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Access to banking, savings and consumption smoothing in rural India ☆

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ABSTRACT

To what extent does access to banking help poor households to save and smooth consumption? To answer this fundamental question, we combine a field experiment that randomly provides access to a bank account with weekly interviews on household finances. Access to banking does not change average consumption, but it improves consumption smoothing by alleviating savings constraints. Indeed, the control's expenditures follow income more closely than the expenditures of the treated. The latter handle variations in income by engaging in pro-cyclical saving in their account. These results provide an important new insight into the role of banking in low- and middle-income countries.

1. Introduction

How much households consume and save, and whether they can smooth consumption over time, are two fundamental questions. Access to formal banking is increasing in many countries and can shed new light on these questions, by creating the opportunity to study consumption decisions when saving is facilitated. We do this in India, by combining a randomized control trial that helps people open bank accounts, and detailed information from weekly interviews. The randomized trial enables the identification of causal effects on average savings, consumption and earnings. The weekly surveys record changes in savings and consumption patterns within a household over time, allowing us to assess changes in consumption smoothing.

The study sample is drawn from villages of Chhattisgarh. As a result of India's financial inclusion policies, a formal bank recently started operating in each of those villages. The bank operates close to the villagers' houses (300 m on average), which greatly improves the accessibility of formal banking and reduces transaction costs. We randomly selected villagers who had not yet opened an account and provided help to open an account to half of them. Next, we organized a practical information session for the treated households. We showed them how to deposit and withdraw money, and demonstrated how a fingerprint recognition tool protects their savings. Once they were familiar with the features and security of their account, we started the weekly interviews. In total, we organized seventeen interviews per respondent between February and May, and July and August 2014. The interviews took place on the same day of the week at a centrally located room in the village.

The first important observation is that treated households actively used the new account. As the treatment did not impact the flow and stock of other financial assets, the household's total stock of liquid savings increased by about INR 290.[1,2] We do not find an effect, on average, on other household level outcomes such

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[1] This is substantial. At baseline, the median weekly food expenditures equal INR 320 and the weekly median income is INR 429.

[2] When we extend the definition of savings and include the stock of grain, livestock and jewelry, the impact remains large, but the estimates are less precise and not statistically significant.

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as expenditures, loans, transfers and income. These results contribute to the literature on the promotion of formal banking among low income populations, by focusing on a new setting. In recent years, the potential benefits of financial inclusion have received a lot of attention from several governments and the international community. Concomitantly, there have been a number of projects that investigate the topic: Dupas et al. (2018) provide an excellent review and Steinert et al. (2018) conduct a meta-analysis. Remarkably, existing papers focus on the average impact of financial inclusion, but give limited attention to its role in consumption smoothing.

The absence of an impact on average expenditures does not preclude an effect on *when* the participants make those expenditures. In the second part of the paper, we use the detailed weekly interviews to test whether the new savings tool translates into improved consumption smoothing —do people spend their incomes immediately, or does access to a savings account allow them to dissociate the timing of income and of expenditures? The approach we follow was introduced by Cochrane (1991), Mace (1991), Townsend (1994), Hayashi et al. (1996) and has recently been adopted by e.g. Kinnan and Townsend (2012), Alem and Townsend (2014) and Meghir et al. (2022). Starting from the classical lifecycle model of consumption and savings, we derive an Euler equation that allows us to estimate the impact of an exogenous change in access to banking on consumption smoothing. If markets are complete and households aim to equalize their marginal utility of consumption over time, expenditures should not correlate with weekly variations in income. The results are promising. For control households, the expenditures (and the caloric value of their food expenditures) vary with income, leading us to reject the hypothesis of complete markets and indicating the presence of liquidity constraints. The expenditures of treated households (and their caloric intake), however, do not respond significantly to variations in income.

We perform two additional analyses to substantiate our interpretation that the savings accounts reduced liquidity constraints. First, following the strategy introduced by Zeldes (1989), we show that savings constraints can explain the observed *excess sensitivity* of expenditures to income variations among the control and that access to banking alleviates those constraints. Indeed, income variations positively correlate with expenditures for control households that are savings constrained at baseline, but not for the treated or for unconstrained control households. Second, we investigate how treated and control households handle variations in income. The treated households engage in pro-cyclical saving in the account (they deposit more when income spikes), while the control households adjust the flow of their transfers. The latter approach, however, appears insufficient for control households to perfectly smooth consumption.

Our paper makes two important contributions to the literature on consumption smoothing. First, we study consumption choices through the lens of the lifecycle model in a low-income setting. This approach has been followed extensively in high-income countries with well-developed credit and savings markets (see Fuchs-Schündeln and Hassan (2016) for a review), but there is limited evidence from lower-income countries.

Second, the degree to which people smooth consumption and the strategies they use received a lot of attention in low-income settings with incomplete markets.[3] Only a small number of papers, however, study the role of banking in mitigating risk and facilitating consumption smoothing. Those papers have two limitations. First, they only focus on the credit component of access to banking.[4] Along with Pomeranz and Kast (2022) and Agarwal et al. (2017), we are among the first to study the role of savings.[5] This is important, in particular given the recent literature emphasizing that savings constraints are as important for the poor as credit constraints (Bachas et al., 2021; Brune et al., 2021; Dupas and Robinson, 2013; Karlan et al., 2014). Second, there is no exogenous change in access to credit markets in these papers. Instead, they are descriptive or rely on panel estimations with household fixed effects to control for time-invariant observables (which we also do) and on the distance to financial services, assuming this is exogenous. While we do not contest the importance and value of the insights in these papers, the estimates may suffer from an endogeneity bias.

In conclusion, the main contribution of our paper is the randomized access to bank accounts as a source of variation to identify how access to a new savings tool alleviates savings constraints and improves consumption smoothing. As such, it bridges the gap between the randomized trials that focus on the average impacts of access to banking but provide limited insights on its role in consumption smoothing, and papers that focus on consumption smoothing but lack exogenous variation in access to financial services through formal institutions, and in particular its savings component. To the best of our knowledge, the only similar work is Pomeranz and Kast (2022), who find that providing access to a bank account improves consumption smoothing in Chile. While we collected precisely estimated information on expenditures through weekly surveys, they ask participants during the endline survey whether they had to cut back consumption on a series of specific items in the preceding three months. Our approach allows measuring the impact at the intensive margin, including on important variables, such as the intake of calories.

The paper is organized as follows. In Section 2 we provide more details on India's financial inclusion plan, our experimental design, the data, attrition and spillover effects. In Section 3, we study the impact of being offered help to open a bank account on savings, expenditures, loans, transfers and income at the household level. We investigate consumption smoothing in Section 4 and conclude in Section 5. We report additional results and robustness tests in the online appendix.

2. Background, experimental design and data

In this section, we discuss India's financial inclusion program and then we describe the experimental design of our study, introduce the data, provide baseline characteristics and discuss attrition and spillover effects.

2.1. Financial inclusion in India

The financial landscape has changed markedly in India. In 2006 the Reserve Bank introduced the Business Correspondents model, which led to a rapid increase in bank account penetration. Between 2011 and 2014, the share of banked adults increased from 35 to 53 percent (Demirgüç-Kunt et al., 2015). The model allows banks to appoint Business Correspondents (BCs), who provide financial and banking services on their behalf (RBI, 2006; RBI, 2008). In the region of our survey, Axis bank appointed the financial inclu-

[3] Some of the seminal references are Rosenzweig and Stark, 1989; Deaton, 1991; Deaton, 1992; Paxson, 1992; Paxson, 1993; Rosenzweig and Wolpin, 1993; Townsend, 1994; Udry, 1994; Townsend, 1995; Kochar, 1995; Morduch, 1995; Morduch, 1999; Dercon, 2002; Dercon, 2005, more recent evidence includes Merfeld and Morduch, 2022.

