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Saving by Default: Evidence from a Field Experiment in Rural India

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Saving by Default: Evidence from a Field Experiment in Rural India

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Abstract

Worldwide, people are gaining access to a formal bank account, which allows account-based instead of cash payments. Based on a novel randomized control trial, we document that the payment method is an important determinant of savings behavior. In rural India, we study the effect on savings of allocating identical weekly payments on a bank account (treated) or in cash (control). The treatment impact is huge: savings increase by 110% within three months, and the effect is long-lasting. Villagers paid in cash do not save more in other assets, but increase consumption. Therefore, we infer that being paid on a bank account has a net positive impact on total savings. When we twist the design and start paying everyone in cash, savings and consumption patterns no longer differ between the treated and control. We interpret these findings as the outcome of the default option, and shed light on six plausible underlying mechanisms.

JEL: D14, C93, D03, G21, O16.

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1 Introduction

The possibility to provide banking services through mobile networks allows a rapid decrease in the number of unbanked adults in developing countries. In India, the government has made it a priority to provide a bank account to all households in the country. The next step is to pay public transfers directly on the recipient's bank account, instead of in cash.

We hypothesize that savings behavior will change once people receive income on a bank account, especially in an economy where most transactions are handled in cash. We know from other contexts that the *default option* - the outcome that results when people do not make an active choice - is a strong predictor of human behaviour. When people are paid on their account, money is saved by default, unless they take the active decision to withdraw. By contrast, transfers given in cash are ready to be spent, unless people make the active choice to deposit, or to save in other tools. As a result, a change in the payment method may have large effects on savings. We test this hypothesis by setting up a randomized control trial in three different districts of Chhattisgarh, a Central-Eastern state of India.

As a result of India's financial inclusion policies, formal banks have started operating in villages that were previously unbanked. We sampled 442 villagers in seventeen of those villages. All of them either had an account, or opened one with our help. As a next step, we organized a practical information session for the 442 participants in the study. We showed them how to deposit and withdraw, and demonstrated how a fingerprint recognition tool protects their money. Once the villagers were familiar with the features of their account, we started weekly interviews that we conducted for about 10 consecutive weeks. At the end of each interview, the villagers received Rs 150, an amount equivalent to the salary for a day of work under the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA). The only difference was the payment method: we randomly allocated them to being

paid on the account (treated) or in cash (control). The interviews provided detailed information on the financial life of the respondents. This allowed us to measure the impact of a differential payment method on expenditures and on savings tools, in addition to the bank account for which we obtained bank records.

The setting of the experiment is ideal. First, the transaction costs are negligible, and the bank is located at the villager's doorstep. The average distance between the location where the weekly interviews took place and the local banker is 55 meter. Therefore, individuals could easily withdraw or deposit on their way home. Furthermore, the average distance between the villager's house and the local banker is 290 meters only. Finally, the accounts are individually owned, do not require a minimum balance or any other commitment, and the clients are free to deposit or withdraw the amount they want. Therefore, if the individuals in our study behave like standard economic agents, we should not observe a difference in the savings behavior of the treated as compared to the control.

Our main findings contradict this. First, being paid on the account instead of in cash increases the account balance by around 111 percent (or Rs 420) after three months of weekly payments. Second, the effects are long lasting: five months after the last weekly payment, the balance of the treated is still twice the one of the control. Third, the villagers who were paid in cash do not save more in other assets, such as cash at home. However, they increase expenditures on regular consumption, such as rice, vegetables, fuels, and soap with about Rs 387. The increase in consumption expenditures by villagers paid in cash, is remarkably similar to the increase in the savings of the villagers paid on the account. Therefore, we conclude that the treatment has a net positive impact on the respondent's total savings. Finally, the effects are uniform across different sub-samples. The treatment has similar effects on men and women, and on old and new account holders, two characteristics on which we had stratified our sample.

About seven weeks after we finished the first phase of payments and interviews,

we twisted the original design. We repeated the interviews for another four weeks during which we paid everyone in cash. We explicitly told the villagers that the use of the accounts did not change, but that they have to deposit themselves the amount they want to save on the account. The effect of paying everyone in cash is remarkable: we no longer observe a difference in bank account use and consumption patterns between the treated and control. The account balance of the treated remains higher, but - as both groups deposit and withdraw similar amounts - there are no significant differences in the evolution of the account balance.

We interpret our findings as the outcome of the default option: the account-based payments are saved by default, while the cash payments are ready to be spent unless the recipient takes the active step to deposit. We use our experimental design and rich data - from several surveys, lab experiments and bank account details - to provide evidence that the default effect can be explained by (i) transaction costs, i.e. the (minimal) time and effort it takes to do a transaction, (ii) a lack of self-control, and (iii) mental accounting. We argue that the impacts are not due to (iv) a higher trust and better relationship with the banker (measured by playing incentivized lab in the field games), (v) experimenter demand effects, or (vi) redistributive pressures.

The first contribution of our paper is to the understanding of savings behavior. While recent research emphasizes the importance of genetic predispositions and education in explaining individual savings propensities (Cronqvist and Siegel, 2015), we show that savings can be importantly affected by the *choice architecture*. We thereby directly contribute to the literature on the importance of behavioral biases in explaining savings behavior (Thaler and Shefrin, 1981; Samuelson and Zeckhauser, 1988; Shefrin and Thaler, 1988; Akerlof, 1991; Thaler, 1994; Bernheim, 1997; Laibson et al., 1998; O'Donoghue and Rabin, 1999a,b; Lusardi, 1999; Thaler and Benartzi, 2004). According to the economic models of decision-making, individuals select their most preferred alternative in accordance with well-defined preferences. The decision is not influenced by the status quo alternative or default option. However,

individuals tend to stick to the default option more frequently than the canonical model would predict. Therefore, the default option can be used as an effective tool to "nudge" people towards a particular outcome from which they may benefit. Well-known examples include the expression of end-of-life treatment preferences (Kressel and Chapman, 2007), organ donation decisions (Johnson and Goldstein, 2003; Abadie and Gay, 2006), and the 401(k) savings plans in the United States of America (Madrian and Shea, 2001; Choi et al., 2002, 2004; Carroll et al., 2009). To the best of our knowledge, we conducted the first field experiment that tests the importance of a default payment method.[1] Our work also complements the 401(k) savings literature for two important reasons. First, the setting is very different. Our sample consists of rural poor in a low income country. As deposits and withdrawals do not involve nontrivial costs, the default can be "undone" at any time, at a small cost (the time and effort it takes to deposit or to withdraw). Second, apart from identifying a difference in savings due to a different default, the design of our experiment allows us to document further consequences for the financial lives of the poor.

We also contribute to other strands of literature. First, as micro-credit showed its limitations, savings gained importance. The substantial demand for savings among the poor has been reflected in their willingness to invest in risky assets, such as jewellery, animals, money under the mattress, and different forms of informal savings arrangements. Therefore, it is important to provide poor households access to savings products of higher quality, such as bank accounts. The literature on access to bank accounts is well-developed, and can be classified in three categories. First, papers investigating the impact of providing formal bank accounts (e.g., Dupas et al., 2012; Dupas and Robinson, 2013a; Kast and Pomeranz, 2014; Prina, 2015);

[1]Related experiments are now being implemented. Brune et al. (2015) compare the effects of a one time payment on an account, or in cash. Brune et al. (2016) study the impact of being offered a new bank account, combined with the possibility to have cash crop harvest proceeds being deposited into the account. In contrast with our set-up, not every farmer has an account, and it remains at his discretion whether or not he will be paid on the account.

second, papers measuring the effect of offering different types of accounts (e.g., Ashraf et al., 2006, 2010; John, 2015); and finally papers focusing on the impact of providing additional banking services along with a bank account (e.g., Karlan et al., 2010; de Mel et al., 2013; Callen et al., 2014). The papers show large take-up rates and important effects on recipients' finances. However, a striking pattern is the low usage of those accounts. Karlan et al. (2014) emphasize that the gap between take-up and usage of formal bank accounts remains to be explained. Our paper contributes to this literature by showing that the gap can be reduced by moving from cash to account based payments.

Second, our study also contributes to the ongoing policy research on financial inclusion. The shift to account based payments is on the political agenda in a wide range of countries.[2] In India, the debate about providing access to formal banking for all, and the move towards account-based public transfers is ongoing. Indeed, Phase I of the national Mission on Financial Inclusion focuses on providing bank accounts, and Phase II - which is to be achieved by August 2018 - proposes to channel all Government benefits to the accounts of the beneficiaries (including Direct Benefit Transfers, and MGNREGA). While the political debate and scientific research focus on public administration issues (Muralidharan et al., 2014), we draw attention to the potential impact of account based payments on the recipient's finances.

The paper is organized as follows. In Section 2 we provide more details on India's financial inclusion plan, our experimental design, and the data. We present the main results in Section 3 and plausible mechanisms in Section 4. We conclude in Section 5.

[2]One famous example is Brazil, where almost twenty percent of the beneficiaries of the Bolsa Familia program receive their transfers on a bank account (Numbers obtained from the *Ministério do Desenvolvimento Social* in January 2015).

2 Background, Experimental Design and Data Collection

In this section, we first discuss India's financial inclusion plans. Next, we describe our experimental design and the data used in our analysis.

2.1 Financial Inclusion in India

In the previous decade, bank account penetration in India was estimated at 35 percent, with disparities along income and gender lines: only 21 percent of adults in the poorest income quintile, and 26 percent of women reported having an account (Demirguc-Kunt and Klapper, 2012). To achieve greater financial inclusion, the Reserve Bank of India (RBI) introduced the Business Correspondents Model in 2006. The model, which is based on recommendations of the 2004 Khan Commission for financial inclusion, allows banks to appoint Business Correspondents (BCs) as intermediaries in providing financial and banking services on their behalf (RBI, 2006). In a notification sent out in August 2008, the RBI allowed BCs to hire Business Correspondents Sub-Agents or *BCSAs*, i.e. grass-root level entities who can render the services of the BCs (RBI, 2008).

In the region where we conducted our survey, Axis bank appointed the financial inclusion company Basix Sub-K as a BC. Basix Sub-K - which is our main partner - is one of the pioneers in the BC model and already reaches 980,000 people. Its main responsibilities are selecting one grocery shop owner per village to become the BCSA, training the new local banker, and providing the necessary equipment: a mobile phone, a finger print recognition device and a receipt machine that are interconnected through bluetooth. Basix Sub-K also pays the BCSA, helps wherever needed and provides a customer service for the clients.

