American Economic Journal: Applied Economics 2018, 10(3): 39–66
https://doi.org/10.1257/app.20160547

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

By Vincent Somville and Lore Vandewalle*

Access to banks is rapidly increasing worldwide, and allows account-based instead of cash transfers. We conduct a randomized experiment documenting the impact of the payment method on savings behavior. In India, we allocate identical weekly payments into a bank account (treated) or in cash (control). Savings in the account increase by 131 percent within 3 months, and the effect is long lasting. We also show that cash payments increase consumption and that—once everyone is paid in cash again—the savings patterns no longer differ. We interpret these findings as a default effect, and we further discuss plausible mechanisms. (*JEL* C93, D14, D90, G21, O12, O16)

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 into the recipient's bank account, instead of in cash.

We hypothesize that savings behavior will change once people receive income into 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 behavior. When people are paid into 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, to keep cash under the mattress, or to save in other assets. As a result, a change in the payment

* Somville: Chr. Michelsen Institute and NHH Norwegian School of Economics, Helleveien 30, 5045 Bergen, Norway (email: vincent.somville@nhh.no); Vandewalle: The Graduate Institute of International and Development Studies, Maison de la paix, Chemin Eugène-Rigot 2, 1202 Genève, Switzerland (email: lore.vandewalle@graduateinstitute.ch). We acknowledge the financial support from The Research Council of Norway through the NORGLOBAL-INDNOR project number 225871. We are grateful to Jean-Marie Baland, Stephan Klasen, Karl Ove Moene, and Bertil Tungodden for helpful discussions and suggestions on the design of the project, and Rohini Somanathan for support in India. We thank the editor, two anonymous referees, Anirban Mitra, Pascaline Dupas, Jessica Goldberg, and Diego Ubfal for comments that helped improve the paper, and seminar participants at the ASWEDE conference, Barcelona GSE Summer Forum, Bocconi University, the Chr. Michelsen Institute, Delhi School of Economics, DIW in Berlin, the EEA conference, the EUDN conference, Georg-August-Universität Göttingen, the Graduate Institute, the 6th GREThA International Conference, Heriot-Watt, the Indian Statistical Institute, IPA, KU Leuven, the Norwegian School of Economics, Portsmouth Business School, the RES conference, the SEEDEC conference, SSES Annual Congress, and the Universities of Alicante, Geneva, and Namur. We are also grateful to Rahul Mehrotra, Sanjay Prasad, and Julia Seiermann for excellent research assistance and the Basix Sub-K's staff for their continued support and collaboration.

[†] Go to https://doi.org/10.1257/app.20160547 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

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 east-central 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 17 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 ten 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 into 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 meters. 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. 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 into the account instead of in cash increases the account balance by around 131 percent (or Rs 463) 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 into 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 subsamples. 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 in 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, to put aside money at home, or to save in other tools. 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 a lack of self-control, and—to a certain extent— by transaction costs, i.e., the (minimal) time and effort it takes to do a transaction. We argue that the impacts are not due to a higher trust and better relationship with the banker (measured by playing incentivized lab in the field games), experimenter demand effects, or 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 toward 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; Choi, Laibson, and Madrian 2004; Carroll et al. 2009). Our work 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. Furthermore, 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. Finally, we would like to emphasize two studies that are conducted in Malawi and related to ours. First, Brune et al. (2016b) study the impact of a one-time payment that was allocated into a bank account or in cash. Contrary to our study, the payment is large and unanticipated. Furthermore, the bank is not located at the doorstep, but at an average distance of 3.7 kilometers. The authors find that a one-time payment has limited impacts on savings and consumption. This is consistent with the pattern we observe. Indeed, we find that the impact of the payment

method becomes significant only after several weeks of repeated payments. Second, Brune et al. (2016a) study the impact of offering different types of bank accounts, combined with the possibility of having cash crop harvest proceeds being deposited into the account. The treatment leads to higher savings and an increase in the use of agricultural inputs. In contrast with our set-up, not every farmer has an account, and it remains at the farmer's discretion whether or not he will be paid into the account. In addition, our data allows measuring the impact on consumption and testing underlying mechanisms.

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 jewelry, 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); second, papers measuring the effect of offering different types of accounts (e.g., Ashraf, Karlan, and Yin 2006, 2010; Ashraf et al. 2015; Brune et al. 2016a; John 2015); and finally papers focusing on the impact of providing additional banking services along with a bank account (e.g., Karlan et al. 2016; de Mel, McIntosh, and Woodruff 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, Ratan, and Zinman (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.[1] In India, the debate about providing access to formal banking for all, and the move toward 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, Niehaus, and Sukhtankar 2016), we draw attention to the potential impact of account-based payments on the recipient's finances.

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

[1] One famous example is Brazil, where almost 20 percent of the beneficiaries of the Bolsa Familia program receive their transfers into a bank account (numbers obtained from the *Ministerio do Desenvolvimento Social* in January 2015).

I. 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.

A. *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 (Bhaskar 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 (Bhaskar 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 application 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 finalized, the customer can perform standard transactions: 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 realize their temporary existence.[2]

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

On August 15, 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 into 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.

B. *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 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 1 in online 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 be the head of the household or the head's spouse, not plan to leave the village, and belong to a household in which nobody has a savings account with another institution.[3]

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 until 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 weeks.[4] We compensated the villagers because they had to leave their house to be

[3] 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.

[4] 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.

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.[5] We randomized the way this weekly compensation was paid at the individual level. Half the respondents received Rs 150 directly into their account (treated), while the other half received it in cash (control). The intervention and randomization are summarized in Figure 2 of the online Appendix A.

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

We will refer to the first part of the experiment—during which the treated respondents were paid into 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 into the account. Phase 2 was not announced and could not have been anticipated by the respondents.

C. *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 into 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 all the 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

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

automatically shown, as to make sure that the enumerator would not forget to update the necessary information. During the pilot study, we realized 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 prespecified. The online Appendix C categorizes our results depending on whether they were foreseen in our pre-analysis plan or not.

D. *Attrition*

Shortly after the baseline survey, one shopkeeper 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.

E. *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.[6] 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 opened a new one. This implies that 46 percent of the sample received a new account. In terms of

[6] Table 5 in online Appendix B shows that the outcome variables are balanced at baseline as well.