[4] For example, Islam and Maitra (2012) document how microcredit in Bangladesh facilitates consumption smoothing. Kinnan and Townsend (2012) and Gertler et al. (2009) provide comparable evidence for formal and informal credit transactions in Thailand and Indonesia, respectively. Jack and Suri (2014) show how the development of mobile banking improves insurance between people by reducing the cost of transfers in Kenya.

[5] Beaman et al. (2014) focus on savings as well, but through informal tools. They show that (randomized) access to savings groups improves consumption smoothing during the lean season in Mali.

sion company Basix Sub-K, which is our main partner on the project. Basix Sub-K's responsibilities are selecting one person per village to become the Business Correspondent Sub-Agent (BCSA) or bank agent, training the person, and providing equipment: a mobile phone, a fingerprint recognition device and a receipt machine that are all interconnected via bluetooth. Basix Sub-K also pays the bank agent, assists wherever needed and provides customer service for the clients.

The bank agent opens the villagers' *BCSA account*. To do so, he sends the customer's application form and a photo to Axis bank. The bank opens the account and communicates the unique account number to the bank agent. The account is then activated by registering the customer's fingerprints. Once this procedure is finalized, the customer can perform standard transactions on the account: deposits, withdrawals, money transfers, and balance inquiries. Transactions that reduce the account balance or provide information about the balance require a signature through the fingerprint recognition device. The customer has to pay an enrollment fee of INR 25, but transactions are free.[6]

In August 2014 —after we finalized our data collection— the government announced the National Mission for Financial Inclusion (PMJDY). This led to an additional boost in bank account penetration.[7]

2.2. Experimental design

The experiment was conducted in 17 villages in rural Chhattisgarh, an east-central state of India. We selected villages according to two criteria. First, we excluded villages with a cooperative, rural or commercial bank branch, to make sure the bank agent is the only person offering formal banking services at the doorstep. Second, we needed a cluster of villages that are sufficiently close to one another, as the survey team had to travel between them on a weekly basis. The sampled villages are located in three different districts, pictured in Fig. A1 in the appendix.

In each village, we randomly selected 12 villagers who did not yet have a BCSA account. We allocated a random number to each person on the voter list and approached them in ascending order. Apart from not yet having opened a BCSA account, villagers had to meet three additional conditions for inclusion in our sample: (i) being the head of the household or the head's spouse, (ii) not having plans to leave the village, and (iii) belonging to a household in which nobody had a savings account with another institution. We allowed post office or other accounts that were opened to receive payments from welfare schemes, or Mahatma Ghandi National Rural Employment Guarantee Act (MGNREGA). We also allowed cooperative bank accounts that were used to receive income from the sale of crops. Villagers cannot deposit into post office or cooperative bank accounts and rarely do so into the other accounts, either because they are not protected (there is no secret code or biometric authentication to protect savings), or because the bank is too far away. Indeed, during the 17 weeks of the experiment, only 17 deposits were reported, or 0.006 percent of the total number of observations. Villagers usually withdraw the money at once shortly after a payment is made.

To obtain a sample that is stratified by gender, we approached people until we had selected six men and six women in each village. Three men and three women were randomly allocated to the treatment group, and the others to the control group.

We conducted a baseline survey at the respondent's home in the fall of 2013. Shortly after the interview, Basix Sub-K started the paperwork to open a BCSA account for each of the treated respondents. All of the accounts were activated by the spring of 2014. To make sure the respondents understood how to use their BCSA accounts, we organized a practical information session. We showed the treated respondents how to deposit and withdraw money, and demonstrated how the fingerprint recognition tool protects their savings.[8] This means that the intervention is twofold: Providing help to open a savings account available at the doorstep (by filling out the documents and taking a photo) and organizing training in how to use the account. These interventions remove important barriers for the population under study. Indeed, while the control could have opened an account, only two people did so during the course of the study.

Between February and May, and July and August 2014, we organized seventeen weekly interviews, which took place on the same day of the week at a centrally located room in the village. On average, the respondents needed about three hours to travel, wait their turn, be interviewed and go back home. To compensate them for their time we paid INR 150, delivered in a closed envelope at the end of each interview.

2.3. Data and pre-analysis plan

We use four sources of data. First, the baseline survey included questions on the characteristics of the participants and their household members, as well as on the household's expenditures, investments in businesses and agriculture, transfers, loans, and informal savings.

Second, Basix Sub-K gave access to all the transactions that were made during the survey period by all villagers in the study area.

Third, inspired by Collins et al. (2009) and Dupas and Robinson (2013) we conducted weekly interviews. These yielded detailed information on the incomes and expenditures of all the household members. The income sections covered wage labor, self-employment, the sale of goods, livestock, crops and forest products and renting out of assets and land. In addition to a list of 195 consumption items for which we recorded the amounts purchased, the expenditure details included expenditure on business and agricultural inputs, and the rent of assets. We also collected details on loans, transfers and remittances. To gather this weekly information, we created a "dynamic" questionnaire that compares the answer with the previously recorded value for all financial variables. In practice, the respondent provided the total amount of deposits and withdrawals over the past week (or the amount of reimbursements received or given for a certain loan or credit) and the value of the current balance. In the background, the program calculated the current balance, using the balance in the previous week and the recorded transactions. If there was a mismatch, the reviewer was prompted to go over the questions again. Details about account ownerships, memberships of savings groups, outstanding loans, etc. were automatically shown, to assure the enumerator would update the necessary information. We believe this process greatly improved the quality of the data and minimized measurement errors, which is particularly important to measure consumption smoothing.

Finally, we conducted an endline survey, where we gathered details about the respondent's relationship to the other partici-

[6] The bank experimented with (very low) charges on withdrawals after the start of our experiment. Withdrawals remained free if the average quarterly balance (AQB) in the account was above INR 500, but customers were charged INR 1 per withdrawal if their AQB was between INR 200 and INR 500 and INR 2 per withdrawal if their AQB was less than INR 200. These charges were abandoned on July 1, 2014. From the endline survey we learned that customers did not realise the existence of temporary charges. We only found out about them shortly before they were abandoned.

[7] Details are available on the PMJDY website: http://pmjdy.gov.in.

[8] The session focused on the use of the technology only, there was no discussion of the importance of savings, or any other aspect related to financial literacy.

pants in the study. The survey questionnaires are available on github.

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). The main deviation from the pre-analysis plan is the analysis on consumption smoothing. We discuss this in more detail in Appendix D.

2.4. Baseline characteristics and balance check

Our pre-analysis plan defines the baseline characteristics to test balance. Table 1 presents the characteristics at the level of the household (which is the level of analysis in our paper) and appendix Table D1 at the level of the respondent. The sample includes 204 households. The first column provides the means (and standard deviations) in the full sample and the second column the coefficient estimates (and standard errors) of the difference between the baseline means in the treatment and control group. All the coefficient estimates are small and only one is significantly different from zero at the 90 percent level of confidence.[9] This suggests that the randomization was successful at making the treatment orthogonal to observed baseline characteristics. The appendix Tables A1 and A2 show that the outcome variables are balanced at baseline as well. All of the 28 coefficient estimates are small and only one is significantly different from zero at a significance level of 0.1. According to the F-test of overall significance (regressing the treatment indicator on all the variables in the balance tables), we do not reject the hypothesis that the coefficients are jointly equal to zero and that the treatment arms are balanced (p-value = 0.85).

As we stratified the sample on gender, 50 percent of the respondents are women. In terms of demographic characteristics, households are mainly Other Backward Classes.[10] The sample is quite poor: Households own about one acre of land and 55 percent live in a house that is made of mud (katcha). Finally, the average distance from the house to the bank agent is about 300 meters as the crow flies. The last variable in the table is not measured at baseline but shows that, on average, we have 13 weeks of information per household. This is balanced as well between treated and control.

