the largest source of business credit came from a formal source. The sources of business credit in SUSENAS are categorized into government empowerment programs (called PNPM), other government programs, banks, cooperatives, individual lenders, and other sources. According to the SUSENAS survey guide, "other government programs" in

Table 2.4: Impact of business credit usage over 12 months

	Any business credit in the last 12 months			Largest source of business credit in the last 12 months: Formal source (banks or unspecified government programs including KUR)			Largest source of business credit in the last 12 months: Other specified source (government empowerment programs, cooperatives, or individuals)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
High BRI density x Post ($\geq$ 2008)			0.0063			0.0050**			0.0017
			(0.0047)			(0.0022)			(0.0032)
High BRI density x 2005	0.0031	-0.0014		0.0009	-0.0011		0.0012	-0.0006	
	(0.0046)	(0.0044)		(0.0023)	(0.0021)		(0.0027)	(0.0029)	
High BRI density x 2006	0.0001	-0.0033		0.0001	-0.0013		-0.0004	-0.0020	
	(0.0039)	(0.0038)		(0.0020)	(0.0020)		(0.0026)	(0.0026)	
(Intervention in 2007, omitted)									
High BRI density x 2008	0.0018	0.0022		0.0055**	0.0051**		-0.0018	-0.0015	
	(0.0043)	(0.0045)		(0.0023)	(0.0024)		(0.0029)	(0.0029)	
High BRI density x 2009	0.0116**	0.0132**		0.0081***	0.0080***		0.0026	0.0045	
	(0.0055)	(0.0062)		(0.0026)	(0.0028)		(0.0040)	(0.0046)	
High BRI density x 2010	-0.0050	-0.0010		0.0006	-0.0002		-0.0053	-0.0006	
	(0.0053)	(0.0057)		(0.0027)	(0.0029)		(0.0038)	(0.0038)	
Number of observations	990,385	990,385	990,385	990,385	990,385	990,385	990,385	990,385	990,385
Mean dependent variable in 2007	0.050	0.050	0.050	0.024	0.024	0.024	0.023	0.023	0.023
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trend controls									
Province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline financial access	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

This table reports difference-in-difference estimates of the program impacts using repeated cross-sections of national household survey data from 2005 to 2010. Standard errors are clustered at the district level and reported in parentheses: *10% significance level; **5% significance level; ***1% significance level. I specify the year immediately prior to the intervention (2007) as the base year. All regressions use district fixed effects and control for provincial time trends.

2009 and 2010 include KUR loans. I thus consider business credit through banks or other government programs as the most relevant measure of formal business credit in measuring the impact of KUR.

Figures 2.2(a) and (b) follow the differential trends in the growth of business credit usage between the treatment and comparison districts. Credit usage before the intervention is remarkably balanced between the two areas. Figure 2.2(a) shows a clear diversion from the prior trend in the treatment districts after 2007; the usage of formal business credit appears to catch up in the comparison districts in 2010 when KUR was expanded to a large number of regional development banks. Similarly, Figure 2.2(b) indicates a differential increase in overall business credit usage in treatment districts in 2008-2009, even though the gap is noticeably smaller than that in Figure 2.2(a).

Table 2.4 reports the regression estimates from a difference-in-difference model in Equation (1). For each outcome, I report the vector β, which captures the differential changes in the treatment districts relative to the comparison districts, compared to the difference in the base year 2007. I include the interactions between year fixed effects and a baseline measure of financial access in the second and the third columns for each outcome.

The results show that the treatment districts saw a significant increase in the likelihood of reporting the largest business credit from a formal source in 2008 and 2009. Between these two years, this likelihood increased by 1.31 percentage points, or 54% of the mean likelihood immediately prior to the KUR implementation in 2007. Consistent with the pattern in the cross-sectional correlations between BRI density and KUR disbursement figures reported in Table 1, the effect disappears in 2010. Table 2.4 also shows no significant increase in other types of credit as the largest credit source and a large increase in the overall likelihood of having business credit in 2009. The estimates on all outcomes remain stable when I control for the differential trends by baseline financial access index.

In Appendix Table B.3, I report the program effects on individual credit sources. The most robust effects are on bank loans. In addition, the likelihood of receiving the largest loan from an unspecified government source differentially increased in the treatment districts in 2009, when the survey guide specifically mentioned that KUR was categorized as such. These findings provide consistent and strong evidence that KUR expanded the usage of business credit at the extensive margin through improved access to formal loans.

These results by themselves do not imply an overall increase in formal credit uptake because I observe only the usage of business credit. However, I find suggestive evidence that this was in fact the case. First, business credit in SUSENAS is defined as loans received to help run or expand business activities regardless of how the household actually uses the proceeds.[42] Access to formal credit among low-income households in the informal sector is often limited to productive credit programs at microfinance banks and cooperatives. Furthermore, BRI's annual report in 2008 shows that consumer loans accounted for only 19% of the total loan portfolio, and 88% of these consumer loans were salary-based. In other words, less than 2.5% of the loan portfolio, at most, comprised consumer loans for non-wage earners.

These facts suggest that an increased usage of formal business credit among households without wage employment would be a reasonable indication of greater usage of *any* formal credit. Unfortunately, the repeated cross-sectional data bring a challenge to examining heterogeneous effects. Instead, I assess the breakdown of the increased usage of business credit under KUR by the presence of wage employment in the household. In Table 2.5, I report the impacts of KUR on the likelihoods of 1) having wage employment in the last 7 days and having received business credit in the last 12 months and 2) having no wage employment and having used business credit. Comparing Columns (1)-(2) and (3)-(4), it is clear that KUR resulted in a more robust increase in the share of non-wage earners with formal business credit than in the share of wage earners with formal business credit. The coefficients in Column (4) suggest that the share of households that have formal business credit without wage employment increased by 63% in the treatment districts between 2008-2009. Furthermore, Column (5) shows that this effect for non-wage earners is not driven by

[42] Microfinance borrowers use formal credit for a wide range of purposes: Johnston and Morduch (2008) report that half of KUPEDES loans are used for non-business purposes even though KUPEDES is considered a business credit program. The measure of business credit in SUSENAS includes these loans.

the change in the composition of wage earners and self-employed: if anything, there is a weak increase in wage employment in the treatment districts in 2009. These results provide generally convincing evidence that KUR contributed to credit additionality at the extensive margin, in particular by expanding access to formal credit among non-wage earners.

Table 2.5: Impact of KUR on business credit usage by presence of wage worker in the household

	HH has a wage worker and…		HH has no wage worker and…		
Dependent variable:	any business credit	the largest business credit from a formal source	any business credit	the largest business credit from a formal source	Household has a wage worker
	(1)	(2)	(3)	(4)	(5)
High BRI density x 2005	-0.0003	-0.0006	-0.0011	-0.0006	-0.0107
	(0.0025)	(0.0014)	(0.0026)	(0.0012)	(0.0070)
High BRI density x 2006	-0.0026	-0.0005	-0.0008	-0.0008	-0.0104
	(0.0020)	(0.0012)	(0.0025)	(0.0013)	(0.0072)
(Intervention in 2007, omitted)					
High BRI density x 2008	-0.0010	0.0015	0.0032	0.0036**	-0.0040
	(0.0028)	(0.0017)	(0.0029)	(0.0016)	(0.0064)
High BRI density x 2009	0.0052	0.0034*	0.0080**	0.0046***	0.0074
	(0.0036)	(0.0017)	(0.0034)	(0.0017)	(0.0075)
High BRI density x 2010	-0.0020	-0.0001	0.0010	-0.0002	-0.0050
	(0.0029)	(0.0015)	(0.0035)	(0.0019)	(0.0070)
Number of observations	990,385	990,385	990,385	990,385	990,385
Mean dependent variable in 2007	0.021	0.011	0.029	0.013	0.394
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Trend controls					
Province	Yes	Yes	Yes	Yes	Yes
Baseline financial access	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-difference estimates of the program impacts using repeated cross-sections of national household survey data from 2005 to 2010. Standard errors are clustered at the district level and reported in parentheses: *10% significance level; **5% significance level; ***1% significance level. I specify the year immediately prior to the intervention (2007) as the base year. All regressions use district fixed effects and control for provincial time trends.

2.4.2 What Proportion of KUR Micro Loans Contribute to Credit Additionality?

The main results on business credit usage show that KUR contributed to credit additionality at the extensive margin: the key question for policymakers is how efficiently the intervention achieved this outcome. To shed light on this matter, I calculate the proportion of program borrowers that newly gained access to formal business credit because of KUR by comparing the observed impact estimates against the reported number of KUR micro borrowers at BRI.

First, BRI reported in 2010 that a total of 2.3 million MSMEs received KUR micro loans between 2008 and 2009. The Indonesian population in 2008 was 234 million, or roughly 60 million households in 2008, implying that 383 of every 10,000 households received a KUR micro loan.

Second, the difference in the BRI densities between the treatment and comparison districts is 0.52 BRI units per 10,000 households. The estimates in Table 2.4 Column (5) thus imply an increase in the usage of formal business credit of 2.51 percentage points for an additional BRI unit per 10,000 households. At the end of 2007, there were 4,544 BRI village units across the country, or 0.757 units per 10,000 households. Multiplying the observed impact for an additional BRI unit per 10,000 households by the actual BRI density, I estimate that KUR expanded formal business credit to an additional 190 of every 10,000 households over the two years. These calculations suggest that roughly 50% of KUR micro loans were directed toward unbanked households that would not otherwise have accessed formal business credit, but that the other half went to MSMEs that would have received formal business credit even in the absence of KUR. This estimate is similar to the results of Zia (2008), who finds that nearly half of guaranteed loans in Pakistan were misallocated to financially unconstrained firms.

2.4.3 Did KUR Increase the Volume of Formal Credit?

Next, I explore the implication for the overall volume of credit supply. The increase in the usage of formal credit at the extensive margin may not lead to an increased volume of credit if BRI shifted credit supply away from existing borrowers by reducing their loan sizes. Without direct measurements of loan sizes, I am unable to statistically evaluate this question. The trends in BRI's loan disbursements, however, provide qualitative insight that KUR in fact increased the total volume of credit supply. Figure 2.2(a) illustrates the trends in the size and the number of outstanding loans at BRI separately for KUPEDES and KUR using the statistics reported by Prawiranata (2013) and BRI's annual reports. First, notice that the number of KUPEDES loans dropped sharply in 2008 with the introduction of KUR. This figure corroborates my finding that a significant proportion of KUR micro loans went to borrowers who would have received formal business credit even in the absence of KUR. The average KUPEDES loan size, in contrast, increased sharply in 2008. Given that the average KUR micro loan is substantially smaller than an average KUPEDES loan, these changes appear to indicate that small KUPEDES loans were relabeled as KUR micro loans.

The key question in assessing the implication for the overall credit volume is whether BRI, as a result of KUR, reduced the loan sizes of the KUPEDES borrowers and of a proportion of KUR borrowers who would have received KUPEDES loans in the absence of KUR. If the trend in the average loan size among this group of clients did not significantly change after 2007, it would be a reasonable indication that KUR increased the overall volume of credit supply. Assuming that only 50% of KUR loans actually went to the previously unbanked, based on my earlier result, I relabel 50% of KUR loans as KUPEDES and recalculate the average size and the number of outstanding KUPEDES loans in 2008-2009. Figure 2.2(b) illustrates the adjusted trends. The average loan size follows the pre-intervention trend and continues to increase after 2007. While only suggestive, this exercise indicates that the program not only increased the usage of formal credit at the extensive margin, but also increased the overall volume of credit.

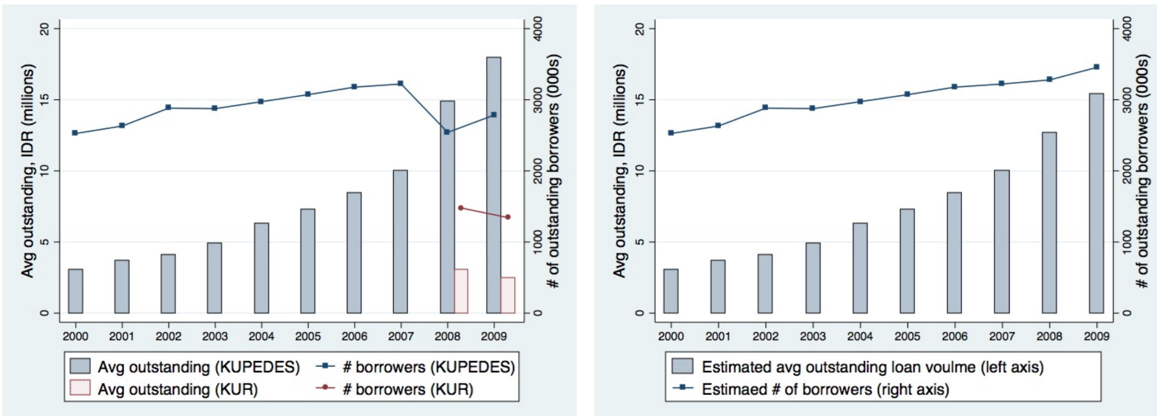
2.4.4 Did KUR Increase the Overall Usage of Credit?

The above analysis suggests that KUR increased the usage and volume of formal credit as well as the usage of any business credit. In assessing the implication for overall credit usage regardless of source and purpose, I draw on the available data on loan amounts by credit source from SUSENAS and IFLS. First, Appendix Figure B.2 shows that productive loans from informal sources tend to be small compared to bank loans and KUR micro loans. Nearly 70% of the informal business loans are below the average KUR micro loan

Figure 2.2: Changes in the size & the number of oustanding loans at BRI

(a) Reported changes for KUPEDES and KUR micro loans

(b) Estimated changes for non-KUR loans (assuming 50% of KUR micro loans were previously banked)



Notes: These graphs illustrate the actual and estimated changes in the average outstanding loan size and the number of outstanding borrowers by the type of BRI's microcredit program: KUPEDES, BRI's flagship microcredit program, and KUR. Figure 2(a) uses the statistics from Bank Rakyat Indonesia, Laporan Statistik BRI Unit (2009) reported in Prawiranata (2013) and BRI's annual report in 2009. In Figure 2(b), I assume that 50% of KUR loans went to the previously banked, based on the observed impact estimates of KUR, and re-calculate the average size and the number of borrowers for the existing borrowers.

size. Second, according to the 2007 IFLS data, consumer loans from informal sources (e.g., moneylenders, family and friends, etc.) are even smaller. The median loan sizes for non-productive purposes are IDR 300,000 for moneylender loans and IDR 500,000 for loans from family and friends, 30-50% of the minimum amount of a KUR micro loan and only 6-10% of the maximum. Therefore, even if the observed effect of KUR were driven by non-wage earners shifting from informal consumption loans to formal business loans, the overall credit volume would have increased for the majority of these borrowers.[43]

2.4.5 Impact on Enterprise and Consumption Decisions

Finally, I examine whether increased usage of credit through KUR among non-wage earners resulted in immediate changes in household economic and financial decisions. Note that I do not observe investments or savings decisions in the SUSENAS data. My analysis is therefore

[43]It is also plausible that KUR borrowers did not substitute away from informal loans. For instance, (Banerjee et al., 2015b) shows that formal credit is complementary to informal loans for productive entrepreneurs with existing business prior to the entry of formal financial institution.

limited to the participation in and the size of self-employment and household consumption. The short-term effects on these outcomes are ambiguous. High-ability entrepreneurs may increase enterprise investment and revenues, but business performance, such as size, survival, and income, may not immediately respond. Similarly, under increased opportunity costs of consumption and increased future income, short-term household consumption could either increase or decrease. Furthermore, heterogeneity in entrepreneurial ability could push these outcomes in one direction for some and in the opposite direction for others, as demonstrated by Banerjee et al. (2015b).[44] A series of randomized experiments of microcredit programs offers mixed evidence on these short-term outcomes (see Banerjee et al. (2015a) for the review).

Table 2.6 reports the difference-in-difference estimates of participation in self-employment and household consumption. First, I find no evidence that KUR affected the participation in self-employment or the composition of the self-employed with and without workers. Table 2.6, Columns (1)-(4) report the estimates of the program impact on the likelihood of having any own-account workers and any self-employed with temporary/permanent workers in the non-agricultural sector. The coefficients on the interaction between *High BRI density* and program years show no consistent pattern in either outcome. Again, these results are not surprising. KUR was designed to mitigate credit constraints for existing entrepreneurs without sufficient collateral rather than to encourage entry into entrepreneurship. In addition, a qualitative study BRI conducted in 2010 documents that many businesses use KUR micro loans as working capital to manage inventories and purchase inputs rather than as investment capital to hire workers or upgrade technologies.[45] These accounts support my results that KUR micro loans did not bring dramatic changes to the productivity or the size of beneficiary enterprises in the short term.

Turning to the results on consumption, Columns (5)-(8) show that KUR, on average,

[44] Banerjee et al. (2015b) suggest that existing enterprises prior to the entry of microfinance tend to be high-ability with high returns to capital. But in a setting where low-income households operate enterprises for survival, there is likely to be a large heterogeneity in abilty among existing entrepreneurs.