Table 1—Summary Statistics and Balance Check of Baseline Characteristics

	Mean (standard deviation) (1)	Coefficient on *paid into account* (standard error) (2)
Paid into account (percent)	50.00 (50.06)	
New account (percent)	46.15 (49.91)	−0.00 (0.05)
Woman (percent)	49.77 (50.06)	0.00 (0.05)
Caste category: ST (percent)	12.90 (33.55)	0.00 (0.03)
Caste category: SC (percent)	11.76 (32.26)	−0.01 (0.03)
Caste category: OBC (percent)	74.66 (43.54)	0.00 (0.04)
Caste category: FC (percent)	0.68 (8.22)	0.00 (0.01)
Literate (percent)	48.19 (50.02)	0.00 (0.05)
Married (percent)	88.24 (32.26)	0.01 (0.03)
Age	43.00 (12.61)	0.43 (1.20)
Wage labor in agriculture (percent)	29.19 (45.51)	0.00 (0.04)
Wage labor outside agriculture (percent)	13.80 (34.53)	0.01 (0.03)
Self-employed in agriculture (percent)	45.48 (49.85)	−0.01 (0.05)
Self-employed outside agriculture (percent)	4.07 (19.79)	−0.01 (0.02)
Land (acres)	1.17 (1.74)	−0.05 (0.17)
Dwelling type: katcha (percent)	52.49 (49.99)	0.01 (0.05)
Accounts held (#)	1.17 (0.60)	0.00 (0.06)
Savings groups (#)	0.16 (0.38)	0.00 (0.04)
Takes savings decision at home (percent)	84.84 (35.90)	0.02 (0.03)
Trusts the BCSA and banks (percent)	73.30 (44.29)	0.03 (0.04)
Impatient (percent)	42.08 (49.42)	0.04 (0.05)
Distance to the BCSA (km)	0.29 (0.22)	−0.03 (0.02)
Balance in BCSA account before start weekly surveys (Rs)	109.39 (645.49)	−21.41 (61.47)
Weeks interviewed (#)	9.73 (3.05)	−0.44 (0.29)
Observations	442	442

Notes: 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.

demographic characteristics, respondents are mainly Other Backward Castes (OBC)[7], and less than half of them are literate. A great majority are 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 percent 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 IB). 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.[8] In terms of time preferences, 42 percent of the participants are impatient, i.e., they prefer money today instead of a larger amount in one week. The average distance from the house to the BCSA is about 290 meters as a crow flies. The last two variables in Table 1 are not included in the regressions, but provide some important information: the money in the BCSA account was balanced the day 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.

II. Impact of the Payment Method

When people are paid into 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 into the account can withdraw. In Section IIA, we first graphically asses whether the payment method makes a difference before estimating the treatment effect on savings and expenditures in Section IIB. The results confirm our hypothesis: the villagers paid into 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 into 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

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

[8] 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.

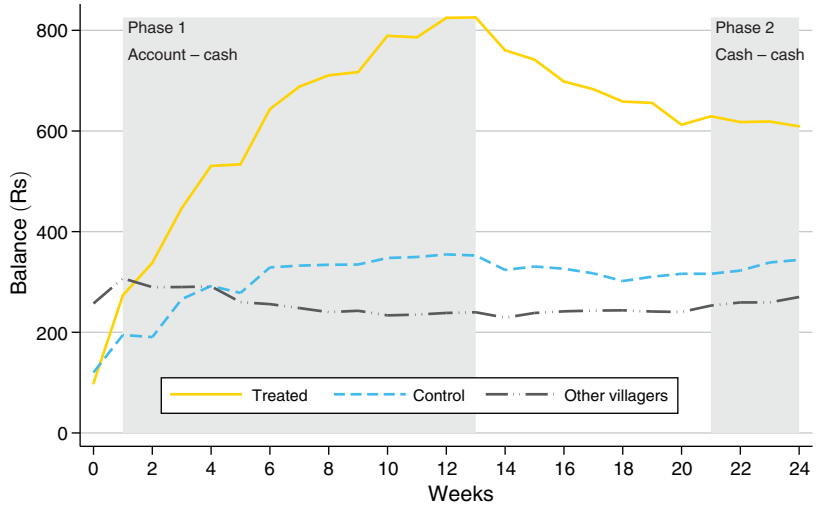

FIGURE 1. EVOLUTION OF THE ACCOUNT SAVINGS OF THE TREATED AND CONTROL

a difference in bank account savings and consumption patterns between the treated and control. Finally, in Section IIC, we show that the respondents transact actively. However, they do not empty 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.

A. *A Graphical Assessment of the Treatment Impact*

We provide summary statistics on both phases of the experiment using a graphical representation. In Figure 1, the horizontal axis shows the number of weeks since the start of the experiment and the vertical axis shows the balance in 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.[9] Overall, the balance of the treated first-order stochastically dominates the balance of the control.[10] 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 in the BCSA account and in the mean cumulative expenditures on frequent goods.[12] Figure 2 presents a connected line plot for both phases.

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

[10] Figure 3 in online Appendix B provides the equivalent for consumption.

[11] We obtained data on the transactions of the other customers in the 17 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 IIB.

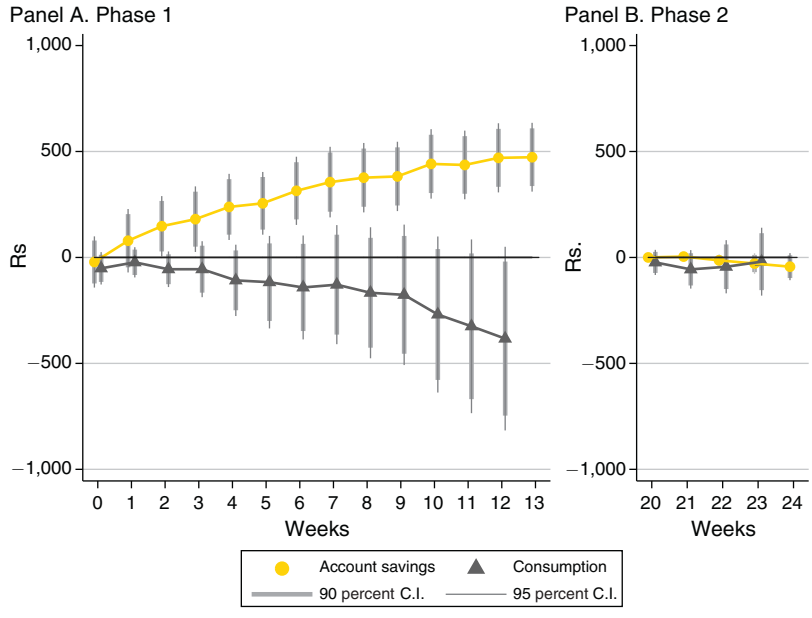

FIGURE 2. DIFFERENCES BETWEEN THE TREATED AND CONTROL IN SAVINGS AND CONSUMPTION

The horizontal axis still shows the number of weeks since the start of the experiment, while the vertical axis now represents the difference in savings and expenditures between the treated and control.[13] Note that we normalized the savings and consumption during Phase 2 as to start at zero again.