Table 1
Summary statistics and balance check of baseline household characteristics.

	Mean (Std. dev.) (1)	Coefficient on *Offered account* (Std. errors) (2)
Offered account	0.50	
	(0.50)	
Woman	0.50	0.00
	(0.50)	(0.07)
Caste category: ST	0.16	0.03
	(0.37)	(0.05)
Caste category: SC	0.16	0.00
	(0.36)	(0.05)
Caste category: OBC/FC	0.68	−0.03
	(0.47)	(0.07)
Land (acres)	1.10	0.28
	(1.59)	(0.22)
Dwelling type: katcha	0.55	−0.07
	(0.50)	(0.07)
Distance to the BCSA (km)	0.31	0.05*
	(0.21)	(0.03)
Weeks interviewed (#)	13.16	0.56
	(3.68)	(0.52)
Observations	204	204

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

2.5. Attrition and spillover effects

Before presenting the results, it is important to discuss the extent of attrition and the likelihood of spillover effects. We intended to work in 18 different villages. However, shortly after the baseline survey, one bank agent stopped his banking activities because they were not as profitable as his other business. Because there is only one bank agent per village, we had to exclude the village from our experiment. As the bank agent's decision was unrelated to our study, the attrition is orthogonal to the experimental treatment assignment.

Of the 204 respondents in our study, only three never attended the weekly interviews. As shown in Table 1, the average person was interviewed 13.2 times and there is no statistically significant difference between the treated and control households. Furthermore, appendix Table A3 shows we cannot predict well the number of weekly interviews a respondent attended based on observables: the R-squared is only 0.14. The final sample consists of 2685 interviews from 201 households over 17 weeks.

The risk of spillover effects should be limited given that we sampled very few villagers per village, but they met while attending the weekly interviews and may thus have interacted. We, therefore, look further into this. At endline, we asked respondents about their relationship to all the other participants who reside in the same village. This gives us network data with 2134 links.[11] On average, they know 68% of the other respondents. In the six months preceding the interview, they visited the house of 24% of them, and 23% of the other participants visited their house. In the same period, transfers (and loans) were given to or received from about 3.5% (and 3.6%) of the other respondents. In conclusion, while participants know each other, they engage in few transactions. In addition, only two control households opened a bank account. Therefore, there is limited evidence that spillover effects matter in this context.

3. Savings and downstream outcomes

We first estimate the impact of being offered help to open a bank account on savings and downstream outcomes. To do so, we use information from the weekly interviews and from the administrative data. Our main specification is a pooled panel model on a sample of 2685 interviews from 201 households over 17 weeks:[12]

$$Y_{ikt} = \alpha_0 + \alpha_1 T_{ik} + \alpha_2 F_{ik} + \alpha_3 Y_{ik0} + V_k + W_t + \epsilon_{ikt} \tag{1}$$

Y_{ikt} is the hyperbolic sine transformation of the outcome variable of interest for household i in village k measured during the interview of week t, T_{ik} is a dummy indicating the household is treated and F_{ik} indicates that the respondent is a woman (the variable on which we stratified the sample). For stocks, we also control for the baseline value of the outcome, Y_{ik0}. Finally, V_k and W_t are village and time fixed effects, and ϵ_{ikt} is the error term.[13] The standard errors are clustered at the household level.

[9] Appendix B.3 shows the results are robust to the inclusion of this baseline characteristic.

[10] Castes are classified in the following categories: ST (Scheduled Tribe), SC (Scheduled Caste), OBC (Other Backward Classes), and FC (Forward Caste). We put the latter two categories together as there are few forward castes in our sample.

[11] We were able to interview 195 respondents at endline. We asked them about the 11 other respondents in their village.

[12] The weekly interviews were delayed in some villages to facilitate a close follow-up of the enumerators in the first couple of weeks. As a result, we did 17 interviews in 11 villages, 16 interviews in two villages, 13 in three villages, and 11 in the final one.

[13] As there is only one banker per village, the village fixed effects also absorb all banker fixed effects.

To control the *false discovery rate*, we correct the p-values to account for multiple hypotheses testing using the procedure proposed by Benjamini and Hochberg (1995). In each table, we group the outcomes together in one family (the only exception is Table 9, where we specify the families in the table notes). In case we test more than one independent variable, we group the p-values per family of outcomes and per independent variable.

We apply the inverse hyperbolic sine transformation (IHST) to all the outcome variables.[14] Bellemare and Wichman (2020) provide a practical guidance on the interpretation of results. If the untransformed mean of the dependent variable is roughly greater than 10, the impact of a dummy variable can be interpreted as in a standard log transformation.

Table 2 presents the impact of being offered help to open a bank account on savings at the household level (for completeness, Appendix C presents the results at the level of the respondent).[15,16] For each asset, we present the impact on a dummy indicating a household member made a transaction during the seven days that precede the interview (panel A), on the flow (deposits minus withdrawals, panel B) and on the stock (the balance, panel C).

We aggregated the household's savings tools into five categories: (i) the BCSA account, (ii) self-help groups (SHGs) and money guarded by others, (iii) post offices, agricultural cooperatives and other accounts, (iv) cash at home and (v) the total stock of jewelry, grain and livestock.[17] For the last two categories, we only have information about the balance, not about the weekly flows.[18] In column (5), we provide the total for the most liquid tools (the first four categories), and in column (7) for all categories together.

We expect the participants to use "liquid savings" rather than "illiquid assets" to smooth consumption. Illiquid assets are mainly used as longer-term savings tools or to absorb larger shocks. For instance, jewelry is given at weddings or as part of a dowry, or stored as inheritance for the next generation (see e.g., Anukriti et al., 2022; Bhalotra et al., 2020, among others) and livestock is sold in response to important shocks (Islam and Maitra, 2012). Our data corroborates the different usage of illiquid versus liquid savings tools: Only four percent of the sample reports a change in illiquid savings during the study period, while 91 percent reports a change in liquid savings.

Treated households have a higher transaction frequency on the BCSA account, and their flow of savings is 114 ($e^{0.76} - 1$) percent higher than the control.[19] As the coefficients are systematically close to zero for other financial assets, there is a positive impact on the flow and on the balance of total liquid financial savings (column 5).[20] If we extend the definition of savings and include other assets such as livestock, grains and jewelry, the increase is not sufficient to significantly impact total savings (column 7 in panel C).[21]

Next, we examine the impact on the flows of expenditures, transfers and revenues. Table 3 shows a precisely estimated zero impact on the hyperbolic sine transformation of the weekly expenditures on (1) frequent purchases, (2) temptation goods, (3) non-frequent products, (4) investments, (5) the total over these goods and (6–8) food. Apart from the additional "food" categories, the classification is the same as in Somville and Vandewalle (2018): Frequent purchases is the sum of expenditures on goods that are bought frequently by the average household, and temptation goods are products that are not *survival necessities* (Banerjee and Mullainathan, 2010).[22] "Food" is the sum of expenditures over all the items that are edible, independent of whether these are bought frequently, infrequently or can be classified as temptation goods (such as snacks from the market). We further split up food into items that have to be consumed within a short period of time (perishable), and items that can be put aside (non-perishable).[23]

Next, Table 4 shows that the net inflow of loans and transfers and the total household income do not differ between the treated and control households. The net inflow of loans is the hyperbolic sine transformation of the total amount borrowed minus the total amount lent, plus the net amount of reimbursements received. Similarly, the net inflow of private transfers is the hyperbolic sine transformation of the total amount received, minus the total amount given. Public transfers are transfers received by the government. The final variable —total income— sums the revenues from seven different sources: wage labor, self-employment, the sale of goods, livestock, crops and forest products and renting out of assets and land.