The first task of the BCSA is to help villagers opening a bank account. The procedure is as follows. First, the BCSA has to send the customer's filled-in ap-

plication form and a photo to Axis bank. Next, the bank opens the account and communicates the unique bank account number to the BCSA. Finally, the BCSA activates the account by registering the finger prints of the customer. Once this procedure is finalised, the customer can perform standard transactions on the account: deposits, withdrawals, money transfers, and balance inquiries. Balance inquiries and transactions that lead to a reduction of the balance require a signature through the finger print recognition device. The customer is charged an enrollment fee of Rs 25 when the account is used for the first time. Deposits and balance inquiries are free. However, the bank experimented with (very low) charges on withdrawals after the start of our experiment. Customers were charged Rs 2 per withdrawal if their average quarterly balance (AQB) was less than Rs 200, and Rs 1 per withdrawal if the AQB was between Rs 200 and Rs 500. Withdrawals were free if the AQB was above Rs 500. These charges were abandoned on July 1, 2014 and from the endline survey we learn that customers did not realise their temporary existence.[3]

On the 15th of August 2014, the new Government announced the *Pradhan Mantri Jan-Dhan Yojana* financial inclusion plans. Ever since, bank account penetration has increased at an amazing speed. The next phase - which is to be achieved by August 2018 - proposes to channel all Government benefits to the accounts of the beneficiaries, including MGNREGA. Our study simulates those payments by offering an amount similar to MGNREGA payments, in exchange for an interview.

2.2 Experimental Design

The experiment was conducted in Chhattisgarh, an east-central state of India. We selected 18 villages in collaboration with Basix Sub-K according to two criteria. First, we excluded villages with a cooperative, rural or commercial bank branch, as to be sure that the BCSA was the only person providing formal banking services at the doorstep. Second, we opted for clusters of villages that are sufficiently close

[3]We only got to know about the existence of temporary charges shortly before it was abandoned.

to one another, as the survey team had to travel between them within a reasonable amount of time. The selected villages are located in three bordering districts: five in the Magarload block of the district Dhamtari, seven in the Rajim block of the district Gariyabandh, and six in the Abhanpur block of the district Raipur. These villages are close, but not contiguous, as can be seen from Figure 5 in Appendix A. The average distance between the BCSAs is 20.5 km.

We randomly sampled 26 participants in each village. The BCSA's customer list was used to select 14 villagers who already had a BCSA account, and the voter list to sample 12 villagers without a BCSA account. Each person on the customer and the voter list was allocated a random number. The sequence in which the villagers were approached respected the ascending order of those numbers. To be sampled, a villager should (i) be the head of the household or the head's spouse, (ii) not plan to leave the village, and (iii) belong to a household in which nobody has a savings account with another institution.[4]

In the fall of 2013, trained enumerators visited the sampled participants at home to administer a baseline survey. At the end of the interview, the respondents without a BCSA account received help to open one. Basix Sub-K took care of the paperwork and the associated costs. Next, we organized a practical information session for all the participants in the study. We showed them how to deposit and withdraw money, and demonstrated how the fingerprint recognition tool protects their account. We emphasized that the account can be used as a protected place to keep savings for any purpose.

From February till May 2014, we hired a centrally located room in each village, where we interviewed the participants on a weekly basis for a total of 7 to 13

[4]We allowed for post office or other accounts that were opened to receive payments from welfare schemes, or MGNREGA. We also allowed for cooperative accounts that were used for the payment of paddy or other grains only. Respondents usually withdraw money from these accounts at once shortly after payments are made, either because they are not protected (there is no secret code or biometric authentication), or because the bank is too far away.

weeks.[5] We compensated the villagers, because they had to leave their house to be interviewed, and because the surveys took a substantial amount of time (on average, respondents needed about three hours to come, wait their turn, be interviewed and go back home). They received Rs 150 at the end of each interview, which is close to the salary of MGNREGA wage labor.[6] We randomized the way this weekly compensation was paid at the individual level. Half the respondents received Rs 150 directly on their account (treated), while the other half received it in cash (control). The intervention and randomization are summarized in Figure 1.

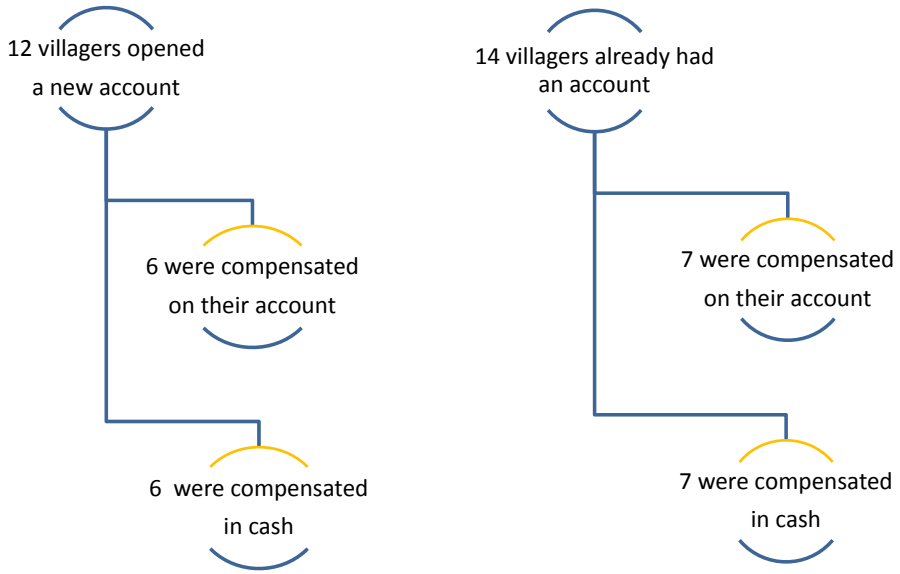

Figure 1: Sampling Strategy within Villages

To guarantee a desired heterogeneity analysis in terms of gender, we stratified the sample. The sub-groups of six villagers who opened a new account consist of three men and three women. To accomplish the same for villagers who already had an account, we sampled eight men and six women in nine randomly chosen villages,

[5]We delayed the weekly interviews in some villages because (i) we wanted to follow-up the enumerators as closely as possible in the first couple of weeks, and (ii) it took longer than expected to open the bank accounts in a subset of villages.

[6]When we started the weekly interviews, the MGNREGA salary was Rs 146 per day. In March 2014, it increased to Rs 157 per day.

and six men and eight women in the other nine villages. Half the men and women were paid on their account, the other half in cash.

We will refer to the first part of the experiment - during which the treated respondents were paid on the account, and the control in cash - as *Phase 1*. At the end of these weekly interviews, we took a break of about seven weeks. After the break, we did interviews for another four weeks, but we paid everyone in cash. We refer to this part as *Phase 2*. We explicitly told the respondents that the use of the accounts did not change, but that they have to deposit themselves the share of their income they want on the account. Phase 2 was not announced and could not have been anticipated by the respondents.

2.3 Data and Pre-Analysis Plan

We use four sources of data. First, our baseline survey included questions on characteristics of the participants and their household members, such as education, marital status, occupation, land ownership, and membership of savings groups. It included a detailed asset module, as well as information on the household's expenditures, investments, transfers, loans, and informal savings. We also gathered detailed information on decision making responsibilities within the household, time preferences, and trust in various institutions.

Second, Basix Sub-K provided data on the use of the BCSA accounts. The data consists of all the deposits, withdrawals and transfers made on the accounts and was used to construct our main dependent variables of interest.

Third, we gathered detailed information on the evolution of the household composition and on the various expenditures of the household members over the past seven days. To do so, we created a "dynamic" questionnaire, that compared the answers over the different weeks. For example, if the respondent had an outstanding loan in week x, the enumerator would be reminded about it in week $x + 1$, unless the last repayment took place in week x. A separate section asks about new loans

taken in week x. Those loans are then part of the outstanding loans from week $x+1$ onwards. Details with respect to accounts, memberships of savings groups, etc. were automatically shown, as to make sure that the enumerator would not forget to up-date the necessary information. During the pilot study, we realised that it was difficult for respondents to provide details with respect to bundles of expenditures, such as cereals and vegetables. To obtain this crucial information, we created an exhaustive list of 120 frequent goods, and 75 non-frequent goods that they were asked about separately. This made the weekly interviews intense, and provided us the opportunity to compensate the villagers for their time.

Finally, we conducted an endline survey to update the information from the baseline. It included the same asset module, and questions about decision making responsibilities and trust in various institutions. We also asked open questions about the treatment, and the use of the account.

Before we received the data, we registered a pre-analysis plan with the American Economic Association's registry for randomized control trials (Somville and Vandewalle, 2015). To further enrich the paper, we also present data and analyses that were not pre-specified. Appendix C categorizes our results depending on whether they were foreseen in our pre-analysis plan or not.

2.4 Attrition

Shortly after the baseline survey, one shop keeper stopped his BCSA activity because it was not as profitable as his other business. Given there is only one BCSA per village, and that it was impossible to appoint a new BCSA within a reasonable amount of time, we had to exclude the village from our experiment. As the BCSA's decision was unrelated to our study, the attrition should be orthogonal to the experimental treatment assignment. The loss of one village implies the sample reduced with 26 villagers. The final sample available for the analysis consists of 442 participants.

2.5 Baseline Characteristics and Balance Check

The baseline survey was administered at the households' homes between October 2013 and January 2014. Table 1 presents the final sample's baseline characteristics.[7] The sample consists of 442 respondents. The first column provides the sample mean and the standard deviation for a series of characteristics. To test for balance across groups, the second column presents the coefficient estimates (and standard errors) of the difference between the baseline means in the treatment and control groups. All of the 23 coefficient estimates are small and none of them is significantly different from zero, suggesting that the randomization was successful at making the treatment orthogonal to observed baseline characteristics.

We stratified the sample on gender, and on whether they had a bank account. In each village, 14 respondents had a bank account, and 12 were opened a new one. This implies that 46.2% of the sample received a new account. In terms of demographic characteristics, respondents are mainly Other Backward Castes (OBC)[8], and less than half of them are literate. A great majority is married, and employed in agriculture (the omitted category is being unemployed). The sample is quite poor. They own about one acre of land, and 52% have a house made of mud (katcha). On average, respondents hold one other account with either a post office, cooperative, rural bank or formal bank. These accounts were opened to receive payments of welfare schemes, paddy or other grains (see Section 2.2). One out of five participants belongs to a neighborhood or self-help group. Most respondents are involved in the household's decision about where and how much to save, and a majority trusts both the BCSA and banks.[9] In terms of time preferences, 42% of the participants are impatient, i.e. they prefer money today instead of a larger amount in one week.

[7]Table 16 in Appendix B shows that the outcome variables are balanced at baseline as well.

[8]Castes are classified in the following categories: ST (Scheduled Tribe), SC (Scheduled Caste), OBC (Other Backward Caste), and FC (Forward Caste).

[9]The respondents were asked whether they trust the BCSA and banks. We build a trust index equal to one if the answer to both questions is "quite a bit of trust" or "a lot of trust". Otherwise, the index is equal to zero.

