

Access to Finance: Ideas and Evidence

The Economics of Saving

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The economics of saving

A large macroeconomic literature exists to understand national savings rates, their determinants, and implications. Policymakers often strive for target rates, but the variance in national savings rates is remarkable in both developed and developing countries (Gersovitz, 1988; Horioka, 2006). As Rosenzweig (2001) and Collins et al. (2009) argue, focusing on the saving rate places emphasis on asset levels at a given point in time, but that misses the value of savings for many poor households. Many poor households may be actively saving even if their assets at any given moment are low; instead, they are building up lumps of money and spending them within a year. To see this, we need to turn to micro data.

Micro household and individual data reveals much about personal savings rates, the decision-making process at the individual and household levels, and the impact on individuals and households from access to different savings services. We will focus specifically on how informal institutions (e.g., ROSCAs) and formal services influence savings decisions. We will then conclude with a discussion about measuring the impacts from deepening access, both in terms of quantity (e.g., lower transaction costs) as well as the quality of access to savings.

Where households in developed countries, including the poor, may have many products available (savings accounts, automatic transfers, savings bonds, certificates of deposit) to them to help them save and build assets, the poor in developing countries face a much more limited menu of options. Those who are able to save are often forced to invest in risky assets like jewelry or animals or to use informal savings arrangements (e.g., ROSCAs). It is easy to imagine that households in poor countries would save more if they were given access to a broader array of quality savings products. Such access would enable the building of safety nets to smooth shocks and greater accumulation for the purchase of indivisible goods. Much of the discussion on how products influence decisions will build from knowledge and innovation from developed countries, with discussion and examples of applications to developing countries—and a focus on the complexities, risks, and infrastructure unique to the poor in developing countries.

Revisiting “the poor can’t save” assumption

The historical emphasis on credit in the microfinance movement implicitly assumes that the poor cannot save up for investments, and instead must borrow at relatively high interest rates in order to make investments. Criticism of this view is not new, and was argued strongly by the rural finance group at Ohio State University in the 1970s and onward, following McKinnon (1973) and Shaw (1973). The argument has been revisited by some donors, notably the Bill and Melinda Gates Foundation, with a renewed focus on encouraging the promotion of savings in rural settings (Guth, 2008). Adams (1978) decries the lack of focus on expanding rural savings capacity and argues that only a handful of countries stress mobilization of voluntary household savings, arguing that policymakers have too quickly assumed that rural households are too poor to save, that there is no excess cash flow from income, and that those that do acquire additional income spend it on consumption or ceremonial expenses.

Yet evidence on rural savings behavior from various economies has shown impressive propensities to save among rural households. Adams cites the remarkable savings rates achieved

in post-World War II East Asia: the average propensity to save in 1973-74 increased to 0.31 in Taiwan (compared to 0.19 in 1960), 0.22 in Japan (compared to 0.10 in 1950), and 0.33 in Korea (compared to 0.04 in 1965)—a product, Adams argues, of pro-saving public policy. The argument continues that while rural households have a substantial capacity for voluntary saving, their capacity is adversely influenced by rural financial markets that tend to discourage savers through subsidized (but unreliable and unsustainable) credit.

The focus on household decision making is not new either. For example, von Pischke (1978) argues that the rural poor can save, but that they have specific needs due to low account balances, seasonal income, remote location, non-cash assets, and high transaction operations. The development challenge in encouraging savings lies in the design of financial technology to serve these specific needs.

Problem with concepts of poverty = destitution

National income accounting and purchasing power parity (PPP) calculations tell us that people in poor countries, on average, do not have a lot of money. But of course what this implies for policy requires much more information. Even for those at the bottom of the income distribution, themselves in countries at the bottom of the global distribution, the fact of earning very little money is insufficient for making reliable inferences about the types of products and services that can be afforded, much less valued.

Take, for instance, Ethiopia, where 23 percent of the population lives below \$1/day (PPP) (UNDP). A family of seven at this international poverty line would consume roughly \$210 worth of goods and services per month – if they were purchased at US prices. PPP measures are debated (Deaton, 2006), with particular questions about the transferability for the bundle of goods that are purchased by the poor (versus that of the median consumer, for example). However, even if these measures are underestimated by a factor of two, this still implies monthly household consumption of the equivalent of \$420 in the United States. These amounts buy so little in terms of meeting an entire family's needs in a rich country that it can be hard to fathom how they get by at all. Going further requires knowing more about the finances of the poor: what do they spend on food, housing, and healthcare? And do they have any money left over at the end of the month?

Banerjee and Duflo (2007) tackle this question: “how actually does one live on less than one dollar per day,” by assembling a dataset comprised of existing detailed survey data (mostly World Bank LSMS surveys and Rand Family Life Surveys) representing the expenditures of poor households (including households living under \$1/day and \$2/day) in 13 countries. The surveys were conducted between 1988 and 2005. They find among those living under \$1 per day between 56 and 78 percent of household income is spent on food (slightly less in urban areas). Only around 2 percent of income is spent on education, and a bit less on healthcare. Housing does not seem to be a major expense, perhaps because many very poor households effectively own their own land (though their holdings are quite small).

Though the very poor often fail to meet minimum caloric requirements they choose to make expenditures on many items like alcohol, tobacco, festivals, and radios. As if to reemphasize

that these households are truly poor despite their disposable income the authors describe the sample from Udaipur, India, for whom they have the most detailed asset data: among the extremely poor most have a bed or a cot but only 10 percent own a chair and 5 percent a table. Less than 1 percent has an electric fan, a sewing machine, or a bullock cart and no one has a phone. Clearly the very poor are choosing to spend their resources in particular ways. Like households everywhere they have to make choices between consumption, investment, and savings.