Finally, Table 5 provides additional details about the seven different sources of income. Column (1) shows the control group's mean revenue, column (3) the proportion of observations with a positive amount in the control group, and the columns (2) and (4) the impact of being offered help to open a bank account on the mean income and on the proportion of positive amounts respectively (coefficient α_1 in Eq. 1). The table reveals two important facts. First, there is no significant impact on any of the income sources at the household level. Second, wage employment is the most important and most regular source of income. Indeed, the average household receives revenues from wage employment in 87 percent of the weeks. The second most important source of income is the sale of agricultural products, which provides revenues in 7.8 percent of the weeks only. These results are important for the analysis in Section 4.

4. Consumption smoothing

The previous section shows that offering help to open a bank account increases total liquid savings at the household level. On

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14 The inverse hyperbolic sine transformation for x is $\ln[x + \sqrt{x^2+1}]$. As explained in Ravallion (2017), it approximates a log transformation, but is also defined for non-positive values. Appendix B.4 shows the results are robust to the use of a log transformation with a small increment instead.

15 The results at the level of the respondent can be summarized as follows. The treated respondents used their bank account actively: During the 17 weeks of the experiment, 62 percent deposited at least once and the average person made three deposits. As a result, being offered help to open a BCSA account has a significant positive impact on their deposits into and savings in the account. As there is no impact on their stock of other financial assets, their total savings increase as well.

16 Appendix A.3 displays the distribution of the outcomes by treatment group.

17 We group post offices, cooperatives and other accounts together because villagers cannot deposit with post offices and cooperatives and they can, but rarely do so, into other accounts. All these accounts were opened with the same purpose, namely to receive payments from welfare schemes, MGNREGA, or the sale of crops. Villagers usually withdraw these payments at once shortly after they have been transferred (see Section 2.2 for more details). We combine money guarded by others with SHGs, as these tools place cash outside of the household. Note that only eight households had money guarded by others for a total of 19 weeks, so it is rarely used.

18 We could calculate the weekly difference to build the flow of each asset category, but did not do so as the sample size would be substantially smaller. We organized the weekly interviews in two phases and would, therefore, lose at least two observations per household.

19 This important difference is not surprising, as only a few respondents in the control group and few other household members had opened an account.

20 Liquid savings equal INR 2818 for the average control, and INR 3108 for the average treated household (see appendix Table B1). The INR 290 difference is close the average control household's weekly food expenditures (see Table 3).

21 Appendix Table B1 shows the results are similar, but less precisely estimated if the flow and stock are measured in levels. From appendix Table B2, we learn the impact is similar when we use the survey measure of BCSA savings instead of the administrative data.

22 Frequent purchases includes expenses on grains, cereals, pulses, lentils, milk products, edible oil, vegetables, fruit, sugar, salt, spices, fuels, soap and washing powder; and temptation goods on pan, alcohol, tobacco, drinks and snacks from the market, hair oil, lotion and perfumes.

23 To define perishable foods, we asked our local research assistant to list all the food that cannot be kept longer than one week outside a fridge (only one household owns a fridge in our sample).

Table 2
Treatment effect on savings behavior.

	BCSA account (1)	SHGs, money guarded (2)	Post offices, cooperatives, other accounts (3)	Cash at home (4)	Total liquid tools (5)	Jewelry, grain and livestock (6)	Total (7)
Panel A: Flow of savings – extensive margin							
Offered account	$0.17^{***}_{☆☆☆}$	0.01	-0.00		$0.14^{***}_{☆☆☆}$		
	(0.02)	(0.02)	(0.01)		(0.03)		
R^2	0.17	0.05	0.04		0.10		
Mean dep (control)	0.01	0.07	0.08		0.14		
Panel B: Flow of savings – intensive margin							
Offered account	$0.76^{***}_{☆☆☆}$	0.04	0.03		$0.70^{***}_{☆☆☆}$		
	(0.11)	(0.09)	(0.06)		(0.14)		
R^2	0.13	0.04	0.02		0.05		
Mean dep (control)							
...IHST	0.07	0.31	0.02		0.33		
...INR	3.88	3.54	0.05		4.66		
Panel C: Stock of savings							
Offered account	$2.77^{***}_{☆☆☆}$	0.04	0.11	0.13	$0.48^{**}_{☆☆}$	-0.11	0.18
	(0.25)	(0.34)	(0.40)	(0.19)	(0.19)	(0.40)	(0.20)
R^2	0.51	0.56	0.29	0.11	0.24	0.52	0.48
Mean dep (control)							
...IHST	0.28	2.98	4.61	5.73	7.44	8.18	9.44
...INR	118	817	1057	827	2818	17896	20714
Observations	2685	2685	2685	2685	2685	2685	2685

The table presents the impact on household savings. For each asset, we present the impact on a dummy indicating a household member made a transaction during the seven days that precede the interview (panel A), on the flow (deposits minus withdrawals, panel B) and on the stock (the balance, panel C). In the panels B and C, we use the inverse hyperbolic sine transformation. The different savings tools are: (1) the BCSA account, (2) SHGs and money guarded by others, (3) post offices, agricultural cooperatives and other accounts, (4) cash at home, (5) the total over the more liquid tools (1 to 4), (6) the total stock of jewelry, grain and livestock, and (7) the total over all the tools. All columns include time and village fixed effects and control for gender. Panel C also controls for the baseline value of the outcome variable. Standard errors are clustered at the household level. Statistical significance is indicated by $* \, p < 0.1, ** \, p < 0.05, *** \, p < 0.01$ for unadjusted p-values and by ☆ $p < 0.1$, ☆☆ $p < 0.05$, ☆☆☆ $p < 0.01$ for p-values that are adjusted for multiple hypotheses testing. When correcting the p-values for multiple hypothesis testing, we group all the p-values from the table in one family.

Table 3
Treatment effect on household expenditures.

	Frequent (1)	Temptation goods (2)	Non-frequent (3)	Investments (4)	Total (5)	All food (6)	Perishable food (7)	Non-perishable food (8)
Offered account	0.06	0.02	−0.00	−0.02	0.05	0.08	0.10	0.07
	(0.09)	(0.16)	(0.20)	(0.22)	(0.11)	(0.10)	(0.11)	(0.11)
R^2	0.12	0.11	0.08	0.06	0.10	0.12	0.12	0.08
Mean control:								
...IHST	6.1	4.2	4.9	1.5	7.0	6.1	5.2	5.2
...INR	323	73	427	332	1155	313	136	176
Observations	2685	2685	2685	2685	2685	2685	2685	2685

The dependent variables are the hyperbolic sine transformations of weekly expenditures on (1) frequent purchases, (2) temptation goods, (3) non-frequent products, (4) investments, (5) the total over these goods, (6) all food and (7) perishable and (8) non-perishable food. All columns include time and village fixed effects and control for gender. Standard errors are clustered at the household level. Statistical significance is indicated by $* \, p < 0.1, ** \, p < 0.05, *** \, p < 0.01$.

Table 4
Treatment effect on loan, transfers and total income.

	Loans (1)	Private transfers (2)	Public transfers (3)	Total income (4)
Offered account	−0.01	0.07	−0.05	0.05
	(0.17)	(0.11)	(0.08)	(0.12)
R^2	0.04	0.09	0.07	0.16
Mean control:				
...IHST	−0.5	−0.8	0.5	6.1
...INR	−24	10	155	981
Observations	2685	2685	2685	2685

The table presents the impact on the inverse hyperbolic sine transformation of loans (the total amount borrowed minus the total amount lent, plus the net amount of reimbursements received), transfers between friends and family (the total amount received minus the total amount given), government transfers and the total household income, which are all measured on a weekly basis. All columns include time and village fixed effects and control for gender. Standard errors are clustered at the household level. Statistical significance is indicated by $* \, p < 0.1, ** \, p < 0.05, *** \, p < 0.01$.