Table 1: Summary Statistics and Balance Check of Baseline Characteristics

	Mean (Std. dev.) (1)	Coefficient on *Paid on account* (Std. errors) (2)
Paid on account (%)	50.00	
	(50.06)	
New account (%)	46.15	-0.00
	(49.91)	(0.05)
Woman (%)	49.77	0.00
	(50.06)	(0.05)
Caste category: ST (%)	12.90	0.00
	(33.55)	(0.03)
Caste category: SC (%)	11.76	-0.01
	(32.26)	(0.03)
Caste category: OBC (%)	74.66	0.00
	(43.54)	(0.04)
Caste category: FC (%)	0.68	0.00
	(8.22)	(0.01)
Literate (%)	48.19	0.00
	(50.02)	(0.05)
Married (%)	88.24	0.01
	(32.26)	(0.03)
Age	43.00	0.43
	(12.61)	(1.20)
Wage labor in agriculture (%)	29.19	0.00
	(45.51)	(0.04)
Wage labor outside agriculture (%)	13.80	0.01
	(34.53)	(0.03)
Self-employed in agriculture (%)	45.48	-0.01
	(49.85)	(0.05)
Self-employed outside agriculture (%)	4.07	-0.01
	(19.79)	(0.02)
Land (acres)	1.17	-0.05
	(1.74)	(0.17)
Dwelling type: katcha (%)	52.49	0.01
	(49.99)	(0.05)
Accounts held (#)	1.17	0.00
	(0.60)	(0.06)
Savings groups (#)	0.16	0.00
	(0.38)	(0.04)
Takes savings decision at home (%)	84.84	0.02
	(35.90)	(0.03)
Trusts the BCSA and banks (%)	73.30	0.03
	(44.29)	(0.04)
Impatient (%)	42.08	0.04
	(49.42)	(0.05)
Distance to the BCSA (km)	0.29	-0.03
	(0.22)	(0.02)
Balance on BCSA account before start weekly surveys (Rs)	116.56	14.77
	(712.63)	(67.87)
Weeks interviewed (#)	9.73	-0.44
	(3.05)	(0.29)
Observations	442	442

The first column reports means (and standard deviations), and the second column shows the coefficient estimates (and standard errors) of the difference between the means in the treatment and control groups. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent

The average distance from the house to the BCSA is about 290 meter in crow flies. The last two variables in Table 1 are not included in the regressions, but provide some important information: the money on the BCSA account was balanced shortly before we started the weekly interviews, and so is the average number of weeks the respondents joined the weekly interviews. On average the respondents were interviewed ten times.

Across villages, the average distance between the location where the weekly interviews took place and the local banker is 55m. This figure is omitted from the table, as it is the same for the treated and control villagers. Indeed, we randomized at the individual level, and interviews were always done at the same location in each village.

3 Impact of the Payment Method

When people are paid on their account, money is saved by default, unless they take the active decision to withdraw. By contrast, transfers given in cash are ready to be spent, unless people make the active choice to deposit, or to save in other tools. As a result, a change in the payment method may affect savings and expenditures. Phase 1 of our experiment is designed to test this hypothesis. The respondents are interviewed at the same place in the village, close to the local banker. They can undo the default on their way home: the villagers paid in cash can deposit, and those paid on the account can withdraw. In Section 3.1, we first graphically asses whether the payment method makes a difference, before estimating the treatment effect on savings and expenditures in Section 3.2. The results confirm our hypothesis: the villagers paid on the account have a higher balance than the ones paid in cash, and the difference can be explained by a difference in consumption patterns. As there is no treatment effect on other savings (measured as the sum over all financial assets), we conclude that the respondents who were paid on the bank account have a higher level of savings at the end of our treatment. Additional evidence is provided by

Phase 2 of the experiment: once everyone is paid in cash, we no longer observe a difference in bank account savings and consumption patterns between the treated and control. Finally, in Section 3.3, we show that the respondents transact actively. However, they do not empty the balance on their account during the withdrawals that follow our last payment, but only take part of it. As a result, the treated respondents have a higher balance up to (at least) 23 weeks after the last payment of Phase 1.

3.1 A Graphical Assessment of the Treatment Impact

We provide summary statistics on both phases of the experiment using a graphical representation. In Figure 2, the horizontal axis shows the number of weeks since the start of the experiment, and the vertical axis the balance on the BCSA account by treatment group. During Phase 1, the balance of the treated increases much faster than the balance of the control, while they evolve similarly during Phase 2.[10] Overall, the balance of the treated first-order stochastically dominates the balance of the control. The stable balance of those who did not participate in our study (*Other villagers*) suggests the absence of any particular event that would affect people's savings in those villages during the experiment.[11]

Next, we graph the savings and consumption differences between the treated and control. For each week, we calculate the difference in the mean savings on the BCSA account and in the mean cumulative expenditures on frequent goods.[12] Figure 3 presents a connected line plot for both Phases. The horizontal axis still shows the number of weeks since the start of each phase, while the vertical axis now represents the difference in savings and expenditures between the treated and

[10] In between both phases, the balance of the treated goes down. This is consistent with a pattern of consumption smoothing over time.

[11] We obtained data on the transactions of the other customers in the seventeen villages where we did our study. We cannot identify them, but we can calculate aggregate figures.

[12] A detailed description of the frequent expenditures is provided in Section 3.2.

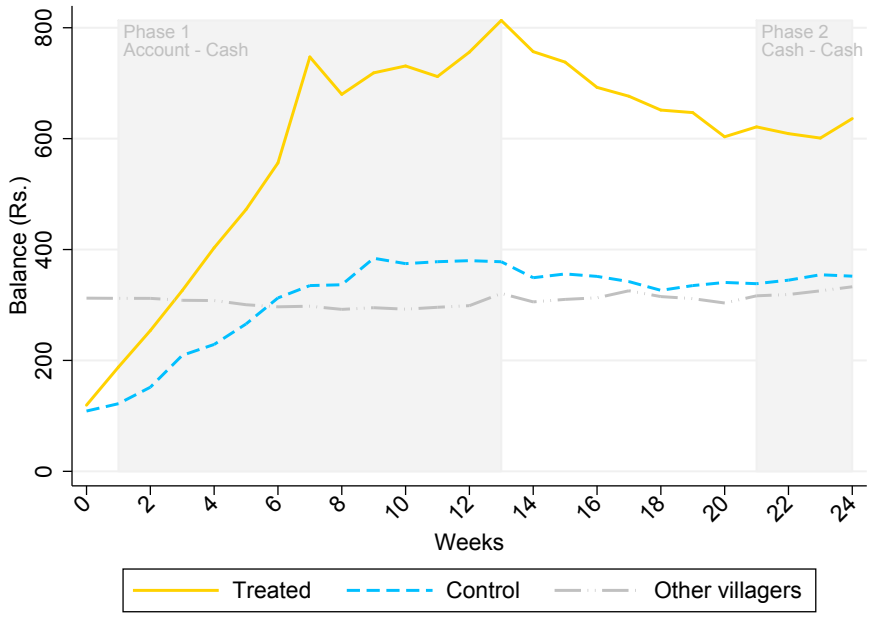

Figure 2: Evolution of the account savings of the Treated and Control

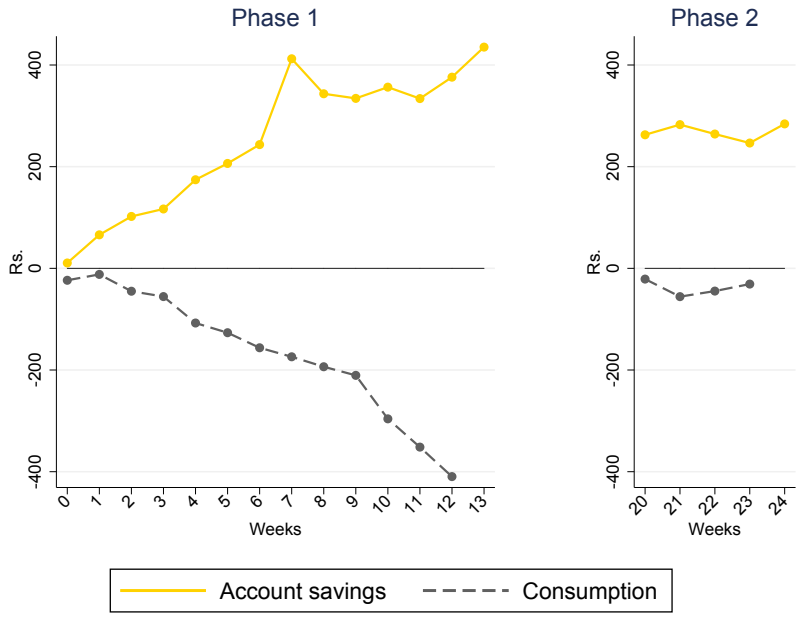

Figure 3: Differences between the Treated and Control in Savings and Consumption

control.[13]

The graph confirms our hypothesis: during Phase 1, transfers given in cash are more likely to be spent, while transfers on the account are more likely to be saved. Once everyone is paid in cash (Phase 2), we no longer observe a differential evolution over time. We will now estimate the impact on savings on the BCSA account, expenditures, and total savings using a regression framework.

3.2 Impact on Savings and Expenditures

We estimate the impact of being paid on the account by running the following regression:

$$Y_{ij} = \beta_0 + \beta_1 T_{ij} + \beta_2 X_{ij} + V_j + \epsilon_{ij} \tag{1}$$

where Y_{ij} is a measure of the savings or expenditures of individual i in village j, T_{ij} is a dummy indicating the respondent was paid on the account during Phase 1, and X_{ij} is a vector of baseline characteristics which includes all but the last two variables that were presented in Table 1. We estimate equation 1 both with and without these individual controls. V_j are village fixed effects that control for differences in time-invariant unobservables across villages, and ϵ_{ij} is the error term.[14]

We estimate the impact on the conditional mean using ordinary least squares, and on the conditional median using quantile regressions. The latter is more robust to outliers. As the compliance is not perfect, we interpret the impact as intention-to-treat estimates.[15] Standard errors are calculated using nonparametric bootstrap-

[13]Phase 1 consisted of 13 weekly interviews in the villages where we started first (see Section 2.2). Therefore, we see the effect on the balance up to week 13. However, we see the effect on consumption till week 12 only, as the information on week 13 would have been obtained during interview 14. Similarly, during Phase 2 we have information on the balance for four weeks, and on consumption for three weeks.

[14]There is one banker per village, hence the village fixed effects also absorb all banker fixed effects.

[15]Twelve respondents never came for an interview, and some others had to skip part of the interviews due to other obligations.

ping, but the significance levels are robust to the use of other methods (more details are provided in Appendix C).

The Tables 2 and 3 provide the main results for Phase 1 and Phase 2 respectively. As explained in Section 2.2, the control respondents were paid in cash, and the treated on the account during Phase 1, while all the respondents were paid in cash during Phase 2. The results include control variables, but the ones without are similar and are provided in the Tables 12 and 13 in Appendix B.[16]

Table 2 provides the impact on savings on the BCSA account, consumption and total savings during Phase 1. First, it shows the impact on two different measures of savings that are constructed using the BCSA account's data: (1) the *final balance* is the respondent's balance the day after we conducted the last weekly interview in the village, and (2) the *average balance* is the average account balance from the day after the first till the day after the last weekly interview in the village.[17]

The other outcome variables are based on information that was gathered during the weekly household surveys. As twelve respondents never showed up for an interview, the sample reduces from 442 to 430 observations, but it remains balanced (see the first two columns of Table 15 in Appendix B). Table 16 in Appendix B shows that all the outcome variables that will be introduced in this Section were balanced at baseline as well.