[45] BRI Access to finance for MSMEs: http://siteresources.worldbank.org/INTINDONESIA/Resources/226271-1170911056314/3428109-1259556842531/18.pdf

Table 2.6: Impact of KUR on economic and financial decisions

	Any self-employment in non-agriculture in the last 7 days				Log of household per capital expenditure in the last 1 month					
	On his/her own		With temporary/ permanent workers		Total		Food expenditure		Non-food expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
High BRI density x Post ($\geq$ 2008)		-0.0037		0.0034		0.0182*		0.0112		0.0285
		(0.0033)		(0.0030)		(0.0108)		(0.0101)		(0.0176)
High BRI x 2005	0.0022		0.0005		-0.0079		-0.0072		-0.0092	
	(0.0069)		(0.0062)		(0.0130)		(0.0122)		(0.0213)	
High BRI x 2006	0.0042		0.0020		-0.0149		-0.0040		-0.0247	
	(0.0058)		(0.0056)		(0.0102)		(0.0081)		(0.0175)	
(Intervention in 2007, omitted)										
High BRI density x 2008	-0.0014		0.0036		--		--		--	
	(0.0057)		(0.0056)							
High BRI density x 2009	0.0009		0.0013		0.0153		0.0146		0.0216	
	(0.0052)		(0.0059)		(0.0117)		(0.0108)		(0.0196)	
High BRI density x 2010	-0.0015		0.0063		0.0061		0.0006		0.0131	
	(0.0060)		(0.0058)		(0.0130)		(0.0136)		(0.0188)	
Number of observations	990,385	990,385	990,385	990,385	826,460	826,460	826,460	826,460	826,460	826,460
Mean dependent variable in 2007	0.187	0.187	0.118	0.118	5.682	5.682	5.159	5.159	4.636	4.636
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trend controls										
Province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline bank access	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-difference estimates of the program impacts using repeated cross-sections of national household survey data from 2005 to 2010. Standard errors are clustered at the district level and reported in parentheses: *10% significance level; **5% significance level; ***1% significance level. I specify the year immediately prior to the intervention (2007) as the base year. All regressions use district fixed effects and control for provincial time trends. Note that the SUSENAS core survey did not collect data on household expenditures in 2008.

had no significant effect on household consumption patterns.[46] The coefficients on the interaction between *High BRI density* and year 2009 are insignificant but positive for both food and non-food consumption. The lower bounds of the confidence intervals are >-0.005 for both outcomes, suggesting that it is highly unlikely that KUR reduced the average consumption level by an economically significant magnitude.

2.4.6 Robustness Check

I carry out falsification tests to verify the robustness of my identification strategy. If the observed increase in credit usage was brought by KUR, I expect to observe no increase in the usage of business credit when estimating the same model using the densities of non-BRI banks. To test this, I estimate Equation (1) using the densities of rural banks and any commercial banks and report the results in Appendix Table B.4. The regression specifications are identical to those of Columns (2), (5), and (8) in Table 2.4. The proportion of households receiving the largest business credit from formal sources does not significantly increase in the districts with either high commercial bank or rural bank densities in 2008-2009. These results verify that the observed program impacts are not simply driven by general trends in the districts with high bank densities.

2.4.7 Conclusion

This study investigates the short-term impact of a large partial credit guarantee scheme in Indonesia on credit additionality. I use the district-level variation in the pre-intervention density of the program bank to estimate the impacts in a difference-in-difference model. The empirical literature on credit guarantee schemes focuses on comparisons between beneficiary and non-beneficiary firms or banks. In addition to the endogeneity problem, these analyses are subject to potential biases from substitution and crowd-out effects within and across banks. My approach overcomes this problem by measuring the net effect of the program at the district level. I find that KUR increased the usage of formal business credit by 1.9

[46] The SUSENAS survey in 2008 did not have the consumption module.

percentage points between 2008 and 2009. Using the observed pattern of program impact for non-wage earners and the descriptive statistics on BRI's loan portfolios and informal loans, I provide qualitative accounts supporting that KUR not only expanded access to formal business credit, but also increased the usage of and the overall volume of any credit.

KUR has largely been considered a successful credit guarantee scheme. In 2012 Global Microcredit Summit presented an award to then President Yudhoyono in recognition of the success of KUR. The Indonesian government continued to expand the program after 2010, on-boarding more banks and increasing their disbursement targets: the total disbursement amount in 2016 reached US$7 billion. The default rate has remained low at below 5%, indicating that participating banks do not take on excessive risks or relabel a substantial number of bad loans.

With increasing resources directed toward the program, however, its cost-effectiveness is starting to be questioned.[47] My analysis highlights substantial mis-targeting, as nearly half of guaranteed loans in the initial two years were absorbed by borrowers who would have received formal credit even in the absence of KUR. Even though this study provides only limited insights into the economic and financial decisions of households, I find no evidence that KUR increased self-employment activities or investments.

The empirical literature on the guarantee schemes has so far focused on the first-stage question of the impact on credit additionality. Despite a large variation in the design features and structure of the guarantee programs, we know little about how different features—from guarantee rates and interest ceilings to incentives/penalties for lenders—would affect program costs and effectiveness. In addition to the need to rigorously assess the long-term effects of guarantees on the growth of MSMEs, future research is warranted to examine how these key features of the guarantee schemes affect the behavior of lenders and borrowers as well as the cost of program implementation.

[47]For example, Indonesia Economic Quarterly (March 2017) points out a significant increase in the program cost after the recent design changes and the need to re-evaluate the mertis of subsidized loans.

Chapter 3

Take-up and Usage of Health Insurance: A Field Experiment from the Philippines[48]

3.1 Introduction

Health risk is a common source of financial vulnerability for the poor. In recent years, many developing countries have adopted social health insurance schemes to provide low-income households with financial protection against health shocks. Expanding outreach among households in the informal sector, however, brings a number of challenges. In addition to high transaction costs and liquidity constraints, the models of information asymmetry predict that private information on individuals' risks would trigger adverse selection in a voluntary insurance scheme. Both public and private insurance providers increasingly use local community organizations as a delivery channel to overcome many of these challenges. In particular, a microfinance institution (MFI), often a trusted financial institution in local communities, could potentially deliver insurance schemes at lower costs than a traditional insurance provider unfamiliar with the target market and reduce adverse selection by

[48]Co-authored with Xavier Giné, Dean Karlan, and Jonathan Zinman

bundling insurance products with other financial services. An MFI could also benefit from lowering default risks due to health shocks through increased insurance coverage of its members.

This study examines the take-up and utilization of a social health insurance scheme delivered by a microfinance bank in the Philippines. In 2003, the Philippines Health Insurance Corporation (PhilHealth) introduced a new program, called KaSAPI, in which it partnered with local financial institutions to market and distribute insurance policies among households in the informal sector. To minimize adverse selection, partner MFIs were incentivized to enroll a large number of members in PhilHealth and in some cases encouraged to mandate PhilHealth enrollment. Requiring members to purchase a PhilHealth policy may not be an optimal policy for an MFI, however, if low-risk members have low demand for insurance and perceive mandatory premium payment as an increase in borrowing costs.

We designed a field experiment to test the relative effects of the compulsory and voluntary KaSAPI policies among the existing borrowers of Green Bank, one of the first KaSAPI partner organizations. We randomly assigned 3,682 clients in the individual loan program to one of three groups: 1) compulsory health insurance: making KaSAPI enrollment a requirement for remaining in the credit program, 2) voluntary health insurance, or 3) no health insurance offer. This design allows us to cleanly isolate selection from moral hazard by comparing baseline risk characteristics and healthcare utilization among the insured clients in the compulsory and voluntary groups.

The intervention significantly increased PhilHealth enrollment during the study period. In the second year of intervention, 59% of clients in the compulsory group[49] and 48% in the voluntary group purchased their own PhilHealth policy for at least one enrollment period (i.e., 3 months), as opposed to 29% in the control group. These effects became smaller but remained significant over time: 46% of clients in the compulsory and 34% in the voluntary groups reported having an active PhilHealth membership at the 2.5-year follow-up survey,

[49] A client was exempted from enrolling in PhilHealth if she presented proof of PhilHealth coverage through her spouse's policy. We verified that 96% of clients in the compulsory group had PhilHealth coverage through her own or her spouse's policy for at least one enrollment period in the first 18 months of the intervention.

as opposed to 27% in the control group. The diminishing effects on PhilHealth enrollment appear to be driven by a high dropout rate from Green Bank's credit program during the study period. An active Green Bank loan in July 2008 is one of the strongest predictors of long-term PhilHealth enrollment, but there are no significant differences in the dropout rates or the characteristics of dropout clients across experimental groups. These findings suggest that reducing the transaction costs of enrollment through an MFI could encourage insurance take-up.

We also find strong evidence for selection in the take-up decision. While none of the observable characteristics predicts PhilHealth take-up in the control group, pre-existing chronic conditions are positively correlated and risky behaviors—regular smoking or drinking—are negatively correlated with PhilHealth enrollment in the voluntary group. These risk characteristics are weakly but positively correlated with insured risks at the follow-up in the control group. Together, our findings indicate the co-presence of adverse and advantageous selection.

In contrast, the intervention did not substantially affect the volume of healthcare utilization for either the overall sample population or the insured pool.[50] Despite a 20 percentage point increase in insurance coverage, compulsory clients do not report a significant increase in health facility visits for covered care[51] compared to clients in the control group. Assuming monotonicity of the treatment effect, this result implies that KaSAPI did not reduce the unmet needs for healthcare or trigger moral hazard.[52] When restricting the sample to insured clients, we again find no large difference in facility visits for covered care between the insured clients in the compulsory and voluntary groups. This second result indicates that the selection effect does not change the aggregate riskiness of the insured pool. We note, however, that confidence intervals for these estimates are relatively large, and thus we

[50]The correlation between insurance coverage and insured risks may be due to adverse selection and/or moral hazard. We are unable to distinguish these two effects on healthcare utilization in this study.

[51]We identify facility visits that are likely covered by PhilHealth, using the self-reported information on the main purpose of the visit and length of stay.

[52]It is plausible that the average effect simply does not capture this effect if the proportion of the risky type in the population is small. Unfortunately, we lack the power to test heterogeneous treatment effects.

cannot rule out the presence of small effects.

More fundamentally, higher insurance coverage through the intervention did not increase insurance utilization. Our effect estimate of the compulsory policy on the likelihood of using health insurance in the last 12 months is one-fourth of the expected effect size, which we calculate using the incidences of insured risk and insurance utilization in the control group and the observed treatment effect on PhilHealth enrollment. The expected effect size is outside the confidence interval we obtain,[53] suggesting that individuals who gained PhilHealth coverage through KaSAPI underutilize insurance.

We turn to the data on 1,044 facility visits for covered care reported among 416 insured clients to explore potential explanations for the lack of treatment effects on insurance utilization. The patterns in their insurance utilization point to three considerations. First, the rate of insurance utilization among all facility visits for covered care is substantially lower for the previously uninsured than the previously insured in all experimental groups, suggesting that the newly insured may face a learning curve in using PhilHealth. Second, information on PhilHealth benefits may not be effectively delivered by a third-party institution. Interestingly, the compulsory KaSAPI policy increases knowledge of the premium schedule and required documents for filing claims but not the knowledge of PhilHealth benefits or coverage restrictions. Among the insured clients, the knowledge of coverage restrictions is in fact *lower* in the KaSAPI treatment groups than in the control group. We hypothesize that the type of information that the bank, as a financial institution, emphasizes in marketing insurance products is different from the information PhilHealth marketers may emphasize. This underscores a potential challenge of KaSAPI's third-party marketing model of KaSAPI. Third, the rate of insurance utilization is particularly low for the newly insured in the compulsory group: 31% in the compulsory group as opposed to 39% in the control and 45% in the voluntary group. Drawing on the literature on behavioral economics, which suggests

[53]We have the power to detect the expected increase in insurance utilization under the compulsory policy at $\alpha = 0.05$ and $\beta = 0.2$. The upper bound of the confidence interval of our estimate is just below this effect size.

a link between active choice and behaviors,[54] we speculate that the compulsory policy may have reduced motivation and commitment for insurance utilization by eliminating active decision-making on insurance enrollment. Although these explanations are tentative, this analysis provides useful insights into the design of micro-health insurance schemes and suggests future research areas.

The growing empirical literature on selection in insurance markets emphasizes the importance of preference heterogeneity. Studies in the US show that individuals' risk characteristics are multi-dimensional and that the correlation between each risk dimension and insured risk may vary across different markets (Finkelstein and McGarry, 2006; Fang et al., 2008; Geruso, 2016). This presents a challenge in identifying information asymmetry in an insurance market. A correlation between insurance coverage and insured risks—a common method of testing for adverse selection—captures the combined effect of selection and moral hazard. With multi-dimensional risk characteristics, even in the absence of moral hazard, this correlation test does not always reveal the presence of information asymmetry. We overcome these challenges by using a field experiment and testing the relative effects of a compulsory vs. voluntary insurance scheme. Consistent with the existing literature in the US, we find that private information about insured risks and risk preference has opposite effects on the take-up decision in a less-developed insurance market.

Our results are also relevant for the literature on the impact of microinsurance schemes. Previous studies on health insurance schemes targeting the informal sector in underdeveloped markets provide limited evidence on the impact on healthcare utilization and well-being, and instead highlight the low demand for health insurance among the poor (see Acharya et al. (2013) and Cole (2015) for reviews). In our study, even with the moderate take-up and incidence of insured risks, the intervention had no effect on the usage of insurance. Our analysis suggests that marketing and enrollment design features that encourage take-up could have unintended consequences for insurance utilization. These

[54] Default rules and active choices are both found effective in increasing program enrollment in the financial domain (**??**). An active choice, however, may be more effective in increasing repeat and follow-up behaviors (**?**) or when learning plays an important role (**?**).

findings underscore the challenge of designing an effective micro-health insurance scheme. Unsurprisingly, our intervention had no impact on downstream household outcomes, such as economic activities, assets, and subjective well-being.

The remainder of the paper is organized as follows. In the next section, we describe the study context and experimental design. We provide details of the empirical method in Section III. We then discuss the determinants of PhilHealth take-up and impacts in Section VI and conclude in Section V.

3.2 Experimental Design

3.2.1 Context

Philippines National Health Insurance Corporation (PhilHealth)

The Philippines was one of the early adopters of a social health insurance scheme among low-income countries. The National Health Insurance Program was first launched in the 1970s, modeled after the US Medicare program (**?**). Philippines National Health Insurance Corporation (PhilHealth), the current government health insurance agency, was created in 1995 under the new national mandate to achieve universal coverage. Enrollment among low-income households, however, has been low. According to **?**, the estimated coverage rate in the early 2000s was lowest among the working poor in the informal sector at 7%: households in the formal sector receive coverage through their employers, and the indigent population qualifies for free coverage through the Sponsorship program.

PhilHealth benefits focused on inpatient care until recently. At the time of the evaluation, the benefits included hospital room and board, doctors' fees, selected medicines and procedures, laboratory tests for inpatient care, maternal and newborn care, and outpatient care for selected illnesses and procedures, such as dialysis, tuberculosis, and SARS. For a fixed annual premium of P1,200 ($\approx$ \$26.50), the coverage extended to the policyholder's immediate family members, including a spouse, children under the age of 21, and parents above the age of 65. The benefits were calculated on a fee-for-service basis up to a ceiling per calendar year. Any cost beyond the ceiling was paid out-of-pocket by the patient.[55] PhilHealth benefits are only applicable to the care received at accredited facilities, which included 38% of public hospitals and 61% of private hospitals in 2005 (**?**).

Overview of the KaSAPI Program

PhilHealth designed the KaSAPI program in 2006 to improve outreach in the informal sector. Under KaSAPI, a partner group—usually a local financial institution such as a rural bank and cooperative—markets insurance policies to its members, collects premiums, and remits payments to PhilHealth on quarterly basis for enrollment. While this procedure enhances efficiency for PhilHealth, the partner group's operational cost inevitably increases. To compensate for the increased administrative cost and encourage enrollment, PhilHealth provides a small discount on insurance premiums for partner groups reaching the pre-agreed target number of enrollments.

KaSAPI provides a number of advantages over the standard Individual-Premium Paying (IPP) program for households in the informal sector. First, it lowers the cost of enrollment. Instead of traveling to a local PhilHealth office in a municipal town, a member makes premium payments through her microfinance lender. Second, it reduces liquidity constraints when the partner group provides financial services to encourage premium payments. Green Bank, for example, offered a direct deduction of insurance premiums from loan proceeds. Third, a member and her dependents become entitled to selected outpatient benefits, most notably the maternity and newborn packages, after one quarter of premium contribution, instead of the nine months required in the IPP program.[56]

[55] Its "first-peso" approach with a benefit ceiling but without price regulation meant that the providers were able to price discriminate against PhilHealth members. In fact, Gertler and Solon (2002) report that up to 86% of PhilHealth benefits were captured by healthcare providers in the early 2000s.

[56] Under the IPP program, a member could make premium payments on a monthly basis, whereas the minimum enrollment term under KaSAPI is three months.