Having the right financial products conveniently available might help them make the choices they prefer over the long term. Though some of the surveys predate the global explosion in microfinance, the lack of financial access is nonetheless striking. While borrowing is quite common among the extremely poor (ranging from 11 percent of households in rural East Timor to 93 percent in Pakistan), little of it is conducted with a formal institution. By and large the extremely poor borrow from relatives, shopkeepers, and fellow villagers. The pattern is consistent in recent data from India reported by Banerjee and Duflo (2007), where only 6.4 percent of borrowing is from a bank or cooperative, even when there is a branch nearby. This informal credit is expensive: borrowers pay nearly 4 percent per month. Collins et al. (2009) report similar data in their samples from Bangladesh, India, and South Africa.

Savings is no better. Except for one notable outlier (Cote d'Ivoire, where 79 percent of extremely poor households have a savings account) the fraction is below 14 percent in the other countries (Banerjee and Duflo, 2007). That may be even more problematic than the lack of credit since, as the data suggest, even the very poor can use help resisting the temptation to spend money on immediate consumption.

1 Basic models of saving

Browning and Lusardi (1996) review nine models used to explain motivations to save: precautionary, life-cycle (to provide for anticipated needs), intertemporal substitution (to enjoy interest), improvement (to enjoy increasing expenditure), independence, enterprise, bequest, avarice, and downpayment. The authors then review the major economic theories of savings, before reviewing empirical evidence on these theories. The data provide a valuable description of who saves and how saving rates have changed over time, but no unique theory explains why people save. We refer the reader to this and other reviews (e.g., Rosenzweig 2001 and Armendáriz and Morduch 2010), and instead here focus on the key motivating and classic models, and then turn to the evidence from developing countries to help understand household decision-making processes.

The lifecycle hypothesis (Modigliani, 1986; Ando and Modigliani, 1963) remains the most influential model of savings. The lifecycle hypothesis (LCH) framework articulates the relationship between consumption, income, wealth, and savings, over the life of individuals. Its central insight is that households have a finite life and a long-term view of their income and consumption needs. They therefore increase their wealth during their working life and use it to smooth consumption during retirement. Wealth itself can come from the accumulation of savings (the difference between “permanent” and “transitory” income) or from bequests. The

life cycle hypothesis was one of the first models used to explain savings; it is supported by many empirical analyses in rich countries and is robust to varying assumptions.

As discussed by Deaton (1997), the life cycle hypothesis requires adaptation to fit the multi-generational households commonly found in developing countries. If, for example, the social norm is that a household unit has grandparents, children and grandchildren, the need to save over one's lifetime is diminished. Rather, intergenerational transfers replace the need for savings and borrowing over one's life. Naturally, demographic transitions (e.g., AIDS epidemics leading to premature deaths of income-earning generations) can wreak havoc on the ability of intergenerational households to transfer wealth, thus making it important to consider how such trends influence life-cycle savings decisions as the LCH suggests.

For poor households, precautionary savings models are often a better fit (Deaton, 1997). The models capture the fact that for many poor households the volatility of income and the inability to borrow to smooth consumption is potentially just as damaging as a persistently low level of consumption. Rutherford (2000b) puts forward a simple prediction, one that also falls out of most models of savings that generate a preference for smooth consumption: the poor need mechanisms to make small deposits and large withdrawals. The idea is that poor people can save and that they want to save in order to meet life cycle needs, cope with emergencies, acquire assets and develop businesses. Most of these needs come in lump-sums, however, whereas income often comes in little installments (cash labor income, or entrepreneurial income). One exception is agricultural income, with a small number of harvests per year, and we will discuss this below.

Putting these points together: the poor have uneven cash flows (thus the need to save) and they have available income (thus an ability to save). What they require is a safe and convenient place to keep their money and a structure with which to discipline the accumulation of lots of small sums and their transformation into a large sum. Rutherford (2000a) eloquently articulates that the poor can and do save using a variety of homegrown mechanisms including rotating savings and credit associations (ROSCAs) and deposit collectors who *charge* the poor to take their savings. Each option presents tradeoffs in convenience, risk, price, and simplicity. The importance of the last dimension, simplicity, should not be underestimated: as programs and products get more complex, they have a greater potential to meet the specific financial needs of the poor—but they also get harder for clients to understand and manage. Good research can help programs identify optimal combinations of all these dimensions by determining which factors “sell” the best to clients and by providing an understanding of the literacy necessary for a product to succeed (both in terms of take-up and proper usage). However, as we discuss below, even the “best” combination of convenience, security, and price may not be enough: just because people make decisions does not necessarily mean they make good decisions—and by “good” we mean decisions about actions that people say *they want to make* — like building a safety net for hard times or saving up for profitable investments.

Features such as transaction costs, liquidity, and interest rates influence the take-up and usage of financial services. But other factors matter, perhaps in some cases even more. In an experiment in South Africa, Bertrand et al. (2010) tested the relative importance of interest rates, marketing features, and choices on a direct mail solicitation to consumer borrowers from a regulated for-

profit microlender. They found that interest rates mattered, but that simple marketing ideas mattered even more. For instance, giving consumers only one choice on loan size, rather than four, increased the take-up of loans just as much as if the lender reduced the interest rate by about 20 percent. Even more striking, they found that if there was a photograph of a woman on the direct mail solicitation, this drove take-up, among both men and women, of the loans just as much as if the lender reduced the interest rate by about 33 percent. These are large effects, particularly given the industry focus on “important” characteristics, such as price.

What does this tell us? We need to pay attention to more than just the pure economics of the choices being offered. The way offers are presented can have just as much to do with take-up and usage as do the terms of the account. Recent work by Richard Thaler and Cass Sunstein has documented a plethora of examples of “choice architecture” in which the way choices are presented and structured may matter just as much, if not more, as the choices themselves (see Thaler and Sunstein (2006), and a popular press book, *Nudge*, (2008)).