The graph confirms our hypothesis: during Phase 1, transfers given in cash are more likely to be spent, while payments into 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 in the BCSA account, expenditures, and total savings using a regression framework.

B. *Impact on Savings and Expenditures*

We estimate the impact of being paid into 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 into the account during Phase 1; and X_{ij} is a vector of baseline characteristics, which includes all but the last two variables

[13] Phase 1 consisted of 13 weekly interviews in the villages where we started first (see Section IB). Therefore, we present the effect on the balance up to week 13. However, we have the effect on consumption util week 12 only, as the information on week 13 would have been obtained during interview 14. Similarly, for Phase 2 we have information on consumption until week 23 only. This was gathered during the last weekly interview of Phase 2, which took place in week 24.

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

The Tables 2 and 3 provide the main results for Phase 1 and Phase 2, respectively. As explained in Section IB, the control respondents were paid in cash, and the treated into 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 Tables 1 and 2 in online Appendix B.[16]

Table 2 provides the impact on savings in 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: (i) the *final balance* is the respondent's balance the day after we conducted the last weekly interview in the village; and (ii) the *average balance* is the average account balance from the day after the first until 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 eight respondents never showed up for an interview, and four respondents came for one only, the sample reduces from 442 to 430 observations. The sample remains balanced though, as can be seen from the first two columns of Table 4 in online Appendix B. Table 5 in online 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 columns 3 and 4, while the impact on investment and durables is presented in Table 3 in online Appendix B. Frequent consumption is the 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

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

[15] Respondents are treated at the end of each interview only. Therefore, we can only measure the treatment impact for respondents who were interviewed at least twice. Eight respondents never came for an interview, and four respondents came only once. Some villagers had to skip an interview due to other obligations, but overall the attendance was high. The average respondent was interviewed ten times, and the number of interviews is orthogonal to the treatment (see Table 1).

[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 into the account to withdraw, and villagers paid in cash to deposit. Otherwise, the difference between treated and control could be artificially inflated.

[18] This includes (we provide the average frequency of expenditures on the goods at baseline in brackets): grains and cereals (0.48), pulses and lentils (0.73), milk products and edible oil (0.73), vegetables and fruit (1.0), sugar, salt, and spices (0.93), fuels and light (0.86), soap (0.83), and washing powder (0.84).

Table 2—Treatment Effect on Savings and Expenditures (*Phase 1*)

	BCSA balance					Total assets	
	Final (1)	Average (2)	Frequent consumption (3)	Temptation goods (4)	Cash at home (5)	Without BCSA (6)	Including BCSA (7)
Panel A. Impact on the conditional mean							
Paid into account	462.7	306.6	−386.8	22.9	−161.1	479.0	959.5
	(94.1)	(67.8)	(210.5)	(46.5)	(447.7)	(444.8)	(438.2)
R^2	0.13	0.09	0.15	0.11	0.13	0.13	0.13
Mean dependent (control)	353	287	3,328	663	1,614	2,436	2,795
Panel B. Impact on the conditional median							
Paid into account	473.1	364.8	−318.2	−27.4	−67.3	−61.8	457.0
	(50.0)	(30.9)	(169.8)	(56.1)	(55.7)	(130.0)	(150.0)
Median dependent (control)	50	43	2,661	470	300	990	1,150
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	442	442	430	430	430	430	430

Notes: Panel A presents the impact on the conditional mean using ordinary least squares, and panel B on the conditional median using quantile regressions. In columns 1 and 2, the dependent variables are different measures of the savings in 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 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 gender, 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 opened a new account, takes savings decisions in the household, trusts both the BCSA and banks, and is impatient.

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 in other accounts, (iii) balance with an agricultural cooperative, (iv) balance in 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 in 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 3 in online Appendix B provides results for the other financial assets.

Being paid into the account has significant positive effects on the different measures of savings in the account. Compared to the control mean, the effects are extremely large: the final balance increases by 131 percent and the average balance by 107 percent. The impact is also important in real terms: it is equivalent to a household's average weekly frequent expenditures (Rs 451, see Table 5, online Appendix B) and amounts to 60 percent of an average household's weekly income prior to the start of our experiment (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 in the

online Appendix B, which pictures the distribution of the final balances. It shows graphically that the treated respondents are much less likely to have a zero balance, and that their mean and median balances are higher.

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 84 percent of 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 473 less in their account.

Finally, the control keep 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 in the BCSA account.

In summary, these results confirm our hypothesis: respondents who are paid into 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 into 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 into 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 in the BCSA account between the day after we finished Phase 2 and the day before we started it. In columns 2 and 3, the outcome variables are the respondents' total expenditures on frequent consumption and temptation goods, and in columns 4 to 6, the change in cash at home and in the total financial assets (excluding and including the savings in the BCSA account).[19]

Being paid into 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 in 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. Remarkably, the difference in 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 into the account, i.e., when the payment method was different.

C. *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 3.19 transactions and a total of 316 respondents (71.5 percent) 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.

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

Table 3—Treatment Effect on Savings and Expenditures (*Phase 2*)

	Change in balance BCSA (1)	Frequent consumption (2)	Temptation goods (3)	Change in cash at home (4)	Change in total assets	
					Without BCSA (5)	Including BCSA (6)
Panel A. Impact on the conditional mean						
Paid into account during Phase 1	−49.8	15.2	−14.4	−177.5	−172.4	−225.9
	(32.7)	(67.8)	(12.8)	(311.8)	(319.5)	(303.5)
R^2	0.06	0.13	0.14	0.09	0.08	0.08
Mean dependent (control)	35	973	212	105	55	93
Panel B. Impact on the conditional median						
Paid into account during Phase 1	−0.0	−26.4	−1.7	21.4	27.0	−113.7
	(1.0)	(66.1)	(20.7)	(72.3)	(84.9)	(92.8)
Median dependent (control)	0	805	170	0	−10	0
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	442	400	400	400	400	400

Notes: 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 in 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 parentheses.