Table 5
Treatment effect on the different sources of income.

	Income			Proportion of positive amounts	
	Control mean INR (Std. dev.) (1)	Control mean IHST (Std. dev.) (2)	Coefficient on *Offered account* (Std. errors) (3)	Control mean (Std. dev.) (4)	Coefficient on *Offered account* (Std. errors) (5)
Wage employment	609	5.7	0.11	87.4	1.27
	(1065)	(2.4)	(0.11)	(33.2)	(1.10)
Agriculture	211	0.6	−0.01	7.8	−0.22
	(1927)	(2.0)	(0.13)	(26.8)	(1.78)
Self-employment	113	0.5	−0.18	6.5	−2.38
	(736)	(1.9)	(0.20)	(24.6)	(2.54)
Livestock	14	0.2	−0.01	2.7	−0.01
	(121)	(1.0)	(0.12)	(16.3)	(1.75)
Sale of goods	22	0.1	−0.01	0.9	−0.01
	(363)	(0.7)	(0.03)	(9.5)	(0.37)
Rents	12	0.0	−0.03	0.6	−0.31
	(219)	(0.6)	(0.03)	(7.8)	(0.31)
Forestry	0	0.0	0.01	0.1	0.15
	(12)	(0.2)	(0.01)	(2.8)	(0.19)
Observations			2685		2685

The table presents the impact on the different sources of (hyperbolic sine transformed) income at the household level. For each source of income, it provides the mean weekly revenue of the control group in levels and transformed (column 1 and 2), the proportion of observations with a positive amount in the control group (column 4), and the impact of being offered help to open a bank account on the inverse hyperbolic sine transformation of weekly income and on the proportion of positive amounts (column 3 and 5, respectively). Columns (3) and (5) include time and village fixed effects and control for gender. Standard errors are clustered at the household level. Statistical significance is indicated by * $p < 0.1$,** $p < 0.05$,***$p < 0.01$.

average, however, there is no effect on other household level outcomes, such as expenditures, loans, transfers and income.[24]

We now turn to consumption smoothing. Starting from the classical lifecycle model of consumption, we derive an Euler equation in Section 4.1 that allows us to estimate the impact of an exogenous change in access to banking on consumption smoothing. The results are presented in Section 4.2. The expenditures (and the caloric value of food expenditures) respond significantly to variations in income for the control households, but not for the treated households.

In Section 4.3, we elaborate upon these patterns. Following the strategy introduced by Zeldes (1989) we show that savings constraints can explain the observed excess sensitivity among the control households and that access to banking alleviates these constraints for the treated households.

Finally, in Section 4.4 we investigate how treated and control households handle variations in income. The treated households engage in pro-cyclical saving on the BCSA account (they deposit more when income spikes), while the control households adjust the flow of their transfers. The latter not being as complete may explain the partial consumption smoothing observed among the control.[25]

4.1. Theoretical background and empirical strategy

Classical theory in consumption and savings holds that people want to keep their marginal utility of consumption constant over time (Modigliani and Brumberg, 1954; Friedman, 1957). If the marginal utility of future consumption (in current, discounted value) is larger than the marginal utility of today's consumption, people prefer to reallocate present consumption to the future.[26] In this framework, the consumer maximizes expected utility over a finite number of periods:[27]

$$\max E_t\left[\sum_t \beta^t U(C_t(\theta^t))\right] \tag{2}$$

subject to a life-time budget constraint:

$$\sum_t p_t(\theta^t)(C_t(\theta^t) - y_t(\theta^t)) \leqslant 0, \forall \theta^t, \tag{3}$$

where β^t is the discount factor for period t, C_t consumption, θ^t the stochastic state-of-the-world, p_t the price of consumption and y_t the resources (income and assets). This formulation relies on strong but widely made assumptions. In particular, we assume that consumers maximize their expected utility, that utility is additive across periods, and that households are unitary. These assumptions and their implications have been extensively discussed in the literature, e.g. in Attanasio and Weber (2010).

If consumers behave according to the Eqs. 2 and 3, the equilibrium depends on their individual preferences, income paths and initial assets. To simplify the problem and to characterize (Pareto efficient) equilibrium allocations, a number of authors used the case of complete markets as a benchmark (see e.g., Cochrane, 1991; Mace, 1991; Nelson, 1994; Townsend, 1994; Attanasio and Davis, 1996; Hayashi et al., 1996). The optimization problem becomes that of an imaginary social planner who maximizes the weighted sum of individual utilities.

As discussed by Townsend (1994), this model can be modified and used as a model of optimal risk-sharing, assuming that the lifetime budget constraint does not have to hold period-by-period because transfers can be state-contingent, transferring resources between states or individuals. While the optimal risk-sharing model is silent about how transfers happen (optimal allocations could be achieved through spot markets for goods, labor and debt with state-contingent prices, transfers between family and friends,

[24] Note that an increase in the average stock of savings is perfectly consistent with no changes in average income and average expenditures. To see this, imagine that person A earns and consumes 10 in week one and earns and consumes 5 in week two. Person B, on the other hand, earns 10, consumes 7.5 and saves 2.5 in week one and earns 5 and consumes 7.5 in week two. Both have the same average income (7.5) and the same average expenditures (7.5), but while person A's average weekly stock of savings equal zero, person B's is equal to 1.25.

[25] This is in line with the literature, e.g. Jack and Suri (2014) point out that informal networks provide an important means to share risk, but that the insurance they provide is often incomplete.

[26] Theoretical and empirical work in this area is very extensive, we refer the reader to Browning and Lusardi (1996, 1992, 2010); Fuchs-Schündeln and Hassan (2016) for overviews.

[27] We follow the notation used in Attanasio and Weber (2010).

or the use of informal financial services such as local money lenders and credit and savings groups), this framework generally provides a fair description of a rural village economy and fits the setting we are studying (Townsend, 1994; Ravallion and Chaudhuri, 1997; Kinnan and Townsend, 2012; Alem and Townsend, 2014; Meghir et al., 2022). The optimization problem in this case becomes:

$$\max_{\{A_\tau, c^j_\tau\}_{\tau>0, j=1,\dots,N}} \sum_i \phi_i \left[\sum_t \beta^t \sum_{\theta^t} \pi(\theta^t) U(C^i_t(\theta^t)) \right] \tag{4}$$

subject to:

$$A_{t+1} \leqslant (1+r)A_t + \sum_i (C^i_t(\theta^t) - y^i_t(\theta^t)), \tag{5}$$

where ϕ_i is the Pareto weight given to consumer $i, \pi(\theta^t)$ the probability of history θ^t, A_{t+1} the assets available to society in the next period of time, and r the real interest rate.

The first-order condition tells us that the marginal utility of consumer i at time t for state of the world θ^t, multiplied by the person's Pareto weight, is equal to the aggregate constraint multiplier for that state of the world $(\mu(\theta^t))$ divided by the probability of that history $(\pi(\theta^t))$:

$$\phi_i \beta^t U_{C^i_t(\theta^t)} = \mu(\theta^t)/\pi(\theta^t) \tag{6}$$

An important insight from this Euler equation is that —in a first-best world with full risk-sharing— the individual's marginal utility of consumption is independent of idiosyncratic risks and that the person's consumption is entirely determined by aggregate resources. This is a key prediction of the model that can be used to test for the completeness of risk-sharing in a particular sample.

In line with most of the literature, we assume a constant relative risk aversion (CRRA) utility function. Log linearizing Eq. 6 then leads to

$$\Delta ln(C^i_t) = -ln\beta/\gamma + \frac{\Delta ln(\mu(\theta^t)/\pi(\theta^t))}{\gamma}, \tag{7}$$

where γ is the risk aversion parameter.