The average respondent was interviewed 10 times, and received Rs 1,500 in total. We do not expect this amount to be sufficient to make a difference in terms of investments, or durables (non-frequent expenditures), but it might impact the expenses on frequent consumption, and temptation goods. The impact on the latter two variables is shown in the columns (3) and (4), while the impact on investment and durables is presented in Table 14 in Appendix B. Frequent consumption is the

[16]The coefficients of the control variables are available upon request.

[17]When constructing the different measures of savings, we use the balance one day after the last interview, as to allow villagers paid on the account to withdraw, and villagers paid in cash to deposit. Otherwise, the difference between treated and control could be artificially inflated.

sum of expenditures on goods that are bought frequently by the average household, i.e. at least once every three weeks.[18] Under temptation goods we classify goods that are not *survival necessities* (Banerjee and Mullainathan, 2010). In line with the literature, it includes pan, alcohol, tobacco, and drinks and snacks from the market. As those goods are more likely to be consumed by men, we also included hair oil, lotion and perfumes.

Finally, we estimate the treatment effect on the financial assets that respondents might own: (i) cash at home, (ii) money on other accounts, (iii) balance with an agricultural cooperative, (iv) balance on a post office account, (v) savings with self-help groups (SHGs) or other informal neighborhood groups, (vi) the sum of those five assets, and (vii) the sum of those assets and the savings on the BCSA account. For each asset, we use the value that was reported during the last interview. Table 2 provides the results for cash at home, and the totals, and Table 14 in Appendix B for the other financial assets.

Being paid on the account has significant positive effects on the different measures of savings on the account. Compared to the control mean, the effects are extremely large: the final balance increases by 111 percent, and the average balance by 85 percent. The impact is also important in real terms: it is equivalent to a household's average weekly frequent expenditures (Rs 451, see Table 16) and amounts to 55% of a household's average weekly income (Rs 770). In the control group, the median of the final and average balances is much smaller than the mean. As the size of the impact on the conditional median is similar to the size of the impact on the conditional mean, it is larger in relative terms. The result can also be seen from Figure 4, which pictures the distribution of the final balances. The treated respondents are much less likely to have a zero balance, and both their mean and median balances are

[18]This includes (we provide the average frequency of expenditures on the goods in brackets): grains and cereals (0.60), pulses and lentils (0.67), milk products (0.44), edible oil (0.47), vegetables (0.99), fruits (0.47), sugar, salt, and spices (0.87), fuels light (0.98), soap (0.66), and washing powder (0.70). We obtain the same categorization if we use the baseline information, and the first week of the interviews only.

Table 2: Impact of Being Paid on the Account on Savings and Expenditures (Phase 1)

	BCSA balance					Total assets	
	Final	Average	Frequent consumption	Temptation goods	Cash at home	without BCSA	including BCSA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Impact on the conditional mean							
Paid on account	420.4***	254.9***	-386.8*	22.9	-161.1	479.0	919.6**
	(78.6)	(45.5)	(210.5)	(46.5)	(447.7)	(444.8)	(447.1)
R^2	0.10	0.08	0.15	0.11	0.13	0.13	0.12
Mean dependent (control)	378	299	3328	663	1614	2436	2821
Panel B: Impact on the conditional median							
Paid on account	401.7***	250.9***	-318.2*	-27.4	-67.3	-61.8	455.9***
	(51.5)	(25.7)	(169.7)	(56.1)	(56.1)	(128.9)	(150.7)
Median dependent (control)	50	40	2661	470	300	990	1156
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	442	442	430	430	430	430	430

Panel A presents the impact on the conditional mean using ordinary least squares, and panel B on the conditional median using quantile regressions. In the columns (1) and (2) the dependent variables are different measures of the savings on the respondent's BCSA account; in column (3) and (4) it is the household's total expenditures on frequent consumption and temptation goods respectively; and in the columns (5)-(7), the respondent's financial assets, measured during the last weekly interview. All columns include village fixed effects and the following baseline characteristics: the respondent's caste category, literacy, marital status, age, occupation, land owned, dwelling type, accounts held, membership of savings groups, and distance to the BCSA. It also includes dummies indicating whether the respondent takes savings decisions in the household, trusts both the BCSA and banks, and is impatient. All columns include village fixed effects. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

higher.

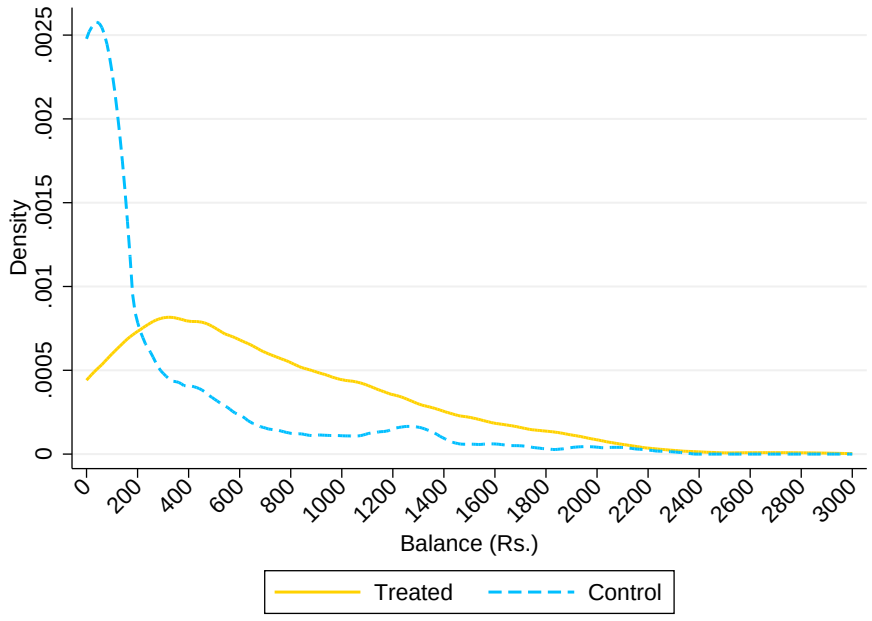

Figure 4: Distribution of the Final Balance

With respect to consumption, there is a significant impact on frequent consumption only: the respondents paid in cash spend Rs 387 more. Remarkably, the size of the treatment effect is almost the same as the impact on the respondent's final balance. The conclusion is similar for the impact on the conditional median: respondents paid in cash spend Rs 318 more, and have Rs 402 less on their account.

Finally, the control keep slightly more cash at home, but the coefficient is not significantly different from zero. The treatment effect on total savings is significant only if we include the balance on the BCSA account.

In summary, these results confirm our hypothesis: respondents who are paid on the account save more, while respondents paid in cash spend more on frequently consumed goods. As there is no treatment effect on other savings, the respondents who were paid on the bank account have a higher level of savings at the end of our treatment.

We now turn to Phase 2 of the experiment. Table 3 presents the effect of being paid on the bank account during Phase 1 on savings and expenditures during Phase 2. In column (1) the dependent variable is the difference in the respondent's balance on the BCSA account between the day after we finished Phase 2, and the day before we started it. In the columns (2) and (3), the outcome variables are the respondents' total expenditures on frequent consumption and temptation goods, and in the columns (4) to (6) the change in cash at home, and in the total financial assets (excluding and including the savings on the BCSA account).[19]

Table 3: Impact of Being Paid on the Account on Savings and Expenditures (Phase 2)

	Change in balance BCSA	Frequent consumption	Temptation goods	Change in cash at home	Change in total assets	
					without BCSA	including BCSA
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Impact on the conditional mean						
Paid on account during Phase 1	18.4	15.2	-14.4	-177.5	-172.9	-217.4
	(69.8)	(67.8)	(12.8)	(311.8)	(319.5)	(301.7)
R^2	0.08	0.13	0.14	0.09	0.08	0.08
Mean dependent (control)	21	973	212	105	56	77
Panel B: Impact on the conditional median						
Paid on account during Phase 1	-6.5	-26.4	-1.7	21.4	27.0	-96.0
	(4.2)	(66.1)	(20.7)	(72.6)	(84.7)	(93.6)
Median dependent (control)	0	805	170	0	-10	0
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	442	400	400	400	400	400

Panel A presents the impact on the conditional mean using ordinary least squares, and panel B on the conditional median using quantile regressions. The dependent variables are the difference in the respondent's balance on the BCSA account between the start and the end of Phase 2, the household's total expenditures on frequent consumption and temptation goods during Phase 2, and the change in financial assets between the start and the end of Phase 2. We include the same baseline characteristics as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

[19]The sample reduces to 400 observations, but it remains balanced. The balance check is available upon request.

Being paid on the account during Phase 1 has no significant effect on the change in the account's balance during Phase 2. Furthermore, the difference is small. As we do not observe a difference in savings on the BCSA account, we do not expect a treatment effect on expenditures and total savings. Indeed, once everyone is paid in cash, the treated and control no longer differ in terms of consumption and savings patterns. Remarkable, the difference on frequent consumption is Rs 15 only.

In conclusion, the balance of the treated as compared to the control increased significantly only, in those weeks where the treated villagers were paid directly on the account, i.e. when the payment method was different.

3.3 Transactions and the Long-Term Effect

There are three remarkable observations with respect to account use. First, villagers transact actively during Phase 1 of the experiment: the average respondent made 3.28 transactions, and a total of 318 respondents (72%) made at least one transaction. Panel A in Table 4 provides summary statistics on the respondent's total number of deposits and withdrawals, and on the average amount per transaction. The figures do not include our payments (we subtracted those before taking averages).

Respondents paid in cash deposited more often than the villagers paid on the account (2.60 times as compared to 0.78 times). In total, 205 respondents deposited at least once: 130 from the control, and 75 from the treated group. The average amount per deposit is close to our payment for the control (Rs 155.6), and slightly higher for the treated (Rs 202.4).

On the other hand, treated villagers withdraw more often than the control (2.80 times as compared to 0.37 times), but less often than the average number of payments made by us (10 times). This implies that the treated respondents did not withdraw systematically after an interview took place, which led to the observed increase in the balance on the account. Indeed, during the weeks in which they were

Table 4: Transactions

Total number			Average amount		
Full sample (Std. dev.) [Obs.]	Paid cash (Std. dev.) [Obs.]	Paid on account (Std. dev.) [Obs.]	Full sample (Std. dev.) [Obs.]	Paid cash (Std. dev.) [Obs.]	Paid on account (Std. dev.) [Obs.]
Panel A: Phase 1 (10 weeks)					
Deposits					
1.69	2.60	0.78	172.7	155.6	202.4
(3.14)	(3.95)	(1.57)	(416.7)	(356.8)	(505.2)
[442]	[221]	[221]	[205]	[130]	[75]
Withdrawals					
1.59	0.37	2.80	467.2	565.3	445.3
(2.68)	(0.99)	(3.22)	(480.8)	(867.2)	(340.2)
[442]	[221]	[221]	[219]	[40]	[179]
Panel B: Phase 2 (4 weeks)					
Deposits					
0.66	0.70	0.63	156.2	147.0	165.9
(1.28)	(1.38)	(1.18)	(164.0)	(139.7)	(186.6)
[442]	[221]	[221]	[139]	[71]	[68]
Withdrawals					
0.15	0.11	0.19	606.9	639.4	592.4
(0.47)	(0.50)	(0.44)	(492.3)	(620.2)	(433.2)
[442]	[221]	[221]	[52]	[16]	[36]

paid on the account, 81% of the treated respondents withdrew at least once, while only 1.8% had a zero balance at the end of those weeks.