Given that the presentation of choice matters, targeted research can help answer the question of how products and processes can be designed to most effectively assist poor people to accomplish their stated goals. Are there certain temptations, for instance, that individuals would actually prefer not to have? For example, do individuals find they purchase items, such as entertainment goods, that they later regret having purchased (when, for example, a health shock occurs)? Commitment savings accounts can help prevent consumption of goods that are later regretted. Altering “default” settings is another example of how product design can *nudge* individuals towards decisions they say they prefer. In most decisions, something must be established as the action that occurs if no alternative action is taken. For example, should a proportion of a remittance or paycheck be set aside automatically into a savings account?

In order to understand how product design might matter, we need to first understand something deeper about the psychology of the decision-making process and the household and societal constraints that lead to certain decisions and coping strategies. Furthermore, employing one design over another requires recognizing that there is rarely a “neutral” option: choices in product design will affect how the product is used, and by whom. Some users may benefit more than others in particular ways. As such, every choice about how to design and offer a product requires taking a normative stand on what will improve outcomes for individuals.

2 Constraints to saving

If people do not save as much as they wish they would, why not: what are the constraints? We think about these as demand-side and supply-side constraints, although clearly there is a relationship between the two.

It is useful to first note the high price the poor are willing to pay for savings services that they can trust. This shows clearly the high value they place on savings more generally, and thus the welfare improvement possible from identifying policies that provide savings options for the poor at lower costs. For example, a ROSCA is a common method of savings that provides zero interest income, and significant loss risk. Wright and Mutesasira (2001) document these risks for informal savings. Through a study of MicroSave data, they indicate that people with access to

the formal sector reported much higher savings than those without access. The percent of those reporting losses from formal mechanisms (15%) was much lower than reported losses in the semi-formal (26%) and informal (99%, albeit loosely defined) sectors. On average, clients in the informal sector lost 22% of their savings. Note that this includes informal savings such as livestock, which can die but also provide a potential return if they live. Thus this is not the ideal analysis to show that, holding returns constant, informal savings are riskier. In this light, the issue of allowing microfinance institutions to offer savings products should be considered in terms of relative risk, since savings are relatively safer at microfinance institutions than in informal mechanisms. Rather than dictate the decision of where the poor save, it is key to help them make informed choices by helping them understand the relative risk of semi formal institutions.

The popularity of deposit collectors also offers clear evidence of the demand for savings, and offers insight into some of the potential behavioral constraints on savings. In Ghana, for instance, individuals pay for informal deposit collection services through susu collectors who travel to individual homes or businesses at regular intervals in order to collect savings deposits. In some cases, payment for this service is high enough that individuals' real return on savings is negative. Aryeetey and Steel (1995) document this and discuss the basic structure of these services. The susu collectors collect deposits from customers (mainly women operating market stalls) every day and return the accumulated savings at the end of the month. They neither pay nor charge interest on the deposits but they keep one deposit per month as a fee. This 3.3 percent fee works out to a negative annual return of 54 percent for those who withdraw their deposits at the end of each month.²

Self and spousal control: arguments for commitment

These issues lead to a re-evaluation of the two views that dominate thinking around the financial behavior of the poor (Mullainathan and Shafir (2008)). One view positions the poor as rational individuals who are methodical and calculating in their financial decisions; the other positions them as impulsive and misguided. Mullainathan and Shafir present an alternative and more realistic perspective, that the poor are neither completely rational nor irrational. Just like everybody else they make good decisions some of the time, and rash or uninformed decisions at other times. They are subject to the same psychological biases as the wealthy; the main difference is that these behaviors have a more profound effect on the poor because of their narrower margins of error due to their adverse financial condition. For example, if a poor individual makes a mistake, it may lead to their telephone being cut off (or in a developing country, to not having sufficient funds for buying "load" on their cellphone). Not having telephone access may then lead to a problem at work, in which the worker is unavailable to call in. This then leads to loss of income, which then leads to further problems. A wealthier

² Despite the high cost Susu services are extremely popular: Aryeetey and Gockel (1991) reports 78 percent of market women in Ghana's largest cities using Susu collectors to save. Susu collectors lend too, but the risk to depositors seems limited: the collectors surveyed by Aryeetey and Steel lent only small amounts to a small portion of their client base. Borrowers are screened based on regular savings history, and the Susu collectors typically lend only half of a month's deposits to a given client. On average less than 10 percent of their portfolios were exposed to lenders.

individual may have made the same initial cognitive error, but it simply did not reverberate and escalate to further problems in that person's life.

Putting psychology into conversation with economics changes the way we think about some basic relationships. Despite a preponderance of arguments for lowered transaction costs as a panacea to savings policy (Robinson, 2001), for example, behavioral theories suggest that higher transaction costs can, in some cases, be welfare enhancing. The problem lies with limited commitment problems, in which individuals want to make certain future decisions but face constraints from their own weaknesses, or constraints from others. Beginning with Strotz (1955) and Phelps and Pollak (1968), theoretical models have been put forth that describe outcomes that arise when there are inconsistencies between current temporal tradeoffs and future temporal tradeoffs. These models often incorporate hyperbolic or quasi-hyperbolic preferences (Ainslie, 1992; Frederick et al., 2001; Laibson, 1997; O'Donoghue and Rabin, 1999), theories of temptation (Gul and Pesendorfer, 2001 and 2004), or dual-self models of self control (Fudenberg and Levine, 2005) to generate this prediction. They all share one consistent prediction: individuals should exhibit a preference for restricting their future choice set, and being able to do so will increase their ex-ante welfare. Naturally, this requires that individuals are self-aware enough to volunteer for such restrictions (much like Odysseus tying himself to the mast to avoid the tempting song of the sirens).

Laibson (1997) looks at decisions made by a consumer with access to illiquid assets – these are assets that generate substantial benefits in the long run, but no immediate benefits (“golden eggs”). The theory suggests that financial innovation, which increases liquidity and reduces implicit commitment opportunities, could have led to the lower savings rates in the US. The model implies that financial market innovation may reduce welfare by providing “too much” liquidity. We revisit the implications of this work below when we discuss technological innovation that lower transaction costs for savings.