Cochrane (1991) and Mace (1991) introduced a test based on this equation: Adding individual income to the right-hand side should lead to an estimated coefficient of zero, or $\alpha = 0$ in

$$\Delta ln(C^i_t) = -ln\beta/\gamma + \frac{\Delta ln(\mu(\theta^t)/\pi(\theta^t))}{\gamma} + \alpha \Delta y^i_t. \tag{8}$$

We derive our estimation directly from Eq. 8 and estimate:

$$C_{it} = \delta_0 + \delta_1 Income_{it} + \delta_2 Income_{it} * T_i + H_i + W_t + \theta_{it}, \tag{9}$$

where C_{it} measures expenditures on different categories of goods by household i during the seven days that precede the interview of week t and $Income_{it}$ is the household's total wage income over the same period. We focus on wage income because it is the most regular and most flexible source of revenues (see Table 5). The unobserved terms $-ln\beta/\gamma$, and $\mu(\theta^t)$ and $\pi(\theta^t)$ in Eq. 8 are absorbed by the household H_i and time W_t fixed effects, respectively. To be able to estimate the differential impact of being provided access to financial services, we follow Kinnan and Townsend (2012) and Alem and Townsend (2014), and include an interaction term between income and the treatment status T_i. To the best of our knowledge, we are the first to estimate this modified Euler equation using an exogenous change in access to banking.

There are two additional remarks to make. First, the inclusion of household fixed effects —which control for all the household characteristics that are constant over time— greatly reduces the possibility of endogeneity biases in the estimates. A bias would occur if the treatment impacts weekly wage income, but the distribution of income is similar in both groups. Indeed, using the Kolmogorov–Smirnov test we cannot reject that the distributions are equal (p-value = 0.269) and Table 5 shows that, on average, there is no significant treatment effect on any source of income. We also check the robustness of our results to three alternative measures of income. In Table 6, panel B, we replace the household's wage income by its total income[28] and in appendix Table B16, we replace the continuous measures by dummies indicating the weekly wage (and total) income are below the household's median wage (and total) income. The latter further limit the potential influence of outliers.

Second, the assumption of a CRRA utility function implies we should use the logarithm of consumption as the dependent variable. To preserve observations with value zero, we use the hyperbolic sine transformation for both the expenditures and for income. As such, the coefficients can be interpreted as the income elasticity of expenditures.[29]

4.2. Results

Panel A in Table 6 shows the results from estimating Eq. 9 for our main measure of income. Total wage income is positively correlated with different expenditures in the control group: On average, a ten percent decrease in total wage income corresponds to a 0.4 percent decrease in frequent purchases and food expenditures, and a 0.3 percent decrease in total expenditures. This indicates the control are able to partially smooth consumption, but not perfectly so.[30] As a result, the findings contradict the prediction of the theory in case of complete markets, namely that δ_1 should equal 0. Being offered a savings account reduces market frictions if $\delta_2 < 0$, which is the case.

If we further split food expenditures into items that are perishable and non-perishable, the difference between the treated and control households is more important for food that has to be consumed within a short period of time than for food that can be put aside for difficult weeks. Taken together, these results suggest the treated respondents' diet may be more balanced over time. The results are robust to the use of total income instead of wage income only (panel B) and to replacing the continuous income measures by dummies (appendix Table B16).

As malnutrition is still severe in India and a large majority of the households in our sample live close to subsistence level (95 percent are on the official "Below Poverty Line" list), we also investigate the impact of access to banking on nutrition smoothing.[31] Large variation in calories obtained from perishable food is evidence against consumption smoothing over time. On the contrary, buying more calories in the form of non-perishable food when income peaks is a plausible consumption smoothing strategy, as these goods can be put aside until income is low.[32]

28 The distribution of total income is similar in both groups as well (the p-value equals 0.838) and Table 4 shows the treatment does not impact total income.

29 As for semi-elasticities, Bellemare and Wichman (2020) show this interpretation is appropriate if the average values of the dependent, independent or both variables are sufficiently large. This is the case in our sample.

30 The size of the effect depends on the context, specification, period under study and measurement of consumption and income. Therefore, it is not straightforward to compare our estimates with other papers. Nonetheless, we refer the reader to Fuchs-Schündeln and Hassan (2016) for a review. The study we consider closest to ours is Meghir et al. (2022), who report a coefficient of 0.165 in Bangladesh. Their estimate —which is not based on weekly, but on annual measures of consumption and income— is larger than ours, but not excessively so.

31 According to the World Health Organization, "India contributes a third of the global burden of undernutrition" (www.searo.who.int/india/topics/nutrition/en/, last visited on 03.06.2019). In particular, the latest Global Nutrition Report emphasizes the importance of anemia and stunting: Around 38 percent of children below five are stunted and around 50 percent of women of reproductive age suffer from anemia (Global Nutrition Report, 2018).

32 To calculate the caloric value of the purchases, we use the *Indian Food Composition Tables 2017* produced by the National Institute of Nutrition (Indian Council of Medical Research).

Table 6
Consumption smoothing.

	Frequent (1)	Temp-tation goods (2)	Non-frequent (3)	Invest-ments (4)	Total (5)	All food (6)	Perishable food (7)	Non- perishable food (8)
Panel A: Wage income								
Wage income	$0.04^{***}_{☆☆}$	0.02	0.04^{*}	0.01	$0.03^{**}_{☆}$	$0.04^{***}_{☆☆}$	$0.05^{***}_{☆☆}$	0.03
	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)
Offered account x	$-0.05^{***}_{☆☆}$	-0.02	-0.05	0.02	-0.03^{*}	$-0.05^{***}_{☆☆}$	$-0.05^{***}_{☆☆}$	-0.03
wage income	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)	(0.03)
Total effect for	-0.01	-0.00	-0.00	0.02	-0.00	-0.01	-0.00	-0.00
treated hh	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)
Panel B: Total income								
Total income	$0.04^{***}_{☆☆☆}$	0.03	$0.07^{***}_{☆☆}$	$0.13^{***}_{☆☆☆}$	$0.06^{***}_{☆☆☆}$	$0.05^{***}_{☆☆☆}$	$0.05^{***}_{☆☆☆}$	$0.04^{*}_{☆}$
	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)
Offered account x	$-0.04^{**}_{☆}$	-0.02	-0.05	-0.01	-0.03	$-0.05^{**}_{☆}$	$-0.04^{**}_{☆}$	-0.02
total income	(0.02)	(0.02)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)
Total effect for	0.00	0.01	0.02	$0.11^{***}_{☆}$	0.03^{*}	0.00	0.01	0.01
treated hh	(0.01)	(0.01)	(0.03)	(0.04)	(0.02)	(0.01)	(0.01)	(0.02)
Mean Control:								
...IHST	6.12	4.20	4.92	1.46	7.04	6.08	5.20	5.23
...INR	323.33	72.51	426.96	332.31	1155.12	312.50	136.28	176.23
Obs.	2685	2685	2685	2685	2685	2685	2685	2685

The dependent variables are the hyperbolic sine transformations of weekly expenditures on (1) frequent purchases, (2) temptation goods, (3) non-frequent products, (4) investments, (5) the total over these goods, (6) all food and (7) perishable and (8) non-perishable foods. All columns include household and time fixed effects. Standard errors are clustered at the household level. Statistical significance is indicated by $*\ p < 0.1, **\ p < 0.05, ***\ p < 0.01$ for unadjusted p-values and by $☆\ p < 0.1, ☆☆\ p < 0.05, ☆☆☆\ p < 0.01$ for p-values that are adjusted for multiple hypotheses testing. When correcting the p-values for multiple hypothesis testing, we group all the outcomes in one family and we correct the p-values per family and per independent variable.

Table 7 presents the treatment impact on the hyperbolic sine transformation of the total amount of calories purchased, and on the calories purchased through perishable and non-perishable food separately.