Individual loan program at Green Bank

Green Bank, a family-owned rural bank, provided a range of credit and savings products to low-income households in the central and southern Philippines at the time. Loan terms under the individual liability credit program, called TREES, varied widely, from a loan of P3,000 ($\approx$ \$67) to P50,000 ($\approx$ \$1,110). Small loans were paid in weekly installments over 3-4 months, whereas large loans offered monthly installments over 12 months. Based on the loan repayment performance and savings accumulation over the loan cycle, a client qualified for an increase in loan amount by up to 50% at each loan renewal. Anecdotally, health shocks were one of the largest default risks the clients faced. Green Bank participated in KaSAPI, recognizing the potential benefits of protecting its clients from financial vulnerability during adverse health shocks. However, the bank was hesitant to adopt the compulsory PhilHealth policy without better understanding its implications for loan performance and retention.

3.2.2 Experimental Fesign

Experimental Treatments

Our experiment randomly assigned existing clients in Green Bank's TREES program to one of three experimental groups. In the first treatment group, PhilHealth was introduced as a compulsory policy. Clients in this group were required to enroll in PhilHealth in order to continue borrowing from Green Bank, unless they already had PhilHealth coverage through a spouse.[57] The bank calculated the required length of coverage based on the loan term and quarterly enrollment cycle so that the client would have PhilHealth coverage for the full duration of the loan term. For example, for a six-month loan released in March, a client paid a premium of P600 ($\approx$ \$13.25) for coverage of the second and the third quarters of the year. In the initial marketing period, credit officers individually visited all clients, provided the information and a brochure on the PhilHealth benefit package, and then informed them of the new bank policy mandating PhilHealth enrollment. During this visit, each client

[57]For each enrollment period, the local PhilHealth office verified the spouse's PhilHealth membership status for all clients who claimed to have PhilHealth coverage through their spouses.

was given the option to enroll on the spot by paying the premium in cash or to enroll at the next loan renewal. For enrollment at loan renewal, she could choose to have the premium deducted from loan proceeds. Most clients opted for premium deduction from loan proceeds. This implies that treated borrowers received smaller loan disbursements in general—the premium payments of P300-600 constitute 3-6% of the median loan amount of P10,000 ($\approx$ \$222).

In the second treatment group, PhilHealth was offered on a voluntary basis. Clients in this group were given the same marketing pitch, materials, and enrollment options. The only difference was the voluntary nature of the PhilHealth offer: they could enroll in PhilHealth for any number of quarters at a loan renewal and discontinue premium payment at any point.

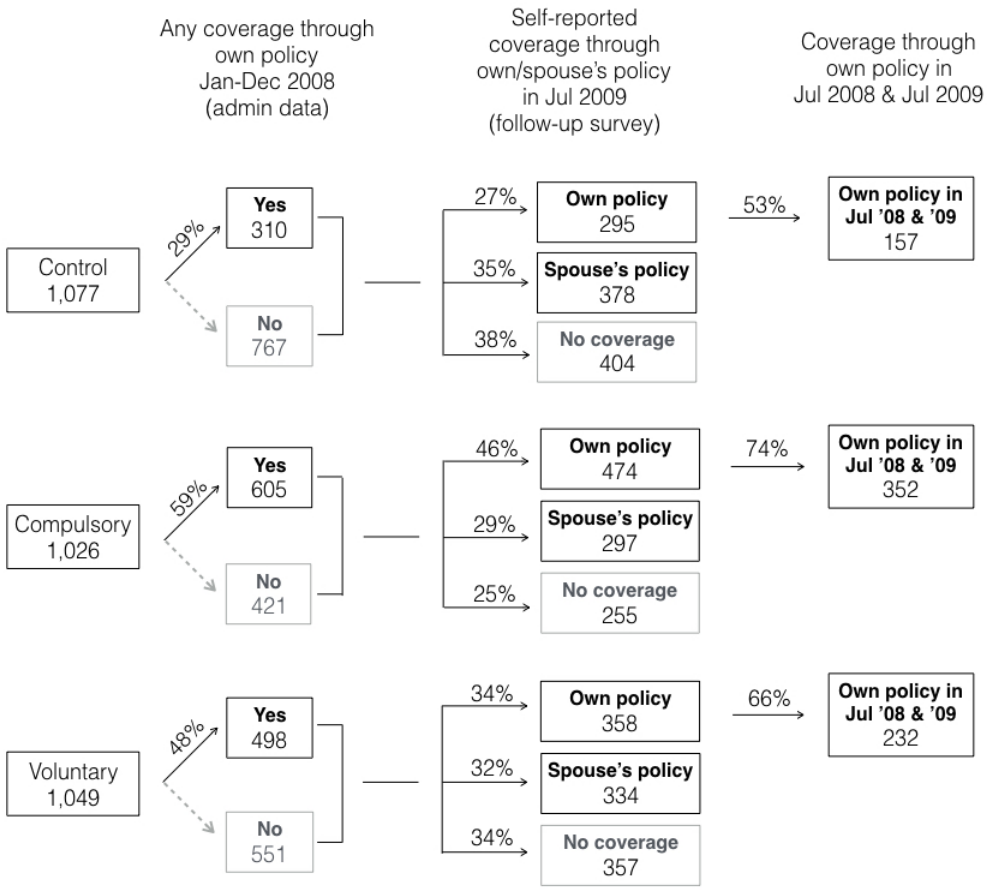
Finally, the third experimental group, which received neither information nor enrollment assistance for PhilHealth, serves as a control group. Note that clients in this group could still enroll in PhilHealth through other programs such as the IPP and the Sponsorship programs. The bank facilitated no interactions among TREES clients. This, we believe, limits potential spillovers of the information on KaSAPI across experimental groups.

Randomization

We implemented the study with 28 Green Bank branches in the central and southern Philippines. The sample was drawn from active TREES clients as of September 2006. We first excluded those with past-due loans or an existing PhilHealth policy from the sample. This gave us a base sample of 3,682 clients. We then stratified the sample by region and randomly assigned them to one of the three experimental groups.

Table **??** verifies that the random assignment created experimental groups with balanced baseline characteristics. We report the OLS estimates of the differences in baseline characteristics between the control group and the two treatment groups as well as the p-values from the equality test of the two treatment groups. The coefficients are generally insignificant for both the clients who completed the baseline survey (Panel A) and those

Figure 3.1: PhilHealth enrollment
Sample: Individuals who completed the follow-up survey



Notes: This figure illustrates PhilHealth coverage across experimental groups during the study period. The compulsory policy did not require the clients covered by a spouse's policy to enroll in PhilHealth. We verified the spouse's PhilHealth membership when a client in the compulsory group claimed that she had coverage through a spouse's policy. As a result, 96% of the compulsory clients had PhilHealth coverage through own or spouse's policy for at least one quarter in the first two years of the intervention.

who completed the follow-up survey (Panel B). The p-values from joint significance tests across baseline characteristics in Column (9)[58] confirm that the experimental groups are overall well-balanced.

[58]These p-values are obtained using an OLS model where we regress the treatment assignment on all baseline characteristics using regional fixed effects.

Sample Characteristics

Table **??** also provides an overview of the sample characteristics. A majority of the clients are women, and the average loan size is P16,850 ($\approx$ \$374). Despite our effort to restrict the sample to those without a PhilHealth policy at baseline, 20% of the clients reported having their own PhilHealth policy in the baseline survey. Overall, nearly half of the sample had PhilHealth coverage through either her own or her spouse's policy. An average household in the control group made 3.4 health facility visits over 12 months, while more than one-third of the households experienced at least one incident where the household was unable to seek care due to costs, suggesting that financial costs are a not-insignificant barrier to seeking formal healthcare.

Data

We use four sets of data to examine the selection and impact of KaSAPI. First, we gathered baseline data on demographic, health and risk characteristics from 80% of the clients randomly selected from the base sample.[59] The fieldwork was carried out in November and December 2006, immediately before the roll-out of KaSAPI marketing. Second, we use administrative data on enrollment from the local PhilHealth office. During the study period, Green Bank's credit officers kept records of the PhilHealth ID numbers for all clients. In the first and the third quarters of 2008 (i.e., the 2nd year of intervention), the local PhilHealth office matched this membership information against the official enrollment database to verify active membership status.[60] Third, we conducted a follow-up survey in July-August 2009 with all clients in the base sample. The survey gathered detailed information on healthcare facility utilization and health shocks in the preceding 12 months, self-reported health status, economic and financial status, and risk behaviors. Fourth, we use administrative data from

[59]We only collected baseline data from a random sample of the clients in the base sample in order to test the effect of the baseline survey on behaviors. The findings of this analysis, along with the results of similar experiments in other settings, are reported in Zwane et al. (2011)). We control for the baseline survey assignment in all analyses that do not restrict the sample to those who completed the baseline survey.

[60]The PhilHealth local office verified the KaSAPI membership for the full sample on a quarterly basis but was unable to verify other types of PhilHealth membership as frequently due to capacity constraints.

Green Bank on basic demographic characteristics and loan performance. These data are only available through August 2008, 12 months before the follow-up survey, due to the migration of the management information system at the bank, which took place in the third quarter of 2008.[61]

Among 3,682 clients in the base sample, 2,924 were randomly selected for the baseline survey and 2,262 completed the interview (77.4%), whereas 3,152 from the base sample completed the follow-up survey interview (85.6%). We construct three sets of samples for our analysis: clients who completed the baseline survey for the analysis of the determinants of PhilHealth take-up, clients who completed the follow-up survey for the analysis of average treatment impacts, and clients who completed both surveys for the analysis of correlations between baseline characteristics and health insurance coverage or utilization reported at the follow-up. Appendix Table C.1 presents differential attrition across experimental groups for different analysis samples. Even though the interaction terms between the treatment indicators and loan amount are weakly significant, the magnitudes of the coefficients indicate little qualitative significance. More importantly, the joint significance test across differential characteristics for each treatment arm suggests that there is no statistically meaningful difference in overall attrition across experimental groups.

PhilHealth Enrollment Measures

Our first outcome of interest is the decision to enroll in PhilHealth. A client receives PhilHealth coverage through either her own or her spouse's policy, and through KaSAPI or other programs (i.e., the IPP and Sponsorship programs). The PhilHealth administrative data we obtained identify quarterly KaSAPI membership for all clients between January 2007 and July 2009 and active PhilHealth membership (through own policy) under any program in the first and the third quarters of 2008. These data do not provide information on spouses' membership status.

[61]Green Bank adopted a new management information system in the fall of 2008. Unfortunately, the bank did not maintain the original customer IDs in the new system, making it nearly impossible to track the loan performance of a large number of clients in our sample.

We construct two measures of PhilHealth coverage. First, we define "PhilHealth enrollment in 2008" as having an active PhilHealth membership in the first and/or the third quarter of 2008 based on the administrative data. We use this as the main outcome for the take-up analysis. Second, we are also interested in identifying clients who had PhilHealth coverage over the 12 months preceding the follow-up survey (i.e., July 2008-2009) because our data on healthcare utilization focus on facility visits made during those months. We thus construct a proxy measure for PhilHealth coverage through own policy between July 2008-2009 by identifying clients who had an active PhilHealth membership in the 3rd quarter of 2008 according to the PhilHealth administrative data *and* reported having PhilHealth coverage through their own policy in the follow-up survey (we call this "long-term PhilHealth enrollment"). In addition, we use self-reported PhilHealth coverage through any policy at the follow-up survey as a supplemental outcome to assess insurance coverage at the extensive margin.[62]

Healthcare Utilization Measures

We gathered detailed information in the follow-up survey about the three most recent visits to formal healthcare facilities for each household member, if they took place within the 12 months preceding the survey. For each visit, the respondent was asked to report the main purpose of the visit, the type of care received, the length of stay, the cost of care,[63] and the use of insurance. Based on the PhilHealth benefit package at the time, we identify the facility visits that would have likely been covered by PhilHealth. A "facility visit for covered care" is defined by any facility visit for dialysis, prenatal care, deliveries,[64] postnatal care, or

[62]We only consider PhilHealth coverage because of the lack of access to other health insurance schemes for this population group. Over 97% of insurance policyholders at the follow-up survey were PhilHealth members.

[63]Unfortunately, there is a large variation in the cost of care, and we do not have the power to test treatment effects on this outcome.

[64]Note that we do not have sufficient information on reported deliveries to determine whether they were covered by PhilHealth (PhilHealth covers complicated deliveries and normal deliveries up to the fourth child). However, over 60% of respondents had fewer than four children at baseline and only 2% of reported health facility visits were for delivery. Excluding those visits from covered care does not affect our results.

the treatment of SARS and TB, or any visit that resulted in a hospital stay over 24 hours. We expect a relatively large margin of error given the difficulty of recalling the details of each visit and often the lack of sufficient information to precisely determine the applicability of PhilHealth benefits. Our categorization, however, strongly predicts insurance usage: Among 5,625 reported facility visits, the respondents with PhilHealth coverage reported using health insurance for 45% of facility visits for covered care, as opposed to 3.3% of other visits.

3.3 Empirical Method

3.3.1 Estimation Strategy

We first analyze predictors of PhilHealth enrollment among 1,732 clients in the compulsory and voluntary groups who completed the baseline survey. In the following OLS model, we estimate D_i, PhilHealth enrollment in 2008:

$$D_i = \alpha + \gamma X_i + \delta(V_i \cdot X_i) + V_i + \theta_s + v_i \tag{3.1}$$

where X_i denotes a vector of individual and household characteristics of client i, V_i the voluntary insurance treatment assignment, θ_s the regional fixed effects, and v_i the individual-specific error term. The vector X_i includes self-reported physical conditions, risk preference and behaviors, financial and human capital, and basic demographic characteristics of the client. If the intervention was implemented as designed, we expect individual characteristics to be uncorrelated with the enrollment decision in the compulsory group (i.e., γ = 0). The vector of coefficients δ captures the differential correlations between clients' characteristics and insurance take-up for the voluntary group. Positive coefficients δ on risk factors, such as poor health conditions, high willingness to take risks, and risky behaviors, would imply the presence of adverse selection.

Second, we estimate the impact of the intervention using the following OLS model:

$$Y_i = \alpha + \beta_C \cdot C_i + \beta_V \cdot V_i + S_i + \theta_s + v_i \tag{3.2}$$

where Y_i denotes a post-intervention outcome of interest for individual i, C and V the random assignments to the compulsory and voluntary treatment groups, respectively, and S_i the baseline survey assignment. The randomization ensures that v_i is uncorrelated with the treatment indicators. The coefficients β_C and β_V thus capture unbiased intent-to-treat (ITT) effects of the KaSAPI treatments. We also report in all tables the relative effects of the compulsory vs. voluntary insurance policy by taking the linear combination of the two coefficients: $\beta_C - \beta_V$.

To test the effect of adverse selection, we also estimate Equation (2) for outcomes on healthcare utilization with a restricted sample of clients with long-term PhilHealth membership. By restricting the sample to insured clients, the comparison of behaviors in the compulsory and voluntary groups could capture the effect of selection, independent of insurance coverage. If the voluntary scheme increases (decreases) the aggregate riskiness of the pool of insured clients, we would expect higher (lower) healthcare utilization in the voluntary compared to the compulsory group.[65]

3.3.2 Multiple Hypotheses Adjustments

We examine the treatment effects on a large number of outcomes related to healthcare-seeking behavior and household conditions across three experimental groups. This raises concerns about over-rejecting the null hypotheses due to their sheer number. We adjust for this multiple inference problem in two ways. First, we are interested in testing the treatment effects on individual outcomes related to PhilHealth take-up and healthcare utilization. For these outcomes we use the False Discovery Rate (FDR) method introduced by Benjamini

[65]The relative effect between the two treatment groups reflects the combined effect of selection and moral hazard unless the effect of moral hazard is homogenous within a population. We do not discuss the effect of moral hazard, which we believe is likely to be small in our study setting for two reasons. First, PhilHealth only provides partial coverage for most treatments. Second, a healthcare facility visit incurs substantial financial and opportunity costs.

and Hochberg (1995) to adjust p-values for the outcomes that belong to the same family.[66] Compared to the Bonferroni corrections, the FDR approach is more powerful because it focuses on the expected proportion of hypotheses that are subject to Type I error rather than the probability that at least one hypothesis is a Type I error (Fink et al., 2014). Second, we are interested in assessing the overall treatment effect on a broad range of downstream outcomes on health and economic conditions. For this analysis, we create an equally weighted index of all related responses from the follow-up survey for each family of outcomes.

3.4 Results

3.4.1 Impact on PhilHealth Take-up

We first examine the impact of the KaSAPI intervention on PhilHealth enrollment. Figure 3.1 illustrates the changes in PhilHealth coverage over time across experimental groups. All measures of PhilHealth coverage through own policy are substantially higher in the compulsory group, followed by the voluntary group.

Table 3.1 presents the ITT estimates of the impact of the compulsory and voluntary insurance policies. In Columns (1)-(3), we report the effects on PhilHealth coverage through the client's own policy. The intervention increased PhilHealth enrollment for both treatment arms, but the effects are significantly larger for the compulsory group. Column (1) shows that the compulsory policy increased enrollment by 30 percentage points in the 2nd year of intervention, a 100% increase relative to the control mean: roughly 60% of the compulsory clients enrolled in PhilHealth in 2008. With nearly one-third of clients covered by a spouse's PhilHealth at baseline, this implies that the compulsory PhilHealth policy was likely well-enforced.[67] Furthermore, Column (4) shows that the compulsory KaSAPI policy resulted in higher overall PhilHealth coverage even after two and a half years: three in four clients has

[66]Note that with three experimental groups, we have three null hypotheses for each regression.