Evidence of such preferences are often difficult to show, but a growing literature is demonstrating that demand exists for such restrictions, and in some cases firms are responding by offering such services. In the United States, Christmas Clubs, popular in the early 20th century, committed individuals to a schedule of deposits and limited withdrawals, typically with zero interest. In more recent years, defined contribution plans, housing mortgages, and tax over-withholding now play this role for many people in developed economies (Laibson, 1997). For example, on the Earned Income Tax Credit, many individuals do not take advances, effectively an interest-free loan to the government, potentially as a costly commitment device to save (see Jones, 2009).

In developing countries, informal institutions have arguably played this role for years. Many theories exist to explain the presence and structure of rotating savings and credit organizations (ROSCAs), but one commonly held belief is that they provide a form of commitment from your future self (Gugerty, 2007) or from your spouse or extended family (Anderson and Baland, 2002). In Gugerty's work from Kenya (2007), qualitative and quantitative evidence from 70 ROSCAs in rural Kenya was consistent with a self-control commitment story: nearly 60 percent of the ROSCAs had an explicit spending agreement, for which members were required to identify in advance their purpose for the pot of funds when their turn is up. Members would

verify each other's purchases to ensure they adhered to the intended purpose (though most used the money for more than one purchase). Drawing causal conclusions from the data is difficult because in practice new members had limited choice in selecting a ROSCA. While spending-agreement ROSCAs show higher savings rates, they also tend to be composed of wealthier members, often with formal-sector income. Much of the qualitative evidence revealed individuals discussing inability to 'save alone.' Married women were *not* more likely to participate than non-married, nor were women whose husbands lived on their compound more likely to join. This suggests that the 'save alone' reference is fundamentally about one's own ability to save, or about claims made by non-spouse family or neighbors.

ROSCAs also have been shown to be spousal control tools, not just self-control tools. In Anderson and Baland (2002), we learn from Kenya (although a more urban setting than the Gugerty paper discussed above) that women with some but not all of the household income are more likely to participate in a ROSCA than those with all or none. The paper works through the following model. It assumes a husband and wife differ only in their preference for an indivisible good, for which the wife values the good, and the husband does not. Assuming that the husband will respect the ROSCA institution (i.e., a man cannot punish a woman for joining the institution, presumably due to a societal norm), then women who need to extract funds from the household in order to save up for an indivisible good will find the need to join ROSCAs. Women who earn all of the income presumably have power, and thus under this model, no need to join a ROSCA.³ Women who have none of the household income, on the other hand, have no control over any income flows, and likewise are unable to join ROSCAs because they are unable to commit to any future deposits. This parabolic relationship is exactly what Anderson and Baland find when trying to predict which type of wives join ROSCAs. Naturally, these two stories are not mutually exclusive, and commitment devices in general can work for very different reasons. Understanding the relevant importance of these models may have important policy implications, such as how to design marketing and account access rules.

The need for commitment from others is not limited to spouses. Platteau (2004) examines the way in which egalitarian norms may inhibit personal savings, and thus growth. In some societies successful individuals are called upon to transfer some of their own wealth (directly and indirectly through transfers and favors) to benefit poorer community members and kin. In cases where the wealthy do not consider these to be legitimate contributions, this even serves as a disincentive to work hard. These individuals may also resort to a number of alternate strategies to hold their wealth that involve high transaction costs, in an effort to reduce demands on their income and wealth. These behaviors result in inefficiency that inhibits economic progress and reduces saving levels.

Whereas these studies argue that indigenous institutions have been formed in order to satisfy a demand for commitment, they leave open the question of whether stronger, more formalized commitment devices could succeed in both attracting the right individuals and helping them to adhere to their stated preferences. In the context of Thaler and Sunstein (2008), formal institutions can "nudge" individuals via product framing and design towards decisions they claim

³ Two alternative stories are worth mentioning. Women who have all of the income may also be too busy for the ROSCA, if the ROSCA is time consuming, and women who earn all of the income in the household may also be better financial managers, less likely to have self-control problems, and thus also less likely to join a ROSCA.

to want. For example, Ashraf, Karlan and Yin (2006b) designed a commitment-savings product, called SEED, that provided clients with a commitment to not withdraw their funds until a goal was reached. SEED clients voluntarily restricted their right to withdraw any funds in their own accounts until they reached a self-specified goal. Clients could opt to restrict withdrawals until a specified date (e.g., in a month when school fees were due), or until a specified savings amount was reached (e.g., a certain amount of money for a new roof). The clients had complete flexibility to choose which of these restrictions they would like on their account. However, once the decision was made it could not be changed, and SEED clients could not withdraw funds from the account until they met their chosen goal amount or date.

To evaluate the impact of this new product the bank implemented a randomized control trial where it assigned individuals to either receive an offer to open the SEED account or not, or a third group which received a marketing pitch about the importance of setting goals for savings (but no access to a new commitment savings account). Among those offered the account, 28 percent opened one, and, importantly, those who exhibit inconsistent time preferences in survey questions about hypothetical alternatives were more likely to open an account.⁴ After twelve months, average balances increased by 80 percent in the treatment group (i.e., those who got the SEED offer) compared to the control group. The account offer was also associated with a significant increase in women's decision-making power within the household (as measured both qualitatively by asking who has power over certain decisions, as well as more objectively by observing the gender-bias of durable goods purchased) (Ashraf et al., 2010).

A question remains, however, to what extent such commitments are about binding one's behavior, or are in fact merely about creating structure. The previous section described how those wanting to save might opt for credit simply because it provides a defined schedule for making regular deposits. Karlan et al. (2009) test the effects of simply making savings more salient by sending clients simple reminders to make deposits. They find even with no commitment, the reminders can be successful in increasing savings rates (by 6%) and helping clients meet savings goals (a 3% increase in the likelihood of reaching one's goal). Similar positive impacts on savings were found by a deposit collection services tested in Ashraf et al. (2006a), as well as Dupas and Robinson (2009).

