



Do saving promotion interventions increase household savings, consumption, and investments in Sub-Saharan Africa? A systematic review and meta-analysis

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ABSTRACT

Saving promotion interventions have gained momentum in international development in recent years. Our analysis investigates whether saving promotion can effectively increase savings, consumption, and future-oriented investments in Sub-Saharan Africa. In an extensive search of 28 academic and policy-focused databases in the fields of economics, psychology, and social sciences, 9330 titles and abstracts of published and unpublished studies were screened and 27 randomized controlled trials on saving promotion interventions fulfilled the inclusion criteria. Of these, 24 studies reporting on an aggregated sample of 87,025 individuals provided sufficient information to be included in the meta-analysis. 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 and significant 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. Results from meta-regressions suggest that supply-based programs are superior to demand-enhancing program types such as financial education. They further reveal reduced program effectiveness for women. Overall, findings from this analysis suggest that saving promotion schemes are highly relevant in reducing poverty in Sub-Saharan Africa, and that future efforts should focus on expansion of banking services to the poor as well as gender-sensitive programming.

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1. Introduction

Saving is widely recognized as an important means for sustainable cash-flow management and consumption smoothing for the poor (Karlan, Ratan, & Zinman, 2014). In response, scholars and practitioners alike have celebrated saving promotion programs as a promising poverty alleviation strategy for international development. Banerjee and Duflo (2011) go so far as to portray micro-savings as “the next microfinance revolution” (p. 190). Savings can serve as investment capital, for instance for business, education or job search (Curley, Ssewamala, & Han, 2010; Dupas & Robinson, 2013a; Karlan et al., 2012; Flory, 2016; Karlan & Linden, 2014), as self-insurance against health shocks and property damage (Dupas & Robinson, 2013b; Carter, Laajaj, & Yang, 2015),

and help smooth consumption over income contingencies (Brune, Giné, Goldberg, & Yang, 2015).

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

It remains to be seen whether saving promotion is an effective poverty reduction tool. Some scholars have depicted saving as a symptom of market failures in insurance systems (Hubbard & Judd, 1987; Loayza, Schmidt-Hebbel, & Servén, 2000) or as flawed in view of high inflation rates (Ndikumana, 2000). Conversely,

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other observational studies have pointed to beneficial impacts of savings on economic wellbeing (e.g. Beck, Demirgüç-Kunt, & Levine, 2007; Jalilian & Kirkpatrick, 2005), but remain limited by issues of reverse causality and endogeneity. More recently, randomized controlled trials (RCTs) have been popularized as a strong tool for generating causal evidence on the impact of development programs. In result, the number of RCTs evaluating the effectiveness of savings interventions has increased. Many of these have focused on the Sub-Saharan African region where a high percentage of people still live below the poverty line. New insights on the viability of saving promotion in reducing poverty are therefore highly relevant for designing adequate policies and programs in this region.

Using state-of-the-art systematic review methodology and meta-analysis techniques, the aim of this study is to quantitatively synthesize evidence on the effectiveness of saving promotion in Sub-Saharan Africa. Meta-analysis has several key strengths over individual quantitative studies: First, while a single study can generate findings with high internal validity, a systematic synthesis across multiple studies allows for more generalizable conclusions. Second, by pooling results across several studies and thus increasing the sample size, statistical power and precision of estimates is increased. Third, search strategies are set up to also identify and include estimates from unpublished studies, thus correcting pooled estimates for potential publication bias. Last and most importantly, cross-study estimates from a meta-regression can provide insights on how components of program design, intervention types, and participant characteristics may influence outcomes beyond the explanatory power of a single study (Card, 2012). Meta-analysis is therefore a first-choice tool to guide policy design.

Studies for our analysis were selected on three criteria. First, the intervention under evaluation had to feature a saving promotion component (e.g. access to formal bank accounts, savings groups, financial education on saving), excluding any intervention that combines saving promotion with additional components that could hypothetically have an impact on poverty, financial stress, or saving behavior through another mechanism.¹ Second, the intervention had to be evaluated within a randomized controlled set-up. Exclusive focus on randomized controlled trials, considered as the 'gold standard' approach to impact evaluation, aimed to ensure high internal validity of considered studies to obtain the most credible effect size estimates.² Third, the study had to report impacts on saving- and poverty-related outcomes. We allow for a relatively wide range of relevant outcome measures to gain a nuanced understanding of possible impacts. Existing RCTs have 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 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 poverty-related *distal outcomes*, including consumption, education, and health.