[67]In addition, we verified the spouse's PhilHealth membership status with the local PhilHealth office for compulsory clients and found that 96% of the compulsory group had PhilHealth coverage through own or her spouse's policy at least for one quarter in the first 1.5 years of the intervention.

PhilHealth coverage at the time of the follow-up survey.

Table 3.1: Impact on PhilHealth enrollment

	PhilHealth coverage through own policy			PhilHealth coverage through own or spouse's policy at the follow-up
	PhilHealth enrollment in 2008	Long-term enrollment (over the 12 months preceding the follow-up)	Coverage at the follow-up	
	Jan - Dec 2008 (admin)	Jul '08 (admin) & Jul '09 (self-reported)	Jul 2009 (self-reported)	Jul 2009 (self-reported)
	(1)	(2)	(3)	(4)
Compulsory KaSAPI	0.302***	0.197***	0.189***	0.127***
	(0.021)	(0.018)	(0.021)	(0.020)
Voluntary KaSAPI	0.187***	0.075***	0.068***	0.036*
	(0.021)	(0.017)	(0.020)	(0.021)
Compulsory - Voluntary	0.115***	0.121***	0.120***	0.091***
	(0.022)	(0.020)	(0.021)	(0.020)
Mean DV, control group	0.288	0.146	0.274	0.625
Number of observations	3,152	3,152	3,152	3,152

Robust standard errors are in parentheses. Stars indicate the significance level: *p<0.10 **p<0.05 ***p<0.01. The significance levels in the first four columns use the FDR adjustment method introduced by Benjamini and Hochberg (1995). The dependent variable in Column (1) indicates whether a client had any PhilHealth coverage through her own policy between January and December 2008. The dependent variable in Column (2) indicates whether a client had her own PhilHealth policy in July 2008 (admin data) and reported PhilHealth coverage through her own policy in the follow-up survey. The dependent variables for Columns (3) and (4) indicate whether a client reported PhilHealth coverage through her own policy and through her own or spouse's policy, respectively, in the follow-up survey conducted in July 2009. *Compulsory - Voluntary* reports the linear combination of the coefficients on Compulsory KaSAPI and Voluntary KaSAPI. All regressions control for whether the client was randomly assigned to receive the baseline survey and use regional fixed effects.

Turning to the effect of the voluntary policy, the ITT effects presented in Table 3.1 indicate a relatively small increase in PhilHealth enrollment over time. PhilHealth enrollment in 2008 is 18.7 percentage points higher than the control group, or 64.9% of the control mean. By July 2009, the difference in the take-up diminishes to 6.8 percentage points, about one-third of the initial effect size (Column (1) vs. (3)). In addition, the treatment effect on any PhilHealth coverage is almost half of the effect on PhilHealth coverage through own policy, suggesting that the KaSAPI voluntary policy at least in part encouraged PhilHealth enrollment among clients who would have received PhilHealth coverage through their spouses in the absence of the program.

This diminishing voluntary treatment effect over time, however, may not necessarily indicate low demand for insurance. The decline in the treatment effect is highly correlated with the dropout rate from the credit program at Green Bank. Among the clients who remained in the credit program as of July 2008, the voluntary treatment effect on PhilHealth enrollment at the follow-up remains moderately large at 11.5 percentage points, or 40% of the control mean (results not shown). While this is an endogenous effect, we find no difference in the dropout rate or the type of clients who dropped out across experimental groups. These results together suggest that reducing the transaction costs of enrollment could potentially boost voluntary insurance take-up over time.

3.4.2 Determinants of PhilHealth Enrollment

We now examine the determinants of PhilHealth enrollment by estimating Equation (1). To investigate selection effects, we consider individual characteristics that reflect private information about the need for healthcare (i.e., self-reported health conditions and pregnancy), private information about risk preference (i.e., willingness to take risks and risky behaviors), and other relevant characteristics (i.e., financial capability, cognitive capability, and demographic characteristics). Table 3.2 presents the estimates for γ in Column (1) and the estimates for δ in Column (2). Column (1) shows that none of the individual characteristics is correlated with the take-up decision in the compulsory group after controlling for PhilHealth coverage through spouse's policy at baseline.[68] The joint significance p-value is 0.519, indicating no predictive power of individual characteristics for PhilHealth enrollment in the control group. This corroborates the earlier result that credit officers followed the protocol of mandating PhilHealth enrollment in the compulsory group.

Column (2), in contrast, provides evidence for both adverse and advantageous selection. Having chronic conditions increases the differential likelihood of enrollment in the voluntary

[68] We exclude this variable from the joint significance test on individual characteristics for the compulsory group because the mandatory enrollment policy only applied to those who had no coverage through their spouses.

Table 3.2: Determinants of insurance take-up

Dependent variable:	Any PhilHealth coverage through own policy, Jan - Dec 2008	
	Coefficient on the covariate (1)	Coefficient on the covariate interacted with the voluntary treatment indicator (2)
Voluntary KaSAPI	-0.140	
	(0.154)	
High needs for healthcare at baseline		
Self-reported poor health	-0.022	0.042
	(0.038)	(0.052)
Chronic pre-condition	-0.048	0.170***
	(0.038)	(0.052)
Non-chronic condition	0.013	-0.034
	(0.037)	(0.051)
Pregnant now or likely to become pregnant	-0.024	-0.009
	(0.064)	(0.089)
Risk characteristics at baseline		
Risk-loving	-0.002	-0.010
	(0.037)	(0.049)
Regularly drink or smoke	0.049	-0.182**
	(0.053)	(0.071)
Other characteristics at baseline		
Financial barriers to seeking formal healthcare	-0.017	0.015
	(0.037)	(0.053)
Cognitive skills index	-0.002	0.023
	(0.013)	(0.019)
Log of household savings	0.005	0.001
	(0.005)	(0.007)
Any qualified PhilHealth dependent	0.011	-0.006
	(0.011)	(0.015)
Age	-0.002	0.001
	(0.002)	(0.003)
Female	0.000	-0.112*
	(0.049)	(0.065)
Urban	-0.054	0.084*
	(0.034)	(0.048)
Covered by own PhilHealth policy	0.041	0.059
	(0.042)	(0.061)
Covered by spouse's PhilHealth policy	-0.208***	0.038
	(0.040)	(0.055)
Joint significance for individual characteristics (p-value)	0.519	0.006
Number of observations	1,732	
Mean of DV in the compulsory group	0.522	

Robust standard errors are in parentheses. *p<0.10 **p<0.05 ***p<0.01. The dependent variable is the indicator for whether a borrower had any PhilHealth coverage through own policy between January and December 2008. We report the coefficients on individual characteristics in Column (1) and the coefficients on the interactions between individual characteristics and the indicator for the voluntary treatment group in Column (2). Joint significance for individual characteristics in each treatment group is reported at the bottom of the table. Note that *Covered by spouse's health insurance* is excluded from the joint significance test for the compulsory group because borrowers who had existing coverage through a spouse's policy were not required to enroll in PhilHealth. *Pregnant now or likely to become pregnant* indicates whether the respondent (or his spouse) is pregnant now or reported > 50% chance of getting pregnant in the next 12 months. *Financial barriers to seeking formal healthcare* indicates respondents who reported any incident in the past 12 months where the household was unable to visit formal health facilities, unable to complete recommended treatments, or sought care from traditional healers due to the costs of formal healthcare. *Risk-loving* identifies respondents who reported high willingness to take risks (i.e., chose 10 on a 0-10 scale) for general, financial, health, and occupational matters. *Regularly drink or smoke* indicates individuals who reported smoking or drinking at least several times a week. *Cognitive skills index* is the principal component of indicators for having basic numeracy, literacy, and reading comprehension. The regression uses regional fixed effects.

group by 17 percentage points, while individuals who engage in risky behaviors (i.e., regular drinking or smoking) are differentially *less* likely to enroll in PhilHealth by a similar magnitude. These effects are not driven by collinearity: removing one from the specification does not affect the robust predictive power of the other. The joint significance p-value of < 0.01 confirms that these risk characteristics matter in determining the overall take-up decision.

We next verify that our baseline measures of risk characteristics reflect insured risks. Appendix Table C.6 Columns (1)-(2) show that three out of four variables related to the needs for healthcare and risky behaviors are all positively correlated with the likelihood of facility visits for covered care. Even though the overall predictive power of these models is quite low, positive correlations between risk characteristics and insured risks provide qualitative support that our findings on the determinants of PhilHealth enrollment implies the co-presence of adverse and advantageous selection.

Adverse selection on health conditions and advantageous selection on risk preference appear to remain persistent over time. In Appendix Table C.5, we show the correlations between the same set of individual characteristics and long-term PhilHealth enrollment in each experimental group. Columns (5)-(6) show that *self-reported poor health* and *chronic pre-conditions* are both positively correlated and *regular drinking or smoking* is negatively correlated with the outcome, even though individual correlations are relatively weak. When comparing the differential predictive power of these characteristics between the compulsory and voluntary groups using Equation (2), we find strong evidence for adverse selection on poor health and advantageous selection on risky behaviors (results are not shown).

3.4.3 Healthcare Utilization

We next investigate how KaSAPI treatments affect care-seeking and insurance usage patterns. Table 3.3 Panel A reports the ITT effects for the full sample. Interestingly, we find no large differences across experimental groups in any of the outcomes on healthcare utilization.

Table 3.3: Impact on healthcare and insurance utilization

	Facility visits		Facility visits for covered care		Facility visits for which the household used insurance		Financial barriers to seeking formal healthcare
	Indicator (1)	Number of visits (2)	Indicator (3)	Number of visits (4)	Indicator (5)	Number of visits (6)	(7)
Panel A. Full sample							
Compulsory KaSAPI	-0.011	0.006	-0.005	-0.001	0.008	0.003	-0.019
	(0.020)	(0.107)	(0.017)	(0.042)	(0.013)	(0.024)	(0.021)
Voluntary KaSAPI	-0.007	-0.155	-0.004	-0.032	0.012	0.004	-0.001
	(0.020)	(0.099)	(0.017)	(0.041)	(0.013)	(0.022)	(0.021)
Compulsory - Voluntary	-0.004	0.161	-0.000	0.031	-0.005	-0.000	-0.018
	(0.020)	(0.105)	(0.018)	(0.041)	(0.013)	(0.023)	(0.021)
Mean DV, control group	0.708	2.071	0.203	0.385	0.094	0.146	0.404
Number of observations	3,152	3,152	3,152	3,152	3,152	3,152	3,152
Panel B. Individuals who had own PhilHealth coverage in August 2008 (admin) and July 2009 (self-reported)							
Compulsory KaSAPI	-0.065	-0.249	-0.008	0.043	-0.017	-0.064	-0.064
	(0.042)	(0.250)	(0.041)	(0.103)	(0.035)	(0.062)	(0.046)
Voluntary KaSAPI	-0.057	-0.330	-0.030	0.006	0.008	-0.002	-0.065
	(0.046)	(0.256)	(0.044)	(0.110)	(0.039)	(0.068)	(0.050)
Compulsory - Voluntary	-0.009	0.081	0.022	0.036	-0.025	-0.062	0.001
	(0.039)	(0.223)	(0.036)	(0.100)	(0.032)	(0.052)	(0.040)
Mean DV, control group	0.758	2.439	0.242	0.452	0.166	0.255	0.401
Number of observations	741	741	741	741	741	741	741

Robust standard errors are in parentheses. Stars indicate the significance level based on the FDR adjustment method introduced by Benjamini and Hochberg (1995) separately for each panel: *p<0.10 **p<0.05 ***p<0.01. Panel B restricts the sample to those who had PhilHealth coverage through their own policy in August 2008 (administrative data) and in July 2009 (self-reported in the follow-up survey). *Compulsory - Voluntary* reports the linear combination of the coefficients on Compulsory and Voluntary. We construct the dependent variable in Columns (3)-(4) using the follow-up survey data on the three most recent visits to health facilities for each member of the household. *Covered care* includes treatments for SARS and TB, dialysis, prenatal and postnatal care, deliveries, and other inpatient care with a hospital stay over 24 hours. *Financial barriers to seeking care* is an indicator for reporting no incident over the last 12 months in which the household member was unable to seek formal healthcare due to costs. All regressions control for whether the client was randomly assigned to receive the baseline survey and use regional fixed effects.

The point estimates on the indicator for any facility visits for covered care in Column (3) are indistinguishable from zero. Even though the large confidence intervals imply that we cannot rule out small treatment effects (up to the ITT of 2.9 percentage points and the treatment-on-the-treated effect of 14.5 percentage points), our results suggest that increased Philhealth coverage through KaSAPI did not substantially reduce unmet needs for healthcare or induce moral hazard.

In Panel B, we report the differences in care-seeking behavior across experimental groups among PhilHealth enrollees to isolate the effects of selection. Point estimates for the compulsory-voluntary differences in facility visits for covered care (Columns (3) and (4)) are positive but noisy, which makes it difficult to assess whether adverse or advantage selection dominates in aggregate. However, the absence of large relative effects on these outcomes, together with the earlier robust evidence for co-presence of adverse and advantageous selection, suggests that it is unlikely that the aggregate care-seeking behavior is determined by severe adverse selection in this setting.

More surprisingly, we find no treatment effect on insurance utilization during facility visits. In the control group, nearly 25% of members with PhilHealth coverage reported insured risks (i.e., any facility visits for covered care). Out of those, 69% reported using health insurance. We observed a 20 percentage point increase in PhilHealth enrollment and no change in insured risks under the compulsory policy. We thus expect insurance utilization to increase by somewhere around 20% x 25% x 69% = 3.5 percentage points.[69] We can rule out the possibility that the compulsory KaSAPI policy increased insurance utilization at this magnitude: the regression estimate in Panel A Column (5) is less than one-fourth of this effect size with the upper bound of the confidence interval at 0.033.

The pattern in the facility visit-level data corroborates this result. In Appendix Table C.3, we compare PhilHealth coverage and insurance utilization rates across experimental groups for all facility visits for which the respondents provided details. The standard errors are clustered at the household level. The results illustrate large increases in PhilHealth coverage but only a small increase in insurance utilization under the compulsory policy. Columns (3)-(6) show that among the 1,044 health facility visits for covered care, PhilHealth coverage through own policy is 25 percentage points higher for the visits among clients in the compulsory group relative to the control mean of 17.5%, but the increase in insurance usage is <1 percentage point relative to the control mean of 30.4%.[70]

3.4.4 Why Is the Insurance Usage So Low?

Our findings so far suggest that selection matters in the insurance take-up decision, but increased insurance coverage through KaSAPI has a limited effect on case-seeking behaviors.

[69] We have the power to detect an effect of this magnitude at $\alpha = 0.05$ and $\beta = 0.2$.

[70] Note that the control mean of PhilHealth coverage rate among facility visits for covered care is lower than that of the insurance usage rate in Column (4) because the dependent variable in Columns (1) and (3) indicates long-term PhilHealth enrollment (i.e., borrowers with both active PhilHealth membership in July 2008 and self-reported PhilHealth coverage through own policy in July 2009) and therefore only captures a subsample of borrowers who had PhilHealth coverage during health facility visits reported in the follow-up survey.

In this section, we take a closer look at the healthcare and insurance utilization patterns to generate potential explanations for low insurance utilization. This part of the analysis is exploratory in nature, but we believe it sheds light on potentially important design considerations for the successful rollout of a social health insurance scheme.

First, the pattern in insurance utilization suggests that insured clients may face a learning curve in using PhilHealth. The rate of insurance utilization, conditional on experiencing an insured risk, is generally lower for the previously uninsured than for the previously insured. The average utilization rate is 44.2% (Appendix Table C.4), but the rate for the previously uninsured is 15 percentage points lower than the rate for the insured at baseline. In addition, Appendix Table C.4 Panel B illustrates that nearly 20% of the reasons for not using insurance among the insured directly relate to the PhilHealth utilization process (i.e., required documents for filing claims[71] and accredited facilities). This pattern holds across experimental groups, suggesting that the newly insured are slow to use PhilHealth regardless of the enrollment process. Given the complexity of any insurance scheme and the low awareness of the PhilHealth process in our sample, it is highly plausible that the newly insured clients learn how to take advantage of PhilHealth through experience.

Second, we highlight a potential challenge in providing high-quality information about a complex insurance product through a third-party institution. It is striking that the most common reason for not using health insurance shown in Appendix Table C.4 Panel B, by far, indicates that the facility visit was not covered by PhilHealth. The actual limitation of benefits, however, are unlikely to explain the null treatment effect on insurance utilization.[72] Instead, these responses potentially point to the lack of accurate information about benefits. Appendix Table C.2 illustrates low awareness of PhilHealth benefits in general: Columns (3)-(4) show that an average member in the control group correctly identifies less than 50%

[71]For example, if a patient did not have the receipt of premium payment during the facility visit, she would have to make a separate trip to a local PhilHealth office with the medical bills, PhilHealth membership information, and a claims form, which incurs substantial additional financial and psychological costs.