These ideas are gaining acceptance within academia. They are also having impacts in practice. Grameen Bank of Bangladesh, for example, has launched a successful commitment savings device, a "pension" product that requires monthly deposits in fixed amounts and returns savings (with interest) after five or ten years (depending on the product). The savings account is a "pension" in name only, and while it is used to pay for old age, households also use it to accumulate for housing improvements, wedding expenses, migration and the like (Collins et al. 2009, chapter 6).

⁴ Interestingly, this preference for commitment was strongest amongst women. The study did not provide data to help understand heterogeneity across gender, and thus future work on this issue would be fruitful. It is suggestive of an important interaction between gender and the preference and need for commitment contracts, as discussed here.

Planning and financial literacy

A preponderance of evidence shows that financial illiteracy is prevalent around the world, and is correlated with low savings. However, this begs the policy question of whether interventions intended to increase financial literacy can in fact lead to changes in behavior of importance. Evidence is clear that people, everywhere, are financially “illiterate” by many definitions, both described and measured by lack of basic numeracy (e.g., simple compounding), financial knowledge (familiarity with financial products, including credit, savings, and mortgages), and financial planning (e.g., saving for retirement). Lusardi (2007) shows that financial illiteracy even in the US population is widespread, and particularly acute for specific demographic groups, such as those with low education, women, African-Americans, and Hispanics. Moreover, close to half of older workers do not know which type of pensions they have and the large majority of workers know little about the rules governing Social Security benefits. Notwithstanding the low levels of literacy that many individuals display, very few rely on the help of experts or financial advisors to make saving and investment decisions.

Naturally, the mere correlation of financial illiteracy with outcomes such as savings decisions does not imply that financial illiteracy is the cause of the low savings. Several endogeneity issues could be at play, including the simplest, that omitted variables such as motivation to succeed are the true cause of both financial illiteracy and low savings. Or, reverse causality: if one is incapable of saving in quantity then one would be unlikely to invest in knowledge of savings vehicles. Examining the causal impact of financial education programs requires effective methods for establishing the counterfactual.

Two studies on financial literacy examine this very issue, and draw strikingly different conclusions. This demonstrates the difficulty of establishing attribution in evaluation of public policies. The first paper, Bernheim, Garrett and Maki (2001), uses variation in state-mandated financial education to measure the treatment effect of financial literacy training on household savings. Using data from Merrill Lynch, and a telephone survey of 3,500, the authors employ a difference-in-difference approach and assume that timing of the introduction of state-mandated financial education is exogenous. They conclude that the mandates were effective in teaching basic financial literacy, and led to a 1.5 percentage point higher saving rate. However, it turns out there is evidence against their key identification assumption. Cole and Shastry (2008) extend this study with more data and conclude that the Bernheim et al. result was spurious. Census data, and thus a larger dataset, allows for the inclusion of state fixed effects to control for unobserved, time-invariant heterogeneity in savings behavior across states, as well as non-parametric identification of the treatment effect itself (rather than a linear measure of years-since-mandate-began employed by Bernheim, Garrett and Maki). Once these three enhancements are implemented, all treatment effects fall to a precisely estimated zero, thus both eliminating the conclusion that financial literacy as implemented under this program had any effect, and demonstrating a key endogeneity issue that plagues this literature (that rollout of programs is responsive to demand and thus extensive work must be done to create convincing counterfactuals).

Savings, as we have seen, is important to poor households, important enough that they are willing to pay for the service. This commitment to save has led some practitioners to conclude

that savings, rather than credit, is the more practical strategy to promote, especially for the very poor. This is surely true for some households, but is strongly contested by the evidence in Collins et al. (2009).

There are some settings where the poor do not save, even where it is clearly advantageous for them to do so. Why haven't more households saved their way out of credit constraints? Such puzzles suggest that large-scale savings promotion may require a more sophisticated strategy than imagined initially: it will involve better research to understand why the poor do not save when they could, and which strategies can help overcome these barriers.

A related puzzle concerns entrepreneurs who borrow persistently, not for a one-time business expansion but for routine working capital. This is very expensive. In extreme examples vegetable vendors in India are known to borrow small sums each day to purchase vegetables, repaying each afternoon from their daily sales. They pay rates as high as 10 percent per *day*. Among a sample of vendors in Chennai, 50 percent claimed to have been engaging in this type of borrowing for at least ten years! What is so interesting about such borrowing cycles is that it is easy to show that in principle by saving merely one rupee (a few cents) each day (and borrowing that much less) the vendor could be debt free, and able to finance her own working capital, in just 50 days. From there her returns are enormous: her daily profit margin is boosted by the ten percent she was paying to the moneylender. So why do the vendors persist in borrowing at such rates? The answer remains unclear but financial education seems a good place to start. If they understood the true cost of their borrowing, they might well be convinced to save.

In India and the Philippines, Karlan and Mullainathan (2008) explore the impact of debt payoff and financial education on the persistence of high interest debt. These experiments address the aforementioned endogeneity concerns by randomly offering indebted vendors different experimental treatments. Specifically, some vendors are offered an unexpected debt payoff, others financial literacy training emphasizing the utility of savings to finance business expansion and the cost of debt (the training itself was a brief but focused 30 minute session), and a third group are offered both debt payoff and financial training. A fourth group serves as a control group and thus received no experimental treatments. The evidence from the Philippines suggests that debt payoff can have immediate but then dissipating effects on vendors' reliance on high-interest loans: vendors who received one of the payoff treatments were 31 percentage points less likely to have money lender debt almost one month after the payoff and 17 percentage points less likely to have taken a high-interest (defined as greater than 5 percent per month) loan after three months. Preliminary results showed that the effect dissipates over time, however, as individuals gradually go back into debt.

