Courant Research Centre 'Poverty, Equity and Growth in Developing and Transition Countries: Statistical Methods and Empirical Analysis'

Georg-August-Universität Göttingen (founded in 1737)



Discussion Papers

No. 226

Do Saving Promotion Interventions Help Alleviate Poverty in Sub-Saharan Africa? A Systematic Review and Meta-Analysis

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April 2017

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April 12, 2017

Abstract

Saving promotion interventions have gained momentum in international development over the recent years. Our analysis investigates whether saving promotion can effectively reduce poverty and economic hardship in Sub-Saharan Africa. In an extensive database search, 9330 records were screened and 27 randomised controlled trials on saving promotion interventions fulfilled the inclusion criteria. Robust-variance estimations of pooled effect sizes show small but significant impacts on poverty reduction, including increases in household expenditures and incomes, higher returns from family businesses, and improved food security. They also show positive impacts on more intermediate outcomes including total savings, pro-saving attitudes, financial literacy, and investments in small-scale family businesses. Our results do not show significant effects on assets, housing quality, education, or health. Findings from this analysis suggest that saving promotion schemes are highly relevant in reducing poverty in Sub-Saharan Africa, and that formal banking services in particular require adaptation to the needs of the poor.

1 Introduction

Saving has become an important mean for sustainable cash-flow management and consumption smoothing for the poor (Karlan, Ratan & Zinman, 2014). In response, scholars and practitioners alike have promoted saving programmes as a promising poverty alleviation strategy for international development. Savings can serve as investment capital, for instance for business, education, or job search (Curley, Ssewamala & Han, 2010; Karlan et al., 2012; Dupas & Robinson, 2013a; Karlan & Linden, 2014; Flory, 2016), as self-insurance against health shocks and property damage (Dupas Robinson, 2013b; Carter, Laajaj & Yang, 2015), and help to smooth consumption over income contingencies (Brune et al., 2011).

Vis -à-vis other financial planning tools, saving can strengthen a feeling of self-efficacy and self-worth instead of creating dependency (Ssewamala et al., 2016; 2009) and does not hold the risks of clients' indebtedness and defaulting (Hulme, Moore & Barrientos, 2015; Karlan et al., 2014; Duflo et al., 2013; Berg, 2010; Stewart et al., 2010). More importantly, saving promotion can be a cost-efficient alternative to poverty reduction strategies, such as cash transfers, and microloans as it leverages on the management of existing resources instead of the infusion of large sums of external capital.

It remains yet to see whether saving promotion interventions are truly effective in household economic strengthening and poverty reduction. Over the recent years, randomised controlled trials (RCTs) were widely used to investigate the effects of savings interventions. Many of these have focused on the Sub-Saharan African region where a high percentage of people still live below the poverty line and where we observe the lowest penetration of formal financial services worldwide (World Bank, 2016b; Demirguc-Kunt & Klapper, 2012). New insights on the viability of saving promotion in reducing poverty are therefore highly relevant for designing adequate policies and programmes in this region.

Using state-of-the-art systematic review methodology and meta-analysis techniques, the aim of this study is to quantitatively synthesise evidence on the effectiveness of saving promotion in Sub-Saharan Africa. While a single study can optimally generate findings with high internal validity, a systematic synthesis across multiple studies offers a much broader critical evaluation and thus allows for more generalizable conclusions. The computation of aggregate effect sizes across studies can provide insights on how components of programme design, intervention types, and participant characteristics may influence outcomes beyond the explanatory power of a single study.

Our study complements the existing review literature by many important aspects. So far, three systematic reviews have been carried out to investigate the impact of general financial literacy programmes. Yet, these studies are not exclusively savings-oriented and include evidence from developed countries where context and participants exhibit a range of characteristics that differ to low- and middle-income countries (Kaiser & Menkhoff, 2016; O'Prey & Shephard, 2014; Fernandes, Lynch & Netmeyer, 2014). Three further reviews examined a broader range of programmes including microcredit interventions and self-help groups and therefore feature programmatic components that could impact poverty alleviation through channels other than saving (Stewart et al., 2012; Duvendack et al., 2011; Brody et al., 2015). A last review put exclusive focus on formal banking services, thus excluding a range of other saving interventions such as savings groups promotion (Pande et al, 2012). To our knowledge, the present review is the first to quantitatively synthesise evidence on a range of saving promotion interventions, with a special focus on Sub-Sahara African countries where the use of formal financial instruments is still scarce among the population. The present review contributes further, by providing new knowledge on the effectiveness of savings interventions on poverty.

Studies for our analysis were selected on three criteria: the intervention under evaluation solely featured a saving promotion component (e.g. access to formal bank accounts, savings groups, financial education on savings), was evaluated within a randomised controlled set-up, and reported impacts on saving- and poverty-related outcomes. With the first criterion, we ensure an accurate and reliable estimate of the effectiveness of saving promotion, excluding any intervention that combines saving promotion with additional components that could hypothetically have an impact on poverty, financial stress, or saving behaviour.¹ Second, our exclusive focus on randomised controlled trials, considered as the 'gold standard' approach to impact evaluation, aims to ensure high internal validity of considered studies in order to obtain reliable and valid effect size estimates. Lastly, we allow for a relatively wide range of

¹We therefore exclude programmes with components such as microcredit, insurance, mentorship, or cash transfers. We further exclude programmes featuring financial incentives to save, such as provision of monetary top-ups contingent on realised saving amounts (see e.g., Ssewamala et al., 2010a, 2010b, 2009). Incentivisation schemes are equivalent to a conditional cash transfer contingent on saving compliance and may therefore differ from other saving promotion interventions both for necessitating infusion of external capital and manipulating levels of household poverty through channels other than saving.

relevant outcome measures to gain a nuanced understanding of possible impacts. Our exclusive focus on programmes implemented in Sub-Saharan Africa not only targets a region where a better understanding about savings intervention is warranted, but also allows to limit heterogeneity of settings and populations.

Our results show that saving promotion interventions do help households in Sub-Saharan Africa to accumulate savings and, more importantly, have trickle-down effects on poverty-related outcomes. Specifically, we show small but significant impacts on household expenditures and incomes, higher returns from family businesses, and improved food security. Our results to not show significant effects on household assets, housing quality, education, or health.

The remainder of this paper proceeds as follows. The next section discusses the theoretical literature on saving promotion interventions and their outcomes. Section 3 describes the data source and the measurement of variables. Section 4 introduces the statistical methods for effect size aggregation and meta-regression. The main results are presented and discussed in Section 5, Section 6 concludes.

2 Theoretical Framework

Individuals are likely to make sub-optimal financial decisions due to different existing obstacles. While some of these obstacles are faced by individuals across the globe, others are specific to the context of developing countries and therefore make financial management particularly difficult for people living in poverty. Often such barriers may lie in supply, demand or behavioural constraints and can help to explain why the poor tend to undersave.² The existing research literature therefore puts focus on identifying and overcoming such constraints in order to enable people to save. It further examines how increases in savings may help households to avoid or rise out of poverty.

² We define "undersaving" in line with Karlan and colleagues (2014) as "a lower level of savings than one would have in a world with perfect markets (perfect information, zero transaction costs, and perfect competition amongst financial institutions) and fully attentive, fully rational, fully consistent, etc., decision-making" (p. 38).

2.1 Saving Barriers

When formal saving opportunities are unavailable, individuals use second best options such as putting money under a mattress, keeping grain reserves, or buying jewellery, construction material, or life stock.³ In the worst case a lack of secure storage and the risk of theft, loss, and requests for financial assistance from relatives and friends may diminish the motivation to save altogether (Wright and Mutesasira, 2001). In response to this, a range of programmes have been developed to address *supply constraints* (see Karlan et al. 2014, Hulme et al., 2015; Lee et al., 2015; Brune, Giné, Goldberg & Yang, 2011; Mendoza & Thelen, 2008). These programmes provide, for instance, access to formal bank accounts at no or subsidised costs (e.g. Prina, 2015; Pande et al., 2012), or introduce mobile banking schemes to overcome physical distance to bank branches (e.g. de Mel, Herath, McIntosh & Woodruff, 2012). Programmes may further distribute simple savings devices such as lock boxes or mobilise savings groups in order to make saving more secure for poorer households (Dupas & Robinson, 2012).

Other interventions focus on educational or motivational elements to attenuate *demand constraints* that hinder individuals to build savings (see Dupas, Keats, Robinson, 2016; Karlan et al., 2014; Brune et al. 2011). For instance, interventions that emphasise financial literacy may counteract lack of trust in financial institutions and help increase knowledge of the procedures required to open a bank account. These hypothesise that financial knowledge is an antecedent to healthy financial decision-making and that increases in financial literacy will ultimately increase savings (Fernandes, Lynch & Netemeyer, 2014; Karlan et al., 2014; Karlan et al., 2014; Karlan et al., 2014; Soman & Cheema, 2011) in order to increase the uptake of savings products as well as to increase savings.

Finally, a growing body of literature discusses how savings commitment tools can help to work against *behavioural constraints* (Karlan & Linden, 2014; Giné et al., 2012; Brune et al.,

 $^{^3}$ Wright and Mutesasira (2001) report the odds of savings loss comparing various saving "technologies" for Uganda. While the risk of loss was similar for savings kept at banks (15% loss in the last 12 months due to bank break down) and in cash (13% of cash savers lost savings in the last 12 months due to theft), they were worse for in kind savings (25% of savers lost savings due to theft and 25% due to drop in value of saved item due to price fluctuations), which, at the same time, was the most popular savings strategy. In addition, maintaining cash at home was substantially harder than, e.g. at a formal institution, caused by temptation of petty consumption and assistance requests from relatives and friends.

2011; Ashraf, Karlan & Yin, 2006). Commitment tools can take the form of automated withdrawal and transaction regulations in formal banking (e.g. Dupas & Robinson, 2013b; Ashraf, Karlan & Yin, 2010) or of self-established regulatory frameworks (such as in savings groups) that make violations costly through feelings of failure, guilt, and social reputation (Soman & Cheema, 2011; Benabou & Tirole, 2004). The primary function of saving strategies involving group pressure or commitment devices is to increase individuals' self-control and/or limit immediate access to reduce the purchase of temptation goods and present-biased decision-making (Fiorill, Potok & Wright, 2014; Banerjee et al., 2005; Strotz, 1965).

2.2 Intermediate and Distal Outcomes: Savings and Poverty Alleviation

Existing RCTs on saving promotion programmes have applied a broad range of outcome measures. Studies have thereby primarily focused on *intermediate outcomes*, for instance by observing increases in savings and financial literacy levels. However, our analysis intends to move beyond the short-run impacts of saving promotion and investigate its wider, and in particular, longer-term welfare implications. We therefore draw on a body of literature that sheds light on the downstream impacts of increased savings on a range of *distal outcomes*, including consumption, education, and health.

Research on the link between savings and poverty alleviation has mainly focused on three causal hypotheses. First, it has been argued that saving can allow for the accumulation of larger lump sums of money. These may consequently serve as '*opportunity investments*' in productive assets, house repairs, children's education, higher quality food, or health care. This can have a positive impact on a range of poverty-related outcomes such as business profits, higher quality of education, nutrition, and health, as well as improved housing quality and asset portfolios (Rutherford & Arora, 2009; Collins et al., 2009; Stewart et al., 2010; Dupas & Robinson, 2009; Rutherford, 2000).

Second, savings can take the form of a *quasi-insurance* in face of unanticipated economic shocks and adverse events. For instance, illness or death of a household member may likely eliminate important sources of income and necessitate high expenses on medical or funeral costs. Savings can facilitate consumption smoothing by providing a buffer against emergencies and reducing alternative coping mechanisms such as fire sales of high-return assets, reduced food intake, borrowing at disproportionally high interest rates, or removal of

children from school (Hulme et al., 2015; Pande et al., 2012; Dupas & Robinson, 2009; Churchill, 2002; Barnes, Gaile & Kimbombo, 2001; Jacoby & Skoufias, 1997). In consequence, saving may increase resilience to economic shocks and reduce vulnerability to poverty (Klasen, Lechtenfeld & Povel, 2015).

Third, from a more psychological perspective, scholars have described how the earmarking of money for savings purposes can counteract a range of "behavioural anomalies". Once people dedicate a certain amount of their money to the purpose of saving, they are more likely to consider this money as unavailable for other expenses (Stewart et al., 2010; Dupas & Robinson, 2009; Rutherford, 2000; Thaler, 1990). Such *'mental accounting' mechanisms* can induce changes in consumption behaviour through decreasing the perceived immediate availability of cash. In consequence, time-inconsistent decision-making and the purchase of temptation goods become less likely. Consumptive spending can then be directed towards more future-oriented expense categories such as health, education, housing, or the accumulation of assets (Prina, 2015; Soman & Cheema, 2011; Banerjee & Mullainathan; 2010; Bryan, Karlan & Nelson, 2010; Ambec & Treich, 2007; Prahalad & Hammond, 2002).

3 Data

The database for this meta-analysis was built up by an intensive search and screening process of the literature on randomised impact evaluations of savings interventions in Sub-Saharan Africa, identification of relevant studies, and extraction of the respective measures. Data was collected according to the Campbell Collaboration's guidelines for systematic reviews.⁴

3.1 Database Search

In order to objectively identify and process all possibly relevant studies for our analysis, we carried out a comprehensive systematic literature search. We searched 28 electronic databases in the fields of economics, psychology, and social sciences to identify both academic literature as well as grey literature.⁵ In addition, reference lists of all included studies and existing reviews of microfinance, financial literacy, and financial inclusion were hand-searched. We contacted distinguished experts in the field to refer us to further relevant

⁴ A protocol specifying search strategy and methods has been pre-published in the <u>Campbell Collaboration</u> <u>Library</u>.