The figures do not include our payments (we subtracted those before taking averages).

Respondents paid in cash deposited more often than the villagers paid into the account (2.57 times as compared to 0.77 times). In total, 204 respondents deposited at least once: 129 from the control and 75 from the treated group. The average amount per deposit is Rs 156.3 for the control and Rs 220.4 for the treated.

On the other hand, treated villagers withdraw more often than the control (2.69 times as compared to 0.35 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 account balance. Indeed, during the weeks in which they were paid into the account, 79.2 percent of the treated respondents withdrew at least once, while only 3.6 percent had a zero balance at the end of those weeks.

Second, as can be seen from panel B, the treated and control use their accounts in a similar manner when they are all paid in cash. Although 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 into the account, 54.8 percent withdrew at least once, while only 15.2 percent had less than Rs 5 in 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 in 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 131 percent on average. From Table 5, we learn that the final balance

Table 4—Transactions

	Total number			Average amount		
	Full sample (SD) [Observations]	Paid cash (SD) [Observations]	Paid into account (SD) [Observations]	Full sample (SD) [Observations]	Paid cash (SD) [Observations]	Paid into account (SD) [Observations]
Panel A. Phase 1 (10 weeks)						
Deposits	1.67	2.57	0.77	179.9	156.3	220.4
	(3.05)	(3.81)	(1.56)	(425.4)	(358.0)	(521.7)
	[442]	[221]	[221]	[204]	[129]	[75]
Withdrawals	1.52	0.35	2.69	456.0	477.0	451.4
	(2.57)	(0.96)	(3.09)	(468.6)	(552.4)	(450.1)
	[442]	[221]	[221]	[213]	[38]	[175]
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	587.4	615.3	575.8
	(0.47)	(0.49)	(0.44)	(498.7)	(634.2)	(440.4)
	[442]	[221]	[221]	[51]	[15]	[36]

Table 5—Treatment Effect in the Longer Run

	After 15 weeks	After 19 weeks	After 23 weeks
	(1)	(2)	(3)
Panel A. Impact on the conditional mean			
Paid into account	312.2	307.9	227.7
	(70.1)	(77.3)	(78.9)
R^2	0.10	0.10	0.05
Mean dependent (control)	296.0	279.4	276.1
Panel B. Impact on the conditional median			
Paid into account	267.3	259.0	170.1
	(45.2)	(45.2)	(38.9)
Median dependent (control)	50.0	50.0	50.0
Controls	Yes	Yes	Yes
Observations	442	442	442

Notes: 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 in 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 parentheses.

of the treated is (more than) twice the one of the control after 15 and 19 weeks, and still 82 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 in the account up to (at least) 23 weeks after our last account-based payment.[20]

D. *Heterogeneous Effects*

Our pre-analysis plan specifies five baseline characteristics for which we would test for heterogeneity in treatment effects: 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. Therefore, we estimate five separate regressions, which take the following form:

$$(2) \qquad Y_{ij} = \gamma_0 + \gamma_1 T_{ij} + \gamma_2 BC_{ij} + \gamma_3 T_{ij} \times BC_{ij} + \gamma_4 X_{ij} + V_j + \nu_{ij},$$

where Y_{ij} is the balance in respondent i's account the day after we conducted the last weekly interview of Phase 1 in village j, and BC_{ij} is the baseline characteristic. Each column of Table 6 shows the result for a different characteristic.

The treatment effect is positive and significant in all the specifications. The interaction terms are never significant, suggesting there are no robust heterogeneous treatment effects. For gender and having opened an account—the two characteristics on which we stratified our sample—the results suggest that the treatment has similar effects on men and women, and on old and new account holders. However, we could not stratify on the other three characteristics: being impatient, taking savings decisions, and having trust in banks and the BCSA, and are therefore more careful with the interpretation.

III. A Discussion of the Mechanisms

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

Choice 1: Rs 150 into their account or Rs 150 in cash.
Choice 2: Rs 125 into their account or Rs 150 in cash.
Choice 3: Rs 150 into their account or Rs 125 in cash.
Choice 4: Rs 150 into their account or Rs 175 in cash.
Choice 5: Rs 175 into 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 7 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 be deposited into 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 into the

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

Table 6—Heterogeneous Effects: Had to Open an Account, Gender, Being Impatient, Takes Savings Decisions, and Trusts the BCSA and Banks

	Impact on final balance of the following baseline characteristics:				
	Opened new account	Woman	Impatient	Decides savings	Trusts bank and BCSA
	(1)	(2)	(3)	(4)	(5)
Paid into account	426.8	427.9	427.5	555.5	550.7
	(146.4)	(77.9)	(126.1)	(165.3)	(101.6)
Baseline characteristic	−119.1	71.7	30.2	104.3	108.3
	(141.2)	(80.2)	(61.9)	(111.2)	(69.7)
Paid into account ×	77.6	69.9	84.0	−109.0	−120.6
Baseline characteristic	(158.6)	(115.1)	(114.6)	(208.7)	(104.2)
R^2	0.13	0.13	0.13	0.13	0.13
Mean dependent (control)	353	353	353	353	353
Controls	Yes	Yes	Yes	Yes	Yes
Observations	442	442	442	442	442

Notes: Each column presents the heterogeneous effects for a different baseline characteristic: the respondent (1) opened an account, (2) is a woman, (3) is impatient, (4) takes savings decisions at home, and (5) trusts both the BCSA and banks. The dependent variable is the respondent's balance the day after we conducted the last weekly interview of Phase 1. We include the same controls as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parentheses.

Table 7—Treatment Effect on Payment Choices

	Choice 1	Choice 2	Choice 3	Choice 4	Choice 5
	(1)	(2)	(3)	(4)	(5)
Paid into 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	0.07	0.06	0.09
Mean dependent (control)	0.12	0.04	0.64	0.06	0.59
Controls	Yes	Yes	Yes	Yes	Yes
Observations	424	424	424	424	424

Notes: The dependent variables are dummies that equal one if the respondent prefers to receive (1) Rs 150 into his account instead of Rs 150 in cash, (2) Rs 125 into his account instead of Rs 150 in cash, (3) Rs 150 into his account instead of Rs 125 in cash, (4) Rs 150 into his account instead of Rs 175 in cash, and (5) Rs 175 into 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 parentheses.