Second, the treated and control use their accounts in a similar manner when they are all paid in cash. As can be seen from Panel B, they do the same average number of transactions, and transact similar amounts per transaction. Given that Phase 2 was shorter than Phase 1, the average number of transactions remains substantial.

Third, the respondents did not withdraw the remaining funds at once when we left the region: during the five months that followed the last payment on the account, 61.5% withdrew at least once, while only 8.6% had less than Rs 5 on the account at the end of those months.

Therefore, it is worth examining the treatment effect in the long run. To do so, we estimate equation 1, where Y_{ij} is the balance on the account 15, 19 and 23 weeks

after the last interview of Phase 1. Table 2 showed that the treatment increased the final balance by 111 percent on average. From Table 5 we learn that the final balance of the treated is (more than) twice the one of the control after 15 and 19 weeks, and still 77 percent higher after 23 weeks. The size of the impact on the median balance is smaller, but the relative impact - compared to the median balance of the control - is (much) higher.

In conclusion, the treated respondents have a higher balance on the account up to (at least) 23 weeks after our last account based payment.[20]

Table 5: Treatment Effect in the Longer-Run

	After 15 weeks (1)	After 19 weeks (2)	After 23 weeks (3)
Panel A: Impact on the conditional mean			
Paid on account	319.9***	292.0***	207.4**
	(70.6)	(70.5)	(84.5)
R^2	0.08	0.08	0.04
Mean dependent (control)	297.3	280.0	268.7
Panel B: Impact on the conditional median			
Paid on account	215.0***	173.5***	81.8**
	(47.4)	(47.8)	(34.3)
Median dependent (control)	42.1	35.0	35.0
Controls	Yes	Yes	Yes
Observations	442	442	442

Panel A presents the impact on the conditional mean using ordinary least squares, and panel B on the conditional median using quantile regressions. The dependent variables are the respondent's balances on the BCSA account 15, 19 and 23 weeks after the last interview of Phase 1. We include the same baseline characteristics as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

[20]We have the bank account data up to that date.

4 A Discussion of the Mechanisms

In the endline survey, we asked the respondents to choose between receiving:

Choice 1 Rs 150 on their account or Rs 150 in cash

Choice 2 Rs 125 on their account or Rs 150 in cash

Choice 3 Rs 150 on their account or Rs 125 in cash

Choice 4 Rs 150 on their account or Rs 175 in cash

Choice 5 Rs 175 on their account or Rs 150 in cash

They then draw a random number ranging from one to five to select the choice that is paid out.

Table 6 presents the treatment impact on the preference for account-based payments, e.g. *choice 1* is a dummy that equals 1 if the respondent prefers Rs 150 on the bank account (and 0 if he prefers Rs 150 in cash). The treatment has a significant impact on each choice: the treated are more likely to prefer an account-based payment, even when it implies receiving a lower amount. This implies that (i) treated respondents have a higher preference for being paid on the account, and (ii) that they are more willing to lose money in order to have someone depositing the payment for them. The latter result suggests serious barriers to depositing, as they would otherwise choose the highest amount and deposit themselves.

Therefore, the treatment impact on the account savings can be explained by a change in (i) *the optimal amount that villagers want to save on the account* and in (ii) *their ability to actually reach their optimal level of savings.* To shed further light on the underlying mechanisms, we discuss different factors that may have influenced both the optimal level of account savings and the capacity to reach that level. In particular, the treated may: (1) have to spend less time and effort to save on the account (because the control face the additional hurdle of depositing themselves);

Table 6: Treatment Effect on Payment Choices

	Choice 1 (1)	Choice 2 (2)	Choice 3 (3)	Choice 4 (4)	Choice 5 (5)
Paid on account	0.13***	0.05**	0.11***	0.05**	0.11**
	(0.04)	(0.02)	(0.04)	(0.02)	(0.05)
R^2	0.06	0.06	0.08	0.06	0.10
Mean dependent (control)	0.12	0.03	0.64	0.06	0.59
Controls	Yes	Yes	Yes	Yes	Yes
Observations	419	419	419	419	419

The dependent variables are dummies that equal one if the respondent prefers to receive (1) Rs 150 on his account instead of Rs 150 in cash, (2) Rs 125 on his account instead of Rs 150 in cash, (3) Rs 150 on his account instead of Rs 125 in cash, (4) Rs 150 on his account instead of Rs 175 in cash, and (5) Rs 175 on his account instead of Rs 150 in cash. We include the same baseline characteristics as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

(2) have a higher trust and a better relationship with the banker if the treatment pushed them to interact more with him; (3) be subject to experimenter demand effects and believe they are expected to leave the money on the account; (4) face a lower pressure to share their Rs 150 with the kith and kin; (5) suffer less from self-control problems and (6) *label* the account and - through mental accounting - manage to save more on it.

In the following sections, we provide evidence that the treatment impacts can be explained by the time and effort it takes to do a transaction, a lack of self-control, and mental accounting. We argue that they are not due to a higher trust and a better relationship with the banker, experimenter demand effects, or redistributive pressures.

4.1 Time and Effort to Transact

There are limited monetary costs to depositing and withdrawing (see Section 2.1), but it takes some time and effort to go to the banker and to make a transaction.[21]

[21]The respondents rarely face problems once they reached the BCSA. Indeed, only 5.4% of the 419 respondents who were interviewed at the endline reported that the BCSA was not available,

As a proxy for those costs, we use the geographical distance to the banker's shop. In each village, the treated and control were interviewed on the same day, at the same place, at an average distance of 55 meter from the banker. Therefore, on average, the treated and control face an equal cost to transact immediately after the interview. On other days, the average transaction cost should not differ by treatment group either, as the geographical distance from their home to the banker is balanced (see Table 1). In addition, Table 7 shows that the treatment impact on savings does not depend on whether the distance to the banker is above or below median.[22]

Table 7: Heterogeneous Treatment Effect by Distance to the Banker

	Final balance (1)	Average balance (2)
Paid on account	355.0***	238.5***
	(122.4)	(66.9)
Distance to BCSA above median	-124.1	-91.9
	(145.4)	(127.0)
Treatment x Distance to BCSA above median	134.3	40.5
	(174.2)	(116.0)
R^2	0.10	0.08
Mean dependent (control)	378	299
Controls	Yes	Yes
Observations	442	442

The dependent variables are the respondents final and average balances as defined in Table 2. "Distance to BCSA above median" is a dummy that equals one if the geographical distance between the respondent's home and the banker is higher than the median distance. We include the same baseline characteristics as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

We do not find evidence of important distance effects in explaining the results. But there remains the fact that compared to the treated, the control face the additional hurdle of depositing themselves. That transaction requires very little time and effort, but as we know from the *default* literature, tiny costs can have large

or did not have enough cash at some moment during the previous month.

[22]We obtain similar results if we use binary variables corresponding to the distance quartiles, or a continuous measure.

consequences (see for instance Johnson et al., 1993; Ted O'Donoghue, 1999; Choong Whan Park, 2000; Madrian and Shea, 2001; Choi et al., 2002; Johnson et al., 2002; Johnson and Goldstein, 2003; Choi et al., 2004; Abadie and Gay, 2006; Kressel and Chapman, 2007; Carroll et al., 2009). Therefore, we cannot completely rule out this channel.

4.2 Trust in the BCSA

Recent evidence emphasizes the importance of trust in explaining formal bank account savings (Coupe, 2011; Dupas et al., 2012; Iyer and Puri, 2012; Sapienza and Zingales, 2012; Bachas et al., 2015; Mehrotra et al., 2016). The share of villagers who did at least one transaction is 50% higher in the group who was paid on the account. Therefore, the treated are more likely to interact with the BCSA, which might affect their trust in or empathy towards the banker, and hence their willingness to keep a higher balance on the account. To test whether this is the case, we played trust and dictator games with real payments shortly after Phase 1.

We did not inform the respondents in advance about our intention to play games, due to which only 381 respondents were present in the village. We test the balance of the reduced sample in the last two columns of Table 15 in Appendix B. All of the 23 coefficient estimates are small and none of them is significantly different from zero. This suggests that the treatment is still orthogonal to observed baseline characteristics in the reduced sample.

First, the respondents were asked to play a trust game in the role of the trustor, while the BCSA was the trustee. They had to allocate a fixed endowment X of Rs 50 between themselves and the BCSA using multiples of 10. The BCSA received triple the amount sent, $3X$, and could send back any amount Y between 0 and $3X$, using multiples of 10 (0; 10; 20;...; $3X$). The respondent earned (50 - X + Y) and the BCSA ($3X$ - Y). The BCSA did not know who gave the money, he only knew it came from a person in his village.

Next, each respondent was asked to play a triple dictator game in the role of the dictator. The respondent had to allocate a fixed endowment of Rs 50 between himself and the BCSA, using multiples of 10. The villager earned (50 - X) and the BCSA $3X$. Again, the BCSA did not know who gave the money, he only knew it came from a person in his village.[23]

We estimate Equation 1, where Y_{ij} is the amount sent to the BCSA by respondent i in village j in the trust and the triple dictator game, respectively. The first column of Table 8 presents the results for the trust game, and the last column for the triple dictator game.

Table 8: Treatment Effect on Trust and Empathy

	Trust game (1)	Dictator game (2)
Paid on account	-1.59	0.53
	(1.22)	(0.91)
R^2	0.07	0.06
Mean dependent (control)	21.5	10.1
Controls	Yes	Yes
Observations	381	381

In the first column the dependent variable is the amount sent to the BCSA in a trust game, and in the second column the amount sent in a triple dictator game. We include the same baseline characteristics as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Being paid on the account has no significant effect on the amounts sent to the BCSA. The difference is also negligible in monetary value. Therefore, the lab experiment suggests that the treatment did not have an impact on the trust in, or empathy towards the local banker.[24]

[23] We used the strategy method to obtain the amounts sent back by the BCSA as a function of the amounts that he received.

[24] A complete description of this laboratory experiment is given in Mehrotra et al., 2016.

4.3 Experimenter Demand Effect

Experimenter demand effects are a potential threat to most experimental studies. In our case, they play a role if the treated were reluctant to withdraw, and the control to deposit, because they thought we did not want them to do so. However, elements in the design of our experiment, and several observations from the field go against their existence.