⁵ See Appendix 1 for the list of databases and search string.

studies. Screening of titles and abstracts was conducted by the first author. A subset of 10% of identified titles were double-screened by a second reviewer, yielding high inter-rater reliability (>0.95).

3.2 Data Extraction Process

Data from included studies was independently extracted by two review authors and entered into a pre-piloted data extraction form. We extracted a range of study-level characteristics as well as key statistics on all outcomes. We aligned our operationalisation of poverty with the multidimensional approach that moves beyond money-metric measures and additionally considers wider aspects of human wellbeing (see Sen, 1993). *Distal outcomes* therefore comprised business profits, food security, investments in and status of health, investments in education and educational attainment, and household poverty measured through assets and quality of housing or expenditure/income. *Intermediate outcomes* included increases in total savings⁶, financial literacy, savings attitudes, and investments in profitable businesses. In view of substantial inconsistencies in the conceptualisation and measurement of resilience to economic shocks and consumption smoothing, these outcome categories were excluded from the meta-analysis. If information was missing, study authors were contacted with up to four follow-up emails over the course of six months. We were unable to collect sufficient information on three studies which therefore had to be excluded from the meta-analysis.⁷

3.3 Risk of Bias

Since a meta-analysis of unreliable or biased results may lead to misleading conclusions, it is essential to critically appraise the validity of included studies. We used the Cochrane Risk of Bias Assessment Tool for Randomised Controlled Trials to rate the quality of included studies (Higgins et al., 2011). The tool was adapted for this review in collaboration with the Campbell Collaboration International Development group. Nine domains were assessed for risk of bias and quality of evidence, whereby three of the categories were added to the existing tool to improve adequacy for complex international development programmes. These

⁶ It is crucial to account for potential crowd-out effects that can arise from the shifting of resources to the saving device endorsed by the interventions. We have therefore made efforts to focus on *total* household savings and otherwise sought to aggregate all information on savings held in different places to reach an average effect.

¹ For Eissa, Habyarimana & Jack (2014) and McConnell, Mullainathan & Zinman (2010) we could not retrieve information on the sample size for control and intervention group and for Cole et al. (2014) information on standard deviations/standard errors (as well as p-values for a possible t-test) were lacking.

included: 1) random sequence generation, 2) allocation concealment, 3) blinding of participants/personnel, 4) blinding of outcome assessors, 5) incomplete outcome data, 6) selective outcome reporting, 7) implementation fidelity, 8) balance at baseline, and 9) potential for contamination or spill-over. Risk of bias was rated independently by two reviewers and classified as 'low', 'unclear' (if sufficient information was lacking), or 'high'.

4 Methods

4.1 Calculation of Effect Sizes

In order to aggregate effect sizes across studies, we calculated standardised effect sizes for all outcomes. Standardised effect sizes are scale-free and provide comparable information about the magnitude and direction of each effect. For continuous outcome measures, standardised mean differences (SMDs) were calculated. To adjust for potential bias from small sample sizes, we used Hedges' g correction for all effect sizes. For outcomes that were measured on a continuous scale in some studies and dichotomised in other studies (e.g. increases in saving amounts), we transformed odds ratios into SMDs and used Hedges' g correction as described above (for transformation, see: Borenstein et al., 2009; Sánchez-Meca, Marín-Martínez & Chacón-Moscoso, 2003). For outcomes predominantly measured on a binary scale (e.g. school enrolment), odds ratios were reported as effect size measure.

We further carefully assessed how clustered study designs were reflected in the estimation of effect sizes. If unit of treatment allocation and unit of analysis differ, unit of analysis errors can arise. Most cluster RCTs in our sample have adjusted standard errors accordingly (25 out of 27). If studies did not account for clustering, we applied corrections by multiplying standard errors with the variance inflation factor as suggested by Littell, Corcoran & Pillai (2008).⁸

4.2 Meta-Analysis and Meta-Regression

When pooling effect sizes across studies it is important to consider the underlying dependency structure of the data. Most of the studies we identified have reported several effect sizes estimates for the same subjects, e.g. multiple outcome measures for one overarching construct (such as poverty). To take the correlation and non-dependency between

⁸ We calculate the adjusted standard error as the unadjusted standard error times $\sqrt{1+(m-1)}$ multiplied by the intra-cluster correlation, where *m* is the average cluster size.

effect sizes into account, we adjust standard errors using robust variance estimation (RVE) meta-analysis. RVE is considered as superior to standard meta-analysis for avoiding loss of information as all effect sizes can be included in the analyses. It has the further advantage of accommodating for the correlated data structure without requiring knowledge of the underlying covariance pattern between effect size estimates (Fisher & Tipton, 2015; Tipton, 2013; Hedges, Tipton & Johnson, 2010).

Following Tanner-Smith, Tipton & Polanin (2015), we estimate the simple RVE model in a first step:

$$y_{ij} = \beta_0 + u_j + e_{ij}, \qquad (1)$$

where y_{ij} is the estimated effect size $i=1...k_j$ in study j=1...m, and β_0 is the true effect size. Further, u_j is a study level random effect, $Var(u_j)=\tau^2$ is the between-study variance component, and e_{ij} represents the residual for the *i*th effect size in the *j*th study.

Sources of heterogeneity were examined by testing whether effect size estimates varied significantly by intervention type, duration of the intervention, participant sex, and participant age. In a similar vein, sensitivity analyses were run to check whether effect sizes differed significantly by time to follow-up and risk of bias rating. For this purpose, the above model was augmented by adding covariates, resulting in a mixed-effects model of the form:

$$y_{ij} = \beta_0 + \beta_1 x_{1ij} + \dots + \beta_p x_{pij} + u_j + e_{ij} , \qquad (2)$$

where $x_{1ij},...,x_{pij}$ represent characteristics on study or effect size level. In the terminology of mixed effects models $\beta_1 x_{1ij},...,\beta_p x_{pij}$ are often called "fixed effects" as $\beta_1,...,\beta_p$ vary only as a function of known characteristics.

As mentioned above, standard meta-regression cannot account for statistical dependency within data, which would result in inappropriately small standard errors. The key difference between RVE models and standard meta-regression therefore lies in the estimate of the variance. Accordingly, the robust variance of the estimate b of $\beta = (\beta_1, ..., \beta_p)$ is obtained by

$$\mathbf{V}^{\mathbf{R}}(\mathbf{b}) = (\sum_{j=1}^{m} \mathbf{X}_{j}^{'} \mathbf{W}_{j} \mathbf{X}_{j})^{-1} (\sum_{j=1}^{m} \mathbf{X}_{j}^{'} \mathbf{W}_{j} \mathbf{A}_{j} \mathbf{e}_{j} \mathbf{e}_{j}^{'} \mathbf{A}_{j}^{'} \mathbf{W}_{j} \mathbf{X}_{j}) (\sum_{j=1}^{m} \mathbf{X}_{j}^{'} \mathbf{W}_{j} \mathbf{X}_{j})^{-1}$$
(3)

where X_j is a design matrix, W_j is a diagonal weight matrix⁹, A_j is an adjustment matrix to correct for small-sample bias, and e_j is the estimated residual vector (see Tipton, 2015, Tanner-Smith et al., 2015; Tanner-Smith & Tipton, 2014).

The null hypothesis for the effect of β_k (H0: $\beta_k=0$) is then tested using the robust variance estimator V^R as:

$$t_k = \frac{\beta_k}{\sqrt{V} \frac{R}{k}} \tag{4}$$

where V_k^R denotes the robust variance estimate of b_k (estimate of β_k). For small sample sizes, t_k approximates a t-distribution (see Tipton, 2015). If this condition is not satisfied, type I error can exceed the p-value that is specified (Tanner-Smith et al., 2015).

In order to increase power and in view of the limited number of individual studies, we did not conduct meta-regressions with more than one explanatory variable. Also, following Cochrane Collaboration conventions, meta-regressions were considered as inappropriate for outcome categories composed of less than ten individual studies (see Higgins & Green, 2011).¹⁰

5 Results

5.1 Identified Studies

Our database search identified 9330 records of which a total of 27 studies met the eligibility criteria of this review. A flowchart that details the stages of the search and screening process is provided in Figure 1. Characteristics of included studies are summarised in Table 1. As can be seen from Table 1, merely 5 out of 27 studies were academic publications while the majority of records were grey literature outlets or working papers. The 27 identified studies feature four broad program components: supply of formal (7 studies, e.g. bank account, mobile money) or semi-formal (13 studies, e.g. savings group, money box) savings infrastructure or reduction of financial and administrative barriers to use existing infrastructure, delivery of financial education curricula around savings (14 studies), and

⁹ Tanner-Smith and Tipton (2014) propose the following weights for the correlated effects model: $w_{ij}=I/\{(v_{ij} + \tau 2 [1+kj-1\rho])\}$ where v_{ij} is the mean of the within-study sampling variances (v_{ij}) for each effect size k_j in study j, τ^2 is the between-study variance, and ρ is the assumed within-study correlation between effect sizes. We ran all analyses assuming $\rho = 0.8$, which is a common assumption across the literature. We further conducted sensitivity tests for different values of ρ and found that results hold up to the fifth decimal.

¹⁰ All data analyses were conducted in R 3.3.2 using the 'robumeta' package (Fisher & Tipton, 2015).

lastly commitment schemes for promoting saving self-discipline, either through imposing hard commitments (flexibility constraints or economic penalties) or soft psychological commitments (10 studies). There was substantial variation in programme set-up, ranging from one-day awareness raising campaigns (such as in Coville et al. 2014) to complex multi-component interventions (such as in Dizon, Gong & Jones, 2016 or Dupas & Robinson 2013b). Further, studies were heterogeneous in terms of time to follow-up (ranging from two months to three years) and duration of the intervention itself, with some brief once-off programmes and others lasting for several months. Although saving promotion is aimed at poverty reduction, only about half of the included studies do in fact look at more distal outcomes such as household expenditures and incomes. Figure 2 depicts the geographic scope of randomised studies on savings programmes in Sub-Saharan Africa. While most trials were implemented in Kenya, Malawi, and Uganda, no studies have been carried out – to date – in any of the most fragile and impoverished countries on the continent.

5.2 Pooled Effect Sizes

We report effect sizes for each outcome category separately. Outcome categories are grouped into intermediate outcomes and poverty-related distal outcomes. We provide pooled RVE effect sizes as well as I^2 - and τ^2 -statistics for a first assessment of heterogeneity. Grand mean pooled estimates should be interpreted with caution if heterogeneity between studies is high.¹¹ Corresponding forest plots visualize individual effect sizes as well as grand pooled estimates for studies in each outcome category and are presented in Appendix 2.

Intermediate Outcomes

Table 2 reports pooled effect sizes for intermediate outcomes. Column (1) shows a positive and significant effect on *total savings* ($g_{pooled}=0.077$, p<0.001). More precisely, the intervention considered in our analysis lead to an overall increase in total savings that is significantly different from zero. Similarly, Column (2) shows that the pooled effect size for *pro-savings attitudes* is positive and borderline significant ($g_{pooled}=0.061$, p<0.1), thus pointing to a trend towards improvement in financial attitudes across included studies. Findings further show a trend towards increases in *financial literacy* levels ($g_{pooled}=0.12$.

¹¹ Acknowledging that I^2 and τ^2 are less reliable with a small number of individual studies, we avoid the use of simple thresholds to diagnose heterogeneity.

p<0.10), see Column (3). Further, Column (4) depicts that *business investments* are positively related to savings interventions (g_{pooled} =0.045, p<0.10). Although the effect size for investment is small, we see significant downstream impacts on *business returns and profits* (g_{pooled} =0.044, p<0.01) as reported in Column (5).

While all pooled effect sizes are positive and (borderline) significant, heterogeneity in included effect sizes for some outcomes is high: an I^2 statistic of 86% indicates that substantial variations in the effects on financial literacy exist across included studies. Further, the I^2 statistic for savings and business investment is high and moderately high with 69,6 % and 43,9 %, respectively.

Distal Outcomes

With respect to distal, poverty-related outcomes, our analysis reveals interesting findings. Table 3 shows significant increases in households' *expenditures and incomes* (g_{pooled} =0.066, p<0.01) (Column (1)). With regards to the wider aspects of household poverty and wellbeing, our results further point to significant increases in *food security* (g_{pooled} =0.052, p<0.05) as reported in Column (2). In contrast, we do not find significant impacts across interventions on *asset ownership and housing quality* (Column (3)). Possibly measures considered in our analysis may either need longer follow-up periods for visible change or are generally more stable across time and therefore less malleable to change (see also Suri & Jack, 2016). In a similar vein, we do not find indication of programme effectiveness with regards to health and education. Columns (4)-(5) show that educational investment (g_{pooled} =0.009, p>0.1) and school enrolment (g_{pooled} =0.059, p>0.1) fail to reach significance across included studies. Likewise, savings interventions show no downstream impacts on general health status or health investments (g_{pooled} =0.010, p>0.1) as reported in Column (6). The latter result is emphasised by the fact that health effect sizes are quite homogenous (with I² of 2.7 %).

Heterogeneity in the remaining outcome categories is high to moderate (I^2 ranging from 38.5 % to 65.9 %). Particularly, variation seems emphasized in expenditure/income and asset/housing outcome categories.