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 in 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: (i) have a higher trust and a better relationship with the banker if the treatment pushed them to interact more with him; (ii) be subject to experimenter demand effects and believe they are expected to leave the money in the account; (iii) face a lower pressure to share their Rs 150 with the kith and kin; (iv)

have to spend less time and effort to save in the account (because the control face the additional hurdle of depositing themselves); and (v) suffer less from self-control problems.[21]

We discuss each factor in turn and draw the following conclusions: the treatment impact can be explained by a lack of self-control and, to a certain extent, by the time and effort it takes to transact. It is not due to a higher trust and a better relationship with the banker, experimenter demand effects, and redistributive pressures.

A. *Trust in the BCSA*

Recent evidence emphasizes the importance of trust in explaining formal bank account savings (Coupé 2011; Dupas et al. 2012; Iyer and Puri 2012; Sapienza and Zingales 2012; Bachas et al. 2017; Mehrotra, Somville, and Vandewalle 2016). The share of villagers who did at least one transaction is almost 40 percent higher in the group who was paid into the account. Therefore, the treated are more likely to interact with the BCSA, which might affect their trust in or empathy toward the banker, and, hence, their willingness to keep a higher balance in 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 4 in online 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 $(0; 10; 20; \ldots ; 3X)$. 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.[22]

[21] A sixth factor is mental accounting, which has recently been emphasized as an explanation for higher account savings (Thaler 1999; Dupas and Robinson 2013b). If transfers into the bank account are mentally allocated to a specific use, it may be easier to control withdrawals for other purposes. As a result, we expect the treatment impact to be larger for participants who labeled their account. It is important to note that reverse causality is possible as well: being paid into the account increases the balance, which in turn may inspire the creation of savings purposes and labeling of the account. In our data, we observe a simultaneous increase in the balance, and in the share of respondents who save for a particular purpose, but we cannot establish a causal relation.

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

Table 8—Treatment Effect on Trust and Empathy

	Trust game (1)	Dictator game (2)
Paid into 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

Notes: 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 parentheses.

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.

Being paid into 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 toward the local banker.[23]

B. *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 IB). 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 IIC shows that both the treated and control transact during Phase 1. The average respondent made 3.62 transactions during those 10 weeks, and a total of 323 respondents (73.1 percent) 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 in the account. Panel

[23] A complete description of this laboratory experiment is given in Mehrotra, Somville, and Vandewalle (2016).

B of Table 4 shows that there were slightly more deposits than withdrawals during those four weeks: they made, on average, 0.66 deposits and 0.15 withdrawals.

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 into the account, 54.8 percent withdrew at least once, while only 15.2 percent had less than Rs 5 in 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 in the account.[24]

C. *Redistributive Pressures from the Household and from Others*

Compared to savings in 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 if it makes them sufficiently illiquid. Redistributive pressures have been emphasized as important barriers to savings in other contexts (Baland, Guirkinger, and Mali 2011; di Falco and Bulte 2011; Boltz, Marazyan, and Villar 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, 98.4 percent 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 (i) the spouse (for married women only), or (ii) any household member. The negative sign suggests that respondents paid into the account are subject to fewer requests, but the coefficient is very small and not statistically significant. Conditional on having been asked for money, the average respondent gives Rs 66 to the spouse ($N = 67$) and Rs 71 to the spouse or any other household member ($N = 130$).

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 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.

This is in line with our baseline information on limitations to saving: only 2.3 percent of the respondents answered affirmative to our question on whether they have difficulties in saving because relatives or friends make a claim on it. To increase variation, we construct an indicator of "pressure to share savings" that takes value one if at least one of the following conditions is satisfied: (i) the respondent declares having difficulties in saving because relatives or friends make a claim on it, and (ii) the household gave or received at least one transfer in the six months preceding the

[24] We compared the declared balance from the last weekly interview with the real account balance, and cannot reject the equality of the means (the difference is an insignificant Rs 6 only).

Table 9—Treatment Effect on Loans and Transfers

	Asked money by			
	Spouse	Any hh member	Loans	Transfers
	(1)	(2)	(3)	(4)
Paid into account	−0.05	−0.09	471.4	27.3
	(0.05)	(0.06)	(1,319.7)	(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	373	424	430	430

Notes: 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 parentheses.

baseline survey. The indicator takes value 1 for 32.4 percent of the respondents. Its heterogeneous impact is given in the first column of Table 10, and confirms that pressure for sharing with others is not a major barrier to savings in our context.

In conclusion, the data show that the respondents paid into 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.

D. *Time and Effort to Transact*

There are limited monetary costs to depositing and withdrawing (see Section IA), but it takes some time and effort to go to the banker and to make a transaction.[25] 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 meters 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, the second column of Table 10 shows that the treatment impact on savings does not depend on whether the distance to the banker is above or below median.[26]

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 consequences (see for instance Johnson et al. 1993; O'Donoghue and Rabin 1999a; Park, Jun,

[25] The respondents rarely face problems once they reached the BCSA. Indeed, only 5.4 percent of the 424 respondents who were interviewed at the endline reported that the BCSA was not available, or did not have enough cash at some moment during the previous month.

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

Table 10—Heterogeneous Effects: Pressure to Share Savings, Distance to the Banker, and Self-Control Status

	Impact on final balance of the following baseline characteristics:		
	Pressure to share savings	Distance to BCSA above median	Sufficient self-control
	(1)	(2)	(3)
Paid into account	469.0	446.6	491.8
	(109.6)	(133.3)	(100.5)
Baseline characteristic	43.5	−100.2	−54.4
	(103.4)	(122.8)	(108.9)
Paid into account × *baseline characteristic*	−22.7	35.6	−238.3
	(135.8)	(162.2)	(144.6)
R^2	0.13	0.13	0.13
Mean dependent (control)	353	353	353
Controls	Yes	Yes	Yes
Observations	442	442	442

Notes: Each column presents the heterogeneous effects for a different baseline characteristic: (1) "Pressure to share savings" is an index that equals one if the respondent reports "*having difficulties in saving because relatives or friends make a claim on it*," or if the household gave or received at least one transfer in the six months preceding the baseline survey; (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; and (3) "Sufficient self-control" equals one if the respondent does not report "*having difficulties in saving because it is hard to control spending.*" The dependent *v*ariable is the respondent's balance the day after we conducted the last weekly interview of Phase 1. We include the same controls as in Table 2, and village fixed effects. Bootstrapped standard errors are given in parentheses.

and MacInnis 2000; Madrian and Shea 2001; Choi et al. 2002; Johnson, Bellman, and Lohse 2002; Johnson and Goldstein 2003; Choi, Laibson, and Madrian 2004; Abadie and Gay 2006; Kressel and Chapman 2007; Carroll et al. 2009). Therefore, we cannot completely rule out this channel.