The results are in line with our previous findings. Being offered an account does not affect the average calories purchased (panel A) but it significantly improves the smoothing of calories over time (panels B and C). In the control households, a ten percent decrease in weekly wage earnings translates into a decrease of around one percent in the amount of calories purchased, and a decrease of around 0.8 percent in the calories purchased as perishable food. For the treated households, the calories purchased do not vary with income. There is weaker evidence that households purchase more calories as non-perishable food when income increases, but this consumption smoothing strategy does not differ between treated and control households.

Table 7
Calories purchased.

	All food (1)	Perishable food (2)	Non-perishable (3)
Panel A: Impact on calories purchased			
Offered account	0.02	0.10	-0.05
	(0.17)	(0.18)	(0.25)
Panel B: Wage income			
Wage income	$0.10^{***}_{☆☆☆}$	$0.08^{***}_{☆☆☆}$	0.09
	(0.03)	(0.03)	(0.06)
Offered account x	$-0.11^{***}_{☆☆☆}$	$-0.09^{***}_{☆☆☆}$	-0.02
wage income	(0.04)	(0.03)	(0.07)
Total effect for	-0.01	-0.01	0.07
treated hh	(0.02)	(0.02)	(0.06)
Panel C: Total income			
Total income	$0.10^{***}_{☆☆☆}$	$0.09^{***}_{☆☆☆}$	0.08
	(0.03)	(0.03)	(0.05)
Offered account x	$-0.09^{**}_{☆☆}$	$-0.08^{**}_{☆☆}$	0.03
total income	(0.04)	(0.03)	(0.07)
Total effect for	0.01	0.01	0.11^{*}
treated hh	(0.02)	(0.02)	(0.06)
Mean Control:			
...IHST	11.69	9.36	10.61
...kJ	245388	24104	221284
Obs.	2685	2685	2685

The dependent variables are the hyperbolic sine transformations of the weekly calories purchased in total (1), on perishable food (2) and on non-perishable food (3). Panel A includes controls for gender and village and time fixed effects. Panels B and C include household and time fixed effects. Standard errors are clustered at the household level. Statistical significance is indicated by $*\ p < 0.1, **\ p < 0.05, ***\ p < 0.01$ for unadjusted p-values and by $☆\ p < 0.1, ☆☆\ p < 0.05, ☆☆☆\ p < 0.01$ for p-values that are adjusted for multiple hypotheses testing. When correcting the p-values for multiple hypothesis testing, we group all the outcomes in one family and we correct the p-values per family and per independent variable.

In conclusion, we reject δ_1 equals zero for control households, but we cannot reject $\delta_1 + \delta_2$ equals zero for the treated ones. Therefore —contrary to treated households —the control households do not allocate their consumption (and nutrition) optimally. These findings may be driven by households being savings constrained. We test this hypothesis in Section 4.3.

4.3. Testing for savings constraints

In line with parts of the literature, we find "excess sensitivity of consumption" to current income for the control households, or $\delta_1 > 0$ in Eq. 9. This does not hold for treated households. Excess sensitivity of consumption can be a sign of liquidity constraints. Given we observe excess sensitivity for the control only, we hypothesize the existence of savings constraints that are alleviated by providing access to a new savings tool, namely formal banking at the doorstep.

To test for the existence of constraints, Zeldes (1989) proposes comparing the correlation between income and consumption variations for participants who are more and less constrained at baseline.[33] To allow a differential correlation for the treated and control, we extend the classical approach and estimate the following equation:

[33] This approach became the standard method to assess the importance of liquidity constraints in the literature (see e.g., Aaronson et al., 2012; Agarwal et al., 2007; Agarwal and Qian, 2014; Bertrand and Morse, 2009; Broda and Parker, 2014; Gelman et al., 2014; Gross et al., 2014; Hsieh, 2003; Johnson et al., 2006; Leth-Petersen, 2010; Mastrobuoni and Weinberg, 2009; Misra and Surico, 2014; Parker, 1999; Parker et al., 2013; Scholnick, 2013; Shea, 1995; Souleles, 1999; Stephens and Melvin, 2006; Stephens and Melvin, 2008; Stephens et al., 2011).

Table 8
Consumption smoothing by being savings constrained.

	Frequent (1)	Temp-tation goods (2)	Non-frequent (3)	Invest-ments (4)	Total (5)	All food (6)	Perishable food (7)	Non-perishable food (8)
Wage income (ζ_1)	$0.04^{***}_{☆☆☆}$	0.02	0.04	-0.00	0.03^{*}	$0.04^{***}_{☆☆☆}$	$0.06^{***}_{☆☆☆}$	0.03
	(0.01)	(0.02)	(0.03)	(0.04)	(0.01)	(0.01)	(0.02)	(0.02)
Offered account x	$-0.05^{**}_{☆}$	-0.01	-0.04	0.04	-0.03	$-0.05^{**}_{☆}$	$-0.05^{***}_{☆}$	-0.02
wage income (ζ_2)	(0.02)	(0.02)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)
Unconstrained x	-0.02	-0.02	-0.00	0.05	-0.01	-0.02	-0.04^{*}	0.00
wage income (ζ_3)	(0.04)	(0.05)	(0.05)	(0.08)	(0.03)	(0.03)	(0.03)	(0.05)
Offered account x	0.00	-0.04	-0.04	-0.10	-0.03	-0.01	0.02	-0.09
unconstrained x	(0.04)	(0.06)	(0.08)	(0.11)	(0.04)	(0.04)	(0.03)	(0.07)
wage income (ζ_4)								
Total effect for …								
…unconstrained control households ($\zeta_1 + \zeta_3$)								
	0.03	0.01	0.04	0.04	0.02	0.02	0.01	0.03
	(0.04)	(0.04)	(0.04)	(0.07)	(0.02)	(0.03)	(0.02)	(0.05)
…unconstrained treated households ($\zeta_1 + \zeta_2 + \zeta_3 + \zeta_4$)								
	-0.02	-0.05	-0.04	-0.02	-0.03	-0.04^{*}	-0.02	-0.07
	(0.02)	(0.03)	(0.05)	(0.07)	(0.02)	(0.02)	(0.02)	(0.04)
…constrained treated households ($\zeta_1 + \zeta_2$)								
	-0.00	0.01	0.01	0.04	0.00	-0.01	0.00	0.02
	(0.01)	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.01)	(0.02)
Observations	2685	2685	2685	2685	2685	2685	2685	2685

The dependent variables are the hyperbolic sine transformations of weekly expenditures on (1) frequent purchases, (2) temptation goods, (3) non-frequent products, (4) investments, (5) the total over these goods, (6) all food and (7) perishable and (8) non-perishable foods. All columns include household and time fixed effects. Standard errors are clustered at the household level. A household is "unconstrained" if they reply yes to the baseline question asking whether the household saves enough. Statistical significance is indicated by $*\ p < 0.1$, $**\ p < 0.05$, $***\ p < 0.01$ for unadjusted p-values and by $☆\ p < 0.1$, $☆☆\ p < 0.05$, $☆☆☆\ p < 0.01$ for p-values that are adjusted for multiple hypotheses testing. When correcting the p-values for multiple hypothesis testing, we group all the outcomes in one family and we correct the p-values per family and per independent variable.

$$C_{it} = \zeta_0 + \zeta_1 Income_{it} + \zeta_2 Income_{it} * T_i + \zeta_3 Income_{it} * UC_i + \zeta_4 Income_{it} * UC_i * T_i + H_i + W_t + \vartheta_{it} \quad (10)$$

where UC_i equals one for unconstrained households and zero for households that are savings constrained at baseline.

We classify a household as being savings constrained, if they reply no to the baseline question asking whether the household saves enough. This holds for 81.8 percent of the control and 80.4 percent of the treated households. This measure reflects the household's judgment regarding their own savings level. We could have used the median baseline savings as a cut-off, but in this case, a household's ranking would not only depend on their constraints, but also on the other households' preferences.