First, during the practical information session, we introduced the account as a protected place to save to all the respondents (see Section 2.2). Both the treated and control attended the session, and were paid for the baseline survey at the end of it only. During the experiment, the research team's instructions were to let the respondents use their account according to their convenience and not to comment on the choices they made.

Second, Panel A of Table 4 in Section 3.3 shows that both the treated and control transact during Phase 1. The average respondent made 3.28 transactions during those 10 weeks, and a total of 318 respondents (72%) made at least one transaction.

Third, at the start of Phase 2 - during which the treated were also paid in cash - we explicitly mentioned to all the respondents that the role of the account did not change, but that they have to deposit themselves the amount they want on the account. Panel B of Table 4 shows that there were more deposits than withdrawals during those four weeks. Indeed, they made on average 0.66 deposits and 0.15 withdrawals, and while 32% (31%) of the control (treated) deposited at least once, only 7% (16%) of the control (treated) withdrew at least once.

Finally, the respondents did not withdraw the remaining funds at once when we left the region: during the five months that followed the last payment on the account, 61.5% withdrew at least once, while only 8.6% had less than Rs 5 on the account at the end of those months. It is important to remark that this is not driven

by a lack of knowledge about the remaining balance on the account.[25]

4.4 Redistributive pressures from the household and from others

Compared to savings on a bank account, cash at home is more likely to be within the reach of others, and therefore more prone to demands by the kith and kin. Account-based payments might help dealing with those demands if it allows hiding the transfers, or makes them sufficiently illiquid. Redistributive pressures have been emphasized as important barriers to savings in other context (Baland et al., 2011; di Falco and Bulte, 2011; Boltz et al., 2015; Jakiela and Ozier, 2016) and we want to assess their importance in our study.

To explore the role of intra-household pressure, we asked a set of specific questions during the endline survey. First, 99% of the spouses knew that the respondent received Rs 150 after each interview. Therefore, bank account payments could not help hiding the existence of transfers. Second, in the first two columns of Table 9 we estimate whether there is a treatment effect on the likelihood of being asked to share the weekly payments with (1) the spouse (for married women only), or (2) any household member. The negative sign suggests that respondents paid on the account are subject to fewer requests, but the coefficient is very small and not statistically significant. Conditional on having being asked for money, the average respondent gives Rs 67 to the spouse ($N = 67$), and Rs 54 to the spouse or any other household member ($N = 172$). The amount given does not differ significantly between the treated and control.

Next, we explore the demands by relatives and friends outside the household. In the last two columns of Table 9, we investigate whether the treatment has an effect on the net inflow of loans and transfers to the household. We measure the net inflow

[25]We compared the declared balance during the last weekly interview, with the real balance on the account: the correlation is 0.86, and the difference between the average declared amount and the average on the bank account is an insignificant Rs 18 only.

of loans as the total amount borrowed, minus the total amount lent, plus the net amount of reimbursements received during the experiment. Transfers are measured in a similar manner: it equals the total amounts received, minus the total amounts given. The treatment does not have a significant impact on the net inflow of loans and transfers.

Table 9: Treatment Effect on Loans and Transfers

	Asked money by		Loans	Transfers
	Spouse	Any hh member		
	(1)	(2)	(3)	(4)
Paid on account	-0.05	-0.09	471.4	27.3
	(0.05)	(0.06)	(1319.7)	(172.5)
R^2	0.08	0.06	0.04	0.06
Mean dependent (control)	0.21	0.35	573	-140
Controls	Yes	Yes	Yes	Yes
Observations	367	424	430	430

The dependent variables are constructed as follows: "Asked money by spouse" is a dummy that takes value one if the spouse asked a share of the weekly transfer (for married women only), and "Asked money by hh member" if any household member asked for a share (including the spouse). "Loans" is the net inflow of loans into the household during the experiment: It is the total amount borrowed, minus the total amount lent, plus the net amount of reimbursements received. Finally, "transfers" is the total amounts received, minus the total amounts given. We include the same baseline characteristics as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parenthesis. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

This is in line with our baseline information on limitations to saving: only 3% of the respondents replied positive to our question on whether they have difficulties in saving because relatives or friends make a claim on it (on the contrary, 89% says that they have difficulties in saving because it is hard to control spending).

In conclusion, the data show that the respondents paid on the account and in cash face similar requests from the kith and kin, and respond to them in the same manner. Therefore, redistributive pressures cannot explain the impact we find.

4.5 Lack of Self-Control

The control immediately increase expenses on frequent consumption, while the treated have more savings at hand for consumption smoothing and medical expenses in the future (see Section 4.6 for details on the use of savings). The respondents put forward that a lack of self-control provides a possible explanation for this pattern.[26] First, during the baseline survey, 89% of them declare "*having difficulties in saving because it is hard to control spending*". Second, at the end of the experiment, we organized focus group discussions to explain the purpose of our study, and the final results. The respondents in the control group said it was difficult to save, as "*cash generates need*". At the moment they go to the market, they count the amount of money in their pocket. The treated respondents agreed and said they faced the same problem during the weeks in which they were paid in cash.

A subset of villagers realize that the payment method helps them to control spending. During the endline survey, we asked the respondents to explain their preference for payments in cash or on the account using an open question. The respondents who prefer being paid in cash mainly do so because they can "*use the money immediately and flexibly*" (63%). The respondents who prefer being paid on the account do so because it "*allows to save, while cash is spent too easily*" (91%).

In conclusion, the respondents suggest that a lack of self-control can explain why the control consume more and save less.[27]

[26] See Karlan et al., 2014 for a review of the research on self-control and savings in low income countries.

[27] A lack of self-control can have various origins, among which time-inconsistent preferences has been the focus of much theoretical and empirical research. We measure whether respondents exhibit more impatience for present financial trade-offs than for future ones by asking hypothetical time-preference questions during the baseline survey. We learn that respondents who exhibit time-inconsistent preferences - 11.8% of our sample - save less on their bank account. We however do not find evidence that the treatment affects time-inconsistent villagers differently. As withdrawals were not restricted, and transaction costs very small, the accounts might not have provided the necessary commitment to overcome time-inconsistent preferences (this finding is in line with Dupas and Robinson, 2013b). The estimations are available upon request.

4.6 Mental Accounting

Mental accounting has recently been emphasized as an explanation for higher account savings (Thaler, 1999; Dupas and Robinson, 2013b). At baseline, we asked the respondents who already had a BCSA account about the specific objectives for which they are saving on the account. The descriptive statistics are provided in the first two columns of Panel A in Table 10.[28] We asked the same question during the endline survey, and provide the results in the last two columns.

The descriptive statistics reveal two interesting patterns. First, only 15% of the respondents saved for a specific purpose at baseline, while 64% did so at the endline.[29] Second, while there were no differences between the treated and control at baseline, the respondents are more likely to save for a specific purpose at the endline if they were paid on the account instead of in cash. The most frequent purpose is dealing with emergencies.

In Panel B we only use the sample of respondents who have a positive balance. We no longer observe a difference between the treated and control.

We also asked the respondents how they spent their largest withdrawal since the start of the experiment. The exact amount was taken from the bank records, and its use was asked for during the endline survey. We make two observations based on the descriptive statistics that are provided in Table 11. First, the declared reasons to withdraw are roughly in line with the goals of saving: "emergencies" (consumption smoothing, and medical expenses) are the most important, followed by marriages, and businesses.[30] Second, the distribution over the different purposes is similar for

[28] At baseline, we could only ask the question to respondents who already had an account. The endline includes both old and new account holders.

[29] If we restrict the sample to the respondents who already had an account at baseline, it is even slightly higher at the endline.

[30] It is not surprising that the frequencies are not exactly the same in both tables, as the households may have different purposes to save in Table 10, while in Table 11, we obtain information about the largest withdrawal only. Furthermore, they did not necessarily reach the amount needed, or the moment to use it.

Table 10: Account Use

Do you save on your BCSA account for:	At baseline: Paid cash (Std. dev.)	At baseline: Paid on the account (Std. dev.)	At endline: Paid cash (Std. dev.)	At endline: Paid on the account (Std. dev.)
Panel A: All respondents				
Business investments	0.0	0.0	9.0	8.5
	(0.0)	(0.0)	(28.6)	(27.9)
Loan repayment	0.0	0.8	9.4	13.2
	(0.0)	(9.2)	(29.3)	(33.9)
Education expenses	1.7	1.7	10.4	10.4
	(12.9)	(12.9)	(30.6)	(30.6)
A marriage	1.7	0.0	9.4	10.4
	(12.9)	(0.0)	(29.3)	(30.6)
Repair of the house	1.7	0.0	9.0	9.9
	(12.9)	(0.0)	(28.6)	(29.9)
Your old day	0.8	0.8	8.5	9.0
	(9.2)	(9.2)	(27.9)	(28.6)
Emergencies	14.3	10.9	53.3	68.4***
	(35.1)	(31.3)	(50.0)	(46.6)
Any of the reasons	16.8	12.6	56.6	71.7***
	(37.6)	(33.3)	(49.7)	(45.2)
Observations	119	119	212	212
Panel B: Respondents who had a positive balance				
Business investments	0.0	0.0	11.9	9.0
	(0.0)	(0.0)	(32.5)	(28.7)
Loan repayment	0.0	3.4	12.6	14.0
	(0.0)	(18.6)	(33.3)	(34.8)
Education expenses	5.9	6.9	13.8	11.0
	(23.9)	(25.8)	(34.6)	(31.4)
A marriage	5.9	0.0	12.6	11.0
	(23.9)	(0.0)	(33.3)	(31.4)
Repair of the house	5.9	0.0	11.9	10.5
	(23.9)	(0.0)	(32.5)	(30.7)
Your old day	2.9	3.4	11.3	9.5
	(17.1)	(18.6)	(31.8)	(29.4)
Emergencies	50.0	44.8	71.1	72.5
	(50.8)	(50.6)	(45.5)	(44.8)
Any of the reasons	58.8	51.7	75.5	76.0
	(50.0)	(50.9)	(43.2)	(42.8)
Observations	34	29	159	200

We indicate the significance of the difference in the means of the treatment and control in the columns (2) and (4) as follows: *** at 1 percent, ** at 5 percent, * at 10 percent.

the treated and control.[31]

Table 11: Use of the Largest Withdrawal

	Paid cash		Paid on the account	
	%	Avg amount	%	Avg amount
Consumption smoothing	22.5	1031	34.0	477
Medical expenses	16.3	440	14.9	858
A marriage	12.5	935	14.4	693
Business or agriculture	10.0	838	9.6	949
Transportation costs	11.2	272	7.4	446
Festivals, clothes	10.0	781	7.4	589
Repayment loan	6.3	560	3.2	617
Education expenses	3.7	667	3.2	708
House repair	3.7	833	3.2	3131
Does not like account	2.5	5500	1.6	500
To pay bill	1.3	500	1.1	350
Observations	80	80	188	188

In conclusion, we find that the treated are more likely to have labelled their account and to use it for a well-defined purpose. However, conditional on having savings, both the treated and control are equally likely to use the account for a well-defined purpose. We observe that the treatment leads simultaneously to a higher probability to have savings and a higher probability to label the account. It could be that the treatment had a direct mental accounting effect, or that the treatment increased savings, which then pushed the respondents to define a savings' purpose. On this basis, we cannot rule out that mental accounting participates in explaining the treatment's impact on savings and consumption.