Our database search identifies 27 eligible randomized controlled trials on saving promotion programs. Results from our meta-analysis 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.

To our knowledge, the present review is the first to quantitatively synthesize evidence on a comprehensive range of saving promotion interventions. Three systematic reviews have been carried out to investigate the impact of general financial literacy programs. 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 from low- and middle-income countries (Fernandes, Lynch, & Netemeyer, 2014; Kaiser & Menkhoff, 2016; O'Prey & Shephard, 2014). Three further reviews examined a broader range of programs, including microcredit interventions and self-help groups and therefore feature programmatic components that could impact poverty alleviation through channels other than saving (Brody et al., 2015; Duvendack et al., 2011; Stewart et al., 2012). Another review put exclusive focus on formal banking services in low- and middle-income countries, thus excluding a range of other saving interventions such as promotion of savings groups (Pande, Cole, Sivasankaran, Bastian, & Durlacher, 2012).

The remainder of this paper proceeds as follows. The next section defines the geographic scope of our review. Section 3 discusses the theoretical literature on saving promotion interventions and their outcomes. Section 4 describes the data source and the measurement of variables. Section 5 introduces the statistical methods for effect size aggregation and meta-regression. The main results are presented and discussed in Section 6, before the conclusions set out in Section 7.

2. Geographic scope

This systematic review is focused on Sub-Saharan Africa as motivated by two key considerations. First, Sub-Saharan Africa remains one of the most impoverished and under-served regions and its study is therefore justified from an equity perspective. Financial inclusion on the continent continues to lag behind and penetration of formal banking is the lowest globally. Across Sub-Saharan Africa, only 35% of adults hold a bank account (largest access rates are in Kenya and South Africa), compared to at least 50% in Asia, Latin America, and the Caribbean, and 95% in high-income countries (World Bank, 2016; Demirgüç-Kunt & Klapper, 2012). The aggregated savings rates in Sub-Saharan Africa only amounts up to 15% of the gross national income. While savings rates have been rising in other regions over the past few decades (e.g. doubled in East Asia), they have stagnated in Sub-Saharan Africa (Loayza et al., 2000). Apart from this, the global disease burden is still highest in Sub-Saharan Africa and financial mechanisms to alleviate the impact of associated income shocks are therefore most warranted. For instance, both prevalence and mortality rates from HIV/AIDS exceed those of other regions, and 75% of all new global HIV infections in 2015 were registered in Sub-Saharan Africa (see Wang et al., 2016). In absence of formal insurance mechanisms, precautionary saving can be a crucial protection mechanism against the financial burden resulting from death or chronic illness of a breadwinner.

Second, our geographic focus is essential in limiting heterogeneity of settings and populations. In line with previous scholars, we argue that "context matters" for program design and particularly for an underlying theory of change (see Bates & Glennster,

¹ We therefore exclude programs with components such as microcredit, insurance, mentorship, or cash transfers. We further exclude programs featuring financial incentives to save, such as provision of monetary top-ups contingent on realized savings amounts (see Ssewamala, Han, Neilands, Ismayilova, & Sperber, 2010a; Ssewamala & Ismayilova, 2009; Ssewamala et al., 2010b.). Incentivization 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.

² Although there is a range of high-quality quasi-experimental study designs, comparison analyses have pointed to discrepancies in findings when compared to truly experimental study designs, with a tendency of the former to over-estimate effect sizes (Glazerman, Levy, & Myers, 2003; Shadish & Ragsdale, 1996).

2017; Pritchett & Sandefur, 2014). For instance, the design of a program would need to factor in existing levels of saving demand, financial literacy, and structural constraints. By focusing on Sub-Saharan Africa, we aim to generate evidence and policy recommendations that are locally and culturally relevant. In consequence, our aggregated findings from a range of RCTs can be more easily transferred and scaled-up to similar contexts within Sub-Saharan Africa – and are thus more useful for policy-making in general.

3. Previous literature

Mobilization of savings has been viewed as critical, both on a macroeconomic as well as on an individual level. Higher gross national saving rates tend to be correlated with economic growth and scholars have thus pointed to a virtuous cycle of saving and prosperity (Karlan et al., 2014; Kriekhaus, 2002; Loayza et al., 2000). Accordingly, Gurley and Shaw (1955) have highlighted that economic development heavily depends on the sophistication of financial intermediation (particularly between savers and investors) and is in many developing countries hindered by reliance on self-finance.