Table 8 provides the results using our main measure of income, namely the hyperbolic sine transformation of the household's total wage income. Appendix Table B17 shows the results are robust to replacing the continuous wage measure by total income.

We test two hypotheses. First, we hypothesize savings constraints exist, and test this by comparing the correlation between income and consumption variations for participants who are more and less constrained in the control group. Total wage income is positively correlated with different expenditures, but only for households that are savings constrained at baseline (ζ_1). For unconstrained control households ($\zeta_1 + \zeta_3$), the correlation is smaller and never statistically significant (the interaction term is negative, though not always statistically significant). Second, we hypothesize that the introduction of bank accounts alleviates savings constraints for constrained households. To test this, we compare the correlation for constrained control households (ζ_1) with constrained treated households ($\zeta_1 + \zeta_2$). While ζ_1 is positive and statistically significant for the most important expenditure categories, ζ_2 is negative and statistically significant. As a result, we cannot reject the hypothesis that the introduction of bank accounts alleviated savings constraints ($\zeta_1 + \zeta_2$).

In conclusion, we find evidence of "excess sensitivity of consumption" due to savings constraints, and access to bank accounts seems to alleviate those constraints.

4.4. How do households smooth consumption?

The theoretical framework and empirical tests do not detail *how* the treatment helps households to smooth consumption. We now investigate this further. Households that are not savings constrained can save more when income is higher. We test this by estimating Eq. 9, where C_{it} measures the change in household i's savings in the seven days that precede the interview of week t. The change in savings is the difference between the deposits and the withdrawals made by any household member using the savings tools they own. Table 9 provides the results for savings in the BCSA account and appendix Table B18 for the other savings tools: SHGs on the one hand and post offices, agricultural cooperatives and other accounts on the other hand. For completeness, appendix Table B19 reports the results for savings tools for which we only have information on the stock, namely cash at home and the total stock of jewelry, grain and livestock.

Households can also smooth consumption by relying on informal insurance. There is an extensive literature that focuses on informal insurance in low- and middle-income countries.[34] To understand its importance in our context, we replace the dependent variable by the inverse hyperbolic sine transformation of the net amount of transfers received (the difference between money that household members received and gave away over the past week).

Table 9 shows the results for net deposits into the BCSA account in column (1) and for net transfers received in column (4). To understand whether these net effects are driven by inflows or outflows, we decompose the changes in savings into the amounts deposited and withdrawn (column 2 and 3), and the changes in transfers into the amounts received and given (column 5 and 6). The results suggest that treated and control households use different tools and the effects are important: A ten percent higher wage income is associated with 0.7 percent higher savings in the BCSA account for the treated households and 1.3 percent lower net

[34] See, for instance, the seminal contributions of Townsend (1994), Udry (1994), Townsend (1995), Morduch (1999), Dercon (2002) and Dercon (2005).

Table 9
Smoothing through savings and transfers.

	BCSA account			Transfers		
	Net flow (1)	Deposited (2)	Withdrawn (3)	Net flow (4)	Received (5)	Given (6)
Panel A: Wage income						
Wage income	-0.01	-0.00	$-0.00^{**}_{☆}$	$-0.07^{***}_{☆☆☆}$	$-0.06^{***}_{☆☆☆}$	$0.05^{***}_{☆☆☆}$
	(0.01)	(0.00)	(0.00)	(0.02)	(0.02)	(0.02)
Offered account x wage income	$0.05^{**}_{☆☆}$	$0.06^{**}_{☆☆}$	-0.01	0.02	0.02	$-0.08^{***}_{☆☆☆}$
	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.03)
Total effect for treated hh	$0.05^{**}_{☆☆}$	$0.05^{**}_{☆☆}$	-0.01	$-0.06^{**}_{☆☆}$	$-0.04^{**}_{☆}$	-0.03
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Panel B: Total income						
Total income	-0.00	-0.01	$-0.01^{**}_{☆}$	$-0.14^{***}_{☆☆☆}$	$-0.09^{***}_{☆☆☆}$	$0.05^{**}_{☆☆}$
	(0.01)	(0.01)	(0.00)	(0.04)	(0.02)	(0.02)
Offered account x total income	0.06^{*}	0.05^{*}	-0.01	$0.11^{**}_{☆☆}$	0.04	$-0.08^{**}_{☆☆}$
	(0.03)	(0.03)	(0.01)	(0.05)	(0.03)	(0.03)
Total effect for treated hh	0.06^{*}	0.04	-0.02	-0.02	-0.05^{**}	-0.03
	(0.03)	(0.03)	(0.01)	(0.04)	(0.02)	(0.03)
Mean Control:						
...IHST	0.07	0.07	0.01	-0.79	0.33	1.12
...INR	4	5	2	10	40	30
Obs.	2685	2685	2685	2685	2685	2685

The dependent variables are the hyperbolic sine transformations of the net deposits into the BCSA account in column (1) and for net transfers received in column (4). Column 2 and 3 decompose the changes in savings into the amounts deposited and withdrawn, and column 5 and 6 decompose the changes in transfers into the amounts received and given. All columns include household and time fixed effects. Standard errors are clustered at the household level. Statistical significance is indicated by $* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$ for unadjusted p-values and by ☆ $p < 0.1$, ☆☆ $p < 0.05$, ☆☆☆ $p < 0.01$ for p-values that are adjusted for multiple hypotheses testing. When correcting the p-values for multiple hypothesis testing, we group the outcomes in two families, the BCSA savings (columns 1–3) and the transfers (columns 4–6), and we correct the p-values per family and per independent variable.

transfers received by the control households. Indeed, when income is higher, treated households deposit more into the account and control households give more and receive less transfers. Appendix Tables B18 and B19 show that the correlation between the different measures of income and the other savings tools does not differ between the treated and control households.

In conclusion, the results suggest the treated households cope with income fluctuations using their savings in the newly received account. Given that there are no differences between the treated and control households for other savings tools, this provides additional evidence of the treatment alleviating savings constraints. Control households cope with income fluctuations through transfers, which allow partial consumption smoothing only.

5. Conclusions

Most papers that study the impact of providing access to a bank account observe some impact on savings, but limited average effects on expenditures and related variables. We confirm these findings for a representative sample of unbanked villagers in rural India. We gathered highly frequent data up to six months after the accounts were opened, in the form of weekly interviews. This has allowed us to observe weekly changes in savings and consumption within households over time, and led to an important conclusion: Treated households smooth consumption (and nutrition) better than control households. Indeed, while the expenditures (and the caloric value of food expenditures) vary with income for control households, the expenditures of treated households (and their caloric intake) do not respond significantly to variations in income. To substantiate our interpretation that these effects are driven by the savings accounts reducing liquidity constraints, we first show that savings constraints can explain the observed excess sensitivity of expenditures to income variations among the control and that access to banking alleviates those constraints. Second, we demonstrate that the treated households engage in pro-cyclical saving in the account, while the control households adjust the flow of their transfers. The latter appears insufficient for control households to perfectly smooth consumption.

Our experiment bridges an important gap between two major strands of literature, the impact of banking on the one hand and its role in mitigating risk and facilitating consumption smoothing on the other hand. Indeed, the existing experiments on access to banking provide limited insights into consumption smoothing, and papers that link frictions in the savings and credit market with risk mitigation lack exogenous variation in access to financial services.

Our results are important given the attention that has been given to access to banking by the international community and several governments. While the existing studies reported mixed effects on average outcomes, our study shows that access to banking can improve consumption smoothing even without changing mean consumption. In this perspective, simplifying access to a convenient savings tool is an important development strategy.

Data availability

Data will be made available on request.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2023.104900.

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