5.3 Meta-Regression: Heterogeneity in Effect Sizes

In order to investigate how program and participant characteristics affect outcomes it is necessary to pool effect sizes into broader categories to meet the requirement of at least 10 individual studies per regression (Higgins & Green, 2011). A common problem when pooling effects sizes is the trade-off between gaining more statistical power by adding up individual outcomes versus maintaining comparability of effect sizes within a pooled category and thus ensure validity of resulting estimates. In line with our theoretical framework, we generate three broad categories namely (i) savings, (ii) consumption proxies, and (iii) future-oriented investments. In our main analysis, we follow a relatively conservative approach, only including outcomes that closely represent the mentioned categorical concept. Yet, in a robustness check (see Appendix 5), we provide estimates based on a wider definition of the three categories: First, for the savings category, we include all kinds of savings measures (e.g. account deposits, cash savings, total savings, etc.) in our conservative approach, while we also add life stock and household asset indices for the broader definition. Second, the conservative consumption category comprises measures such as food and household expenditures, income/profits from agricultural activity and small-scale business, while for the wider definition we further add actual food insecurity, and frequency of meals. Finally, for the conservative investment category, we combine human capital investment (i.e. expenditures in health and education,) as well as investments in agriculture and small-scale business. We add broader measures of actual health and education for the wider definition. As both strategies, i.e. using the conservative and wider definition of categories, yield relatively similar results, we only discuss the conservative approach in depth in the following sections (see Tables 4-5) and present the regression tables with the wider categories to Appendix 5.

Intervention Design and Components

In our first set of regressions we investigate whether variations in outcomes can be explained by differences in programmatic characteristics (see Table 4). For this purpose, we recorded whether interventions featured supply-enhancing components, demand-enhancing components, or any form of behavioural constraints (see Appendix 3 for coding of components). Most programmes under investigation feature some kind of supply component, which is often combined with either a demand or behavioural component. Further, a few studies in our sample feature stand-alone demand-enhancing interventions. We do not observe any stand-alone behavioural intervention in our sample (behavioural components are usually tied to a supply enhancing component).

Based on their prevalence, supply-based programmes were used as base category in all regressions. Intercepts therefore determine the magnitude and significance level of pooled effect sizes for these programmes. We find that supply-based programmes show consistently positive and significant effect sizes for all three outcome categories (see constants in Columns (1)-(9)).¹² Among these, programmes with formal supply components (i.e. increasing access to bank or mobile money accounts) appear tentatively more effective when compared to programmes with informal supply components (i.e. initiation of group-based savings schemes or supply of money boxes) in promoting actual savings amounts (β =-0.08, p<0.1) as reported in Column (1). Yet, we do not find a significant difference in effect sizes for consumption and investment-related outcomes. While difference coefficients are small, standard errors are large and hence prohibit any clear assessment.

Further, we do not find evidence for an add-on effect for the combination of supply-based components with literacy or motivational components (i.e. demand promotion) when compared to supply-only programmes (see Table 4, Columns (2), (5), (8)) Although coefficients are positive in all outcome categories in this group, the standard errors are quite large. It is therefore conceivable that low statistical power makes it impossible to detect some small but true differences between pooled effect sizes. Turning to the behavioural components, we find no support for the hypothesis that "tying one's hands" through programs that include *external controls* such as purpose-labelled accounts, peer pressure, and commitment to a fixed cycle with institutionalised withdrawal constraints can increase effectiveness of supply-based programmes (see Columns (3), (6), and (9)). Note, however, that standard errors are relatively large and it is therefore conceivable that low statistical power makes it impossible to detect true differences between pooled effect sizes.

We further compare once-off/one-day programmes with longer programmes featuring several weekly meetings or complex curricula (see Table 5). We find no evidence that longer programme duration yields higher effect sizes (see Columns (1), (6), (11)). In contrast, programs with longer duration seem to be less effective in improving consumption outcomes

 $^{^{12}}$ If we use demand instead of supply as base category, constants turn non-significant (for savings: 0.07, p=0.28, consumption: 0.00, p= 0.83, investment: 0.01, p=0.67), suggesting that demand-based programmes are not associated with significant changes in the three outcomes.

 $(\beta$ =-0.06, p<0.001, Column (6)). This finding feeds back into the above discussion of programmatic components: While longer programmes tend to target demand aspects (e.g. financial education curricula), we may still expect to see higher effects from a once-off programme with a strong formal supply component. It would be interesting to examine the impact of programme intensity and duration for the sub-group of literacy programmes only, however, we did not have a sufficiently large number of studies to proceed with such a posthoc analysis.

Participant Characteristics

In the next set of regressions (see Table 5, Columns (2), (7), (12)), we seek to elucidate whether programme effectiveness varies with participant characteristics. Our analyses reveal relatively large programme effects for male participants across all three outcomes as well as some substantial decreases in heterogeneity statistics (for savings I^2 is reduced from 69.9% to 58.7%, for consumption from 45.8% to 42.4%, and for investments from 35.2% to 16.3%). Our finding is somewhat at odds with existing research literature that suggests higher impacts for female programme beneficiaries (see Suri & Jack, 2016) and a tendency of women to prioritise future-oriented and child-friendly expenses (Dupas & Robinson 2013a; D'Espallier, Guérin & Mersland, 2011; Ashraf, 2009; Ashraf, Karlan & Yin, 2003). We can only speculate about the underlying mechanisms of this difference in effectiveness by gender. One interpretation, which is in line with previous research, would be that women as more riskaverse when it comes to portfolio investments such as the purchase of business assets (see Coleman, 2000; Scherr, Sugrue & Ward, 1993; Brush, 1992). Another explanation might be that our finding is an artefact of the composition of samples in the studies of this review. While the majority of included interventions focus on women and target specifically vulnerable and economically deprived samples, there are only three studies with a more specific focus on men who are, in these cases, small entrepreneurs and farmers (Ksoll et al., 2016; Carter et al., 2015; Brune et al., 2015). It is therefore conceivable that these interventions turn out more effective because of the socioeconomic background of participants rather than their sex. We did not have sufficient data to examine the hypothesis across all studies.

In terms of participants' age, interventions seem to be somewhat more effective in promoting savings and consumption when targeting adults rather than school children and adolescents

(see Table 5, Columns (3), (8), (13)). This might partly be explained by the fact that some programme types such as access to bank accounts are not feasible with young populations. It would be interesting to see whether other outcomes such as pro-savings attitudes and financial literacy are more malleable to change when implemented in younger populations, especially with children. However, limited data availability leaves this question to future research.

Study Design

Turning to study design characteristics, we observe that effect sizes for consumption significantly decrease with the time to follow-up, pointing to a 'fading out' of programme impact (β =-0.02, p<0.05, see Table 5, Column (9)). We run post-hoc sub-group analyses that reveal that the pooled effect size for household poverty lies at g_{pooled} =0.12 (95% CI [0.05, 0.19]) after 6 months of programme delivery and is diminished to effectively zero after more than two years (g_{pooled} =0.02, 95% CI [-0.01, 0.04]). We do not find indication for diminishing effects over time for savings (see Table 5, Column (4)) and for investments (see Table 5, Column (14)).

5.4 Risk of Bias

The quality of included studies ranges from moderate to high as detailed in Figure 3 (as well as Appendix 4). Four points are noteworthy. First, blinding of participants is notoriously difficult in non-medical trials and was thus not ensured in most included studies. However, some innovative study designs included quasi-placebo treatment arms that received the same intervention (e.g. public movie screening) without specific financial content (see Berg & Zia, 2014; Coville et al. 2014; Dupas & Robinson, 2013b; Eissa et al. 2014). Second, documentation on process evaluation was difficult to identify for most included interventions, although quality of programme implementation and fidelity may partly explain variations in outcomes (see Durlak & DuPre, 2008; Dane & Schneider, 1998). Third, none of the 27 included studies used any corrections for multiple testing (such as family wise error rate or false discovery rate adjustments) (see Fink, McConnell & Vollmer, 2014; Anderson, 2008; Kling, Liebman & Katz, 2007).

Sensitivity analyses were conducted to explore whether study quality was associated with magnitude and significance of effect sizes (see Table 5, Columns (5), (10), (15)). Risk of bias

did not explain heterogeneity in treatment effects for any of the three outcomes, thus suggesting that the findings from our meta-analysis are quite robust.

6 Discussion

This study set out to examine the poverty-alleviating potential of savings interventions in Sub-Saharan Africa. Financial inclusion and saving promotion are being increasingly researched in the region, as expressed by 27 studies included in this review and more studies still underway. Our findings reveal that savings interventions do indeed show significant impacts on poverty-related aspects when looking at distal outcomes, including increases in household expenditures, incomes, and improved food security, as well as on intermediate outcomes such as increases in total savings amounts and promotion of small-scale family businesses.

Our findings further suggest that programmes with formal supply-enhancing components, e.g. formal banking/mobile money, appear to be more effective in raising savings when compared to other components. This links back to our theoretical framework, suggesting that undersaving may primarily stem from barriers in supply. In fact, over 80% of poor people in developing countries lack access to formal banking up to date (Demirguc-Kunt & Klapper, 2012). However, it has to be cautioned that studies with formal supply components often rely on administrative data to analyse impact, while studies without such a component often have to rely on self-reported saving measures. The difference we observe between these components may, hence, rather be an expression of the error in measuring group- or home-based savings.¹³ Although pointing in a similar direction, effects sizes do not vary significantly with the type of programme component for consumption and investment aggregates.

Apart from this, we do not find conclusive evidence on the question of whether the addition of a demand or behavioural component alters the effectiveness of an existing supply component. While we find positive add-on coefficients for an additional demand component across all outcomes, none is close to statistical significance and standard errors are large. Similarly, add-on coefficients for behavioural components are all close to zero with large confidence intervals, thus not allowing for any further conclusion. That is, if any add-on

¹³ See e.g. Karlan and Linden (2014) for direct comparison of self-reported and administrative data.

effects does in fact exist, they may be small and the capacity to detect such effects lies beyond the statistical power of this study.

Results from the range of trials included in this study provide strong empirical grounds to iron out the misconception that poor people are "too poor" (or worse: too unsophisticated) to save. The findings therefore point to a gap between the demand for savings and the actual provision of reliable, safe, and easily accessible institutionalised savings devices. Mobile banking technology reduces dependence on the brick-and-mortar presence of bank branches and has recently gained prominence across the African continent. It may have the potential to considerably boost supply in the coming years.

While our findings are encouraging to some extent, it must be stressed that effect sizes, albeit significant, were very small across all outcome categories ($<g_{pooled}=0.20$). Cohen (1988) classifies effect sizes of 0.20 as small; 0.40 as a medium; and 0.80 as large. This could either mean that take-up of the savings tools offered is low^{14,15} or that programme impact, overall, is too small to substantially lift individuals out of poverty. More importantly, our results suggest that programme effects tend to fade out over time and that possible reductions in poverty levels may not be sustained over a longer period. In a similar vein, Ashraf et al. (2006) present evidence from the Philippines where bank accounts were not actively used one year after programme roll-out, not even by those who were registered as high-frequent users in the first couple of months. This finding calls for continued support to those who take up savings and for a follow-up outreach one to two years after a programme's launch. Future research will need to include cost effectiveness analyses that weigh overall programme costs against material as well as psychosocial benefits for target populations.

However, the effect sizes in this review do not differ vastly from those of other interventions in the field of international development. For instance, a meta-analysis of the impact of conditional cash transfers on educational outcomes finds effect sizes similar to ours for primary and secondary school enrolment (Saavedra & Garcia, 2012). Likewise, McEvan (2015) examines a range of school-based interventions in developing countries and finds that monetary grants and school-based deworming programmes have effect sizes close to zero. The review reveals the largest effect size for technology and computer training which still does not exceed a standardised mean difference of 0.15. Lastly, a meta-analysis on technical

¹⁴ For instance, a three-country study finds that take-up rates for formal bank accounts were as low as 17% in Chile, 54% in Uganda, and 69% in Malawi (Dupas, Karlan, Robinson& Ubfal, 2016).

¹⁵ Given that included studies applied intent-to-treat analyses it is likely that they offer realistic estimates of take up in the "real world".

and vocational training for youth in low- and middle-income countries finds a mean effect size (Hedges' g) of 0.13 on income which is, again, quite similar to what we find (Tripney & Hombardos, 2013).

While our meta-analysis intends to specifically move beyond intermediate outcomes by examining the multi-dimensional aspects of household poverty, it provides little empirical insights on the causal mechanisms at play. Future research will need to open this 'black box' and examine how increased savings and improved budgeting can translate into poverty-relevant outcomes. Also, it is essential to scrutinise why hypothesised trickle-down effects on education and health have failed to materialise.

Findings from this analysis may raise caution against anticipations of a 'revolution' in the global fight against poverty. And yet, they are promising enough to position saving promotion somewhere at the top of the agenda in international development. All in all, our findings have shown that the poor in Sub-Saharan Africa are indeed able to save their money. The 2030 Sustainable Development Agenda propagates to *"[s]trengthen the capacity of domestic financial institutions to encourage and expand access to banking, insurance and financial services for all"*. Our research alludes to the above policy claim, by providing empirical grounds that urge for an expansion of the formal financial sector to the world's poor and a better adaptation of services to their specific financial needs.

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¹⁶ For studies with multiple records, we use the latest record as study reference marked in bold.