On an individual level, savings can be essential for securing the livelihoods of poor households, namely by smoothing consumption, providing a buffer stock for coping with adverse events such as health emergencies or death of a family member, and securing participation in culturally relevant obligations such as weddings or funeral ceremonies (Chowa, 2006). Traditional theoretical models such as the *life-cycle hypothesis* (Modigliani, 1966) and the *permanent income hypothesis* (Friedman, 1954) assume that individual saving behavior is determined by balancing current versus anticipated consumption, for instance through accumulating savings while earning (more) and dissaving when retired. However, these models are less applicable to the African context (and to more collectivist societies) since responsibility for care of the elderly is borne inter-generationally rather than by the individual (e.g. Aron, 2007; Roberto & Jarrott, 2008; Smith, 1998).

Saving behavior in Sub-Saharan Africa is thus more likely driven by other externalities, most prominently by imperfections of insurance and credit markets and inadequate financial intermediation (see Liu & Woo, 1994). While a rational response to underdeveloped financial markets would imply increases in investment-motivated and precautionary savings (Zeldes, 1989), poor households, in fact, tend to under-save³ (Karlan et al., 2014). Accordingly, individuals make sub-optimal financial decisions due to a range of different saving barriers. The existing research literature thereby puts focus on identifying and overcoming three particular types of barriers: supply, demand, and behavioral constraints to save. It further examines how increases in savings may help households to rise out of poverty. We discuss these barriers and mechanisms below.

3.1. Saving barriers

When formal saving opportunities are unavailable or unattractive, individuals use second best options such as putting money under a mattress, keeping grain reserves, buying jewellery, construction material, or livestock (Karlan et al., 2014; Kimuyu, 1999; Rutherford & Arora, 2009). Yet, these types of savings are often subject to substantial loss rates, through theft, damage, and

requests for financial assistance from relatives and friends (Wright & Mutesasira, 2001).⁴ At worst, the lack of safe storage may diminish the motivation to save altogether (Wright & Mutesasira, 2001). In response to this, a range of programs have been developed to address such *supply constraints* (see Hulme et al., 2015; Karlan et al., 2014; Lee et al., 2015; Mendoza & Thelen, 2008). These programs provide, for instance, access to formal bank accounts at no or subsidized costs (e.g. Pande et al., 2012; Prina, 2015). Other programs introduce mobile banking schemes that allow monetary transactions via text messages to overcome physical distance to bank branches (e.g. Munyegera & Matsumoto, 2016; Suri & Jack, 2016). Programs may further distribute simple saving devices such as lock boxes or seek to semi-institutionalize saving by mobilizing savings groups (such as Rotating Savings and Credit Associations) with the intention to make saving more secure and more regular for poor households (Berry et al., 2015; Dupas & Robinson, 2013b).

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). For instance, interventions that emphasize financial literacy may counteract lack of trust in financial institutions and help increase knowledge of the procedures required to open a bank account. These programs hypothesize that financial knowledge is an antecedent to healthy financial decision-making and that increases in financial literacy will ultimately increase savings rates (Fernandes et al., 2014; Karlan et al., 2014). Motivational components may include visual representations of saving goals, drafting of detailed saving plans, and text- or mail-delivered saving reminders. These programs are thereby built on the assumption that money earmarked for savings is perceived as less available for some more present-oriented temptation spending. In consequence, both savings and the uptake of savings products are increased (Soman & Cheema, 2011).

Finally, a growing body of literature discusses how saving commitment tools can help to work against *behavioral constraints* (Ashraf, Karlan, & Yin, 2006; Dupas & Robinson, 2013b; Karlan & Linden, 2014; Banerjee & Duflo, 2007). While behavioral anomalies such as myopia or temptation spending are characteristic for individuals across the globe, they are more consequential for the poor who have less resources for absorbing such behavioral biases (Banerjee & Mullainathan, 2010; Bryan, Karlan, & Nelson, 2010). Several interventions have introduced commitment tools, for instance, automated withdrawal and transaction regulations in formal banking or accounts that only authorize transfers for health- or education-specific purposes (e.g. Dupas & Robinson, 2013b; Ashraf, Karlan, & Yin, 2010). Commitment devices can also consist of self-established regulatory frameworks (such as those in savings groups) that make violations of saving intentions costly through feelings of failure, guilt, and damage to social reputation (Benabou & Tirole, 2004; Soman & Cheema, 2011). The primary function of saving strategies that involve group pressure or commitment schemes is to increase individuals' self-control and constrain the liquidity of money so that the purchase of temptation goods and present-biased decision-making are mitigated (Strotz, 1956).