Of course a measured reduction in household consumption is not necessarily a good thing. More research will be required to look deeper into the specifics of how household consumption decisions change as a result of greater financial literacy. Are households choosing to save rather than spend excess income? Are they making immediate sacrifices in the interest of financing longer-term business goals? Another question that remains unanswered is whether the impacts observed in the debt payoff and financial literacy training experiment can be multiplied by providing high-interest borrowers with an effective savings mechanism. These and other

questions require further research to disentangle how product offerings can change the cash management and thus savings and consumption decisions of the poor.

Pricing

Just as we discussed earlier with respect to credit, many have assumed that because the poor are willing to actually pay to save, this implies that their demand is inelastic with regard to price. Naturally one does not imply the other, since willingness to save at zero or negative real interest rates simply means that at that price demand is positive, but at higher prices of course demand could still be higher. Hirschland and Owens (2005) provide a useful overview of a typical practitioner's perspective on the considerations for how to set price for savings, including competitive analysis and cost drivers. Clear information on elasticities, however, would help tremendously.

In Karlan, Mullainathan and Zinman (2008), just as in earlier work on credit elasticities, the authors conducted a series of field experiments in which the interest rate on the savings account was randomized, and data collected on how this influenced the decision to open an account as well as the volume of savings held in the account. Specifically, the treatments tested are high, low, and “reward”, in which the high rate was 1.0 percent per annum higher than the low, and the bonus was also 1.0 percent but only awarded if the individuals savings goal amount was reached by their goal date. In the second experiment, the design was slightly modified, and the increase in interest rates and bonus was 1.5 percent per annum instead of 1.0 percent. While they find that the product take-up rates among the high-interest group are higher than that of the low-interest group in both experiments, and both lead to 3 percent higher savings balances, still after 7,000 offers (with an average take-up rate of 23 percent and average balance of about US\$10), neither of those results are statistically significant.

Loss aversion and mental accounting

The concept of loss aversion is well established in our understanding of human behavior but not well adopted in the design of savings products. The 2002 Nobel Prize in Economics was awarded to Daniel Kahneman for, among other ideas, the simple demonstration of this. The canonical classroom experiment involves mugs (Kahneman et al., 1990). Half of the students are randomly selected in a lottery to each get a mug. Everyone in the class is then given the opportunity to trade. In an “efficient” world of no loss aversion, the half with the highest valuations should end up with the mugs, irrespective of whether or not they “won” the lottery. But winning the lottery changes one’s valuation of the mug. Why? Because once the mug is won, the reference point is shifted. Giving up the mug is now a loss, whereas, for everyone else acquiring the mug is a gain. Losses loom larger than gains, and few people trade. Those who won the mugs mostly fail to find someone willing to pay them enough to make them part with their moments-ago-won mug. Instead, if the professor asked for students to announce their valuation *before* receiving the mugs, then handed them out and made everyone adhere to their stated valuations, one would expect, on average, half of the mugs to be traded.

Now let’s apply loss aversion theory to the decision to make a deposit into a savings account. What is a savings deposit in a mental accounting system? It is trading off some current (salient)

consumption for some future (nebulous) consumption. Ignore the timing for a moment (more on that below), and what does one have? A sure loss, in exchange for an unclear gain. Thus loss aversion may drive the individual to consume, rather than save.

How does one tackle this? Can product design effectively convert the savings deposit into a gain? Some of the problem is driven by the vagueness of the future gain. Perhaps making the future gain more salient, one can convert the savings deposit into, at a minimum, a “neutral” and, at best, a “gain.” Ashraf, Karlan and Yin (2006), in the study in the Philippines discussed earlier, employed a “placebo” treatment involving marketing only, in which bank marketing agents visited homes of prior and current clients to encourage them to consider setting savings goals. This treatment was intended in that study as a placebo, in order to make sure the treatment effect from the commitment savings product was due to the mechanism design of the product, and not the labeling and goal setting promotion. In fact, the impact of the goal-setting treatment (referred to there as “marketing” treatment) was positive, although not statistically significant. Labeling can also matter. In developed countries, we have seen accounts “labeled” through marketing (and tax advantages): savings accounts for *education*, savings accounts for *health*, savings accounts for *retirement*. These ideas do not require fancy infrastructure, but rather mere marketing and packaging. Does account labeling allow one to make the later gain from saving more salient in the present, and thus cancel out the loss from foregoing current consumption? This is an open question.

More generally stated, one could imagine many methods for converting the “loss” of the foregone consumption due to making a deposit be converted to a gain. Doing so relies heavily on the *lack* of fungibility of money, a violation of most traditional models in economics. Mental accounting, as put forward by Thaler (1985) provides a framework to interpret such behavior.

Default options

Extensive evidence exists to show that default option matter. This is true in many facets of life, and savings behavior is no exception. Madrian and Shea (2002) document this clearly for individuals making retirement decisions in the USA. There, Madrian and Shea show that setting automatic enrollment as the default participation in retirement plans leads to a 50 percentage-point increase in the likelihood of participation due to automatic enrollment, and similarly large effects are found on the type of investments individuals choose.

A more complex example comes from Thaler and Benartzi (2004). Here, the authors implemented a program called “Save More Tomorrow™” (SMarT) in which individuals weakly commit (by “weakly”, we mean they can reverse this decision at any time) a portion of their *future* salary increases toward retirement savings. The paper cited above reports findings based on evidence from the first implementation of the SMarT program, through four raises. Key findings are (1) a high proportion (78 percent) of those offered the plan joined, (2) the majority (80 percent) remained in the program through the fourth raise and (3) the average savings rate increased from 3.5 to 13.6 percent over 40 months. This program has now been adopted for many corporate retirement plans in the United States. In a survey of 146 employers in the United States, Hewitt Associates found that 31 percent of plans have an automatic escalation program,

and of the 69 percent that do not, 42 percent said they plan to incorporate such a component into their plan (Hewitt, 2007).