[72]The compulsory policy increased PhilHealth coverage without changing the riskiness of the insured pool or the incidence of insured risks. Thus, we would expect a higher level of insurance utilization under higher insurance coverage in a compulsory group compared to the control group.

of coverage restrictions and covered treatments/procedures. Even though the compulsory policy increases knowledge of the premium schedule and required documents for claims filing, we find no increase in the knowledge of benefit details. Furthermore, the insured clients in the treatment groups have a somewhat *lower* level of knowledge of PhilHealth benefit restrictions (Panel B). We speculate that this may in part reflect the challenge of delivering complex insurance products through a third-party institution. Even though credit officers attended a full-day seminar on the PhilHealth benefits and enrollment policies, they are unlikely to be perfect substitutes for well-trained PhilHealth marketers.

Third, the compulsory nature of the treatment may have affected the motivation for insurance utilization. The rate of insurance utilization is nearly 20 percentage points lower for the previously uninsured than the insured in the compulsory group as shown in Appendix Table C.4 Panel A. This is twice as large as the corresponding gap in either the control or the voluntary group. This pattern indicates potential presence of another barrier in the compulsory group. Although we are unable to identify a specific mechanism, the behavioral literature point to one plausible story: eliminating an active decision to enroll in an insurance policy, the compulsory treatment may have reduced the motivation or commitment for utilizing insurance.

Again, the discussion in this section is somewhat speculative. However, it highlights how the designs of marketing and enrollment processes may influence later decision-making on insurance utilization.

3.4.5 Downstream Household Outcomes

Absent the effects on healthcare and insurance utilization, the KaSAPI treatments are unlikely to affect downstream household outcomes. We show in Table 3.4 that the intervention, in fact, had little effect on six indices that capture the following domains of household behaviors and conditions: risk behaviors, subjective well-being, assets, health shocks, economic activities, and informal risk-sharing. Each index takes the mean of several indicator variables on related survey responses. (See Appendix C.7 for the definitions) Out of 18 coefficients we report, only two are significant at the 10% level. Overall, these results provide no evidence that the intervention had any meaningful effect on household conditions.

Table 3.4: Impact on household outcomes

	Risky behaviors	Subjective well-being	Asset index	Health shocks	Economic activities	Informal risk-sharing
	(1)	(2)	(3)	(4)	(5)	(6)
Compulsory KaSAPI	-0.007	0.006	0.004	-0.022*	-0.009	0.001
	(0.010)	(0.007)	(0.006)	(0.013)	(0.014)	(0.013)
Voluntary KaSAPI	-0.001	-0.013*	-0.002	0.000	0.005	-0.012
	(0.010)	(0.007)	(0.006)	(0.013)	(0.013)	(0.013)
Compulsory - Voluntary	-0.006	0.019	0.005	-0.022	-0.014	0.013
	(0.010)	(0.007)	(0.006)	(0.013)	(0.014)	(0.013)
Mean DV, control group	0.283	0.611	0.515	0.232	0.837	0.349
Number of observations	3,152	3,152	3,152	3,152	3,152	3,152

Robust standard errors are in parentheses. Stars indicate the significance level: *p<0.10 **p<0.05 ***p<0.01. *Compulsory - Voluntary* reports the linear combination of the coefficients on Compulsory and Voluntary. The definition of each index is summarized in Appendix Table 6. Panel B restricts the sample to those who had PhilHealth coverage through their own policy in August 2008 (administrative data) and in July 2009 (self-reported in the follow-up survey). All regressions control for whether the client was randomly assigned to receive the baseline survey and use regional fixed effects.

3.4.6 Retention and Loan Performance at the Bank

Similarly, offering PhilHealth through KaSAPI brought no benefits to Green Bank. Reducing default risks caused by health shocks and improving satisfaction and retention among members are the two main motivations for banks and cooperatives to participate in KaSAPI. We did not observe an exodus of clients when mandating PhilHealth coverage, unlike a similar study in India (Banerjee et al., 2014). Table 3.5 shows, however, that neither the compulsory nor the voluntary policy had any positive effect on retention, loan disbursements, or loan performance over 18-24 months. Without improvements in loan portfolio and retention, partner institutions may find it inefficient to provide the PhilHealth enrollment service.

3.5 Conclusion

The microinsurance industry continues to innovate product and process designs in an effort to improve outreach and impacts. KaSAPI was designed to increase the efficiency of health

Table 3.5: Impact on borrowing behavior at Green Bank

Dependent variable: Data source:	Active loan index (admin/survey)	Total loan amount (admin/survey)	Past-due loan (admin)	Proportion of weeks with missed payments (admin)
	Jan 07- Jul 09	Jan 07- Jul 09	Aug 2008	Jan 07- Aug 08
	(1)	(2)	(3)	(4)
Compulsory KaSAPI	0.012	2029.302	-0.001	0.004
	(0.018)	(4391.329)	(0.010)	(0.007)
Voluntary KaSAPI	0.006	2742.327	0.016	0.011
	(0.018)	(4838.830)	(0.010)	(0.007)
Compulsory - Voluntary	0.006	-713.025	-0.017	-0.007
	(0.018)	(5050.115)	(0.011)	(0.007)
Mean DV, control group	0.640	59,561.86	0.056	0.142
Number of observations	3,152	3,152	3,152	3,152

Robust standard errors are in parentheses. Stars indicate the significance level based on the FDR adjustment method introduced by Benjamini and Hochberg (1995): *p<0.10 **p<0.05 ***p<0.01. *Compulsory - Voluntary* reports the linear combination of the coefficients on Compulsory and Voluntary. Dependent variables are constructed using the administrative data from Green Bank through August 2008 and the follow-up survey data collected in July 2009. *Active loan index* is an equally weighted index of having any active loan in August 2008 (admin data) and having any active loan over the 12 months prior to the follow-up survey in July 2009 (self-reported). *Total loan amount* aggregates the amount of loans taken out between January 2007 and August 2008 (admin data) and the amount of loans taken out over the 12 months prior to the follow-up survey (self-reported). *Total loan amount* is winsorized at the 99th percentile. All regressions control for whether the client was randomly assigned to receive the baseline survey and use regional fixed effects.

insurance delivery among low-income households in the informal sector by outsourcing the marketing and enrollment process to local financial institutions. Using a field experiment, this study examined the relative effects of the compulsory and voluntary KaSAPI policies among existing clients of a rural bank in the Philippines. Our analysis led to three main findings. First, providing PhilHealth policies through a rural bank on a compulsory and voluntary basis increased PhilHealth enrollment without reducing retention in the credit program. Two and a half years later, self-reported PhilHealth enrollment rates in the compulsory and voluntary groups are, respectively, 18.9 and 6.8 percentage points higher than the control mean of 27.4%. Second, contrary to the conventional view that a voluntary scheme would increase the riskiness of the insured pool, we find the co-presence of adverse and advantageous selection in the take-up decision and no substantial increase in healthcare utilization among the insured clients in the voluntary group compared to their counterparts in the control group.[73] Third, despite a large increase in insurance coverage, the intervention did not encourage insurance utilization among those who made facility visits for covered care.

The growing research on micro-health insurance schemes suggests that the demand for insurance is often low among the poor, and where there is a demand, impacts on healthcare utilization and health outcomes are limited. The literature has identified a number of barriers to purchasing insurance (Cole, 2015), but less is known about barriers to *using* insurance. Our analysis suggests that there may be a learning curve to using insurance when familiarity with the insurance process is low. Other studies have documented similar findings on the underutilization of PhilHealth. According the 2003 Demographic Health Survey, for example, the most commonly cited reasons for not filing insurance claims are related to a lack of information about benefits and the requirements for claims filing, rather than access to (accredited) healthcare facilities (**?**). A more recent survey of 2,046 PhilHealth members in Manila also documents that lack of knowledge on filing claims and unawareness of benefits are the top two reasons for underutilization of PhilHealth (**?**). In the presence of barriers to information, design features that help beneficiaries learn about benefits and processes and internalize those information would be key to increasing utilization. Third-party marketing and the compulsory nature of the treatment may have run counter to this insight in our setting.

The literature provides increasing evidence that "making it easy" helps improve take-up of a range of welfare-enhancing products and services. Our findings highlight the complexity of insurance products in that certain design features that reduce constraints on insurance take-up may also increase constraints on utilization. To bridge the gap in take-up and utilization, barriers to take-up and usage must be jointly considered in designing an

[73] It is important to note that our analysis has limited power to detect small effects on healthcare utilization. Due to the unexpectedly high dropout rate from the credit program at Green Bank across experimental groups, the treatment effects on insurance coverage became significantly smaller over time. Even though our results consistently suggest the absence of large treatment effects on care-seeking behavior, they do not provide robust evidence for null treatment effects.

insurance scheme.

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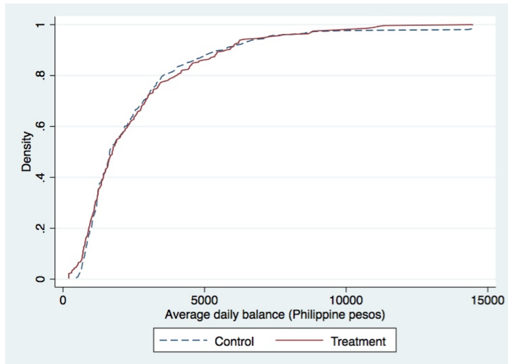
Appendix A

Appendix to Chapter 1

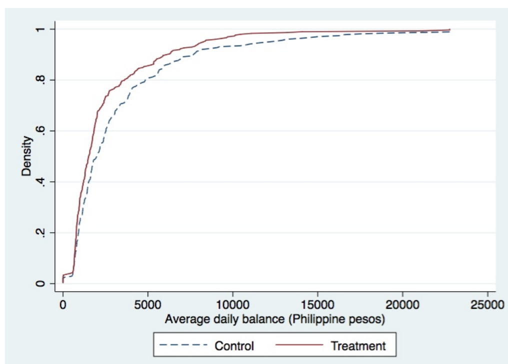
A.1 Supplemental Figures and Tables

Figure A.1: CDFs of average daily balances

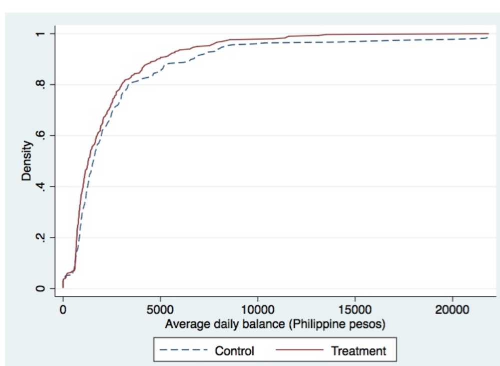
(a) 1 year before the intervention



(b) 15 months after the intervention
(before 3 control centers received mobile banking)



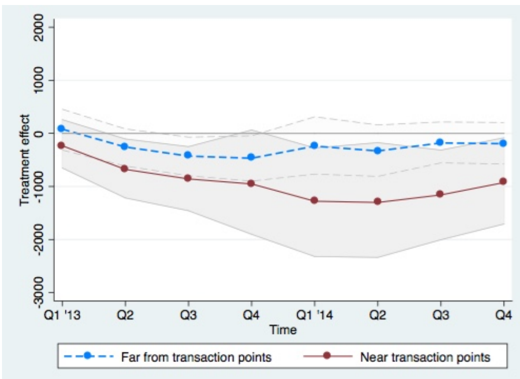
(c) 2 years after the intervention
(after 3 control centers received mobile banking)



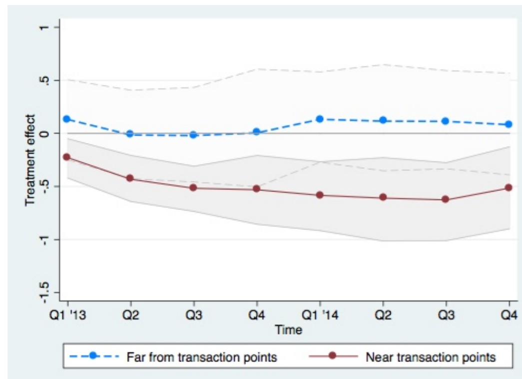
Notes: These graphs plot cumulative density functions for average daily balances in the treatment and control centers before and after the intervention. The red solid line shows the cdf for the treatment group and the blue dashed line shows the cdf for the control group.

Figure A.2: Quarterly treatment effects on Bank savings and loan outcomes by baseline proximity to transaction points over time

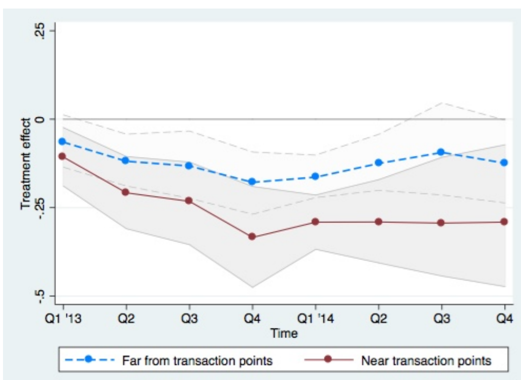
(a) Average daily balance (winsorized at the 99th percentile)



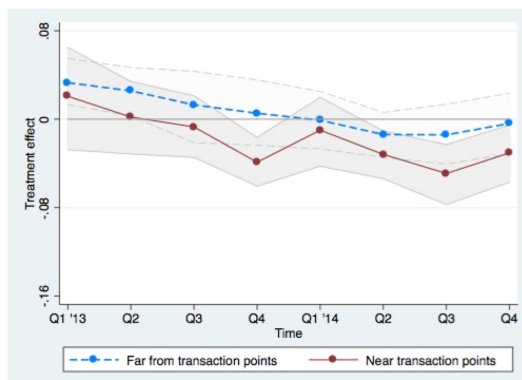
(b) Log of average daily balance



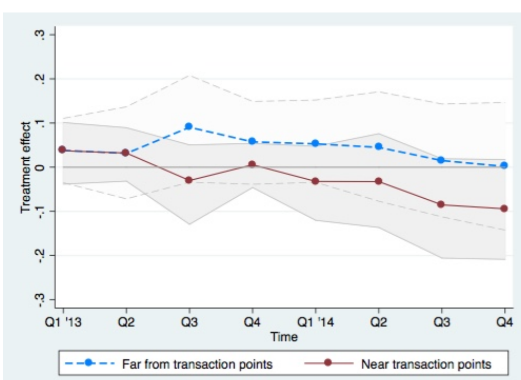
(c) Deposit likelihood



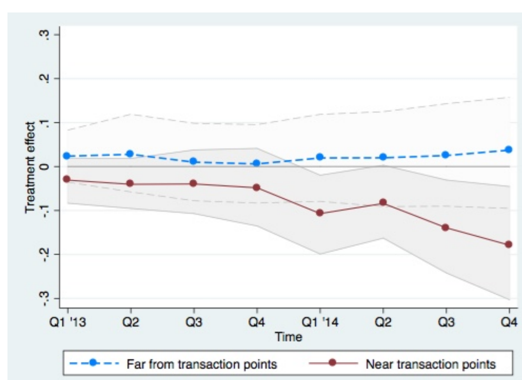
(d) Withdrawal likelihood



(e) Active loan



(f) Active savings account



These graphs plot the changes in quarterly treatment effects over time separately for members close to and far from transaction points. The blue dashed line and red solid line show the treatment effects for members far from transaction points and those close to transaction points, respectively. Gray shaded areas show the wild bootstrap 95% confidence intervals.

Table A.1: Follow-up survey attrition
Sample: Full sample

Dependent variable: Completed the follow-up survey			
	Coefficient on Treatment indicator	Clustered std error	Wild bootstrap p-value
	(1)	(2)	(3)
Treatment	-0.022	0.108	0.850
Age	0.006*	0.002	0.052
Treatment x Age	0.051	0.077	0.730
Female	0.116*	0.058	0.055
Treatment x Female	-0.001	0.002	0.584
Regular borrower	0.053	0.090	0.617
Treatment x Regular borrower	0.002	0.067	0.978
Control mean	0.790		
Number of observations	575		

This table reports the differential treatment effects on the likelihood of completing the follow-up survey by demographic characteristics. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. The regression uses fied effects for center-pair.

Table A.2: Baseline characteristics
Sample: Members who completed the follow-up survey

	Control mean	Coefficient on Treatment indicator	Clustered std error	Wild bootstrap p-value
	(1)	(2)	(3)	(4)
Owns a mobile phone	0.72	0.033	0.038	0.584
Household operates a microenterprise	0.679	-0.025	0.048	0.799
Proximity index to transaction points	0.170	-0.473	3.017	0.456
Access to the nearest bank office				
Time	23.88	-1.096	3.032	0.813
Cost	21.06	-0.255	1.968	0.951
Distance	7.847	-1.754**	0.673	0.113
Access to the center meeting location				
Time	11.16	-2.006	1.140	0.291
Cost	1.736	0.154	0.850	0.943
Distance	0.590	-0.081	0.090	0.631

This table reports the descriptive statistics on baseline characteristics, retrospectively collected during the follow-up survey. Columns 2-4 report the coefficient on the treatment indicator, clustered standard errors, and wild bootstrap p-values from the regression estimating the correlation between the treatment assignment and each of the baseline characteristics in Equation 5. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. *Proximity index* takes the first principal component score of quarter indicators for self-reported travel cost, time, and distance to reach the nearest bank office and the center meeting location in 2012. This regression uses fixed effects for center-pair.