[31]We have a larger number of observations for the treated, because they were more likely to withdraw at least once.

5 Conclusions

Several products have been designed to encourage households to save more, from simple technologies such as a box with a key (Dupas and Robinson, 2013b), to savings reminders (Karlan et al., 2010), and commitment savings accounts (Ashraf et al., 2006, 2010). Although the overall impact is positive, these technologies have a limitation: they still require an active decision to save, and therefore some self-control. In developed countries, products have been designed that overcome the need of an active savings decision. The best known example are automatic transfers to 401(k) savings plans. In developing economies, where most economic transactions are settled in cash, direct transfers on a bank account could serve the same purpose without the need of a commitment component. We tested this hypothesis in rural India. We compared the savings on bank accounts, the savings in other financial assets, and the consumption patterns of villagers who received identical weekly payments, but were randomly allocated to being paid in cash (control) or on the bank account (treated). We find that being paid on the account increases savings by about 111 percent, or Rs 420 after three months. Being paid in cash increased the total expenditures on frequent consumption, such as rice, vegetables, and other regular household expenses, by a similar amount over the same period: Rs 387. The treated and control use other financial assets, such as cash at home, in a similar way. To the best of our knowledge, this is the first experiment to test how bank account transfers instead of cash payments affect recipients' savings and consumption. The large effects that we find are in line with the general *default* literature.

We interpret our findings as the result of the default option, and further discuss plausible mechanisms. We provide evidence that the treatment impacts can be attributed to (i) the (minimal) time and effort it takes to do a transaction, (ii) a lack of self-control, and (iii) mental accounting. We argue that they are not due to (iv) a higher trust and a better relationship with the banker, (v) experimenter

demand effects, or (vi) redistributive pressures.

The marginal savings rate is relatively high in our experiment. People received Rs 1500 on average, out of which the treated saved almost one third more than the control. An important outstanding research question is how savings and consumption would be affected if the main income source is paid on an account instead of in cash.

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Appendix A: Study Area

The study took place in three districts of Chhattisgarh, a Central-Eastern state of India. Figure 5 shows the location of Chhattisgarh in India, of the three districts in Chhattisgarh, and finally, of the villages in the three districts.

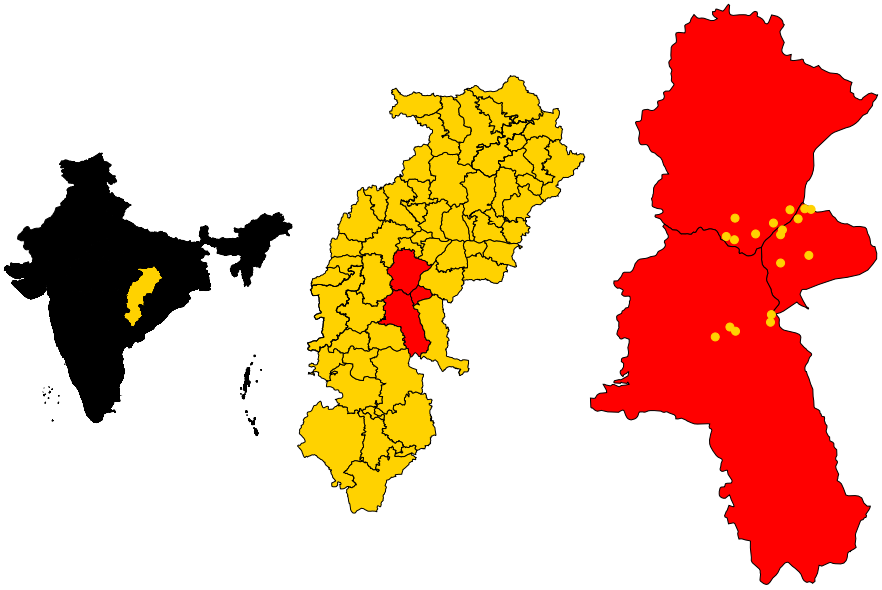

Figure 5: Map of the Study Area

Appendix B: Additional Results and Balance Checks

Additional Results

The Tables 12 and 13 provide the treatment effects that were presented in the Tables 2 and 3, but for regressions without control variables.[32] There is one interesting difference, namely the significant impact on the conditional median of cash at home during Phase 1: the control save Rs 425 less on their BCSA account, but they consume Rs 310 more, and save Rs 100 more at home.

Table 14 presents the treatment effect for two extra expenditure categories: (1) non-frequent expenditures and (2) investment. Both outcome variables are measured as the total amount spent during Phase 1. The investment category includes investments on livestock, businesses, and agricultural tools and inputs, such as fertilizers. All irregular expenses are classified as non-frequent. It includes expenditures on durable goods, education, services, rent, water charges, house repair, clothes, footwear, bedding, kitchen utensils, and furniture. To categorize the remaining goods as frequent or non-frequent, we calculate how often households consume or spend on each category during the weekly household surveys. Frequent consumption includes all goods on which the average household spends at least once every three weeks, while the other goods - meat, toothpaste, and shaving articles - were added to the category of non-frequent goods.

The average respondent was interviewed 10 times, and received Rs 1,500 in total. We do not expect this amount to be sufficient to make a difference in terms of investments, or irregular expenses such as expenditures on durable goods. Indeed, the first two columns show that there are no systematic differences between the treated and control.

The columns (3) to (6) of Table 14 show details on the respondent's financial assets that we aggregated in Table 2: (i) money on other accounts, (ii) balance with

[32]The quantile regression did not converge for the difference in the balance on the BCSA account during Phase 2 (column (1) in Table 13).

Table 12: Impact of Being Paid on the Account on Savings and Expenditures (Phase 1) - Without Control Variables

	BCSA balance					Total assets	
	Final	Average	Frequent consumption	Temptation goods	Cash at home	without BCSA	including BCSA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Impact on the conditional mean							
Paid on account	435.2***	269.9***	-400.5*	23.4	-208.1	389.5	839.3*
	(66.8)	(35.8)	(219.0)	(43.5)	(490.4)	(462.4)	(457.5)
R^2	0.07	0.05	0.02	0.00	0.00	0.01	0.01
Mean dependent (control)	378	299	3328	663	1614	2436	2821
Panel B: Impact on the conditional median							
Paid on account	425.0***	251.2***	-310.0*	-36.5	-100.0**	-110.0	461.6***
	(47.5)	(26.7)	(159.4)	(56.5)	(50.4)	(126.8)	(149.9)
Median dependent (control)	50	40	2661	470	300	990	1156
Controls	No	No	No	No	No	No	No
Observations	442	442	430	430	430	430	430

Panel A presents the impact on the conditional mean using ordinary least squares, and panel B on the conditional median using quantile regressions. In the columns (1) and (2) the dependent variables are different measures of the savings on the respondent's BCSA account; in column (3) and (4) it is the household's total expenditures on frequent consumption and temptation goods respectively; and in the columns (5)-(7), the respondent's financial assets, measured during the last weekly interview. We only control for the variables we stratified on (gender, and whether they had a bank account). All columns include village fixed effects. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Table 13: Impact of Being Paid on the Account on Savings and Expenditures (Phase 2) - Without Control Variables

	Change in balance BCSA	Frequent consumption	Temptation goods	Change in cash at home	Change in total assets	
					without BCSA	including BCSA
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Impact on the conditional mean						
Paid on account during Phase 1	6.6	5.0	-11.5	-157.0	-154.1	-193.9
	(63.9)	(63.7)	(14.7)	(318.6)	(329.6)	(313.9)
R^2	0.00	0.00	0.01	0.01	0.01	0.00
Mean dependent (control)	21	973	212	105	56	77
Panel B: Impact on the conditional median						
Paid on account during Phase 1		39.0	-7.0	50.0	-0.0	-40.6
		(73.9)	(17.3)	(63.2)	(56.2)	(71.5)
Median dependent (control)		805	170	0	-10	0
Controls	No	No	No	No	No	No
Observations	442	400	400	400	400	400

Panel A presents the impact on the conditional mean using ordinary least squares, and panel B on the conditional median using quantile regressions. The dependent variables are the difference in the respondent's balance on the BCSA account between the start and the end of Phase 2, the household's total expenditures on frequent consumption and temptation goods during Phase 2, and the change in financial assets between the start and the end of Phase 2. We only control for the variables we stratified on (gender, and whether they had a bank account). All columns include village fixed effects. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

an agricultural cooperative, (iii) balance on a post office account, and (iv) savings with self-help groups (SHGs) or other informal neighborhood groups.[33] In Table 2, we find that the treatment effect on total savings - measured as the sum over these four assets and cash at home - is not significant (column (6)). However, the treatment seems to have a positive effect on the mean balance with agricultural cooperatives. This is due to a small number of respondents who sold crops shortly before the last interview of Phase 1. If we exclude the top two values of balances with agricultural cooperatives for both the treated and control (0.9% in total), the treatment effect on balances with agricultural cooperatives is no longer significant, and the other results do not change substantially.[34] Therefore, we conclude there is no significant impact on financial assets.[35]

Balance Check of Outcome Variables and Restricted Samples

In Table 15 the first two columns provide a balance check for the respondents who joined the weekly interviews (Section 3.2), and the last two columns for the participants in the lab-in-the-field games (Section 4.2). The odd columns provide the sample mean and the standard deviation, and the even columns present the coefficient estimates (and standard errors) of the difference between the baseline means in the treatment and control groups. All the coefficient estimates are small and not significantly different from zero, suggesting that the treatment is orthogonal to observed baseline characteristics in the restricted samples as well.

Table 16 provides a balance check for the baseline value of the outcome variables that are presented throughout the paper. The only significant difference between the

[33]Only 5.5% of the control, and 7.5% of the treated have a positive balance with a post office. Therefore, the quantile regression did not converge.

[34]The results are available upon request.

[35]We provide details for financial assets only, as we consider it most likely that those would be influenced by our weekly payments. However, for completeness, we also tested the impact on other assets, namely on food grain, livestock, and jewelry. We also compared the control and treated's ownership of a long list of assets measured in the endline survey (for example electronics such as radio or television, cattle, bicycle, ...). There was no significant impact on any of those either.