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TABLES

Table 1. Summary of Included Studies

Study	Country/ Setting	Participants	Intervention Type	Intervention Duration	Intermediate Outcomes	Distal Outcomes	Trial Design	Sample Size	Time to Follow-Up
Annan, Bundervoet, Seban & Costigan 2013 (grey literature)	Burundi	Poor families with children	• Savings group (VSLA)	For Savings group: 3 months training and 9 month cycle, in addition weekly discussion sessions (2 h/session)	• N/A	 Expenditures/ Consumption Poverty level Household assets 	cRCT	 Intervention: 805 individuals Control: 743 individuals (across 77 self-help groups) 	12 months
Batista & Vicente 2013 (working paper)	Mozambique	Household heads of rural dwellers	 Access to formal bank accounts Mobile banking scheme 	N/A	 Adoption of mobile savings Financial literacy/knowledge Intention/ willingness to save Trust in financial services Deposit amounts 	• N/A	cRCT	 Community outreach & agent: 1020 individuals (51 EA) Information leaflet: 204 individuals Control: 1020 individuals 	2 months
Beaman, Karlan & Thuysbaert 2014 (working paper)	Mali	Female household members	• Savings group (VSLA)	Introductory village meeting led by NGO agent, savings group meets on weekly basis for pre-determined cycle (varies in length)	 Uptake of Savings Savings Consumption Smoothing 	 Food security Business profits Health/ health expenditures Investments in education Housing quality/ assets Expenditures 	cRCT	 Intervention: 209 village, 2508 women Control: 291 villages, 3492 women 	3 years
Berg & Zia 2014 (grey literature)	South Africa	Medium- to low-income households	• Financial Literacy	Screening of 26 episodes over a period of two months	 Financial Knowledge Saved money in the past 6 months 	• N/A	iRCT	Intervention: 553Control: 478	4 months

Berry, Karlan & Pradhan 2015 (working paper)	Ghana	School children in grades 5 & 7	Financial literacyDistribution of lock boxes	 Honest Money Box arm: 8 weekly one- hour sessions Aflatoun arm: approx. 24 hrs in total and continued school-based saving clubs 	 Savings Savings behaviour Savings attitudes Financial literacy 	• N/A	Multi-arm cRCT	 Honest Money Box: 45 schools, 1800 students Aflatoun: 45, 1800 Control: 45, 1800 	9 months
Brune, Giné, Goldberg & Yang 2015 (published)	Malawi	Smallholder cash crop farmers	 Access to formal bank accounts Saving commitment schemes 	2 months	 Deposits into savings accounts Savings balances Uptake of bank account Agricultural input 	• Total expenditure last 30 days Profit from farming	Multi-arm cRCT	 Ordinary accounts: 1804 individuals Commitment accounts: 1763 individuals Control: 583 individuals 	1-1.5 years
Buehren 2011 (conference proceeding)	Uganda	Microfinance borrowers	 Financial literacy Saving mobilisation through microfinance organisation 	6 months, weekly sessions	• Savings	• N/A	cRCT	 Treatment arm: 809 individuals (270 Microfinance groups) Control: 628, 135 	6 months
Carter, Laajaj & Yang 2015 (unpublished manuscript)	Mozambique	Farmers	 Access to formal bank accounts Financial Literacy 	3 sessions (duration of each not specified)	• Formal Savings	 Per capita consumption Total household assets Expenditures on education 	Multi-arm cRCT (but only focus on 1 arm as other arms include incentive scheme)	 Intervention: 269 households Control: 258 households (1 individual per household) 	Three waves: 5 months, 1.5 years, approx 2 years
Cole et al. 2014 (not included in quantitative synthesis) (grey literature)	South Africa	Members of burial society and women's business development group	• Financial Literacy	1 day (8 hours)	Financial LiteracySavingsExpenditure	• N/A	cRCT	 Intervention: 589 individuals Control: 661 individuals 	6 months

Coville et al. 2014 (grey literature)	Nigeria	Micro Entrepreneurs	 Access to formal bank accounts Financial Literacy 	Once-off, 8-11 am film screening	Intentions to saveFinancial literacy	• N/A	Multi-arm iRCT	 Movie Screening: 327 Bank account: 287 Movie & Bank Account: 307 Control Arm: 309 	4 months
Dizon, Gong & Jones 2016 (unpublished manuscript)	Kenya	Vulnerable women (female sex workers, single/ widowed women	 Access to formal bank accounts Weekly savings reminders Soft commitment through account labelling 	6 months	 Takeup of mobile banking Consumption smoothing Savings 	N/A	iRCT	 Intervention: 304 women Control: 323 women 	8-12 months
Dupas et al. 2016 (unpublished manuscript)	Uganda, Malawi, (Chile)	Household heads of unbanked rural households	• Access to formal bank accounts	Once-off	Uptake of savings productSavingsBusiness investment	 Income Assets Expenditures Food expenditures Education expenditures Health expenditures Housing quality expenditures 	iRCT	 Intervention: Uganda 1079, Malawi 1053 Control: Uganda 1081, Malawi 1054 	4, 8, and 20 months after treatment
Dupas, Keats & Robinson 2016 (working paper)	Kenya	Household heads around three market centers	• Access to formal bank accounts	Once-off home visit for delivery of bank vouchers	 Uptake of savings product Usage of bank account Savings 	Food SecurityExpenditures	Multi-arm cRCT	 Intevention: 198 single-headed/ 404 dual-headed households Control : 283 households 	2 years
Dupas & Robinson 2013a (published)	Kenya	Market vendors and taxi drivers	• Access to formal bank accounts	Not specified	Account usageSavingsBusiness investment	Business profitExpenditures	iRCT	 Treatment: 195 (130 female market vendors) Control: 197 (132 female market vendors) 	6 months
Dupas & Robinson 2013b	Kenya	Members of a ROSCA	• Distribution of saving	1 ROSCA meeting (and then	 Take-up of savings technology 	• Resilience to health emergencies	Multi-arm cRCT	• Safe Box: 20 ROSCAS, 354	6 and 12 months, 3

(published)			devices (e.g. lock boxes) • Saving commitment schemes • Earmarking/ peer pressure	ROSCA cycle)		• Investment in preventative health products		individuals • Lock Box: 26,458 • Health Pot: 23,311 • Health Savings Accounts: 26,470 • Control: 18,320	years with a random subsample
Eissa, Habyarimana & Jack 2014 (not included in quantitative synthesis)	Kenya	High School students in last 2 years of school	• Financial Literacy	Weekly treatment materials for a period of 6 weeks	SavingsFinancial literacy	• N/A	Multi-arm cRCT	 Comic & CD with financial education materials: 60 schools, 1140 students Full financial literacy program: 54, 1140 Placebo (comics without financial education): 52 	6 months
(grey literature)								 1140 Control: 51: 1140 	
Flory 2016 (working paper)	Malawi	Households in central Malawi	• Financial literacy (happening in conjunction with expansion of mobile banking)	Trained assistants visited treatment communities 1-2 times/month, visits lasted up to a few hours	 Awareness of financial services Uptake of saving devices Savings (only analysed for subgroup of account opener) Investment in agricultural business (land and fertiliser) 	 Crop Income Food consumption (only analysed for subgroup of account openers) 	cRCT	 Intervention: 56 clusters, 1003 households Control: 56 clusters, 1003 households 	2 years
Jamison, Karlan & Zinman 2014 (working paper)	Uganda	Members of Youth Clubs	 Access to formal bank accounts Financial Literacy 	15-hour course delivered over 10 weeks	 Financial literacy (financial knowledge, awareness, and numeracy) Savings 	 Income School attendance Expenditures Nutrition 	Multi-arm cRCT	 Financial literacy: 60 clubs, 702 individuals Bank Account: 60, 702 Both: 60, 702 Control: 60, 702 	9-12 months
Karlan et al. 2012	Ghana, Malawi,	Low-income households	Savings Group (VSLA)	Cycle usually between 8-12	 Uptake of VSLA membership Saving (total and 	 Business Profits Household poverty (assets and 	cRCT	 Intervention: Ghana: 88 villages/ 2640 individuals, 	Ghana: 2 years, Malawi &

(working paper)	Uganda			months	weekly contributions)Investment in agriculture	consumption)Food securityEducationHealth		Malawi: 95 / 2265, Uganda: 98 / 2270 • Control: Ghana 87//2231, Malawi 95/ 2265, Uganda 98 /2270	Uganda: 3 years
Karlan & Linden 2014 (working paper)	Uganda	Students grades 4-7	 Saving commitment schemes Saving Account 	5 school terms, regular visits by intervention team	 Saving (both administrative data and self-reported) Savings attitude Uptake of saving product 	 Expenditure on education (school fees School attendance 	Multi-arm cRCT	 Cash treatment: 1350 students, 39 schools (with parent outreach: 19, without: 20) Voucher treatment: 1350 students, 39 Schools (with PO: 19, without PO: 20) Control: 2007 students, 58 schools 	2 years
Ksoll et al. 2016 (published)	Malawi	Household heads in rural Malawi	• Savings Group (VSLA)	Varies by cycle, typically 12 months	 Uptake of VSLA membership Total Savings Agricultural input 	 Food Security Expenditures Income/Poverty level Housing Quality Agricultural output 	cRCT	 Intervention arm: 23 villages, 568 households Control arm: 23, 569 	2 years
Lee et al. 2015 (working paper)	Ghana	Low-income youth	 Marketing outreach for formal bank accounts 	3-7 visits to schools by bank staff over the course of a year	Account openingSavings	• N/A	Multi-arm cRCT	 In-School banking: 25 schools, 5501 students Marketing outreach: 25 schools, 7207 students Control: 50 schools, 9760 students 	2.5 years
McConnell 2012 (not included in quantitative synthesis)	Ghana	Market vendors	 Marketing of Savings (information and levels of convenience to open an account), 	NA	 Account opening Account usage Intention of account usage 		iRCT	Unclear	1 month & 3 months (not clear)

(unpublished manuscript)			SMS reminders						
Sayinzoga, Bulte & Lensink N/A (unpublished manuscript)	Rwanda	Representativ es of village banks	• Financial Literacy	5 days (8 am to 5 pm)	SavingsFinancial literacy	• N/A	cRCT	 Intervention: 180 village banks, 174 individuals Control: 180, 167 	15 months
Schaner 2015 (published)	Kenya	Low-income married couples	Access to formal bank accounts	One day for opening the account, interest rate running for six months	Account usageSavings	IncomeAssets	Multi-arm iRCT	Intervention: 3372Control: 1302	6 months, 3 years
Shephard, Kaneza & Moclair (under review)	Rwanda	Teachers & students	• Financial literacy	Full Aflatoun curriculum	General financial capabilitySaving attitudes	• N/A	cRCT	 Intervention: 875 students, 125 teachers Control: 875, 125 	midline 3-4 months, endline 7 months
Supanantaroek (in press) (grey literature)	Uganda	School children	Financial literacy	3 months, 40 hours in school	SavingsSaving attitudes	• N/A	cRCT	 Intervention: 22 schools, 936 students Control: 22 schools, 810 students 	3 months

	Savings	Saving Attitudes	Financial Literacy	Business Investment	Business Profits
	(1)	(2)	(3)	(4)	(5)
Hedges' G	0.077***	0.061 [†]	0.12 [†]	0.045 [†]	0.041**
(SE)	(0.02)	(0.02)	(0.05)	(0.02)	(0.01)
95% CI	[0.03, 0.12]	[-0.02, 0.09]	[-0.01, 0.24]	[-0.00, 0.09]	[0.01, 0.07]
I^2	69.6%	24.4%	85.9%	43.9%	20.8%
Tau ²	0.004	0.001	0.017	0.002	0.000
N of studies	18	4	7	9	7
N of effect sizes	43	8	23	28	13

Table 2. Pooled Effect Sizes for Intermediate Outcomes

Notes: [†] p<0.1, *p<0.05, **p<0.01, ***p<0.001. Note that some studies are multi-arm trials and therefore contribute effect sizes to two or more intervention type categories.

	Expenditures/ Income	Food Security	Assets/ Housing	Education Investment	Enrolment (binary)	Health / Health Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Hedges' G	0.066**	0.052*	0.038	0.009	0.059	0.010
(SE)	(0.02)	(0.02)	(0.02)	(0.01)	(0.05)	(0.01)
95% CI	[0.02, 0.12]	[0.01, 0.10]	[-0.01, 0.09]	[-0.03, 0.05]	[-0.18, 0.3]	[-0.01, 0.03]
I ²	61.7%	38.5%	65.9%	41.9%	39.7%	2.7%
Tau ²	0.003	0.001	0.003	0.000	0.005	0.000
N of studies	11	8	9	6	3	5
N of effect sizes	38	18	23	17	11	17

Table 3. Pooled Effect Sizes for Distal Outcomes

Notes: [†] p<0.1, *p<0.05, **p<0.01, ***p<0.001. Note that some studies are multi-arm trials and therefore contribute effect sizes to two or more intervention type categories. Pooled effect sizes for *Enrolment (binary variable)* are log odds.