E. *Lack of Self-Control*

The control increase expenses on frequent consumption, while the treated have more savings at hand for consumption smoothing and medical expenses in the future.[27] The respondents put forward that a lack of self-control provides a possible explanation for this pattern.[28]

First, during the baseline survey, 89 percent of the respondents declare "*having difficulties in saving because it is hard to control spending*." This implies only 11 percent of them report having sufficient self-control. The third column of Table 10 shows the treatment effect is driven by respondents who report having self-control problems. Indeed, being paid into the account increases the final balance with Rs 492 for respondents who lack self-control and Rs 254 for respondents with sufficient

[27] We asked details about the declared purpose of each respondent's largest withdrawal. We obtained the amount from the administrative data, and asked about its use during the endline survey. *Emergencies* (consumption smoothing and medical expenses) is the most important declared purpose (45.7 percent of the withdrawals), followed by marriages (13.8 percent) and businesses (9.3 percent).

[28] See Karlan, Ratan, and Zinman (2014) for a review of the research on self-control and savings in low-income countries.

self-control. This test is analogous to the one carried out by Ashraf et al. (2015) and suggests that the treatment helps dealing with self-control problems.[29]

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.

Third, 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 into 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*" (70.7 percent). The respondents who prefer being paid into the account do so because it "*allows to save*, *while cash is spent too easily*" (96.5 percent).

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

IV. 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. 2016) and commitment savings accounts (Ashraf, Karlan, and Yin 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 is automatic transfers to 401(k) savings plans. In developing economies, where most economic transactions are settled in cash, direct transfers into a bank account could serve the same purpose without the need for a commitment component. We tested this hypothesis in rural India. We compared the savings in 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 into the bank account (treated). We find that being paid into the account increases savings by about 131 percent, or Rs 463 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.

[29] 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 percent of our sample—save less in 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 were 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.

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 a lack of self-control and, to a certain extent, to the (minimal) time and effort it takes to do a transaction. We argue that they are not due to a higher trust and a better relationship with the banker, experimenter demand effects, or redistributive pressures.

The marginal savings rate is relatively high in our experiment. People received Rs 1,500 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 into an account instead of in cash.

REFERENCES

Abadie, Alberto, and Sebastien Gay. 2006. "The impact of presumed consent legislation on cadaveric organ donation: A cross-country study." *Journal of Health Economics* 25 (4): 599–620.

Akerlof, George A. 1991. "Procrastination and Obedience." *American Economic Review* 81 (2): 1–19.

Ashraf, Nava, Diego Aycinena, Claudia Martínez A., and Dean Yang. 2015. "Savings in Transnational Households: A Field Experiment among Migrants from El Salvador." *Review of Economics and Statistics* 97 (2): 332–51.

Ashraf, Nava, Dean Karlan, and Wesley Yin. 2006. "Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines." *Quarterly Journal of Economics* 121 (2): 635–72.

Ashraf, Nava, Dean Karlan, and Wesley Yin. 2010. "Female Empowerment: Impact of a Commitment Savings Product in the Philippines." *World Development* 38 (3): 333–44.

Bachas, Pierre, Paul Gertler, Sean Higgins, and Enrique Seira. 2017. "Banking on Trust: How Debit Cards Enable the Poor to Save More." National Bureau of Economic Research (NBER) Working Paper 23252.

Baland, Jean-Marie, Catherine Guirkinger, and Charlotte Mali. 2011. "Pretending to Be Poor: Borrowing to Escape Forced Solidarity in Cameroon." *Economic Development and Cultural Change* 60 (1): 1–16.

Banerjee, Abhijit, and Sendhil Mullainathan. 2010. "The Shape of Temptation: Implications for the Economic Lives of the Poor." National Bureau of Economic Research (NBER) Working Paper 15973.

Bernheim, B. Douglas. 1997. "Rethinking Saving Incentives." In *Fiscal Policy: Lessons from Economic Research,* edited by Alan J. Auerbach, 259–311. Cambridge: MIT Press.

Bhaskar, P. Vijaya. 2006. *Financial Inclusion by Extension of Banking Services—Use of Business Facilitators and Correspondents*. Reserve Bank of India. Mumbai, January.

Bhaskar, P. Vijaya. 2008. *Financial Inclusion by Extension of Banking Services—Use of Business Correspondents* (*BCs*). Reserve Bank of India. Mumbai, August.

Boltz, Marie, Karine Marazyan, and Paola Villar. 2015. "Income Hiding and Informal Redistribution: A Lab-in-the-Field Experiment in Senegal." https://www.parisschoolofeconomics.eu/IMG/pdf/jobmarket-paper-boltz-pse.pdf.

Brune, Lasse, Xavier Giné, Jessica Goldberg, and Dean Yang. 2016a. "Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi." *Economic Development and Cultural Change* 64 (2): 187–220.

Brune, Lasse, Xavier Giné, Jessica Goldberg, and Dean Yang. 2016b. "Savings Defaults and Payment Delays for Cash Transfers: Field Experimental Evidence from Malawi." World Bank Policy Research Working Paper 7807.

Callen, Michael, Suresh De Mel, Craig McIntosh, and Christopher Woodruff. 2014. "What are the Headwaters of Formal Savings? Experimental Evidence from Sri Lanka." National Bureau of Economic Research (NBER) Working Paper 20736.

Carroll, Gabriel D., James J. Choi, David Laibson, Brigitte C. Madrian, and Andrew Metrick. 2009. "Optimal Defaults and Active Decisions." *Quarterly Journal of Economics* 124 (4): 1639–74.

Choi, James J., David Laibson, and Brigitte C. Madrian. 2004. "Plan Design and 401(k) Savings Outcomes." *National Tax Journal* 57 (2): 275–98.