³ We define "under-saving" in line with Karlan et al. (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).

⁴ Wright and Mutesasira (2001) report the odds of savings loss by comparing various saving "technologies" for Uganda. While the risk of loss was similar for savings held at banks (15% of savers lost their savings in the past 12 months due to bank breakdown) and held in cash (13% loss due to theft), it was higher for in-kind savings (25% loss due to theft and 25% loss due to drop in value from price fluctuations). In addition, maintaining cash at home was substantially harder than at a formal institution in consequence of temptation of petty consumption and assistance requests from relatives and friends.

3.2. Savings and poverty alleviation

Assuming that the mechanisms outlined above can in fact help the poor to save (or to save more), it remains yet to examine how increased savings rates may causally translate into poverty reduction (and namely have a trickle-down effect on the distal outcomes that we define in here). Research on the link between savings and poverty alleviation has thereby focused on three causal hypotheses.

3.3. Opportunity investments

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 higher quality of education, nutrition, and health, as well as improved housing quality and asset portfolios (Collins, Morduch, Rutherford, & Ruthven, 2009; Rutherford & Arora, 2009; van Rooyen, Stewart, & de Wet, 2012). Investments in the future not only have the potential to increase the general economic situation of a household but may also reduce the inter-generational transmission of poverty.

It needs to be cautioned, however, that inflation rates in African economies are usually high and interest rates on savings therefore negative. High inflation introduces general economic uncertainty and discourages saving for long-term investment projects.⁵ For instance, Ndikumana (2000) examines predictors for domestic investment in a sample of 30 Sub-Saharan African countries and identifies inflation as one of four key factors that were negatively correlated with investment activities. Likewise, other studies have pointed out, both empirically and theoretically, that high inflation – as well as the resulting economic uncertainty – can be a major impediment to investment (McKinnon, 1973; Shaw, 1973; Tessema, 2011). Saving programs that are focused on financial literacy and demand promotion would thus ideally integrate information on inflation and interest rates.

3.4. Quasi-Insurance

Second, savings can take the form of a *quasi-insurance* in the 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 disproportionately high interest rates, or removal of children from school (Hulme et al., 2015; Pande et al., 2012; Churchill, 2002; Jacoby & Skoufias, 1997). In consequence, saving may increase resilience to economic shocks and reduce vulnerability to poverty (Klasen, Lechtenfeld, & Povel, 2015).

3.5. Mental accounting mechanism

Third, scholars have described how the earmarking of money for savings purposes can not only increase savings rates but also has the potential to counteract a range of “behavioral anomalies”. Scholars have referred to this as a ‘*mental accounting*’ mechanism whereby

the perceived immediate availability of cash is reduced and can therefore induce changes in overall consumption behavior (Rutherford & Arora, 2009; Thaler, 1990). Accordingly, money dedicated to a specific purpose (such as savings) is perceived as less fungible for other expenses as these would evoke feelings of guilt and failure. This psychological enforcement mechanism then helps reduce discrepancy between present and future consumption choices and thus promotes long-term planning (Soman & Cheema, 2011; Strotz, 1956; Thaler, 1990). In consequence, household spending is directed from temptation goods towards more future-oriented expense categories such as health, education, housing, or the accumulation of assets (Prina, 2015; Soman & Cheema, 2011; Banerjee & Mullainathan, 2010; Ambec & Treich, 2007; Bryan et al., 2010; Prahalad & Hammond, 2002). These more deliberate spending patterns and the prioritization of the future over the present can then have important implications for household poverty.

4. Data

The database for this meta-analysis was built up by an intensive search and screening process of the literature on randomized impact evaluations of saving interventions in Sub-Saharan Africa, identification of relevant studies, and extraction of the respective measures. Data was collected according to the guidelines for systematic reviews as outlined by the Cochrane Collaboration and the Campbell Collaboration (Higgins & Green, 2011; Steering Group of the Campbell Collaboration, 2017). A protocol specifying search strategy and methods has been pre-published in the Campbell Collaboration Library (Steinert, Movsisyan, Zenker, Filipiak & Shenderovich, 2016).