		Savings			Consumption			Investment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Informal Supply	-0.08†			-0.01			-0.01		
	(0.04)			(0.04)			(0.02)		
Additional Demand	()								
Component		0.01			0.04			0.01	
		(0.05)			(0.03)			(0.03)	
Additional Behavioural									
Control Component			-0.02			-0.01			-0.00
			(0.05)			(0.03)			(0.02)
Const. (Formal supply)	0.11**			0.08**			0.04*		
	(0.03)			(0.02)			(0.01)		
Const. (Any Supply)		0.07*	0.09*		0.06**	0.07**		0.04*	0.04*
		(0.02)	(0.04)		(0.01)	(0.02)		(0.01)	(0.01)
Sample		Supply			Supply			Supply	
N (Studies)		15			12			13	
N (Effect Sizes)		39			58			51	
I^2 in % (original, resid.)	72.1, 68.1	72.1, 73.8	72.1, 72.8	68.2, 67.1	68.2, 66.2	68.2, 66.6	35.4, 36.0	35.2, 36.0	35.4, 38.5

Table 4. Meta-Regression: Intervention Components (Conservative Approach)

Notes: [†] p<0.1, *p<0.05, **p<0.01, ***p<0.001. P-values do not match regular t-statistics due to small sample correction as suggested in Tipton (2015). Parentheses around significance stars indicate coefficients with adjusted degrees of freedom below 4. Standard errors from robust variance estimation are in parentheses. Sample only includes study arms that feature any kind of supply component for better interpretability. Intervention channels are defined as follows: (i) *formal supply* indicates that intervention includes a component that grants or improves access to institutionalised banking, (ii) *informal supply* indicates that intervention includes a component that induces or supports savings groups or provides moneyboxes etc., (iii) *additional demand component* indicates a literacy or motivational scripts/outreach component in addition to a supply component, and (iv) *additional behavioural control component* indicates the addition of a commitment devices, peer pressure and regulatory frameworks in groups (e.g. fixed cycles), or strict earmarking of savings. For definition of outcome categories, see Section 5.3.

			Savings			Consumption				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Duration	-0.06					-0.05*				
	(0.05)					(0.02)				
Female	. ,	-0.16*					-0.06*			
		(0.05)					(0.02)			
Mixed		-0.16†					-0.00			
		(0.06)					(0.03)			
Youth			-0.07†					-0.04*		
			(0.04)					(0.01)		
Time to Follow Up				0.00					-0.03 t	
Ĩ				(0.02)					(0.01)	
Risk of Bias					0.00					0.02
					(0.01)					(0.01)
Const.	0.17†			0.07	0.09	0.10**			0.15**	0.15*
	(0.08)			(0.05)	(0.04)	(0.02)			(0.04)	(0.04)
Const. (Male)		0.21†					0.09*			
		(0.05)					(0.02)			
Const. (Adults)			0.10**					0.07***		
			(0.03)					(0.01)		
Sample			Full					Full		
N (Studies)			18					12		
N (Effect Sizes)			43					62		
I^2 (original vs. resid.)	69.6, 69.0	69.9, 58.7	69.9, 68.8	69.9, 71.0	69.9, 71.7	64.5, 64.8	64.5, 60.7	64.5, 66.8	64.5, 58.4	

Table 5. Meta-Regression: Study Design, Participant Characteristics and Bias

Notes: [†] p<0.1, *p<0.05, **p<0.01, ***p<0.001. P-values do not match regular t-statistics due to small sample correction as suggested in Tipton (2015). Parentheses around significance stars indicate coefficients with adjusted degrees of freedom below 4. Standard errors from robust variance estimation in parentheses. *Intervention duration* is a dichotomous variable, coded 0 for brief ('once-off' or one day) and 1 for longer programmes. Savings groups programmes were coded as long given that groups meet in regular intervals over a longer cycle. *Participant sex* has three categories for primarily male, female, or mixed programme beneficiaries. The threshold for primarily male/female was defined as more than 75% of all participants. *Participant age* has three categories for adults, children/youth (up to 24 years), or both. *Time to follow-up* has

four categories: 0-6 months, >6 months - 1 year, >1-2 years, and > 2 years. *Risk of Bias* was coded as a continuous variable with higher scores reflecting higher risk of bias. A summative scale score was created for each individual study by coding low risk of bias as -1, unclear risk of bias as 0, and high risk of bias as +1.

			Investment		
	(11)	(12)	(13)	(14)	(15)
Duration	-0.01				
	(0.02)				
Female		-0.07*			
		(0.02)			
Mixed		-0.06*			
		(0.02)			
Youth			-0.03		
			(0.01)		
Time to Follow Up				-0.01	
				(0.01)	
Risk of Bias					0.00
					(0.00)
Const.	0.05			0.07	0.05
	(0.04)			(0.04)	(0.03)
Const. (Male)		0.09*			
		(0.02)			
Const. (Adults)			0.04*		
			(0.01)		
Sample			Full		
N (Studies)			13		
N (Effect Sizes)			54		
I^2 (original vs. resid.)	35.2, 37.5	35.2, 16.3%	35.2, 38.7	35.2, 35.8	35.2, 37.8

Table 5. ctd.

Notes: [†] p<0.1, *p<0.05, **p<0.01, ***p<0.001. Standard errors from robust variance estimation in parentheses.

•

FIGURES

Figure 1. Flow Chart



Note: The flow chart depicts the flow of information that was processed throughout the different phases of the systematic review. The chart maps out the number of records identified, the records included and excluded, and the reasons for exclusions (see Moher et al. 2009).



Figure 2. Geographic Distribution of RCTs on Saving Promotion Interventions

Note: The figure shows the geographic Distribution of the 27 randomised controlled trials on saving promotion interventions in Sub-Saharan Africa that were identified in this systematic review.



Figure 3. Risk of Bias of Included Studies

APPENDIX 1. Search Strategy

List of databases searched:

- 1. Cochrane Collaboration Library
- 2. Campbell Collaboration Library
- 3. EPPI-Centre Library
- 4. 3ie Database for Systematic Reviews
- 5. DFID Database for Systematic Reviews
- 6. PsycINFO
- 7. International Bibliography of Social Science (IBSS)
- 8. SCOPUS
- 9. Web of Sciences
- 10. Applied Social Science Index and Abstracts (ASSIA)
- 11. Conference Proceedings Citation Index Social Science & Humanities
- 12. JOLIS (database of 14 World Bank and International Monetary Fund libraries)
- 13. ECONLIT
- 14. IDEAS/ RePEc
- 15. Business Source Premier
- 16. 3ie Impact Evaluation repository (indexed list of impact evaluations)
- 17. 3ie RIDIE (list of ongoing/registered impact evaluations)
- 18. Abdul Latif Jameel Poverty Action Lab (J-PAL)
- 19. Innovations for Poverty Action (IPA)

20. Eldis

- 21. USAID Development Experience Clearing House:
- 22. World Bank Impact Evaluation Working Paper Series
- 23. Research4Development (DFID)
- 24. African Development Bank Evaluation Reports:
- 25. http://www.afdb.org/en/documents/evaluation-reports/
- 26. Agence Française de Développement: Impact Evaluations
- 27. Asian Development Bank Evaluation Resources
- 28. Inter-American Development Bank Evaluations

Search String:

An example of the search string used for ProQuest interface is presented below:

Search number	Search
#1	(AB,TI (saving* OR microsaving* OR ROSCA* OR stokvel* OR "savings
[Program]	group*" OR (rotating saving credit association) OR VSLA OR (village
-	saving* loan association) OR ASCA OR "village bank*" OR (financial
	NEAR/2 (literacy OR literate)) OR Aflatoun OR YouthSafe OR (financial
	NEAR/2 education) OR (economic NEAR/2 (literacy OR literate)) OR
	(economic NEAR/2 education) OR (financial NEAR/2 skills) OR
	(economic NEAR/2 skills) OR (financial NEAR/2 training) OR (economic
	NEAR/2 training) OR (financial NEAR/2 knowledge) OR (economic
	NEAR/2 knowledge) OR "financial inclusion" OR banking OR budgeting
	OR "money manag*" OR earmarking OR "saving NEAR/2 account*" OR
	"bank account*" OR "youth account*" OR "lock box" OR "piggy bank" OR

OR (SU(financial inclusion OR banking OR financial literacy OR saving*#2(AB, TI ("randomized control* trial" OR "randomised control* trial" OR randomised OR randomized OR RCT OR randomly OR trial OR experiment* OR "control group" OR "comparison group")) OR (SU(randomised control study OR randomised control trial OR randomised controlled trial OR randomized control trial OR randomized control trial OR randomised control study OR randomised control trial OR randomised controlled trial OR randomized control trial OR randomized))
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[Study Design] randomised OR randomized OR RCT OR randomly OR trial OR experiment* OR "control group" OR "comparison group")) OR (SU(randomised control study OR randomised control trial OR randomised controlled trial OR randomized control trial OR randomized	
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APPENDIX 2. Forest Plots

1. Forest Plot for Total Savings

		F	orest Plot				
Studies						Effect Size	Weight
Batista & Vicente 2013 Average value of deposits Average number of deposits						0.005 0.065	80.634 80.634
Beaman et al. 2014 Total savings						0.072	203.047
Berry et al. 2015 Total savings (Allatoun arm) Total savings (Honest Money Box arm) Saving amount past week (Honest Money Box arm) Saving amount past week (Honest Money Box arm)			ŧ			-0.007 -0.029 -0.042 -0.039	46.724 46.724 46.724 46.724
Dizon et al. 2016 Total savings (6 months follow up) Total savings (1 year follow–up)				=		0.162 0.157	47.003 47.003
Brune et al. 2015 Total deposits into savings account Deposits into saving accounts				<u>.</u>		0.162 0.206	76.662 76.662
Buehren 2011 Total savings						-0.002	141.513
Dupas, Keats & Robinson 2016 ROSCA contributions (dual-headed household) ROSCA contributions (single-headed household) Bank deposits (dual-headed household) Bank deposits (single-headed household) Deposits to home savings (single-headed household) Deposits to home savings (dual-headed household)				_		0.055 0.049 0.200 0.022 0.028 0.052	14.452 14.452 14.452 14.452 14.452 14.452 14.452
Dupas & Robinson 2013a Bank savings (market vendor subsample) Bank savings (full sample)			 			0.275 0.260	34.322 34.322
Jamison et al. 2014 Total savings (Banking arm) Total savings (Literacy arm) Total savings (Baking & Literacy arm)						0.046 0.101 0.100	47.017 47.017 47.017
Karlan & Linden 2014 Self-reported total savings (cash & parent outreach arm) Self-reported total savings (voucher & parent outreach arm) Self-reported total savings (cash only arm) Self-reported total savings (voucher only arm)						-0.026 -0.106 -0.011 -0.072	39.961 39.961 39.961 39.961
Karlan et al. 2012 Total saving deposits			-			0.042	220.525
Ksoll et al. 2016 Total savings (log)						0.142	128.797
Carter et al. 2015 Formal savings			- 1			0.376	83.635
Schaner 2015 Savings (individual account) Savings (pouse account) Savings (joint account)			=			0.105 -0.062 -0.052	44.690 44.690 44.690
Lee et al. 2015 Average monthly net savings (marketing outreach arm) Average monthly net savings (In-school banking arm)			- <u>+</u> -			0.098 0.127	87.726 87.726
Supanantroek, Lensik & Hansen (in press) Total Savings			_ _			0.038	152.861
Sayinzoga et al. N/A Total Savings			<u>.</u>			0.280	62.253
Dupas et al. 2016 Total deposits Uganda Total deposits Malawi Total monetary savings Uganda Total monetary savings Malawi Total savings stock (administrative records) Uganda Total savings stock (administrative records) Malawi			+++++-+			0.033 0.001 0.088 0.062 0.054 0.060	27.256 27.256 27.256 27.256 27.256 27.256 27.256
	-1	-0.5	I 0 Effect Size	0.5	1		

2. Forest Plot Savings Attitudes

Studies						Effect Size	Weight
Berry et al. 2015 Saving Attitudes Index (Aflatoun Arm) Saving Attitudes Index (Money Box Arm)						0.010 0.034	292.821 292.821
Karlan & Linden 2014 Savings Attitude Index (Cash & Parent Arm) Savings Attitude Index (Voucher & Parent Arm) Savings Attitude Index (Cash Arm) Saving Attitudes Index (Voucher Arm)			╶╴ ╶┿ ╺┿╴			0.023 0.064 0.039 0.015	94.072 94.072 94.072 94.072
Supanantroek, Lensik & Hansen (in press) Saving Attitudes Index			-			0.117	343.243
Shephard et al. (under review) Saving Attitudes Index						0.102	343.935
			\				
	-1	-0.5	0	0.5	1		
			Effect Size				

3. Financial Literacy

Studies						Effect Size	Weight
Batista & Vicente 2013			:				
Knowledge about savings						0.172	25.832
Knowledge about deposits			÷∎			0.178	25.832
			i				
Berry et al. 2015							
Financial literacy index						0.009	27.092
Financial literacy index			- -			-0.003	27.092
laminon at al. 2014							
Jamison et al. 2014 Financial knowledge index (Banking arm)			1			-0.017	5 501
Financial awareness index (Banking arm)			I			-0.049	5.501
Financial literacy index (Banking arm)			<u> </u>			-0.048	5.501
Financial knowledge index (Literacy arm)			<u> </u>			0.162	5 501
Financial awareness index (Literacy arm)			<u> </u>			0.040	5.501
Financial literacy index (Literacy arm)			<u>+</u>			0.095	5,501
Financial knowledge index (Banking & Literacy arm)						0.160	5.501
Financial awareness index (Banking & Literacy arm)			——————————————————————————————————————			0.080	5.501
Financial literacy index (Banking & Literacy arm)						0.142	5.501
			1				
Berg & Zia 2014			1				
General financial literacy			— ∎ —!			-0.033	23.523
Content-specific financial literacy			_			0.117	23.523
			i i				
Coville et al. 2014			1			0.010	0.000
Financial literacy (artinmetic index) (Banking arm)			 _			0.042	0.986
Financial literacy (weighted index) (Banking arm)						0.071	0.980
Financial literacy (artinmetic index) (Literacy arm)		-				-0.000	0.900
Financial literacy (weighted index) (Elleracy arm)						-0.059	6.096
Financial literacy (weighted index) (Banking & Literacy arm)		_				-0.028	6.986
I manoial merady (weighted index) (Danking d Ellerady ann)			- i			0.020	0.000
Savinzoga et al. N/A			i				
Financial literacy score						0.288	34.299
			· · -				
Shephard et al. (under review)			! _				
Financial capability				•		0.317	50.876
			i –				
	r				-		
	-1	-0.5	0	0.5	1		
		0.0		0.0			
			Effect Size				