Adapting these ideas to developing countries has tremendous promise, and also could provide ample opportunities for learning *why* this works more precisely, as several factors were employed at once in the design of SMarT. First, since upon sign-up, the default future decision would be to save more, action had to be taken to change this decision. Second, by framing the future increase as “coming out of your next pay increase” (even if the next pay raise was merely an inflation adjustment), money illusion may have led more to sign-up than would otherwise have occurred. Third, the method of presentation, and the skills of the advisors, may have influenced the decision of individuals to participate.

Marketing

Often design and discussion about products by academics focus on the terms and structure and risks of a product. Yet, in many cases, the presentation, framing, and promotion style influences the outcomes. For example, in Landry et al. (2006), in a door-to-door marketing of a fundraising appeal, the authors found that the physical attractiveness of the door-to-door salespeople was far more important than the lottery that was being offered to some but not others. Similarly, in developing country setting, Bertrand et al. (2010) find that adding a photo of a woman to a direct mail solicitation increases the likelihood of borrowing by just as much as dropping the interest rate by about 30%. Al-Bagadi and Cracknell (2005) discuss this more generally in the context of marketing and promotion of savings in developing countries. They argue that microfinance institutions must translate ideas about why potential clients should want to deposit with a particular institutions into a message that motivates them to do so. We suggest that future studies that examine product features as discussed above do not forget to think about the promotional strategy, and ideally incorporate the promotional strategy as an integral part of any study.

Social networks and peer effects

If the poor have limited knowledge of the benefits of saving, then learning about these benefits through social networks could potentially have important impacts on the poor’s savings behavior. Despite its relevance, not much research exists on the role of peer effects in savings decisions. Peer effects are notoriously hard to identify empirically. In many instances individuals’ decisions within a social group are correlated for reasons that have nothing to do with social learning or social imitation. Behavior of individuals may be correlated simply because individuals in the same group have similar unobserved characteristics – e.g. a common propensity to save – or share a common environment. Manski (2003, 2005) lays out some of the difficulties involved in identifying peer effects.

In an empirical contribution, Duflo and Saez (2002) investigate the role peer effects play in retirement savings decisions in the United States. They study whether the decisions of employees of a large university to enroll in a university sponsored Tax Deferred Account plan are affected by the decisions of colleagues in the same department. Instrumenting average participation within peer groups by average salary or tenure and looking at subgroups of peers

within departments, Duflo and Saez find evidence that the individual participation decision is influenced by the decision of one's peers.

Assessing the importance of peer effects in savings decisions matters for the design of policy interventions. If innovations in savings behavior spread through social networks, the impacts of financial education efforts can be much larger than the impacts on the financially educated. An initiative that aims to make effective use of learning from peers to promote savings is Oxfam's Saving for Change program, based on a model used in Nepal and now replicated in Africa. Saving for Change groups are informal self-managed saving and credit groups consisting of 25-30 women. Group members meet weekly to save a predetermined amount and the collected funds are used to make loans to group members. Since loans are repaid with interest, the fund grows over time. At the end of the cycle the fund is divided according to each member's share in the savings. The program has been found to rapidly reach considerable numbers of women. The first group in a village is typically trained by an external agent with the members of the first group then forming and training subsequent groups in the village. The role of social networks and peer effects in the spread of these saving and loan groups is the object of ongoing research.

Peer influences need not work merely through one-on-one interaction. For example, as Garon (2004) points out, East Asian states played a key role in inculcating savings habits and thrift amongst their citizens. Asian values of savings and consumption were created by a regionally adopted model of state promotion of savings, with Japan's colonial presence in these countries acting as one of the major catalysts to these state efforts. The history of the "Japanese Model" includes national campaigns to promote savings, postal savings banks, and a Central Council for Savings Promotion. This model was exported to varying degrees to South Korea, Singapore, and Malaysia. Garon argues that though thrift and savings are not timeless or unique Asian values, many Asian people have come to embrace these as part of their national identities. This begs the question, though: are savings levels cultural, driven by social norms and macro-level policies, or simply the product of one huge omitted variable, e.g., institutions.

Technology

There is a massive effort underway to harness technology to overcome the transaction costs that have prevented the poor from accessing banking services, especially in rural areas. Technology also appears to be revolutionizing the remittance industry, as technology solutions are making remittances easier and cheaper than ever before. As discussed earlier, remittances historically, and still today in many countries, are expensive as a proportion of money sent, particularly for small amounts. Yet unlike credit there are no information asymmetries or default risks to explain these costs, and hence technology solutions are proving to have big impacts on the costs consumers are offered.

The donor consortium CGAP has launched a campaign to bring mobile banking services to 25 million low-income people by 2012. It is early to tell what form these services will ultimately take, but right now attention is focused on point-of-sale devices installed at retail agents (e.g., local stores) who can accept deposits or payments, as well as withdrawals; and banking via mobile handsets, enabling customers to transfer funds electronically. The explosion of accessible technology, no doubt, will change whether and how people interact with banks. This

may have important unintended consequences. As we discussed above, liquidity can actually be undesirable for some. Could electronic banking lead to further problems with self-control and spousal, familial, or community pressure? If hidden savings are not offered in conjunction with increased access to funds through technology, such pressures could lead to worse outcomes. As transaction costs drop, this will become an even more important area for future research and innovation.

Furthermore, much of the advent of microfinance has been through innovations that remove layers of information asymmetries so that banks can profitably lend to the poor (and for microinsurance, so that they can insure the poor). Removing the human touch from banking may have unintended adverse consequences, in that individuals will reach for the “easy” technology but in the process lose the interaction with the credit officer that is necessary for the financial services firms to establish relationships and lend and insure. The personal relationship with a banker may reduce information asymmetries both by allowing the bank to have more information for screening, and also by reducing moral hazard, by instilling a sentiment of reciprocity or personal loyalty in the mind of the borrower, making them less likely to engage in moral hazard.