Table A.3: Impact on Bank savings and loan outcomes: IV estimates
Sample: Full sample/Members who completed the follow-up survey

	Average daily balance		Deposit likelihood	Withdrawal likelihood	Active loan	Active savings account
	Winsorized	Log value				
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Average effects (sample: full)						
Treatment x Post	-811.289**	-0.246	-0.236***	-0.004	0.015	-0.048
	(281.306)	(0.155)	(0.043)	(0.006)	(0.025)	(0.037)
	[0.026]	[0.145]	[0.001]	[0.534]	[0.568]	[0.215]
Number of observations	88,549	88,549	88,549	88,549	88,549	88,549
Control mean (post-intervention)	3170.87	8.126	0.768	0.075	0.396	0.908
Panel B. Heterogeneous effects by proximity to transaction points (sample: completed the follow-up survey)						
Treatment x Post	-384.250*	0.076	-0.151***	0.006	0.049	0.025
	(161.281)	(0.214)	(0.039)	(0.009)	(0.041)	(0.045)
	[0.075]	[0.743]	[0.004]	[0.578]	[0.280]	[0.584]
Treatment x Near x Post	-885.487**	-0.699**	-0.167***	-0.027**	-0.082*	-0.129**
	(256.070)	(0.271)	(0.051)	(0.012)	(0.039)	(0.056)
	[0.013]	[0.022]	[0.008]	[0.041]	[0.061]	[0.033]
Near x Post	369.414	0.404	0.081	0.020**	0.035*	0.071
	(186.996)	(0.218)	(0.036)	(0.008)	(0.025)	(0.051)
	[0.384]	[0.260]	[0.208]	[0.046]	[0.072]	[0.291]
Near	391.610	0.138	0.041*	0.014**	0.156	0.015
	(390.996)	(0.114)	(0.029)	(0.006)	(0.066)	(0.014)
	[0.131]	[0.128]	[0.072]	[0.034]	[0.213]	[0.191]
Total effect for Near						
Wild-bootstrap p-value	0.020	0.002	<0.001	0.014	0.237	0.031
Number of observations	68,159	68,159	68,159	68,159	68,159	68,159
Control mean (post-intervention)	3096.010	8.193	0.803	0.073	0.426	0.426
Week-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the 2SLS estimates of the treatment effects reported in Tables 2 and 3. *Average daily balance* in Column 1 is winsorized at the 99th percentile within each week; the log value of daily average balance in Column 2 uses the natural log transformation. I report the standard errors clustered at the center level in parenthesis and score bootstrap p-value in bracket under each coefficient. Stars indicate *10%, **5%, and ***1% significance levels using two-sided score bootstrap p-values. Score bootstrap uses Rademacher weights and 5000 replications. *Near* indicates members with below-median index of self-reported time, distance, and cost to center meeting and bank office locations within each center-pair.

Table A.4: Quarterly impact on Bank savings and loan outcomes
Sample: Full sample

	Average daily balance		Deposit likelihood	Withdrawal likelihood	Active loan	Active savings account
	Winsorized	Log value				
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x 1st quarter 2013	-217.653	-0.101	-0.091***	0.037***	0.046	0.000
	(148.713)	(0.075)	(0.027)	(0.009)	(0.026)	(0.024)
	[0.195]	[0.207]	[0.009]	[<0.001]	[0.124]	[0.997]
Treatment x 2nd quarter 2013	-597.272***	-0.257**	-0.166***	0.010*	0.044	-0.006
	(182.188)	(0.099)	(0.036)	(0.005)	(0.027)	(0.026)
	[0.009]	[0.024]	[<0.001]	[0.089]	[0.156]	[0.826]
Treatment x 3rd quarter 2013	-697.592**	-0.272*	-0.180***	0.010	0.032	-0.025
	(223.729)	(0.118)	(0.041)	(0.011)	(0.025)	(0.025)
	[0.010]	[0.055]	[<0.001]	[0.384]	[0.246]	[0.363]
Treatment x 4th quarter 2013	-734.506*	-0.260	-0.259***	-0.016*	0.038	-0.024
	(326.803)	(0.174)	(0.045)	(0.008)	(0.023)	(0.027)
	[0.066]	[0.155]	[0.001]	[0.089]	[0.150]	[0.392]
Treatment x 1st quarter 2014	-805.839*	-0.195	-0.226***	-0.007	0.021	-0.051
	(385.344)	(0.159)	(0.025)	(0.011)	(0.031)	(0.034)
	[0.067]	[0.306]	[<0.001]	[0.513]	[0.529]	[0.181]
Treatment x 2nd quarter 2014	-877.680**	-0.195	-0.224***	-0.022**	0.009	-0.045
	(404.457)	(0.171)	(0.046)	(0.007)	(0.044)	(0.041)
	[0.041]	[0.298]	[<0.001]	[0.011]	[0.850]	[0.289]
Treatment x 3rd quarter 2014	-770.990**	-0.201	-0.204**	-0.022*	-0.039	-0.069
	(338.468)	(0.155)	(0.065)	(0.010)	(0.036)	(0.040)
	[0.025]	[0.264]	[0.022]	[0.069]	[0.310]	[0.100]
Treatment x 4th quarter 2014	-670.018*	-0.156	-0.218**	-0.014	-0.042	-0.091
	(335.302)	(0.169)	(0.065)	(0.009)	(0.039)	(0.050)
	[0.051]	[0.432]	[0.013]	[0.142]	[0.302]	[0.122]
Number of observations	88,549	88,549	88,549	88,549	88,549	88,549
Control mean (post-intervention)	3096.010	8.193	0.803	0.073	0.426	0.426

Reported coefficients are interactions between Treatment and post-intervention quarter indicators. *Average daily balance* in Column 1 is winsorized at the 99th percentile within each week; the log value of daily average balance in Column 2 uses the natural log transformation. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. The regressions use center-pair and week-year fixed effects.

Table A.5: Determinants of proximity to transaction points
Sample: Members who completed the follow-up survey

	Proximity index (High values indicate physical proximity to transaction points) (1)	Near (Proximity index below-median) (2)
Any enterprise	0.020	-0.024
	(0.099)	(0.053)
	[0.842]	[0.660]
Main occupation: Salaried employment	-0.052	-0.040
	(0.163)	(0.064)
	[0.776]	[0.593]
Own any transportation asset	0.300	0.086
	(0.187)	(0.048)
	[0.136]	[0.100]
High mobile literacy	-0.069	-0.019
	(0.053)	(0.022)
	[0.210]	[0.413]
Finished secondary school	0.364*	0.150**
	(0.192)	(0.062)
	[0.093]	[0.039]
Age	0.005	0.003
	(0.004)	(0.002)
	[0.301]	[0.143]
Married	-0.014	0.040
	(0.139)	(0.070)
	[0.926]	[0.568]
Female	-0.135	-0.100
	(0.153)	(0.077)
	[0.368]	[0.232]
Log of baseline balance	-0.122	-0.027
	(0.093)	(0.038)
	[0.198]	[0.465]
Borrower	0.116	0.006
	(0.166)	(0.066)
	[0.485]	[0.930]
Bank membership since 2011	-0.009	-0.010
	(0.146)	(0.070)
	[0.944]	[0.909]
Number of observations	448	448
Control mean	0.032	0.484

I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. *Proximity index* is the first principal component score of self-reported cost, time, and distance to center meeting and bank office locations. *Near* indicates below-median proximity index within each center pair. All regressions use center fixed effects.

Table A.6: Impact on weekly deposit and withdrawal amounts
Sample: Full sample/Members who completed the follow-up survey

	Weekly deposit amount		Weekly withdrawal amount	
Winsorized at:	99th percentile	95th percentile	99th percentile	95th percentile
	(1)	(2)	(3)	(4)
Panel A. Average effects (sample: full)				
Treatment x Post	-10.454	-15.577**	-10.736	-4.586
	(8.021)	(5.740)	(11.814)	(2.749)
	[0.240]	[0.025]	[0.391]	[0.145]
Number of observations	88,549	88,549	88,549	88,549
Control mean (post-intervention)	93.167	77.290	108.462	36.132
Panel B. Heterogeneous effects by proximity to transaction points (sample: completed the follow-up survey)				
Treatment x Post	4.225	-5.230	17.583	-0.078
	(6.895)	(5.016)	(12.410)	(4.120)
	[0.570]	[0.341]	[0.189]	[0.981]
Tretment x Post x Near	-29.830**	-22.465***	-53.994***	-10.423**
	(10.665)	(6.732)	(14.954)	(4.676)
	[0.023]	[0.007]	[0.005]	[0.049]
Post x Near	14.409*	10.447	32.560*	6.458*
	(7.190)	(4.799)	(10.216)	(3.108)
	[0.097]	[0.109]	[0.053]	[0.092]
Near	14.136	10.261	20.915	6.973**
	(15.690)	(8.251)	(12.166)	(2.960)
	[0.409]	[0.299]	[0.166]	[0.022]
Total effect for Near				
Wild-bootstrap p-value	0.062	0.003	0.081	0.012
Number of observations	69,055	69,055	69,055	69,055
Control mean (post-intervention)	94.936	79.043	114.814	38.854
Week-year fixed effects	Yes	Yes	Yes	Yes
Center-pair fixed effects	Yes	Yes	Yes	Yes

This table presents the average treatment effects on weekly deposit and withdrawal amounts at the Bank and for the heterogeneous effects by proximity to transaction points. *Near* indicates below-median index of self-reported travel cost, time, and distance to center meting and bank office locations within each center pair. I report the standard errors clustered at the center level in parenthesis and wild bootstrap p-value in bracket under each coefficient. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. Wild bootstrap uses Rademacher weights and 5000 replications.

Table A.7: Impact on the components of group defection index
Sample: Members who completed the follow-up survey

	Group defection index					
		Low importance of …				
	Self-reported meeting absence	Lown status of others	Reputation of new members	No interaction with center members	No interaction with bank staff	Very important to save every week
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Average effects						
Treatment	0.071**	0.083**	0.115**	0.021	0.041	-0.114**
	(0.025)	(0.025)	(0.036)	(0.034)	(0.031)	(0.034)
	[0.040]	[0.019]	[0.029]	[0.728]	[0.434]	[0.023]
Panel B. Heterogeneous effects by proximity to transaction points						
Treatment	-0.025	0.039	0.075	-0.032	0.057	-0.118***
	(0.056)	(0.040)	(0.051)	(0.059)	(0.031)	(0.031)
	[0.666]	[0.425]	[0.246]	[0.680]	[0.128]	[0.006]
Treatment x Near	0.192*	0.099	0.090	0.112	-0.023	0.003
	(0.097)	(0.073)	(0.074)	(0.069)	(0.050)	(0.074)
	[0.069]	[0.216]	[0.249]	[0.137]	[0.633]	[0.968]
Near	-0.024	-0.096	-0.108*	-0.068	-0.062	0.043
	(0.060)	(0.066)	(0.058)	(0.040)	(0.040)	(0.062)
	[0.682]	[0.220]	[0.094]	[0.144]	[0.209]	[0.507]
Total effect for *Near*						
Wild bootstrap p-value	0.006	0.008	0.022	0.028	0.565	0.176
Number of observations	448	448	448	448	448	448
Control mean	0.628	0.606	0.564	0.303	0.908	0.619
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the treatment effects on each component of the group defection index and on the self-reported importance of weekly savings habit. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. *Near* indicates below-median index of self-reported travel cost, time, and distance to center meeting and bank office locations within each center pair. Wild bootstrap uses Rademacher weights and 5000 replications. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. All regressions use fixed effects for center-pair.

Table A.8: Alternative explanations:
Heterogeneous impact by loan status and mobile literacy

	Average daily balance (1)	Deposit likelihood (2)	Withdrawal likelihood (3)	Average daily balance (4)	Deposit likelihood (5)	Withdrawal likelihood (6)
Treatment x Post	-621.446*	-0.202***	0.005	-816.834**	-0.187***	-0.002
	(313.425)	(0.044)	(0.006)	(305.141)	(0.036)	(0.007)
	[0.058]	[0.001]	[0.407]	[0.029]	[0.002]	[0.842]
Treatment x Borrower at baseline x Post	-110.381	0.006	-0.014*			
	(405.730)	(0.040)	(0.008)			
	[0.788]	[0.886]	[0.097]			
Borrower at baseline	55.888	0.165***	0.035***			
	(371.076)	(0.030)	(0.006)			
	[0.884]	[0.006]	[0.006]			
Treatment x Low mobile literacy x Post				407.216	-0.018	-0.013
				(378.616)	(0.028)	(0.009)
				[0.350]	[0.535]	[0.200]
Low mobile literaacy				-459.326	0.007	-0.000
				(398.449)	(0.026)	(0.007)
				[0.357]	[0.820]	[0.986]
Number of observations	87,399	87,399	87,399	68159	68159	68159
Control mean (post-intervention)	3093.321	0.801	0.073	3116.78	0.796	0.082
Week-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Center-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the heterogeneous treatment effects on Bank savings by baseline loan status and mobile literacy. *Average daily balance* is winsorized at the 99th percentile within each week. Borrower at baseline indicates the members who had active loans over 6 months prior to the intervention. Low mobile literacy indicates members who did not own a moblie phone or did not feel comfortable sending an SMS message in 2012. I report the standard error clustered at the center level in parenthesis and the wild bootstrap p-value in bracket under each coefficient. Stars indicate *10%, **5%, and ***1% significance levels using two-sided wild bootstrap p-values. Wild bootstrap uses Rademacher weights and 5000 replications.

Table A.9: Small cluster sample tests

	Treatment effect (β)				Treatment x Near (β^n)			
	Coefficient	Wild bootstrap	Randomization inference	T-based approach	Coefficient	Wild bootstrap	Randomization inference	T-based approach
Table 3 Panel A								
Average daily balance (winsorized)	-622.88	0.025	0.031	0.037				
Average daily balance (log)	-0.220	0.103	0.195	0.182				
Deposit indicator	-0.188	<0.001	0.008	0.002				
Withdrawal indicator	0.006	0.335	0.328	0.122				
Active loan	0.036	0.050	0.055	0.049				
Active savings account	-0.022	0.431	0.313	0.415				
Table 3 Panel B								
Average daily balance (winsorized)	-681.16	0.028	0.047	0.044				
Average daily balance (log)	-0.206	0.169	0.266	0.239				
Deposit indicator	-0.198	<0.001	0.016	<0.001				
Withdrawal indicator	-0.004	0.555	0.609	0.568				
Active loan	0.013	0.585	0.461	0.423				
Active savings account	-0.040	0.232	0.367	0.275				
Table 4								
Average daily balance (winsorized)	-305.123	0.091	0.117	0.089	-747.830	0.014	0.039	0.069
Average daily balance (log)	0.072	0.714	0.828	0.710	-0.589	0.026	0.016	0.026
Deposit indicator	-0.124	0.004	0.023	0.010	-0.140	0.010	0.008	0.007
Withdrawal indicator	0.007	0.474	0.508	0.191	-0.026	0.030	0.055	0.064
Active loan	0.045	0.269	0.305	0.374	-0.074	0.055	0.148	0.171
Active savings account	0.021	0.627	0.664	0.607	-0.105	0.037	0.039	0.031
Table 5								
Proportion of weeks with any arrears	0.041	0.005	0.031	--				
Proportion of weeks with any arrears	0.052	0.001	0.008	--	-0.018	0.316	0.289	--
Proportion of weeks with NPLs	0.015	0.090	0.125	--				
Proportion of weeks with NPLs	0.017	0.015	0.039	--	-0.002	0.792	0.789	--
Average daily value of NPLs	18.99	0.202	0.156	--				
Average daily value of NPLs	15.67	0.379	0.430	--	8.35	0.693	0.656	--
Table 6								
Procrastination tendency	-0.043	0.506	0.500	--				
Procrastination tendency	-0.047	0.246	0.281	--	0.014	0.767	0.781	--
Fee sensitivity	0.081	0.199	0.203	--				
Fee sensitivity	-0.008	0.917	0.983	--	0.190	0.090	0.109	--
Group defection index	0.237	0.151	0.156	--				
Group defection index	0.063	0.702	0.695	--	0.379	0.031	0.063	--
Table 7 Panel A								
Savings at the Bank (99th percentile)	-893.95	0.284	0.305	--				
Savings at the Bank (9tth percentile)	-501.77	0.386	0.367	--				
Non-Bank savings (99th percentile)	-533.88	0.883	0.867	--				
Non-Bank savings (95th percentile)	327.19	0.698	0.531	--				
Total HH savings (99th percentile)	-1274.31	0.723	0.742	--				
Total HH savings (95th percentile)	-303.74	0.825	0.773	--				
Self-employed	-0.027	0.509	0.461	--				
Salaried work	0.021	0.628	0.539	--				
Casual work	0.014	0.759	0.719	--				
Any enterprise in the last 12 months	-0.026	0.686	0.641	--				

Table A.9: (continued)