Table 14: Treatment Effect on Other Consumption and Total Savings

	Non-frequent expenditures	Investments	Balance on other accounts	Balance with cooperative	Balance with post office	Savings with SHGs
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Impact on the conditional mean						
Paid on account	-303.5	321.5	39.1	352.8*	10.3	238.0
	(897.9)	(960.5)	(61.1)	(185.5)	(18.0)	(190.9)
R^2	0.10	0.11	0.07	0.14	0.07	0.20
Mean dependent (control)	5185	2789	171	193	42	416
Panel C: Impact on the conditional median						
Paid on account	-301.3	-47.1	-0.0	0.0		-0.0
	(327.6)	(152.0)	(0.1)	(9.1)		(0.0)
Median dependent (control)	2451	300	0	0		0
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	430	430	430	430	430	430

Panel A presents the impact on the conditional mean using ordinary least squares, and panel B on the conditional median using quantile regressions. The dependent variables are the household's total expenditures on non-frequent goods and investments, and the respondent's financial assets, measured during the last weekly interview of Phase 1. We include the same baseline characteristics as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

treated and control, is their balance with the post office. However, this is driven by a very small number of participants who have a positive balance (9 control, and 13 treated respondents). Therefore, we conclude that the sample is not only balanced for baseline characteristics, but also for outcome variables.

Table 15: Summary Statistics and Balance Check for Restricted Samples

	Weekly interviews		**Lab**	
	Mean (Std. dev.)	Coefficient on *Paid on account* (Std. errors)	Mean (Std. dev.)	Coefficient on *Paid on account* (Std. errors)
	(1)	(2)	(3)	(4)
Paid on account (%)	49.53		47.24	
	(50.06)		(49.99)	
New account (%)	46.98	-0.00	45.67	-0.02
	(49.97)	(0.05)	(49.88)	(0.05)
Woman (%)	50.23	0.00	51.44	0.00
	(50.06)	(0.05)	(50.04)	(0.05)
Caste category: ST (%)	12.79	-0.23	12.86	-0.16
	(33.44)	(3.23)	(33.52)	(3.44)
Caste category: SC (%)	11.86	-0.00	11.55	-0.01
	(32.37)	(0.03)	(32)	(0.03)
Caste category: OBC (%)	74.65	-0.00	74.80	0.00
	(43.55)	(0.04)	(43.47)	(0.04)
Caste category: FC (%)	0.70	0.00	0.79	0.01
	(8.33)	(0.01)	(8.85)	(0.01)
Literate (%)	47.44	-0.01	46.98	0.00
	(49.99)	(0.05)	(49.97)	(0.05)
Married (%)	88.14	0.01	87.40	-0.00
	(32.37)	(0.03)	(33.23)	(0.03)
Age	43.22	0.51	43.57	0.07
	(12.60)	(1.22)	(12.69)	(1.30)
Wage labor in agriculture (%)	29.77	0.01	29.40	0.01
	(45.78)	(0.04)	(45.62)	(0.05)
Wage labor outside agriculture (%)	13.26	0.02	14.17	0.04
	(33.95)	(0.03)	(34.92)	(0.04)
Self-employed in agriculture (%)	45.81	-0.01	44.36	-0.04
	(49.88)	(0.05)	(49.75)	(0.05)
Self-employed outside agriculture (%)	3.95	-0.00	4.20	-0.01
	(19.51)	(0.02)	(20.08)	(0.02)
Land (acres)	1.18	-0.05	1.19	-0.03
	(1.76)	(0.17)	(1.81)	(0.19)
Dwelling type: katcha (%)	52.56	0.00	52.49	0.01
	(49.99)	(0.05)	(50.00)	(0.05)
Accounts held (#)	1.17	0.00	1.17	-0.00
	(0.60)	(0.06)	(0.59)	(0.06)
Savings groups (#)	0.16	-0.01	0.17	-0.01
	(0.38)	(0.04)	(0.39)	(0.04)
Takes savings decision at home (%)	84.65	0.03	84.25	0.02
	(36.09)	(0.03)	(36.47)	(0.04)
Trusts the BCSA and banks (%)	73.26	0.02	72.70	0.01
	(44.31)	(0.04)	(44.61)	(0.05)
Impatient (%)	41.86	0.05	43.31	0.03
	(49.39)	(0.05)	(49.62)	(0.05)
Distance to the BCSA (km)	0.28	-0.02	0.28	-0.02
	(0.21)	(0.02)	(0.20)	(0.02)
Balance on BCSA account before start weekly surveys (Rs)	119.56	16.99	125.15	13.58
	(722.29)	(69.74)	(760.23)	(78.11)
Weeks interviewed (#)	10.00	-0.28	10.18	-0.33
	(2.65)	(0.26)	(2.52)	(0.26)
Observations	430	430	381	381

The first and third column report means (and standard deviations), and the second and fourth column show the coefficient estimates (and standard errors) of the difference between the means in the treatment and control groups. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent

Table 16: Summary Statistics and Balance Check of Outcome Variables at Baseline

	Mean (Std. dev.) (1)	Coefficient on *Paid on account* (Std. errors) (2)
Balance on BCSA account before start weekly surveys	119.6	17.0
	(722.3)	(69.7)
Expenditures on frequent consumption	451.2	-3.2
	(325.7)	(31.4)
Expenditures on Temptation goods	80.5	5.9
	(86.4)	(8.3)
Expenditures on non-frequent expenditures	366.5	-112.2
	(1016.4)	(98.0)
Expenditures on investments	760.4	-14.3
	(1902.9)	(183.8)
Cash at home	554.8	84.1
	(1432.7)	(138.3)
Balance on other accounts	92.8	4.4
	(522.0)	(50.4)
Balance with cooperatives	292.3	268.6
	(1804.9)	(173.8)
Balance with post office	19.5	35.6**
	(167.8)	(16.1)
Savings with SHGs	342.6	-4.4
	(1443.3)	(139.4)
Total financial assets (excluding BCSA savings)	1302.0	388.3
	(2717.0)	(261.7)
Total financial assets (including BCSA savings)	1421.6	405.3
	(2800.8)	(269.7)
Observations	430	430

The first column reports means (and standard deviations), and the second column shows the coefficient estimates (and standard errors) of the difference between the means in the treatment and control groups. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent

Appendix C: Pre-specified and exploratory analysis

We registered a pre-analysis plan with the American Economic Association. It has the identification number AEARCTR-0000387 and can be consulted on www.socialscienceregistry.org (Somville and Vandewalle, 2015). The main results come from the pre-planned analysis. Some tables provide additional analyses that we decided upon after the pre-analysis plan was registered, but that we believe the paper benefits from. This is for example the case for the long-term effect on the balances, or the comparison of the transactions made by the control and treated groups. In the pre-analysis plan we only discussed OLS estimators. However, we also show quantile regressions, as part of our robustness checks.

We make four important deviations from the initial pre-plan. First, we pre-specified that the standard errors would be clustered at the village level. Given the low number of clusters (17), there is the risk to artificially reduce the standard errors. This might indeed be the case, as the standard errors are smaller when they are clustered. We therefore decided to present nonparametric bootstrapped standard errors. The level of significance of the impact is not affected by this deviation from the initial plan, and the results are available upon request.

Second, the pre-analysis plan includes two extra outcome variables, namely the *positive balance*, which is the ratio between the number of days with a positive balance and the total number of days, from the day after the first till the day after the last weekly interview in the village; and *maximum balance*, which is the maximum balance that was recorded on the account. We graphically summarize the impact on *positive balance* in Figure 4. *Maximum balance* did not add much to the analysis, and we therefore did not include it in the paper. The results for both variables are available upon request.

Third, the pre-analysis plan includes the description of a third group of villagers:

those who do not have an account, and were not asked to open one. We do not include the analysis of that group, and the related outcome variables (Y2, Y3 and Y4) in this paper. We are writing a separate paper that specifically looks at Y2, Y3 and Y4 in the three groups of villagers that we sampled: (i) people without an account, (ii) people with a new account, (iii) people who already had an account.

Finally, we specified five characteristics for which we would test for heterogeneity in the treatment effects on savings. We provide the results in the different panels of the Tables 17 and 18. Equation 1 becomes:

$$Y_{ij} = \gamma_0 + \gamma_1 T_{ij} + \gamma_2 H_{ij} + \gamma_3 T_{ij} \times H_{ij} + \gamma_4 X_{ij} + V_j + \nu_{ij} \tag{2}$$

We run five separate regressions in which H_{ij} is a dummy variable taking value one if the respondent (i) opened a new account, (ii) is a women, (iii) is impatient, (iv) takes savings decisions in the household, and (v) trusts both the BCSA and banks.

Each panel first presents the coefficient and standard error of being treated, the dummy of interest and their interaction. Next, it shows the R^2.

The treatment effect is positive and significant in all the specifications. The interaction terms are never significant for both measures of the account balance, suggesting that there are no robust heterogeneous treatment effects. For gender and having opened an account - the two characteristics on which we stratified our sample - Table 17 suggests that the treatment has similar effects on men and women, and on old and new account holders. However, we could not stratify on the characteristics which are shown in Table 18: being impatient, taking savings decisions, and having trust in banks and the BCSA.

Table 17: Heterogenous Effects: Had to Open an Account, and Gender

	Final Balance (1)	Average Balance (2)
Panel A: Treatment effect for the respondents who opened an account		
Paid on account	416.4***	286.3***
	(133.4)	(54.3)
New account	-212.5	-181.3
	(219.4)	(172.0)
Paid on account x new account	8.7	-67.8
	(169.0)	(67.6)
R^2	0.10	0.09
Panel B: Treatment effect by gender		
Paid on account	357.4***	212.8***
	(88.1)	(55.3)
Woman	-1.4	-49.1
	(115.7)	(87.7)
Paid on account x woman	126.6	84.6
	(106.3)	(98.3)
R^2	0.10	0.09
Controls	Yes	Yes
Observations	442	442
Mean dependent (control)	378	299

Each panel presents the main results of testing for heterogeneity in the treatment effects of a different baseline characteristic. The dependent variables are the same as in the first two columns of Table 2. We include the same baseline characteristics as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parenthesis. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Table 18: Heterogenous Effects: Being Impatient, Takes Savings Decisions and Trusts the BCSA and Banks

	Final Balance (1)	Average Balance (2)
Panel C: Treatment effect by impatience		
Paid on account	397.8***	260.8***
	(113.6)	(59.1)
Impatient	50.7	15.6
	(70.8)	(64.1)
Paid on account x impatient	54.0	-14.2
	(119.9)	(69.2)
R^2	0.10	0.08
Panel D: Treatment effect for respondents who take savings decisions at home		
Paid on account	480.6***	322.0**
	(184.2)	(146.0)
Decides savings	188.0	183.1
	(169.4)	(170.4)
Paid on account x decides savings	-70.7	-78.9
	(236.6)	(185.2)
R^2	0.10	0.09
Panel E: Treatment effect for respondents who trust both the BCSA and banks		
Paid on account	371.3**	221.8***
	(147.6)	(84.4)
Trusts bank & BCSA	11.9	61.6
	(104.6)	(54.4)
Paid on account x Trusts bank & BCSA	67.3	45.3
	(123.2)	(90.0)
R^2	0.10	0.08
Controls	Yes	Yes
Observations	442	442
Mean dependent (control)	378	299

See Table 17 notes.