Thus, technological innovations in the user-interface are promising for radical reductions in cost of accessing savings services, but we need to understand better how behavior will change as a result of this easier access. In a quasi-experiment from the field, albeit not in this context, consumption on a particular good (sorting laundry into two washing machines or pooling into one) increased by 50% as the payment mechanism shifted from cash to prepaid cards (Soman, 2003). How will mobile “minutes” be treated, as cash to save or money to spend? This simple mental categorization could have serious implications for the long-term impact on consumption and savings from such technologies.

Early experiences with branchless banking are already showing clients are using the new technologies largely for payment transactions, rather than savings or credit. To some extent this may be the result of marketing campaigns by mobile operators which focus on the transfer of money rather than the storage of money—apparently because the operators are concerned about appearing to market themselves as banks and thus attracting notice by regulators. Despite the great potential for branchless banking to expand financial access, thus far mobile banking customers in developing countries have been wealthier customers in urban areas (Ivatury and Mas, 2008).

3 Impacts of saving

The impact of savings programs can be difficult to measure, both because savings is hard to capture in survey data and because it is hard to isolate savings from other financial services: few institutions offer *only* savings. Three factors particularly complicate measurement: size, timing, and diffusion. Unlike credit inflows, which can be sizable relative to household income, savings flows can be quite small, and balances accumulate slowly. Also, the timing of the change in behavior and outcome is less clear. For households, savings develop slowly through a small reduction in consumption over time, with a large inflow later. At some point the household will

have built up enough savings to protect themselves from shocks (like sickness or unemployment), to pay school fees, or to start a business. But when is that point? It may not be a simple question of waiting for savings to accrue: household cash flows may vary over time. Researchers need to measure savings balances at multiple points in time, often over several years.

The last issue, diffusion, is perhaps the most important. Poor households save in all sorts of different ways. In addition to a formal savings institution they may save cash at home, with deposit collectors, within a savings club (such as a ROSCA), or by lending to family members. They may also save in non-cash assets such as jewelry or livestock. An evaluation that failed to capture these different savings vehicles could understate savings rates, potentially leading to incorrect conclusions about the impact of the program. Or, an evaluation which focused on just one savings channel may incorrectly conclude that net savings goes up, whereas in fact the only impact was a shift from one type of savings to another.

As with credit, it is problematic to compare savers to non-savers. Savers may be better educated or have more disposable income. They may be better planners, or more risk averse. There might also be reverse causality: healthy people might be able to earn more, or spend less on hospital visits, increasing savings balances. Given these issues it is unsurprising that there are few rigorous evaluations of savings. Burgess and Pande (2005), described in the previous section, solve the identification problem, showing that financial access reduces poverty, but they are unable to separate the effect of savings from credit.

Aportela (1999) evaluates the impact of a government savings program in Mexico, Patronato del Ahorro Nacional (Pahnal). In 1993 Pahnal decided to expand its savings operations through the postal office network. By using the postal offices, Pahnal was able to locate close to many people who were otherwise inaccessible, but without the fixed cost of opening a vast network of offices. As part of this expansion, Pahnal offered two savings options: (1) a fixed-term instrument (12, 24 or 36 months) with compulsory monthly deposits of five dollars, and inability to withdraw until maturity, and (2) a liquid savings account, with minimum balance of just over five dollars, with no fees but lower interest than the fixed term. Aportela uses the partial expansion of the program to compare the change in outcomes for those in communities that received the Pahnal expansion to the change in outcomes for those in communities that did not receive the Pahnal expansion. The analysis uses data from the 1992 and 1994 Mexican Household's Income and Expenditure surveys. The advantage of these data, rather than using client data, is that there is no individual selection bias. Given that the program chose its own expansion path, however, it is critical to verify that preexisting savings rates or poverty rates are not correlated with the treatment communities. Aportela finds no correlation, nor any evidence of an operational plan that could have confounded the analysis (e.g., an initial expansion into districts that had expressed high demand for the program). The expansion appears to have more closely followed Pahnal's operational convenience i.e., proximity to its prior branches. Hence an issue remains as to whether Pahnal's prior branches were located strategically such as to create a preexisting trend and thus selection bias.

The impact on formal-sector savings balances was noticeable: the average savings rate increased by 3 to 5 percentage points. For low-income individuals it was even higher: an increase of 5.7 to

8 percentage points. The emphasis of the analysis in the study is on formal sector savings volume, broken down by different income levels. The attempt to measure aggregate savings, including informal savings, fails to reach decisive conclusions because of lack of statistical precision and data limitations: the analysis is unable to rule out substitution from other savings vehicles. Moreover, it does not allow one to isolate which of the two products, or within either product which particular feature, led to the impact. Whether the increase in formal savings represents an increase in net savings or a shift from informal to formal savings it can be considered a positive impact in that the formalization of savings implies safer savings for individuals.

Impact from specific product trials is more limited. The evidence discussed earlier, from Ashraf, Karlan and Yin (2010), showed a potentially good relationship between savings and empowerment. The commitment savings product there led to a significant increase in women's decision-making power within the household (measured by an index of decision-making authority over various types of purchases, as well as family planning and children's education), and an increase of the purchase of female-oriented durable goods. The impacts were particularly strong for women who have below median decision-making power in the baseline data.

Dupas and Robinson (2009) worked with a community bank in rural Kenya to provide incentives to open a savings account to randomly selected entrepreneurs, for whom the researchers paid the fee to open the account and provided the minimum account balance. The control group received no incentives but were not barred from opening an account. The incentives were strong enough that 89 percent of the treatment group opened an account while only three individuals in control group did so. The researchers find remarkable impacts despite substantial transaction fees charged by the bank (\$0.50 or more) and the fact that many never used the account after opening it. In contrast to the Karlan and Zinman (2009) study of the impact of credit in the Philippines and de Mel et al. (2008) study of returns to capital, here the impacts are found only among female entrepreneurs. Four months after opening the account women show 40 percent increases in productive investment, and after six months daily consumption is approximately 40 percent higher than the control group. In this study however women have different enterprises than men, and hence the gender difference may be properly interpreted as a difference generated by occupational choice. Further research and expansion can help understand this important result.

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