	Treatment effect (β)				Treatment x Near (β^n)			
	Coefficient	Wild bootstrap	Randomization inference	T-based approach	Coefficient	Wild bootstrap	Randomization inference	T-based approach
Table 7 Panel B								
Savings at the Bank (99th percentile)	-110.492	0.893	0.891	--	-1799.984	0.032	0.055	--
Savings at the Bank (95tth percentile)	-84.526	0.866	0.922	--	-986.880	0.089	0.148	--
Non-Bank savings (99th percentile)	72.527	0.959	0.773	--	-1710.174	0.331	0.164	--
Non-Bank savings (95th percentile)	545.777	0.404	0.242	--	-497.531	0.539	0.391	--
Total HH savings (99th percentile)	-74.840	0.973	0.727	--	-2991.454	0.183	0.023	--
Total HH savings (95th percentile)	657.102	0.507	0.398	--	-2058.210	0.090	0.008	--
Self-employed	0.018	0.691	0.664	--	-0.093	0.245	0.219	--
Salaried work	-0.003	0.924	0.906	--	0.045	0.474	0.406	--
Casual work	0.030	0.397	0.461	--	-0.031	0.475	0.539	--
Any enterprise in the last 12 months	0.056	0.487	0.617	--	-0.170	0.144	0.234	--
Table 8 Panel A								
Total # of shocks	0.013	0.884	0.797	--				
Withdrew savings	0.053	0.109	0.188	--				
Received gifts from friends	0.026	0.293	0.227	--				
Borrowed/Sold assets/Cut consumption	0.019	0.709	0.414	--				
# of transfers given	0.491	0.730	0.656	--				
# of loans given	0.947	0.491	0.391	--				
# of transfers received	0.594	0.577	0.609	--				
# of loans received	-0.160	0.914	0.836	--				
Net giver	0.064	0.105	0.141	--				
Table 8 Panel B								
Total # of shocks	0.019	0.753	0.422	--	-0.020	0.873	0.656	--
Withdrew savings	0.077	0.076	0.070	--	-0.049	0.498	0.492	--
Received gifts from friends	0.063	0.070	0.039	--	-0.082	0.217	0.195	--
Borrowed/Sold assets/Cut consumption	-0.027	0.425	0.383	--	0.101	0.340	0.383	--
# of transfers given	-0.490	0.748	0.898	--	2.134	0.239	0.258	--
# of loans given	2.264	0.272	0.289	--	-2.744	0.405	0.477	--
# of transfers received	0.143	0.928	0.953	--	1.169	0.641	0.695	--
# of loans received	-1.575	0.309	0.469	--	3.049	0.027	0.094	--
Net giver	0.152	0.072	0.117	--	-0.195	0.137	0.195	--

A.2 Sample selection

In the catchment areas of the two bank officers, there were 105 microfinance centers handled by seven account officers at the time. Out of 105 centers, 31 were removed from the sample due to weak cell phone signal, exposure to mobile banking during the beta testing, and discrepancies in the monthly performance report. The remaining 74 centers were stratified by account officer, travel time from the bank office, and the average loan repayment rate over 6 months. Centers within each stratum were then ranked by center size and paired up with a nearest-ranked center from another village. This process generated 32 pairs of centers. (Not all centers were matched because of the large variation in the number of centers per village.) The Bank randomly chose one pair of centers per account officer for the mobile banking pilot evaluation, and randomly assigned one center within each pair to receive mobile banking.

Appendix B

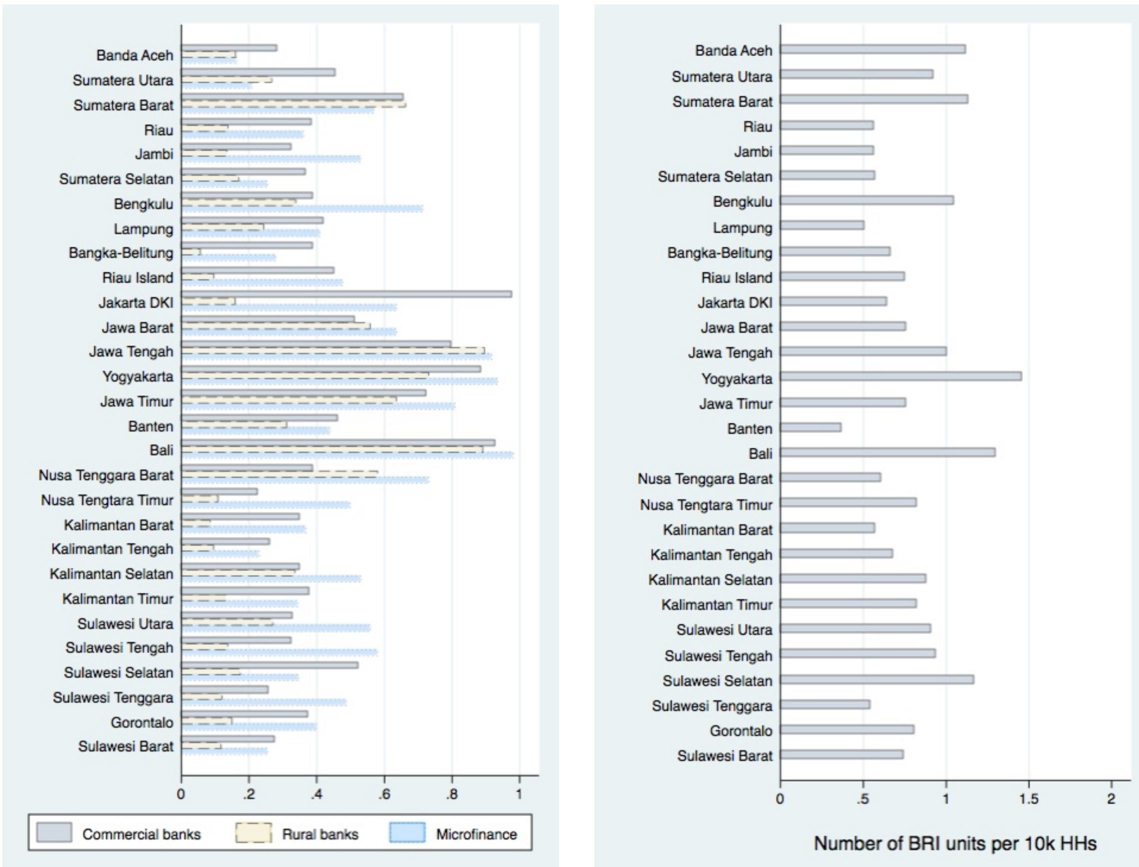
Appendix to Chapter 2

B.1 Supplementary Tables and Figures

Figure B.1: Presence of financial institutions at baseline by province

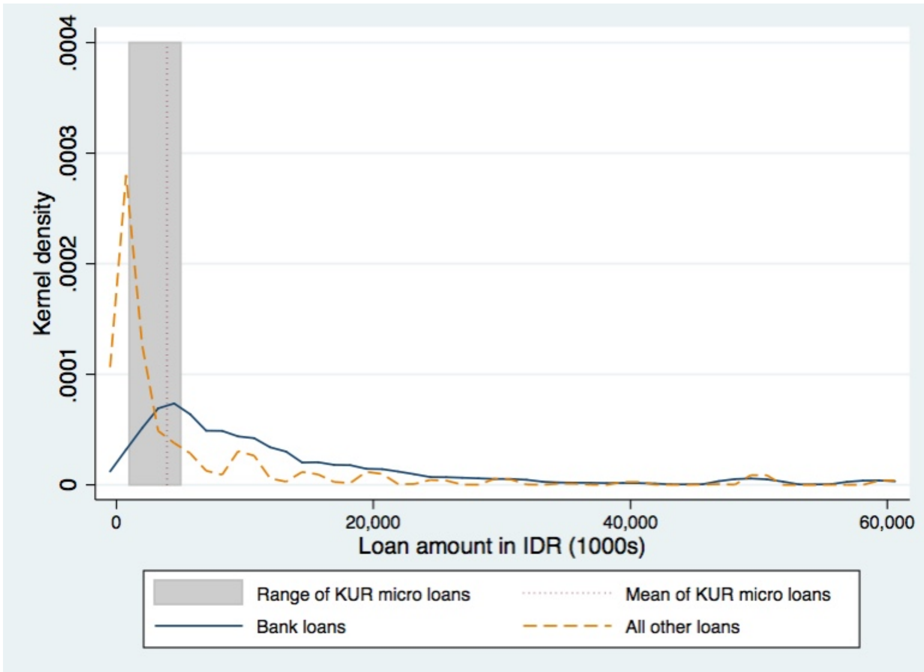
(a) Proportion of villages with different types of financial institutions

(b) Average of district-level densities of BRI village units



I use the village census data from 2005 to obtain the measures of presence of different types of financial institutions illustrated in Figure 1(a) and the location data on BRI village units in 2005 to construct Figure 1(b). The average sub-district size in 2005 (based on village census) is 10,057 households. Figure 1(b), therefore, can be considered roughly as the average number of BRI units per sub-district.

Figure B.2: Kernel densities of the largest loan size by source



Notes: This graph shows the kernel densities of the loan amount for the largest business credit source reported in the SUSENAS core survey in 2005. The outcome is winsorized at the 99th percentile. The shaded area represents the range of KUR micro loans with the dotted vertical line indicating the average loan size at BRI in 2009. The dashed density curve in orange illustrates that 70% of all non-bank loans reported as the largest credit source for the household are below the average KUR micro loan size at BRI.

Table B.1: Correlations between district characteristics and BRI density
Village census data 2005

Dependent variable:	BRI density (Number of BRI units per 10,000 HHs)		
	(1)	(2)	(3)
Proportion of villages with:			
Commercial banks	1.6863**	1.4836**	1.0289
	(0.6819)	(0.6220)	(0.6661)
Rural banks	0.3168	0.8286*	0.5877
	(0.5812)	(0.4656)	(0.5524)
Microfinance institutions	0.1833	0.4709***	0.2113
	(0.1354)	(0.1274)	(0.1970)
Cooperatives	-0.3742	-0.3483	-0.2786
	(0.2974)	(0.3546)	(0.3256)
Legal farm companies	-0.4226	-0.3852	-0.2503
	(0.3367)	(0.2741)	(0.3038)
Agricultural kiosks	0.1391	0.1815	0.1304
	(0.1697)	(0.1669)	(0.1919)
Concrete roads	0.2144**	0.2780**	0.1609
	(0.0983)	(0.1032)	(0.1385)
Density (# per 10,000 households) of:			
Large businesses (> 100 workers)	-0.0691**	-0.0626**	-0.0449
	(0.0325)	(0.0273)	(0.0281)
Medium-size businesses (20-100)	-0.0102	-0.0077	-0.0160
	(0.0121)	(0.0123)	(0.0119)
Small businesses (< 20 workers)	0.0001	0.0000	0.0001
	(0.0001)	(0.0002)	(0.0001)
City	-0.0167	-0.0338	0.1412
	(0.1309)	(0.1057)	(0.1307)
P-value from joint test	<0.0001	<0.0001	0.0004
(Excluding presence of other financial institutions)	0.0054	0.0007	0.0100
Fixed effects	None	Region	Province
Number of observations	254	254	254
Mean dep var, control	0.829	0.829	0.829

Standard errors are clustered at the province level and reported in parentheses: *10% significance level; **5% significance level; ***1% significance level. All independent variables come from the village census in 2005. I report the p-values from the joint tests for independent variables at the bottom of the table, with and without the four variables on the presence of financial institutions.

Table B.2: Pre-intervention trends in household characteristics
SUSENAS Panel 2005-2007

Dependent variable:	Any business credit	Any business credit from banks	Any self-employed in non-agriculture	Any self-employed in agriculture	Log of household expenditure	Standardized joint coefficient
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Full sample						
High BRI density x 2007	0.0118	0.0014	-0.0071	-0.0116	0.0460	0.006
	(0.0102)	(0.0053)	(0.0208)	(0.0210)	(0.0692)	(0.031)
Baseline financial access x 2007	-0.0819*	-0.0461**	0.0129	0.1565*	-0.5093	
	(0.0445)	(0.0200)	(0.1066)	(0.0852)	(0.3765)	
Number of districts	305	305	305	305	305	
Number of observations	18,386	18,386	18,386	18,386	18,386	
Mean dep var, control	0.030	0.013	0.329	0.343	6.737	
Panel B. Restricted analysis sample						
High BRI density x 2007	-0.0032	-0.0060	-0.0033	-0.0193	-0.0009	-0.042
	(0.0121)	(0.0075)	(0.0266)	(0.0282)	(0.0956)	(0.044)
Baseline financial access x 2007	-0.0802	-0.0368	-0.0047	0.1951	-1.0151*	
	(0.0609)	(0.0344)	(0.1462)	(0.1235)	(0.5757)	
Number of districts	206	206	206	206	206	
Number of observations	11,232	11,232	11,232	11,232	11,232	
Mean dep var, control	0.027	0.014	0.312	0.393	6.720	
District fixed effects	Yes	Yes	Yes	Yes	Yes	
Year fixed effects	Yes	Yes	Yes	Yes	Yes	
Controlling for provincial trend	Yes	Yes	Yes	Yes	Yes	

The regression results reported in this table use the SUSENAS panel dataset in 2005 and 2007. Standard errors are clustered at the primary sampling unit and reported in parentheses: *10% significance level; **5% significance level; ***1% significance level. *High BRI density* indicates the districts with an above-median BRI density (i.e., number of BRI units per ten thousand households) within each province in 2005. All regressions use household sampling weights and district fixed effects. In Column (6), I report the standardized joint coefficient for Columns (1) - (5).

Table B.3: Impact of KUR by credit type

	Formal sources (Table 4 Col (4)-(6))					
Dependent variable:	Banks	Unspecified government programs	Government empowerment (PNPM) programs	Cooperatives	Individuals	Other
	(1)	(2)	(3)	(4)	(5)	(6)
High BRI density x 2005	-0.0011	-0.0001	-0.0009	0.0003	0.0001	0.0003
	(0.0021)	(0.0006)	(0.0010)	(0.0014)	(0.0018)	(0.0006)
High BRI density x 2006	-0.0011	-0.0002	-0.0026**	0.0010	-0.0004	-0.0000
	(0.0019)	(0.0006)	(0.0011)	(0.0013)	(0.0017)	(0.0007)
(Intervention in 2007, omitted)						
High BRI density x 2008	0.0047**	0.0005	0.0017	0.0001	-0.0033	-0.0015
	(0.0021)	(0.0007)	(0.0011)	(0.0015)	(0.0024)	(0.0014)
High BRI density x 2009	0.0063**	0.0017**	0.0009	-0.0008	0.0044	0.0007
	(0.0026)	(0.0008)	(0.0012)	(0.0015)	(0.0040)	(0.0011)
High BRI density x 2010	-0.0010	0.0008	-0.0003	0.0003	-0.0006	-0.0003
	(0.0023)	(0.0012)	(0.0020)	(0.0018)	(0.0024)	(0.0008)
Number of observations	990,385	990,385	990,385	990,385	990,385	990,385
Mean dependent variable in 2007	0.021	0.003	0.007	0.008	0.007	0.003
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Trend controls						
Province	Yes	Yes	Yes	Yes	Yes	Yes
Baseline financial access	No	Yes	Yes	Yes	Yes	No

This table reports difference-in-difference estimates of the program impacts using repeated cross-sections of national household survey data from 2005 to 2010. Standard errors are clustered at the district level and reported in parentheses: *10% significance level; **5% significance level; ***1% significance level. I specify the year immediately prior to the intervention (2007) as the base year. All regressions use district fixed effects and control for provincial time trends.

Table B.4: Falsification tests:
Growth in business credit usage by densities of commercial and rural banks

Type of bank:	Commercial banks			Rural banks		
		Largest credit source			Largest credit source	
Dependent variable:	Any business credit in the last 12 months	Formal sources	Other specified sources	Any business credit in the last 12 months	Formal sources	Other specified sources
	(1)	(2)	(3)	(4)	(5)	(6)
High bank density x 2005	0.0067	0.0017	0.0052*	-0.0034	-0.0004	-0.0026
	(0.0046)	(0.0026)	(0.0030)	(0.0043)	(0.0022)	(0.0027)
High bank density x 2006	0.0052	0.0003	0.0057**	-0.0024	0.0010	-0.0024
	(0.0044)	(0.0026)	(0.0027)	(0.0036)	(0.0019)	(0.0025)
(Intervention in 2007, omitted)						
High bank density x 2008	-0.0030	0.0019	-0.0017	0.0018	-0.0008	0.0032
	(0.0053)	(0.0030)	(0.0034)	(0.0042)	(0.0024)	(0.0029)
High bank density x 2009	0.0037	0.0025	0.0017	0.0023	-0.0008	0.0041
	(0.0052)	(0.0031)	(0.0038)	(0.0051)	(0.0029)	(0.0037)
High bank density x 2010	0.0097*	0.0034	0.0075*	0.0020	-0.0000	0.0019
	(0.0054)	(0.0032)	(0.0039)	(0.0052)	(0.0026)	(0.0036)
Number of observations	990,385	990,385	990,385	990,385	990,385	990,385
Mean dependent variable in 2007	0.050	0.024	0.023	0.050	0.024	0.023
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Trend controls						
Province	Yes	Yes	Yes	Yes	Yes	Yes
Baseline financial access	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the changes in credit usage by densities of commercial banks and rural banks using repeated cross-sections of national household survey data from 2005 to 2010. High bank density indicates the districts with above-median bank density within province. Standard errors are clustered at the district level and reported in parentheses: *10% significance level; **5% significance level; ***1% significance level. I specify the year immediately prior to the intervention (2007) as the base year. All regressions use district fixed effects and control for provincial time trends.