4.1. Database search and study identification

In order to objectively identify and process all possibly relevant studies for our analysis, we carried out a comprehensive systematic literature search. Following best practice systematic review guidelines, we made explicit efforts to identify grey literature in addition to publications in peer-reviewed journals. This allows us to exploit one of the main features of meta-analysis, namely a direct assessment of publication bias, which is a potential threat to the external validity of published evidence⁶ (see Higgins, 2011). We searched 28 electronic databases in the fields of economics, psychology, and social sciences.⁷ In addition, reference lists of all included studies and existing reviews of microfinance, financial literacy, and financial inclusion were hand-searched. We also contacted distinguished experts in the field to refer us to further relevant studies.

In a subsequent step, all identified records were assessed for relevance for this systematic review according to the eligibility criteria laid out above, namely (1) featuring a saving promotion component, (2) providing experimental evidence, (3) assessing savings- and poverty-related outcomes, and (4) implemented in Sub-Saharan Africa. Screening of titles and abstracts was conducted by the first author. A subset of 10% of identified titles were double-screened by a second author, yielding high inter-rater reliability (>0.95).

4.2. Data extraction

Data from included studies was independently extracted by two review authors and entered into a pre-piloted data extraction form.

⁵ In development economics theory, the described mechanism is referred to as the McKinnon-Shaw doctrine, suggesting that “financial repression” and negative real interest rates hinder mobilization of savings, the extent of investment, and hence economic growth as such.

⁶ For an empirical investigation of publication bias, see for example: Easterbrook, Gopalan, Berlin, and Matthews (1991) on publication bias in clinical research, Brodeur, Lé, Sangnier, and Zylberberg (2016) in economics research, and Gerber and Malhotra (2008) in political sciences research.

⁷ See Appendix 1 for the list of databases and search string.

We extracted a range of study-level characteristics and key statistics on all outcomes. We aligned our operationalization 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 household expenditures and incomes, food security, investments in and status of health, investments in education and actual educational attainment, and asset ownership and quality of housing. *Intermediate outcomes* included increases in total savings⁸, financial literacy, savings attitudes, and investments and returns from businesses. If information was missing, study authors were contacted with up to four follow-up emails over the course of six months.

4.3. Risk of bias

Since a meta-analysis of unreliable or biased results may lead to erroneous conclusions, it is essential to critically appraise the validity of included studies. This becomes particularly important if the analysis includes unpublished studies that have not been peer-reviewed (yet). We therefore independently rated the quality of included studies using the Cochrane Risk of Bias Assessment Tool for Randomized Controlled Trials⁹ (Higgins et al., 2011). Studies were assessed for the integrity of the trial, specifically randomization procedures and blinding of participants and researchers; for potential imbalance between study arms (control vs. treatment arm) at baseline or between end line completers and “attriters”. We further examined whether reporting of outcomes corresponded to *a priori* trial registration and pre-analysis plans. Lastly, we assessed whether program curricula were implemented as intended (including aspects of attendance rates, facilitator training, and session observations) and whether there was risk of spillover of program content to the control arm. Risk of bias for each individual study was rated independently by two review authors and classified as ‘low’, ‘unclear’ (if sufficient information was lacking), or ‘high’. If there was disagreement between ratings, a third author was consulted for arbitration.

5. Methods

5.1. Calculation of effect sizes

In order to aggregate findings across studies, we calculated standardized effect sizes for all outcomes of interest. Standardized effect sizes are scale-free measures and can thus provide information about the magnitude and direction of a program’s effect that is comparable between different studies. For continuous outcome measures, we calculated **Hedges’ g** effect sizes. Hedges’ g is defined as the standardized mean difference (SMDs) between treatment and control group for any outcome of interest (such as household savings rates) that is then divided by the pooled standard deviation of the respective outcome variable. In addition, Hedges’ g values

are corrected for a potential bias in estimates that could result from low sample size or unequal size of treatment arms in the primary study. For outcomes that were measured on a continuous scale in some studies (e.g. percentage increase in savings amounts) and dichotomized in other studies (e.g. any increases in savings), we transformed odds ratios into SMDs and used Hedges’ g correction for small sample bias as described above (for transformation, see: Borenstein, Hedges, Higgins, & Rothstein, 2009; Sánchez-Meca, Marín-Martínez, & Chacón-Moscoso, 2003). For outcomes predominantly measured on a binary scale (e.g. school enrollment), we reported odds ratios as effect size measure.