4. Forest Plot Business Investment

Studies				Effect Size	Weight
Brune et al. 2015 Total value of agricultural inputs (Banking arm) Total value of agricultural inputs (Commitment arm)		- -		0.063 0.108	125.221 125.221
Dupas, Keats & Robinson 2016 Total spent on farming inputs (dual-headed household) Total spent on farming inputs (single-headed household) total business investment (single-headed household) total business investment (dual-headed household)		<u> </u>		0.156 -0.089 -0.005 0.043	27.749 27.749 27.749 27.749
Dupas & Robinson 2013a Business investment (full sample) Business investment (female vendors)		<u>+</u>		0.172 0.139	41.904 41.904
Flory 2014 Fertilizer expenditures Land expenditures Seedlings expenditures				0.068 0.085 0.045	89.836 89.836 89.836
Jamison et al. 2014 Money spent on business last 6 months (Banking arm) Money spent on business last 6 months (Literacy arm) Money spent on business last 6 months (Banking & Literacy arm)				-0.008 -0.014 -0.027	73.042 73.042 73.042
Karlan et al. 2012 Agricultural inputs use index Total expenditure on agricultural inputs (last 12 months)		5		0.000 0.022	248.631 248.631
Ksoll et al. 2016 Any use of fertilizer on maize				0.160	191.032
Carter et al. 2015 Agricultural investment: Irrigation Agricultural investment: Machinery Agricultural investment: Other Non-agricultural investments: Property Non-agricultural investments: Other Agricultural investment: Land Investment index				-0.002 -0.045 0.195 0.099 -0.032 -0.047 0.035	15.351 15.351 15.351 15.351 15.351 15.351 15.351 15.351
Dupas et al. 2016 Business inventory Uganda Business inventory Malawi Expenditures on agricultural inputs (last month) Uganda Expenditures on agricultural inputs (last month) Malawi				-0.043 0.065 0.001 -0.026	66.887 66.887 66.887 66.887
	-1 -0.5	0 Effect Size	1 0.5 1		

5. Forest Plot for Business Profit

Studies						Effect Size	Weight
Beaman et al. 2014 Value of livestock Business profit Value of agricultural output household (household)			-+ + +			0.066 0.035 0.011	476.190 476.190 476.190
Brune et al. 2015 Farm profit (Banking arm) Farm profit (Commitment arm)			+			0.064 0.101	217.391 217.391
Dupas & Robinson 2013a Business revenues (full sample) Business revenues (female vendors)			- <u>i</u>	_		0.130 0.088	49.020 49.020
Flory 2014 Crop income			÷			0.082	500.000
Jamison et al. 2014 Busines ownership earnings (Banking arm) Busines ownership earnings (Literacy arm) Busines ownership earnings (Banking & Literacy arm)						0.028 -0.004 -0.001	119.048 119.048 119.048
Karlan et al. 2012 Business profits (last 12 months)			, i			0.043	3333.333
Ksoll et al. 2016 Value of agricultural sale (log) Total income from all businesses			<u> </u>			-0.062 0.044	142.857 142.857
	-1	–0.5	¢ 1	Г 0.5	1		
			Effect Size				

6. Forest Plot for Poverty (Income/Expenditures)

Studies						Effect Size	Weight
Beaman et al. 2014 Monthly non-food expenses per adult			- 			-0.010	274,477
Brune et al. 2015 Total expenditure last 30 days (Banking arm) Total expenditure last 30 days (Commitment arm)						0.065 0.105	95.360 95.360
Dupas, Keats & Robinson 2016 Tatal income (single-headed household) Total income (dual-headed household) Tatal expenditures (single-headed household) Tatal expenditures (dual-headed household)			ŧ			0.065 0.082 0.047 0.066	24.406 24.406 24.406 24.406
Dupas & Robinson 2013a Daily total expenditures (full sample) Daily total expenditures (female vendors)						0.152 0.160	38.042 38.042
Jamison et al. 2014 Expenditures in past 7 days (Literacy arm) Expenditures in past 7 days (Banking & Literacy arm) Expenditures in past 7 days (Banking arm) Total earnings last 90 days (Literacy arm) Total earnings last 90 days (Banking arm) Total earnings last 90 days (Banking & Literacy arm)						0.001 -0.007 -0.003 0.099 0.098 0.114	28.769 28.769 28.769 28.769 28.769 28.769 28.769
Karlan et al. 2012 Total selected expenditures (monthly) Living under \$1.25 per day (per capita) Living under \$2.50 per day (per capita)			4			0.017 0.003 0.002	102.776 102.776 102.776
Ksoll et al. 2016 Predicted per capita expenditure (log)			- i			0.110	154.696
Annan et al. 2013 Total expenditures per capita Per capita consumption expenditures Below the poverty line of 1.25 USD (Poverty Headcount) Consumption			÷			0.115 0.105 0.314 0.144	45.317 45.317 45.317 45.317
Carter et al. 2015 Per capita consumption expenditures (log)						0.204	94.847
Schaner 2015 Monthly income (20% interest individual account) Monthly income (20% interest spouse account) Monthly income pooled (individual account) Monthly income pooled (individual account) Monthly income pooled (joint account) Monthly income (4% interest individual account) Monthly income (4% interest spouse account) Monthly income (4% interest joint account) Monthly income (12% interest individual account) Monthly income (12% interest joint account) Mont			-┿┿┤ ┼╎╎╎╎╎╎╎╎╎╎╎╎			0.138 0.017 0.056 0.142 -0.006 0.093 0.159 0.097 0.062 0.140 0.082 0.005	13.419 13.419 13.419 13.419 13.419 13.419 13.419 13.419 13.419 13.419 13.419 13.419 13.419 13.419
	-1	-0.5		0.5	1		
			Effect Size				

7. Food Security

Studies						Effect Size	Weight
Beaman et al. 2014 Weekly food consumption Food insecurity index			•			0.049 0.089	294.962 294.962
Dupas, Keats & Robinson 2016 Food expenditures Food expenditures			<u> </u>			-0.020 0.032	60.224 60.224
Dupas & Robinson 2013 a Daily food expenditures (full sample) Daily food expenditures (female vendors sub-sample)				=		0.168 0.172	44.634 44.634
Jamison et al. 2014 Number of meals w. meat last 7 days (Banking Arm) Number of meals w. meat last 7 days (Literacy Arm) Number of meals w. meat last 7 days (Banking & Literacy Arm)						0.070 0.090 0.100	86.156 86.156 86.156
Karlan et al. 2012 Food insecurity index Total food consumption (last 7 days)						0.012 -0.008	383.934 383.934
Ksoll et al. 2016 Number of months with fewer than three meals a day Number of meals yesterday Food consumption per week per adult equivalent (log)			_ <u>+</u>			0.045 0.136 0.054	74.036 74.036 74.036
Annan et al. 2013 Number of meals per day Food consumption per capita						0.062 0.106	138.800 138.800
Dupas et al. 2016 Food expenditures (last 7 days) Uganda Food expenditures (last 7 days) Malawi			 			0.062 0.044	172.277 172.277
	t.	1	<u>i</u>	1			
	-1	-0.5	0 Effect Size	0.5	1		

Note: A number of studies measured food insecurity and reduction thereof. Effect sizes were therefore reverse-coded.

8. Forest Plot for Household Assets

Studies						Effect Size	Weight
Beaman et al. 2014 Value of lifestock household Housing quality index			-			0.076 0.052	129.954 129.954
Dupas & Robinson 2013a Savings in lifestock (full sample) Savings in lifestock (female vendors)				2		0.168 0.167	37.460 37.460
Flory 2014 Asset index						0.099	194.269
Karlan et al. 2012 Asset index (weighted) Housing quality index						0.006 -0.004	145.032 145.032
Ksoll et al. 2016 Asset count						-0.071	150.028
Annan et al. 2013 Assets index (14 individual items) (DiD Estimation) Ownership of common household goods Assets index (14 individual items) (ANCOVA estimation)						0.113 0.137 0.051	58.017 58.017 58.017
Carter et al. 2015 Asset index (log) Durable goods Livestock						0.148 0.046 0.131	31.015 31.015 31.015
Schaner 2015 Total assets (Individual account) Total assets (Spouse account) Total assets (Joint account) Net assets (Individual account) Net assets (Spouse account) Net assets (Joint account)						0.131 -0.136 0.084 0.160 -0.136 -0.103	25.651 25.651 25.651 25.651 25.651 25.651 25.651
Dupas et al. 2016 Assets (house items and animals) Malawi Assets (house items and animals) Uganda			*			-0.082 0.020	99.079 99.079
	-1	і –0.5	0 Effect Size	0.5	1		

9. Forest Plot Education

Studies						Effect Size	Weight
Beaman et al. 2014 Education index			÷-			0.026	1767.953
Batista & Vicente 2013 Education index			-			0.026	1428.571
Jamison et al. 2014 Own money spent on school fees (past 6 months) (Banking arm) Own money spent on school fees (past 6 months) (Literacy arm) Own money spent on school fees (past 6 months) (Banking & Literacy arm)			+			0.042 0.043 0.053	119.048 119.048 119.048
Karlan & Linden 2014 School supplies index (cash & parent outreach arm) School supplies index (voucher & parent outreach arm) School supplies index (cash only arm) School supplies index (voucher only arm) Attendance index (cash & parent outreach arm) Attendance index (voucher & parent outreach arm) Attendance index (voucher only arm) Attendance index (coucher only arm)						0.083 0.018 -0.039 -0.030 0.015 0.000 -0.038 -0.022	60.976 60.976 60.976 60.976 60.976 60.976 60.976 60.976
Karlan et al. 2012 Education expenses (last 12 months)			, in the second			-0.002	3333.333
Carter et al. 2015 Education expenses						-0.005	131.579
Dupas et al. 2016 Education outcomes index Uganda Education outcomes index Malawi			+			0.002 0.036	263.158 263.158
	-1	I _0.5	0 Effect Size	0.5	1		

10. Forest Plot School Enrolment

Studies						Effect Size	Weight	
Jamison et al. 2014 Currently attending school (Banking arm) Currently attending school (Literacy arm) Currently attending school (Banking & Literacy arm)		-		_		0.000 -0.076 0.062	18.816 18.816 18.816	
Karlan & Linden 2014 Enrollment Rate (Cash & parent outreach arm) Enrollment Rate (Voucher & parent outreach arm) Enrollment Rate (Cash only arm) Enrollment Rate (Voucher only arm)		_		_		-0.123 0.003 0.112 0.003	14.681 14.681 14.681 14.681	
Karlan et al. 2012 Primary school enrollment (girls) Primary school enrollment (boys) Secondary school enrollment (girls) Secondary school enrollment (boys)			1			0.146 0.161 0.091 0.042	34.673 34.673 34.673 34.673	
	-1	- 0.5		0.5	1 1			
			Effect Size					

11. Forest Plot General Health and Health Investment

Studies	,	Effect Size	Weight
Beaman et al. 2014 Health index (serious illness in household)		0.001	1366.934
Dupas & Robinson 2013b		0.128 0.002 0.040 0.169 0.182 0.070 0.273 0.040	21.727 21.727 21.727 21.727 21.727 21.727 21.727 21.727 21.727 21.727
Jamison et al. 2014 Money spent on health-related purposes (Literacy arm) Money spent on health-related purposes (Banking arm) Money spent on health-related purposes (Banking & Literacy arm)		-0.004 0.046 -0.039	117.721 117.721 117.721
Total health expenses		0.010	3016.005
Dupas et al. 2016 I Health Outcomes Index (sickness in household) Uganda Image: Comparison of the second s		0.006 -0.019 0.048 -0.020	129.429 129.429 129.429 129.429
♀ -1 -05 0	1 05 1		
Effect Size	0.0 1		

Note: For Dupas & Robinson 2013b the outcome 'Could not afford medical treatment' was reverse-coded.