Appendix C

Appendix to Chapter 3

C.1 Supplementary Tables and Figures

Table C.1: Follow-up survey attrition
Dependent variable: Completed the follow-up survey

Dependent variable:	Completed the baseline surey (1)	Completed the follow-up survey (2)	Completed the baseline & follow-up surveys (2)
Compulsory KaSAPI	-0.086	-0.105	-0.104
	(0.088)	(0.072)	(0.094)
Voluntary KaSAPI	-0.078	-0.092	-0.082
	(0.088)	(0.072)	(0.094)
Compulsory x Age	0.002	0.001	0.003
	(0.002)	(0.001)	(0.002)
Voluntary x Age	0.004**	0.001	0.003*
	(0.002)	(0.001)	(0.002)
Age	-0.001	0.002*	0.001
	(0.001)	(0.001)	(0.001)
Compulsory x Female	-0.031	0.028	-0.031
	(0.049)	(0.039)	(0.052)
Voluntary x Female	-0.052	0.030	-0.036
	(0.047)	(0.039)	(0.051)
Female	0.020	0.004	0.031
	(0.035)	(0.027)	(0.037)
Compulsory x Loan amount (in thousands)	0.001	0.001*	0.001
	(0.001)	(0.001)	(0.001)
Voluntary x Loan amount (in thousands)	-0.002*	0.001	-0.000
	(0.001)	(0.001)	(0.001)
Loan amount (in thousand)	-0.001	-0.001***	-0.001**
	(0.001)	(0.001)	(0.001)
Joint significance			
Compulsory	0.574	0.357	0.383
Voluntary	0.069	0.465	0.440
Mean DV, control group	0.712	0.856	0.627
Number of observations	3,682	3,682	3,682

Robust standard errors are in parentheses. Stars indicate the significance level: *p<0.10 **p<0.05 ***p<0.01. All regressions control for whether the client was randomly assigned to receive the baseline survey and use regional fixed effects.

Table C.2: Impact on knowledge about PhilHealth and KaSAPI
Sample: Those who completed the baseline surey

Knowledge on:	Premium schedule	Claims filing	Benefit restrictions	Out-patient benefit package	KaSAPI
Dependent variable	Identified correct premium schedule	Identified corrected documents required for filing PhilHealth claims	Knowledge of accredited facilities, eligible dependent coverage and type of services (9 items)	TB, dialysis, eye surgeries, complicated deliveries, newborn care and organ transplants (7 items)	Ever heard of KaSAPI; Knew payment method; Knew the premium contribution period
	(1)	(2)	(3)	(4)	(5)
Panel A. Full sample					
Compulsory KaSAPI	0.078***	0.042*	0.007	0.018	0.085***
	(0.022)	(0.019)	(0.008)	(0.015)	(0.010)
Voluntary KaSAPI	0.031	0.023	0.001	0.017	0.051***
	(0.021)	(0.019)	(0.008)	(0.015)	(0.009)
Compulsory - Voluntary	0.047*	0.018	0.006	0.001	0.034
	(0.022)	(0.020)	(0.008)	(0.015)	(0.011)
Mean DV, control group	0.447	0.255	0.469	0.452	0.057
Number of observations	3,152	3,152	3,152	3,152	3,152
Panel B. Individuals who had own PhilHealth coverage in August 2008 (admin) and July 2009 (self-reported)					
Compulsory KaSAPI	0.097*	0.097*	-0.035*	0.019	0.182***
	(0.047)	(0.045)	(0.015)	(0.032)	(0.022)
Voluntary KaSAPI	0.045	0.078	-0.041*	0.015	0.152***
	(0.051)	(0.048)	(0.017)	(0.035)	(0.024)
Compulsory - Voluntary	0.051	0.019	0.006	0.004	0.031
	(0.041)	(0.040)	(0.015)	(0.029)	(0.026)
Mean DV, control group	0.447	0.255	0.469	0.452	0.057
Number of observations	741	741	741	741	741

Robust standard errors are in parentheses. Stars indicate the significance level based on the FDR adjustment method introduced by Benjamini and Hochberg (1995) separately for each panel: *p<0.10 **p<0.05 ***p<0.01. *Compulsory - Voluntary* reports the linear combination of the coefficients on Compulsory and Voluntary. All regressions control for whether the client was randomly assigned to receive the and use regional fixed effects.

Table C.3: Impact on health insurance coverage and usage
Sample: Three most recent facility visits for the respondent and her family members

Sample:	All visits			Visits for covered care		
Dependent variable:	PhilHealth coverage through own policy (Jul '08 & '09)	Any PhilHealth coverage at follow-up	Used insurance	PhilHealth coverage through own policy (Jul '08 & '09)	Any PhilHealth coverage at follow-up	Used insurance
	(1)	(2)	(3)	(4)	(5)	(6)
Compulsory KaSAPI	0.182***	0.099***	0.006	0.248***	0.122***	0.006
	(0.027)	(0.028)	(0.012)	(0.049)	(0.047)	(0.045)
Voluntary KaSAPI	0.065***	-0.014	0.009	0.084*	-0.029	0.025
	(0.025)	(0.029)	(0.012)	(0.049)	(0.052)	(0.044)
Compulsory - Voluntary	0.117	0.113	-0.003	0.164	0.151	-0.019
	(0.029)	(0.028)	(0.013)	(0.054)	(0.047)	(0.044)
Control mean	0.167	0.633	0.074	0.175	0.669	0.306
Number of observations	5,625	5,625	5,625	1,044	1,044	1,044

Standard errors are clustered at the household level and reported in parentheses. Stars indicate the significance level based on the FDR adjustment method introduced by Benjamini and Hochberg (1995) separately for each panel: *p<0.10 **p<0.05 ***p<0.01. *Compulsory - Voluntary* reports the linear combination of the coefficients on Compulsory and Voluntary. This table reports the treatment effects on insurance coverage and usage. The sample for Columns (1)-(3) is 5,625 facility visits among 2,208 households; The sample for Columns (4)-(6) consists of 1,044 facility visits for covered care among 416 households. *Covered care* includes treatments for SARS and TB, dialysis, pre-natal and post-natal care, deliveries, and other inpatient care with a hospital stay over 24 hours.

Table C.4: Reasons for not using health insurance
Sample: Three most recent visits where the household did not use health insurance benefits among households with health insurance coverage at the follow-up

	Control (1)	Compulsory (2)	Voluntary (3)	Total (4)
Panel A. Insurance usage among the insured borrowers				
Insurance usage rate	44.5%	39.1%	50.5%	44.2%
Number of observations	245	276	212	733
By any PhilHealth coverage at baseline (sample: completed the baseline survey)				
Yes (n = 326)	49.6%	52.9%	56.6%	52.8%
No (n = 195)	39.2%	31.0%	45.1%	37.4%
Panel B. Reasons for not using health insurance				
No benefit coverage	42.6%	45.2%	51.4%	46.0%
Low cost of care	26.5%	23.2%	9.5%	20.8%
Lack of required documents for filing claims	6.6%	12.5%	12.4%	10.5%
Insurance was inactive	8.8%	4.8%	19.0%	9.8%
Facility was not accredited	8.8%	7.7%	3.8%	7.1%
Lack of information	5.9%	4.8%	2.9%	4.6%
Other/no response	0.7%	1.8%	1.0%	1.2%
Number of observations	136	168	105	409

Panel A reports insurance usage rates among 706 facility visits made by 178 households. Panel B reports reasons for not using health insurance during 386 facility visits for covered care reported among 103 households who had PhilHealth coverage but did not use insurance. For each of these visits, we asked why the household did not use health insurance. *Covered care* includes treatments for SARS and TB, dialysis, pre-natal and post-natal care, deliveries, and other inpatient care with a hospital stay over 24 hours.

Table C.5: Predicting long-term PhilHealth enrollment
Sample: Those who completed the baseline surey

Sample:	Control (1)	Control (2)	Compulsory (3)	Compulsory (4)	Voluntary (5)	Voluntary (6)
Active Green Bank account in Jul 2008		-0.011		0.238***		0.115***
		(0.025)		(0.035)		(0.031)
High needs for healthcare at baseline						
Self-reported poor health	0.005	0.005	-0.062	-0.052	0.051	0.055*
	(0.026)	(0.026)	(0.039)	(0.037)	(0.031)	(0.031)
Chronic pre-condition	0.004	0.005	-0.003	0.002	0.046	0.046
	(0.026)	(0.026)	(0.037)	(0.037)	(0.033)	(0.032)
Non-chronic condition	0.028	0.028	0.030	0.018	-0.004	-0.002
	(0.027)	(0.027)	(0.038)	(0.037)	(0.031)	(0.031)
Pregnant now or likely to become pregnant	0.039	0.040	0.050	0.042	-0.078	-0.074
	(0.045)	(0.045)	(0.071)	(0.071)	(0.053)	(0.052)
Risk characteristics at baseline						
Risk-loving	-0.021	-0.021	0.015	0.031	-0.033	-0.017
	(0.030)	(0.030)	(0.041)	(0.039)	(0.036)	(0.036)
Regularly drink or smoke	0.031	0.031	0.088*	0.062	-0.054	-0.060
	(0.039)	(0.039)	(0.053)	(0.053)	(0.038)	(0.038)
Other characteristics at baseline						
Financial barriers to seeking formal healthcare	0.012	0.011	-0.035	-0.011	-0.046	-0.037
	(0.027)	(0.027)	(0.038)	(0.037)	(0.032)	(0.032)
Cognitive skills index	0.010	0.010	0.002	0.000	0.033***	0.034***
	(0.010)	(0.010)	(0.013)	(0.013)	(0.013)	(0.012)
Log of household savings	0.003	0.003	0.001	-0.002	0.006	0.004
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)
Any qualified PhilHealth dependent	0.002	0.002	0.007	0.008	-0.005	-0.006
	(0.007)	(0.007)	(0.011)	(0.011)	(0.009)	(0.009)
Age	0.000	0.000	-0.002	-0.003*	-0.003*	-0.003**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Female	-0.036	-0.036	0.009	-0.002	-0.043	-0.040
	(0.038)	(0.038)	(0.050)	(0.048)	(0.043)	(0.042)
Urban	-0.001	-0.002	-0.051	-0.031	0.014	0.027
	(0.024)	(0.024)	(0.034)	(0.033)	(0.030)	(0.030)
Covered by own PhilHealth policy	0.292***	0.292***	0.163***	0.166***	0.189***	0.186***
	(0.043)	(0.043)	(0.047)	(0.046)	(0.045)	(0.045)
Covered by spouse's PhilHealth policy	-0.046**	-0.045**	-0.202***	-0.193***	-0.098***	-0.103***
	(0.022)	(0.022)	(0.039)	(0.038)	(0.031)	(0.031)
Adjusted R-squared	0.122	0.121	0.087	0.145	0.078	0.095
Mean of dependent variable	0.146	0.146	0.356	0.356	0.229	0.229
Number of observations	783	783	749	749	778	778

Robust standard errors are in parentheses. *p<0.10 **p<0.05 ***p<0.01. *Financial barreirs to seeking formal healthcare* indicates respodents who reported any incident in the past 12 months where the household was unable to visit formal health facilities, unable to complete recommended treatments, or having sought care from traditional healers due to the costs of formal healthcare. *Risk-loving* takes the principal component of self-reported willingness to take risks on a 0-10 scale for general, financial, health, and occupational matters. *Regularly drink or smoke* indicates individuals who reported smoking or drinking at least several times a week. *Cognitive skills index* is the principal component of indicators for having basic numeracy, literacy, and reading comprehension. The regression uses regional fixed effects.

Table C.6: Predicting healthcare utilization
Sample: Those who completed the baseline and follow-up sureys

Dependent outcome:	Any facility visits	Any facility visits for covered care	Any facility visits	Any facility visits for covered care	Any facility visits	Any facility visits for covered care
Sample:	Control	Control	Compulsory	Compulsory	Voluntary	Voluntary
	(1)	(2)	(3)	(4)	(5)	(6)
High needs for healthcare at baseline						
Self-reported poor health	0.013	-0.006	0.030	0.013	-0.009	0.010
	(0.036)	(0.033)	(0.039)	(0.033)	(0.036)	(0.032)
Chronic pre-condition	0.044	0.071**	0.069*	0.051	0.086**	0.009
	(0.037)	(0.031)	(0.037)	(0.032)	(0.036)	(0.032)
Non-chronic condition	-0.001	0.042	0.024	0.019	0.015	-0.002
	(0.035)	(0.033)	(0.037)	(0.033)	(0.036)	(0.032)
Pregnant now or likely to become pregnant	0.097**	0.104*	0.090	-0.040	0.024	0.090
	(0.048)	(0.059)	(0.055)	(0.062)	(0.064)	(0.066)
Risk characteristics at baseline						
Risk-loving	0.004	-0.021	-0.053	-0.079**	0.030	0.047
	(0.037)	(0.034)	(0.040)	(0.035)	(0.041)	(0.037)
Regularly drink or smoke	0.033	0.073	-0.081	-0.038	-0.050	-0.025
	(0.050)	(0.045)	(0.055)	(0.041)	(0.052)	(0.039)
Other characteristics at baseline						
Financial barriers to seeking formal healthcare	0.146***	0.031	0.085**	0.006	0.024	0.034
	(0.035)	(0.034)	(0.037)	(0.032)	(0.037)	(0.033)
Cognitive skills index	0.015	0.005	-0.011	-0.022*	0.036**	0.024*
	(0.013)	(0.012)	(0.014)	(0.012)	(0.014)	(0.013)
Household savings (thousand pesos)	0.007	0.001	0.007	-0.005	0.004	-0.000
	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)
Any qualified PhilHealth dependent	-0.002	0.005	-0.004	0.003	0.002	0.016*
	(0.010)	(0.009)	(0.011)	(0.009)	(0.011)	(0.009)
Age	-0.004**	-0.005***	-0.005**	-0.007***	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Female	0.024	-0.061	0.089*	0.049	0.010	-0.012
	(0.050)	(0.043)	(0.051)	(0.038)	(0.048)	(0.040)
Urban	-0.032	0.012	0.010	-0.002	-0.051	0.012
	(0.033)	(0.029)	(0.034)	(0.028)	(0.033)	(0.030)
Covered by own PhilHealth policy	0.075*	0.023	0.000	0.012	0.033	-0.008
	(0.041)	(0.040)	(0.044)	(0.037)	(0.042)	(0.038)
Covered by spouse's PhilHealth policy	0.004	0.021	-0.001	-0.023	-0.035	0.022
	(0.038)	(0.033)	(0.040)	(0.034)	(0.039)	(0.035)
Adjusted R-squared	0.039	0.032	0.044	0.026	0.009	0.003
Mean of dependent variable	0.709	0.206	0.694	0.186	0.698	0.198
Number of observations	783	783	749	749	778	778

Robust standard errors are in parentheses. *p<0.10 **p<0.05 ***p<0.01. We construct the dependent variable in even columns using the follow-up survey data on the three most recent visits to health facilities for each member of the household. *Covered care* includes treatments for SARS and TB, dialysis, prenatal and postnatal care, deliveries, and other inpatient care with a hospital stay over 24 hours. *Financial barriers to seeking formal healthcare* indicates respondents who reported any incident in the past 12 months where the household was unable to visit formal health facilities, unable to complete recommended treatments, or sought care from traditional healers due to the costs of formal healthcare. *Risk-loving* takes the principal component of self-reported willingness to take risks on a 0-10 scale for general, financial, health, and occupational matters. *Regularly drink or smoke* indicates individuals who reported smoking or drinking at least several times a week. *Cognitive skills index* is the principal component of indicators for having basic numeracy, literacy, and reading comprehension. All regressions use regional fixed effects.

Table C.7: Definitions of household indices

Risky behaviors	Any occupational risk; smoke or drink regularly; pregnant now or likely to be pregnant in the next 12 months
Subjective well-being	Self-reported health in general; self-reported satisfaction with life; limited in the type of work or other activity due to physical health over the last 4 weeks; accomplished less than desired due to physical health over the last 4 weeks
Asset index	Strong house materials; house ownership; piped water; sewing machine; refrigerator; oven; cellphone; air conditioning; telephone; TV; stereo; VCR; washing machine; any savings; savings > 75th percentile; own flush latrine
Health shocks	Any expenditure shock due to health problems in the last 12 months; any income shock due to health problems in the last 12 months; missed any work day due to health problems of household members in the last 30 days
Economic activities	Operated an enterprise in the last 12 months; Any paid or self-employed work in the last 12 months
Informal risk-sharing	Gave any financial transfers in the last 30 days; gave any in-kind transfers in the last 30 days; Received any financial transfers in the last 30 days; Received any in-kind transfers in the last 30 days