Choi, James J., David Laibson, Brigitte C. Madrian, and Andrew Metrick. 2002. "Defined Contribution Pensions: Plan Rules, Participant Choices, and the Path of Least Resistance." *Tax Policy and the Economy* 16: 67–113.

Coupé, Tom. 2011. "Mattresses versus Banks—The Effect of Trust on Portfolio Composition." Kyiv School of Economics Discussion Paper 40.

Cronqvist, Henrik, and Stephan Siegel. 2015. "The Origins of Savings Behavior." *Journal of Political Economy* 123 (1): 123–69.

de Mel, Suresh, Craig McIntosh, and Christopher Woodruff. 2013. "Deposit Collecting: Unbundling the Role of Frequency, Salience, and Habit Formation in Generating Savings." *American Economic Review* 103 (3): 387–92.

Demirguc-Kunt, Asli, and Leora Klapper. 2012. "Measuring Financial Inclusion: The Global Findex Database." World Bank Policy Research Working Paper 6025.

di Falco, Salvatore, and Erwin Bulte. 2011. "A Dark Side of Social Capital? Kinship, Consumption, and Savings." *Journal of Development Studies* 47 (8): 1128–51.

Dupas, Pascaline, Sarah Green, Anthony Keats, and Jonathan Robinson. 2012. "Challenges in Banking the Rural Poor: Evidence from Kenya's Western Province." National Bureau of Economic Research (NBER) Working Paper 17851.

Dupas, Pascaline, and Jonathan Robinson. 2013a. "Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya." *American Economic Journal: Applied Economics* 5 (1): 163–92.

Dupas, Pascaline, and Jonathan Robinson. 2013b. "Why Don't the Poor Save More? Evidence from Health Savings Experiments." *American Economic Review* 103 (4): 1138–71.

Iyer, Rajkamal, and Manju Puri. 2012. "Understanding Bank Runs: The Importance of Depositor-Bank Relationships and Networks." *American Economic Review* 102 (4): 1414–45.

Jakiela, Pamela, and Owen Ozier. 2016. "Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies." *Review of Economic Studies* 83 (1): 231–68.

John, Anett. 2015. "When Commitment Fails—Evidence from a Regular Saver Product in the Philippines." Economic Organisation and Public Policy (EOPP) Discussion Paper 55.

Johnson, Eric J., Steven Bellman, and Gerald L. Lohse. 2002. "Defaults, Framing and Privacy: Why Opting In-Opting Out?" *Marketing Letters* 13 (1): 5–15.

Johnson, Eric J., and Daniel Goldstein. 2003. "Do Defaults Save Lives?" *Science* 302 (5649): 1338–39.

Johnson, Eric J., John Hershey, Jacqueline Meszaros, and Howard Kunreuther. 1993. "Framing, probability distortions, and insurance decisions." *Journal of Risk and Uncertainty* 7 (1): 35–51.

Karlan, Dean, Margaret McConnell, Sendhil Mullainathan, and Jonathan Zinman. 2016. "Getting to the Top of Mind: How Reminders Increase Saving." *Management Science* 62 (12): 3393–3411.

Karlan, Dean, Aishwarya Lakshmi Ratan, and Jonathan Zinman. 2014. "Savings by and for the Poor: A Research Review and Agenda." *Review of Income and Wealth* 60 (1): 36–78.

Kast, Felipe, and Dina Pomeranz. 2014. "Saving More to Borrow Less: Experimental Evidence from Access to Formal Savings Accounts in Chile." National Bureau of Economic Research (NBER) Working Paper 20239.

Kressel, Laura M., and Gretchen B. Chapman. 2007. "The Default Effect in End-of-Life Medical Treatment Preferences." *Medical Decision Making* 27 (3): 299–310.

Laibson, David I., Andrea Repetto, Jeremy Tobacman, Robert E. Hall, William G. Gale, and George A. Akerlof. 1998. "Self-Control and Saving for Retirement." *Brookings Papers on Economic Activity* 28 (1): 91–172.

Lusardi, Annamaria. 1999. "Information, Expectations, and Savings for Retirement." In *Behavioral Dimensions of Retirement Economics,* edited by Henry J. Aaron, 81–115. Washington, DC: Brookings Institution Press.

Madrian, Brigitte C., and Dennis F. Shea. 2001. "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior." *Quarterly Journal of Economics* 116 (4): 1149–87.

Mehrotra, Rahul, Vincent Somville, and Lore Vandewalle. 2016. "Increasing Trust in the Bank to Enhance Savings: Experimental Evidence from India." Chr. Michelsen Institute (CMI) Working Paper 2016:02.

Muralidharan, Karthik, Paul Niehaus, and Sandip Sukhtankar. 2016. "Building State Capacity: Evidence from Biometric Smartcards in India." *American Economic Review* 106 (10): 2895–2929.

O'Donoghue, Ted, and Matthew Rabin. 1999a. "Doing It Now or Later." *American Economic Review* 89 (1): 103–24.

O'Donoghue, Ted, and Matthew Rabin. 1999b. "Incentives for Procrastinators." *Quarterly Journal of Economics* 114 (3): 769–816.

Park, C. Wan, Sung Youl Jun, Deborah J. MacInnis. 2000. "Choosing What I Want Versus Rejecting What I Do Not Want: An Application of Decision Framing to Product Option Choice Decisions." *Journal of Marketing Research* 37 (2): 187–202.

Prina, Silvia. 2015. "Banking the poor via savings accounts: Evidence from a field experiment." *Journal of Development Economics* 115: 16–31.

Samuelson, William, and Richard Zeckhauser. 1988. "Status quo bias in decision making." *Journal of Risk and Uncertainty* 1 (1): 7–59.

Sapienza, Paola, and Luigi Zingales. 2012. "A Trust Crisis." *International Review of Finance* 12 (2): 123–31.

Shefrin, Hersh M., and Richard H. Thaler. 1988. "The Behavioral Life-Cycle Hypothesis." *Economic Inquiry* 26 (4): 609–43.

Somville, Vincent, and Lore Vandewalle. 2015. "Saving, by default—Status quo bias in financial inclusion programs." https://www.socialscienceregistry.org/trials/387/history/6306.