Study	Supply:	Supply:	Demand	External
1 2012	Informal	Formal	Demand	Control
Annan et al. 2013	l	0	0	l
Batista & Vicente 2013	0	l	l	0
Beaman et al. 2014	1	0	0	1
Berry et al. 2015 (Arm 1)	1	0	1	0
Berry et al. 2015 (Arm 2)	1	0	1	0
Dizon et al. 2016	0	1	1	0
Brune et al. 2015 (Arm 1)	0	1	1	0
Brune et al. 2015 (Arm 2)	0	1	1	1
Buehren 2011	0	0	1	0
Dupas, Keats & Robinson 2016 (Arm 1)	0	1	0	0
Dupas, Keats & Robinson 2016 (Arm 2)	0	1	0	0
Dupas, Keats & Robinson 2016 (Arm 3)	0	1	0	0
Dupas & Robinson 2013a	0	1	0	0
Dupas & Robinson 2013b (Arm 1)	1	0	1	0
Dupas & Robinson 2013b (Arm 2)	1	0	0	1
Dupas & Robinson 2013b (Arm 3)	1	0	0	1
Dupas & Robinson 2013b (Arm 4)	0	1	0	1
Flory 2014	0	1	1	0
Jamison et al. 2014 (Arm 1)	0	1	0	0
Jamison et al. 2014 (Arm 2)	0	0	1	0
Jamison et al. 2014 (Arm 3)	0	1	1	0
Karlan & Linden 2014 (Arm 1)	1	0	1	1
Karlan & Linden 2014 (Arm 2)	1	0	1	1
Karlan & Linden 2014 (Arm 3)	1	0	1	1
Karlan & Linden 2014 (Arm 4)	1	0	1	1
Karlan et al. 2012	1	0	0	1
Ksoll et al. 2016	1	0	0	1
Berg & Zia 2014	0	0	1	0
Carter et al. 2015	0	1	1	0
Schaner 2015 (Arm 1)	0	1	0	0
Schaner 2015 (Arm 2)	0	1	0	0
Schaner 2015 (Arm 3)	0	1	0	0
Lee et al. 2015 (Arm 1)	0	1	1	1
Lee et al. 2015 (Arm 2)	0	1	1	1
Supanantroek et al. (in press)	0	0	1	0
Coville et al. 2014 (Arm 1)	0	1	0	0
Coville et al. 2014 (Arm 2)	0	0	1	0
Coville et al. 2014 (Arm 3)	0	1	1	0
Sayinzoga et al. N/A	0	0	1	0
Shephard et al. (under review)	0	0	1	0
Dupas et al. 2016	Õ	1	0	Õ

APPENDIX 3. Coding of Intervention Components

Notes: *Formal supply* includes access to a formal bank account, mobile banking, or 'banks on wheels' programmes or a reduction in usage costs for these products; *informal supply* includes any form of savings groups such as ROSCAs, VSLAs, or school savings clubs, as well as the supply of money boxes; *demand* includes financial literacy programmes and programmes with motivational components such as savings reminders, priming on savings goals, or encouragements to save; external *behavioural control* included formal commitment devices such as automated withdrawal and transaction regulations in formal banking, peer pressure and regulatory frameworks (e.g. fixed cycles) in savings groups, and strict earmarking of usage of savings money.

APPENDIX 4. Risk of Bias Assessment

Study	Random Sequence Generation (Selection bias)	Allocation Concealment (Selection bias)	Blinding of participants (Performance bias)	Blinding of outcome assessment (Detection bias)	Incomplete outcome data (Attrition bias)	Selective Reporting (Reporting bias)	Implementation fidelity	Baseline Differences	Potential for Spillover or Contamination
Annan, Bundervoet.	Low	Low	High	Low	High	Unclear	Unclear	Unclear	Low
Seban & Costigan 2013	Randomisation through public lottery	Randomisation through public lottery	No placebo treatment was possible, outcomes based on self-report	No potential conflict of interest identified	Due to technology failure leading to lost data authors are only able to report only the results of the first cycle; differential attrition after first project cycle, treatment drop- outs less educated and wealthy	No protocol identified	Collection on participant feedback on the delivery and content of the intervention, in the final session groups reflected on the previous sessions, evaluated the program, and discussed their pledges; use of process indicators (Savings, loans, loan size, rates of return)	Descriptive comparison of baseline characteristics but no significance tests	Randomisation at cluster-level to limit spillover
Batista & Vicente 2013	Unclear	Unclear	High	Low	High	Unclear	Unclear	High	High
Vicence 2015	No details on randomisation procedure provided	No details on randomisation & treatment assignment procedure provided	Participants interviewed on the spot right after the intervention had been delivered, no placebo treatment was possible	The study used administrative data in addition to self- reports for savings outcome; no potential conflict of interest identified	No checks for differential attrition were run	No protocol identified		Significant differences on a range of outcomes, including electricity supply, assets & property and income	Potential spillover due to attendance at the community meeting or theatre held for programme dissemination or through social network transmission
Beaman, Karlan &	Low	Unclear	High	Low No potential	Low	Low	Low	Low	Low
Thuysbaert 2014	Randomisation was stratified by commune and re□randomisation procedure was used to ensure balance on village characteristics	Not sufficient detail provided	Self-reported outcomes (e.g. savings attitudes only, no placebo treatment was possible	conflict of interest identified	Authors do not find evidence for differential attrition between treatment arms	Protocol published in AEA registry (AEARCTR- 0000102), discrepancies: intermediate savings outcomes not listed in protocol	Assessment of training of programme agents, randomisation of two training models: 1) structured, 2-day training, certificate; vs. 2) organic, no formal training agents, stronger impact for training approach 1)	Mean comparisons and orthogonality suggest balance across treatment arms	Self□replicating savings groups, but spillover was intended (6% of control group received the intervention), but power calculations was set up to factor this in
Berg & Zia 2014	Low	Low	Low	Low	Low	Unclear	Unclear	Low	High
	Randomisation implemented in	Identifying information on	Control received a quasi-placebo	No potential conflict of interest	General attrition was low (<10%)	No protocol identified		Test of differences in means suggest	No control over who watches TV show,

	STATA using a random number generator	participants anonymised so we as researchers only had access to unique codes that identified participants in the study	treatment: Soap opera on unrelated topic	identified	and no evidence for differential attrition between treatment arms			balance across treatment arms	control group participants do not receive invitation for specific programme but it is still possible that they watch it
Berry, Karlan	Low	Low	High	Low	Low	Low	Low	Low	Low
& Pradhan 2015	Randomisation implemented in STATA using a random number generator	Schools were enrolled in the study before the assignment was done, using STATA helped to prevent that anyone knew the sequence of assignments until everyone had been assigned	Potential for a Hawthorne effect: behaviour change could be an artefact of the intervention (i.e. substituting savings at home for savings at school while no visible change in attitudes)	No potential conflict of interest identified	General attrition was very low (<2%) and no evidence for differential attrition between treatment arms	Protocol published in AEA registry (AEARCTR- 0000107)	Innovation for Poverty Action monitored programme implementation throughout the study period by visiting schools and interviewing teachers and students about the progress and activities of the savings club	Few significant differences at baseline (2 out of the 11 indices not balanced at 10 % level), all outcome analyses include controls for baseline values of outcome measures	Randomisation was conducted at the school- level to limit spillover
Brune et al.	Low	Low	High	Low	Low	Low	Unclear	Low	Low
2013	Random number generator used	Treatment assignment was done independently from those who administered the treatment	Most outcomes were self- reported (except from savings), no placebo treatment was possible	No potential conflict of interest identified	General attrition was low (<10%) and no evidence for differential attrition between treatment arms	Protocol published in AEA registry (AEARCTR- 0000205), discrepancies: only baseline reports of financial literacy		The authors find a range of significant baseline differences, but control for these variables in the regression	Randomisation was conducted at the club- level to limit spillover
Buehren 2011	Low	Low	High	Unclear	High	Unclear	Unclear	High	High
	Randomisation implemented in STATA using a random number generator	It was not possible for participants or for the credit officers to select Microfinance members based on the future treatment assignment	Self-reported outcomes and no placebo treatment was possible	Part of this work was carried out during paid consultancy work for BRAC Uganda	High attrition (>20%) and differential for formal schooling, business ownership, number of household members engaged in an income generating activity	No protocol identified		A range of significant baseline differences not controlled for in the regression	Possible contamination through other aspects of the microfinance programme, risk of spillover was low given that randomisation was conducted at the cluster- level to limit contamination and no indication of spill-over effects, even within households

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Carter, Laajaj & Yang 2015	Low	Unclear	High	Unclear	Low	Low	Low	High	Low
-	Randomisation was conducted by the research team on the computer of one of the PIs		Self-reported outcomes and no placebo treatment was possible	Not sufficient information	General attrition was low (<10%) and no evidence for differential attrition between treatment arms	Protocol published in AEA registry (AEARCTR- 0000239)	Training material and manual provided by authors, fidelity was reported as high, participation in training sessions was around 65% on average	The authors find some significant differences at baseline (due to chance), but fail to control for these in the regression	Randomisation was conducted at the cluster level to limit spillover
Cole et al. 2014	Low	Low	High	Low	Unclear	Unclear	Unclear	Low	Low
	Use of algorithm for random offer of the programme	Computer-based assignment	Self-reported outcomes and no placebo treatment was possible	No potential conflict of interest identified	ITT analysis but no reports on attrition	No protocol identified and method section not specific to outcomes		No significant differences for most baseline variables (exception is risk aversion but differences are numerically small)	Randomisation was conducted at the cluster level to limit spillover
Coville et al.	Low	Low	Low	Low	High	Unclear	Low	Low	High
2014	Randomisation implemented in STATA using a random number generator	Enumerators handing out invitations were not aware of treatment status	Control received a quasi-placebo treatment: Movie Screening with no financial education content and offered short session on hygiene after the screening	No potential conflict of interest identified	One treatment arm was dropped from the analysis due to high attrition, no ITT	No protocol identified	Participant-reported process evaluation: In the follow-up survey, authors asked for self- reported exposure for two reasons a) to confirm attendance, and b) to understand whether people recall activities and messages from the events; Finding: 95% of people recall receiving an invitation and 96% of the people recorded through administrative records as attending the event confirmed that they had	Most variables were balanced across arms, but significant difference in holding a bank account. This was added as a control in the regression	Although randomisation was on the cluster-level, spillover is possible given that clusters only had to be at least 20 meters away from the next closest business
Dizon, Gong & Jones 2016	Low	Low	Low	Low	Low	Low	Unclear	Low	High
	Randomisation implemented in STATA using a random number generator. Re- randomisation was conducted until a satisfactory balance	Researchers gave the implementing partners lists of participants and their treatment assignments	No placebo treatment was possible, but all outcomes use administrative records	No potential conflict of interest identified	General attrition was low (<10%) and no evidence for differential attrition between treatment arms	Protocol published in AEA registry (AEARCTR- 0000323)		177 baseline observables were compared between the treatment and control groups, conditional on geographic cluster and age. Significant	No cluster-level randomisation, information from programme could spill over to control group participants

	was reached							differences only for 4% of the variables.	
Dupas & Robinson 2013a	Low	Low	High	Low	High	Low	Low	Low	Low
	Randomisation implemented in STATA using a random number generator.	Participants were admitted prior to the assignment and at the time of enrolment, allocation concealment was thus possible	No placebo treatment was possible, some data is based on administrative records, but most on logbooks filled out by study participants	No potential conflict of interest identified	No differential attrition for market women but for male taxi drivers, those in treatment group were both more likely to be found and more likely to accept the logbooks	Protocol published in AEA registry (AEARCTR- 0001592)	Scripts for introducing financial products, otherwise no process evaluation required	No significant differences for most variables in both female and male sample, one significant difference was income at baseline, however, including the variable as control does not change the results	No cluster-level randomisation, but unlikely that control group participants can get access to a similar subsidised bank account
Dupas & Robinson 2013b	Low	Low	Low	Low	Low	Low	Unclear	Low	Low
Koonison 20100	Randomisation implemented in STATA using a random number generator.	Participants were admitted prior to the assignment and at the time of enrolment, allocation concealment was thus possible	Control received a quasi-placebo treatment: Encouragement to save for health while not receiving any savings technology	No potential conflict of interest identified and outcomes based on enumerator observations (counting of savings amounts)	Attrition was high (20%) but not differential between control and treatment arms	Protocol published in AEA registry (AEARCTR- 0001169)		Out of 84 coefficients for baseline differences only five coefficients were significant at the 10% level	Randomisation was done at the level of ROSCAS making spillover of health saving technologies to control ROSCAS unlikely
Dupas, Karlan,	Low	Low	Low	Low	Low	Low	Low	Low	Low
Kooinson & Ubfal 2016	Randomisation implemented in STATA using a random number generator	Participants were admitted prior to the assignment and at the time of enrolment, allocation concealment was thus possible	No placebo treatment was possible, but authors use administrative data and windsorize non- administrative data to correct for reporting-bias	No potential conflict of interest identified	Attrition was very low (<5%) and not differential between control and treatment arms	Protocol published in AEA registry (AEARCTR- 0000083)	Researchers monitored take-up and use of the bank accounts, and examined quantitative predictors and participants' explanations of the reasons for not taking up the accounts	Joint F-test finds no significant baseline differences	Unlikely that control group participants can get access to a similar subsidised bank account
Dupas, Keats & Robinson 2016	Low	Low	High	Low	Low	Low	Low	Low	Low
	Randomisation implemented in STATA using a random number generator	Participants were admitted prior to the assignment and at time of enrolment, allocation concealment was	Admin data	No potential conflict of interest identified	Evidence of differential attrition authors show that the experimental arms are balanced post- attrition and also	Protocol published in AEA registry (AEARCTR- 0000740)	Scripts for introducing financial products, otherwise no process evaluation required	Small and non- significant baseline differences across treatment arms	Randomisation was conducted at the cluster level to limit spillover