Somville, Vincent, and Lore Vandewalle. 2018. "Saving by Default: Evidence from a Field Experiment in Rural India: Dataset." *American Economic Journal: Applied Economics.* https://doi.org/10.1257/app.20160547.

Thaler, Richard H. 1994. "Psychology and Savings Policies." *American Economic Review* 84 (2): 186–92.

Thaler, Richard H. 1999. "Mental accounting matters." *Journal of Behavioral Decision Making* 12 (3): 183–206.

Thaler, Richard H., and Shlomo Benartzi. 2004. "Save More Tomorrow: Using Behavioral Economics to Increase Employee Saving." *Journal of Political Economy* 112 (S1): S164–87.

Thaler, Richard H., and H. M. Shefrin. 1981. "An Economic Theory of Self-Control." *Journal of Political Economy* 89 (2): 392–406.

This article has been cited by:

1. Abhijit Banerjee, Martínez A. Claudia, Esteban Puentes. 2025. Better strategies for saving more: Evidence from three interventions in Chile. *Journal of Development Economics* **173**, 103405. [Crossref]
2. Fernanda Marquez-Padilla, Susan W. Parker. 2024. The Leaky Bucket: Transition Costs from Cash to Deposits in a Mexican Transfer Program. *Economic Development and Cultural Change* **73**:1, 37-59. [Crossref]
3. Emma Riley. 2024. Resisting Social Pressure in the Household Using Mobile Money: Experimental Evidence on Microenterprise Investment in Uganda. *American Economic Review* **114**:5, 1415-1447. [Abstract] [View PDF article] [PDF with links]
4. Diego Ubfal. What Works in Supporting Women-Led Businesses? **3**, . [Crossref]
5. Mariya Davydenko, Johanna Peetz. 2024. Does Timing of Self-Control Strategies Matter? How Focusing on Proactive versus Reactive Strategies Affects Monthly Spending. *Basic and Applied Social Psychology* **46**:2, 56-67. [Crossref]
6. Federico A. Bugni, Mengsi Gao. 2023. Inference under covariate-adaptive randomization with imperfect compliance. *Journal of Econometrics* **237**:1, 105497. [Crossref]
7. Syedah Ahmad, Robert Lensink, Annika Mueller. 2023. Uptake, use, and impact of Islamic savings: Evidence from a field experiment in Pakistan. *Journal of Development Economics* **163**, 103098. [Crossref]
8. Hideki Shimada, Tomonori Honda. 2022. What drives households' choices of residential solar photovoltaic capacity?. *Energy Policy* **168**, 113120. [Crossref]
9. Alexander Coutts. 2022. Identifying communication spillovers in lab-in-the-field experiments. *Journal of Development Economics* **157**, 102845. [Crossref]
10. Sayantan Ghosal, Smarajit Jana, Anandi Mani, Sandip Mitra, Sanchari Roy. 2022. Sex Workers, Stigma, and Self-Image: Evidence from Kolkata Brothels. *The Review of Economics and Statistics* **104**:3, 431-448. [Crossref]
11. Johanna Peetz, Mariya Davydenko. 2022. Financial self-regulation. *Social and Personality Psychology Compass* **16**:5. . [Crossref]
12. Paul Christian, Steven Glover, Florence Kondylis, Valerie Mueller, Matteo Ruzzante, Astrid Zwager. 2022. Do private consultants promote savings and investments in rural Mozambique?. *Agricultural Economics* **53**:1, 22-36. [Crossref]
13. Lisa Spantig. 2021. Cash in hand and savings decisions. *Journal of Economic Behavior & Organization* **188**, 1206-1220. [Crossref]
14. PIERRE BACHAS, PAUL GERTLER, SEAN HIGGINS, ENRIQUE SEIRA. 2021. How Debit Cards Enable the Poor to Save More. *The Journal of Finance* **76**:4, 1913-1957. [Crossref]
15. Mariya Davydenko, Marta Kolbuszewska, Johanna Peetz. 2021. A meta-analysis of financial self-control strategies: Comparing empirical findings with online media and lay person perspectives on what helps individuals curb spending and start saving. *PLOS ONE* **16**:7, e0253938. [Crossref]
16. Lasse Brune, Eric Chyn, Jason Kerwin. 2021. Pay Me Later: Savings Constraints and the Demand for Deferred Payments. *American Economic Review* **111**:7, 2179-2212. [Abstract] [View PDF article] [PDF with links]
17. Ute Rink, Yabibal M. Walle, Stephan Klasen. 2021. The financial literacy gender gap and the role of culture. *The Quarterly Review of Economics and Finance* **80**, 117-134. [Crossref]

18. Rahul Mehrotra, Vincent Somville, Lore Vandewalle. 2021. Increasing Trust in Bankers to Enhance Savings: Experimental Evidence from India. *Economic Development and Cultural Change* **69**:2, 623-644. [Crossref]
19. Emily Breza, Martin Kanz, Leora Klapper. Learning to Navigate a New Financial Technology: Evidence from Payroll Accounts **108**, . [Crossref]
20. Jean-Michel Servet, Bruno Tinel. 2020. The behavioral and neoliberal foundations of randomizations. *Strategic Change* **29**:3, 293-299. [Crossref]
21. Michael A. Clemens, Timothy N. Ogden. 2020. Migration and household finances: How a different framing can improve thinking about migration. *Development Policy Review* **38**:1, 3-27. [Crossref]
22. Lorenzo Casaburi, Rocco Macchiavello. 2019. Demand and Supply of Infrequent Payments as a Commitment Device: Evidence from Kenya. *American Economic Review* **109**:2, 523-555. [Abstract] [View PDF article] [PDF with links]
23. Joshua Blumenstock, Michael Callen, Tarek Ghani. 2018. Why Do Defaults Affect Behavior? Experimental Evidence from Afghanistan. *American Economic Review* **108**:10, 2868-2901. [Abstract] [View PDF article] [PDF with links]
24. Nirvikar Singh. 2018. Financial Inclusion: Concepts, Issues and Policies for India. *SSRN Electronic Journal* **95**. . [Crossref]
25. Nirvikar Singh. 2017. Financial Inclusion: Concepts, Issues and Policies for India. *SSRN Electronic Journal* **121**. . [Crossref]
26. Dean S. Karlan, Pascaline Dupas, Jonathan Robinson, Diego Ubfal. 2016. Banking the Unbanked? Evidence from Three Countries. *SSRN Electronic Journal* **121**. . [Crossref]