		thus possible			perform "placebo tests" checking whether treatment effects estimated are already there when estimated on the first survey round, but find that they are not				
Eissa,	Unclear	Unclear	Low	Low	High	Unclear	Unclear	Unclear	Low
Habyarimana & Jack 2014	No details on randomisation procedure provided	No details on randomisation & treatment assignment procedure provided	Control received a quasi-placebo treatment: weekly comics without financial education material	No potential conflict of interest identified	Due to changes in the Kenyan school calendar and teacher strikes a substantial number of schools could not be followed-up with after the intervention, no checks for differential attrition are reported	No protocol identified and method section not specific to outcomes	Exposure to the intervention was varied, not stated how this was assessed	No baseline statistics and tests provided	Randomisation was conducted at the cluster level to limit spillover
Flory 2014	Low	Low	High	Low	Low	High	Unclear	Low	Low
	Randomisation was done using a random number generator	Research assistants did not know the treatment assignment	No placebo treatment was possible and use of self-reported outcomes	No potential conflict of interest identified	Author does not discuss attrition explicitly, but Table reports test for differential attrition across groups which looks like attrition is not a problem, also use of ITT	No protocol identified, but some of the outcomes mentioned in the method section not reported, including savings balances; further there are only selective samples for distal outcomes	Only discussion on gender differences for service representatives: 18% of the information- treated communities were served by female financial services representative, and 82% being served by males, higher effects for women if delivered by female trainer	Only 1 out of 16 coefficients for balance tests was significant	Randomisation was conducted at the cluster level to limit spillover
Jamison,	Low	Low	High	Low	Low	Low	High	Low	Low
Karian & Zinman 2014	Randomisation implemented in STATA using a random number generator	Participants were in pre-existing groups of individuals (youth clubs in various	No placebo treatment was possible, potential 'teaching for	No potential conflict of interest identified	Attrition was very low (<5%) and not differential between control and treatment	Protocol published in AEA registry (AEARCTR- 0000080),	Authors shared detailed manual, but attendance was found to be only 50%, administrative problems were noted	Joint F-test finds no significant baseline differences	Randomisation was conducted at the cluster level to limit spillover

		towns) and then randomized at the cluster level	testing' effect, participants might simply report more savings due to image concerns		arms	discrepancies: food security was not mentioned in the protocol	with account opening		
Karlan & Linden 2014	Low	Low	Low	Low	Low	Low	Low	Low	High
	Randomisation implemented in STATA using a random number generator	No revealing of the random assignment before actual randomisation, then programme was rolled out in each location as per the assignment	No placebo treatment was possible, but authors put more weight on administrative data	No potential conflict of interest identified	Attrition rates are the same for treatment and control arms and there is no evidence of differential attrition	Protocol published in AEA registry (AEARCTR- 0000081)	Analysis of process outcomes as per student reports: 77% percent of treatment students were familiar with the Supersavers program, 39% were saving with Supersavers, and little difference in programme awareness as well as self-reported participation across treatment groups	Assignment to treatment was orthogonal to a list of baseline variables	Process evaluation finds that 11% of control group participants had heard of the programme, and 4% were saving within the programme
Karlan et al. 2012	Low	Low	Low	Low	Low	Unclear	Unclear	Low	High
2012	Randomisation implemented in STATA using a random number generator	No revealing of the random assignment before actual randomisation, then programme was rolled out in each location as per the assignment	Pragmatic, non- intrusive trial, village loan and savings associations pre- exist	No potential conflict of interest identified	Attrition was low (<10%) and not differential between control and treatment arms	No protocol identified		Joint F-test suggests that treatment status is orthogonal to a set of baseline covariates, however authors do find significant but substantially small differences for wealth, educational status and enrollment	Proximity of control villages to treatment villages caused some control villages to adopt the VSLA program
Ksoll et al. 2016	Low	Low	High	Low	Low	Low	Low	Low	High
	The randomisation was carried out under research supervision by field officers from the NGO, who drew village names from seven hats containing the villages in each stratum	The NGO implementing the programme identified 46 villages out of which the researchers allocated 23 to the treatment group and 23 to a waitlist control	No placebo treatment was possible, outcomes based on self-report	No potential conflict of interest identified	Attrition was very low (<5%) and not differential between control and treatment arms	Authors state that outcomes were pre-defined by the implementing partner	Detailed information on implementation/process evaluation obtained by the authors, a number of Implementation Reports monitor fidelity to the manual, largely implemented as intended (some delays to crop cycle due to currency crisis, only 9 instead of	Most variables were balanced across arms, only number of income generating activities was significant at 10% level	Evidence of contamination: More than 20% of households in control villages have become members of a savings group before the roll-out into control villages started

		receiving the same programme two years later					12 months training), no incentives provided but participation was high		
Lee et al. 2015	Unclear	Unclear	Low	Low	Unclear	Unclear	Unclear	High	Low
	No details on randomisation procedure provided	No details on randomisation & treatment assignment procedure provided	No placebo treatment was possible, but all outcomes use administrative records	No potential conflict of interest identified	No mentioning of attrition	No protocol identified		No mentioning of balance at baseline, some students were only recruited at follow-up, no baseline data available	Randomisation was conducted at the cluster level to limit spillover
McConnell 2012	Unclear	Unclear	High	Unclear	High	Unclear	Unclear	Low	High
	No details on randomisation procedure provided	No details on randomisation & treatment assignment procedure provided	No placebo treatment was possible, some outcomes based on self-report	Not sufficient information	There is attrition in the data, but author does not conduct any checks for differential attrition	No protocol identified and method section not specific to outcomes		Most baseline characteristics are balanced but significant differences between arms for loans from friends and	Risk of spillover given that randomisation on individual level and implementation of the programme in a market setting
Sayinzoga,	Unclear	Unclear	High	Unclear	Low	Unclear	Unclear	Low	Low
Buite & Lensink NA	No details on randomisation procedure provided	No details on randomisation & treatment assignment procedure provided	No placebo treatment was possible, outcomes based on self-report	Not sufficient information	Checks suggest no significant differential attrition	No protocol identified		No significant differences in baseline characteristics between treatment and control group	Randomisation was conducted at the cluster level to limit spillover
Schaner 2013	Low	Low	High	Low	Low	High	High	Low	Low
	Respondents took separate draws for each potential account	Field staff were carefully trained not to allow a respondent more than one draw	No placebo for the ATM treatment, for the interest rates, treatment group participants could see which interest rate they drew from the hat and which other results of this drawing would have been possible	No potential conflict of interest identified	Attrition was low (<10%) and not differential between control and treatment arms	A high number of different results reported across papers, protocol was published in the AEA registry (AEARCTR- 0001358) stating less outcomes of interest than were eventually tested in the papers	Authors assesses attendance/takeup, takeup was only 15%	Balance on most characteristics, but couples who received a free ATM card significantly less likely to be subsistence farmers and more likely to be entrepreneurs and have higher incomes, author therefore control for covariates significantly related to the free ATM	Individual-level randomisation, but unlikely that control group participants can get access to similar bank accounts with interest

								throughout the analysis	
Shephard, Kaneza &	Low	Low	High	Low	Low	Low	Low	Low	Low
Moclair (under review)	The list of schools was compiled by the second author in Rwanda and the first author conducted the random allocation in Amsterdam using a single random number generated sequence in Microsoft Excel	Schools were assigned to treatment arm by researchers, seven students from each of the teacher's classes were randomly selected by enumerators	No placebo treatment was possible, outcomes based on self-report	No potential conflict of interest identified	Attrition was high (30%) but within the acceptable range according to the What Works Clearinghouse guidance on attrition in educational interventions, no significant interaction effects between attrition and treatment status and attrition did not predict treatment status	The study protocol was registered at ClinicalTrials.go v as NCT02348580.	Authors assessed exposure and fidelity to the programme via a set of six questions on programme content (e.g. Are you familiar with the Aflatoun's Concept?). Familiarity with the program is 72.8% for treatment arm participants at endline.	Use of change scores to adjust for possible baseline differences	School level randomisation in view of pilot results suggesting that teacher level randomisation would result in spill-over from treatment to control teachers within a single school
Supanantaroek (in press)	High	High	High	Low	Unclear	Low	Unclear	Unclear	Low
	Study was conducted based based on available data set provided by PEDN which is a NGO in Uganda, both randomisation and intervention were done by them, authors therefore no insights into exact randomisation procedure	Authors no insights into exact allocation procedures	No placebo treatment was possible, outcomes based on self-report	No potential conflict of interest identified, research was not carried out by implementing organisation	Authors did not have any information on attrition and could not carry out attrition checks	Questionnaire is provided in the Appendix listing all outcomes	Questionnaire featured questions about the Aflatoun programme (implementation etc.), but not reported further in text	No baseline data available and no tests were conducted for balance at baseline	Randomisation was conducted at the cluster level to lifmit spillover

APPENDIX 5. Robustness Test: Wider Definition of Outcome Categories

	Savings				Consumption		Investment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
Informal Supply	-0.07*			-0.02			-0.01			
	(0.03)			(0.02)			(0.02)			
Additional Demand										
Component		0.02			0.04			0.00		
		(0.03)			(0.03)			(0.03)		
Additional Behavioural										
Control Component			-0.01			-0.02			-0.00	
			(0.03)			(0.02)			(0.02)	
Const. (Formal supply)	0.09**			0.08**			0.04*			
	(0.02)			(0.02)			(0.01)			
Const. (Any Supply)		0.05*	0.07*		0.06***	0.08**		0.04*	0.04*	
		(0.02)	(0.02)		(0.01)	(0.02)		(0.01)	(0.01)	
Sample		Supply			Supply			Supply		
N (Studies)		17			12			13		
N (Effect Sizes)		62			65			59		
I^2 in % (original, resid.)	68.5, 61.2	68.5, 69.4	68.5, 70.0	63.0, 65.0	63.0, 54.2	63.0, 65.2	34.0, 35.5	34.0, 37.6	34.0, 37.0	

Meta-Regression: Intervention Components (Wider Definition)

Notes: [†] p<0.1, *p<0.05, **p<0.01, ***p<0.001. P-values do not match regular t-statistics due to small sample correction as suggested in Tipton (2015). Parentheses around significance stars indicate coefficients with adjusted degrees of freedom below 4. Standard errors from robust variance estimation are in parentheses. Sample only includes study arms that feature any kind of supply component for better interpretability. Intervention channels are defined as follows: (i) *formal supply* indicates that intervention includes a component that grants or improves access to institutionalised banking, (ii) *informal supply* indicates that intervention includes a component that induces or supports savings groups or provides moneyboxes etc., (iii) *additional demand component* indicates a literacy or motivational scripts/outreach component in addition to a supply component, and (iv) *additional behavioural control component* indicates the addition of a commitment devices, peer pressure and regulatory frameworks in groups (e.g. fixed cycles), or strict earmarking of savings. For definition of outcome categories, see Section 5.3.

			Savings					Consumption		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Duration	-0.04					-0.05**				
	(0.02)					(0.01)				
Female		-0.08*					-0.06 [†]			
		(0.03)					(0.02)			
Mixed		-0.07					-0.01			
		(0.04)					(0.03)			
Youth			-0.05					-0.03 [†]		
			(0.04)					(0.01)		
Time to Follow Up				-0.01					-0.02*	
				(0.01)					(0.01)	
Risk of Bias					0.00					0.01
					(0.01)					(0.01)
Const.	0.12**			0.08^{+1}	0.07*	0.10***			0.14**	0.15*
	(0.03)			(0.04)	(0.03)	(0.02)			(0.03)	(0.04)
Const. (Male)		0.12*					0.09*			
		(0.03)					(0.02)			
Const. (Adults)			0.08***					0.08***		
			(0.02)					(0.01)		
Sample			Full					Full		
N (Studies)			20					12		
N (Effect Sizes)			66					70		
I^2 (original vs. resid.)	66.3, 63.9	66.3, 63.0	66.3, 66.9	66.3, 67.6	66.3, 67.2	62.% 63.0	62.5, 61.0	62.5, 65.2	62.5, 59.1	62.5, 62.5

Meta-Regression: Study Design, Participant Characteristics and Bias

Notes: [†] p<0.1, *p<0.05, **p<0.01, ***p<0.001. P-values do not match regular t-statistics due to small sample correction as suggested in Tipton (2015). Parentheses around significance stars indicate coefficients with adjusted degrees of freedom below 4. Standard errors from robust variance estimation are in parentheses. *Intervention duration* is a dichotomous variable, coded 0 for brief ('once-off' or one day) and 1 for longer programmes. Savings groups programmes were coded as long given that groups meet in regular intervals over a longer cycle. *Participant sex* has three categories for primarily male, female, or mixed programme beneficiaries. The threshold for primarily male/female was defined as more than 75% of all participants. *Participant age* has three categories for adults, children/youth (up to 24 years), or both. *Time to follow-up* has four categories: 0-6 months, >6 months – 1 year, >1-2 years, and > 2 years. *Risk of Bias* was coded as a continuous variable with higher scores reflecting higher risk of bias as -1, unclear risk of bias as 0, and high risk of bias as +1.

						Meta-Regression: Study Design,		
	Investment					- Participant Characteristics and Bias (ctd.		
	(11)	(12)	(13)	(14)	(15)	-		
Duration	-0.01							
	(0.02)							
Female		-0.07*						
		(0.02)						
Mixed		-0.06*						
		(0.02)						
Youth			-0.04					
			(0.02)					
Time to Follow Up				-0.02				
				(0.01)				
Risk of Bias					0.00			
					(0.00)			
Const. Const. (Male)	0.05			0.08^{\dagger}	0.04			
	(0.04)			(0.03)	(0.02)			
		0.09*		. ,	· · ·			
		(0.02)						
Const. (Adults)			0.04*					
			(0.01)					
Sample			Full			-		
N (Studies)			13					
N (Effect Sizes)			62					
I ² (original vs. resid.)	33.7, 37.3	33.7, 13.9	33.7, 36.6	33.7, 30.7	33.7, 38.6			

Notes: [†] p<0.1, *p<0.05, **p<0.01, ***p<0.001. Standard errors from robust variance estimation in parentheses.