We further carefully assessed how clustered study designs (i.e. studies that randomized villages, schools, etc. rather than individuals) were reflected in the estimation of effect sizes. If the unit of treatment allocation and the unit of analysis differ, unit of analysis errors can arise. Most cluster RCTs in our sample have adjusted standard errors accordingly (only two out of 24 meta-analyzed studies did not). If studies did not account for clustering, we applied corrections by multiplying standard errors with the variance inflation factor as suggested by Littell, Corcoran, and Pillai (2008).¹⁰

5.2. 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 multiple measures for one overarching outcome construct (such as poverty). If a study, for instance, seeks to examine effects of saving promotion on household poverty, authors may report both past-month earnings as well as past-month expenses for each sampled household. Considering that effect sizes would be correlated and non-dependent in the above scenario, standard meta-analysis approaches have selected only one coefficient per outcome category and omitted those of conceptually similar outcomes (see Borenstein et al., 2009). However, we adopt a more sophisticated meta-analytical method: We include all reported individual outcome measures for one construct, but then apply robust variance estimation (RVE) techniques to correct standard errors for within-study correlation of multiple effect size estimates per outcome (Fisher & Tipton, 2015; Tanner-Smith & Tipton, 2014; Hedges, Tipton, & Johnson, 2010; Tipton, 2013).

Following Tanner-Smith, Tipton & Polanin (2016), the RVE model for pooling effect sizes is defined as:

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

where y_{ij} captures the outcome of interest (e.g. total savings, household income/expenditures) and more specifically the estimated effect size $i = 1 \dots k_j$ in study $j = 1 \dots m$, and β_0 is the true effect size. Further, u_j is a study-level random effect, $\text{Var}(u_j) = \tau^2$ is the between-study variance component, and e_{ij} represents the residual for the i th effect size in the j th study.

The above y_{ij} yields the pooled Hedges’ g effect size estimate across all studies that have observed the respective outcome of interest. For each pooled effect size, we additionally examine the degree of heterogeneity. Heterogeneity denotes the variability between studies, including diversity in interventions, settings, study designs, or operationalization of outcome measures. We calculate both I^2 as well as τ^2 statistics to assess heterogeneity. I^2 describes the percentage of the variability in Hedges’ g estimates that stems from heterogeneity rather than sampling error (see

⁸ It is crucial to account for potential crowd-out effects that can arise from the shifting of saving 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.

⁹ The Cochrane Risk of Bias Assessment Tool evaluates the quality of randomized controlled trials based on the following six categories: 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. 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 (implementation fidelity, balance at baseline, and potential for contamination/spill-over) were added to the existing tool to improve adequacy for complex international development programs.

¹⁰ 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.

Higgins, 2011). τ^2 is a point estimate of the between-study variance of true effect sizes. 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.

In a next step, we move beyond the mere pooling of effect sizes and examine sources of heterogeneity by testing whether effect size estimates varied significantly by a) intervention type, b) duration of the intervention, c) participant sex, and d) participant age. In a similar vein, sensitivity analyses were run to check whether effect sizes differed significantly by e) time to follow-up, f) risk of bias rating, and g) publication status. For this purpose, we augment the model in Eq. (1) by including each of these characteristics as so called “moderators” (i.e. covariates). This results in a mixed-effects model of the following form:

$$y_{ij} = \beta_0 + \beta_1 x_j + u_j + e_{ij}, \quad (2)$$

where x_j represents the respective effect size moderator (e.g. intervention type, intervention duration, etc.), β_1 is the coefficient of interest that denotes changes in effect sizes across studies that are caused by the respective moderator, u_j is a random effect of unobservable study characteristics and e_{ij} is an error term independent of β_1 and u_j . The above mixed effects model incorporates two types of effects: fixed and random. In the terminology of mixed effects models $\beta_1 x_j$ are often called “fixed effects” as β_1 varies only as a function of known characteristics – in the present case characteristics of program design, recipient characteristics, and aspects of study design. However, as we presume that between-study heterogeneity is not exhaustively explained by the observable characteristics x , we incorporate u_j as unobservable characteristics in the form of a random effects model. Given that we lack sufficient statistical power, we add only one moderator at a time in all specifications.¹¹ To further address the limited statistical power, we apply a small sample correction procedure in each meta-regression. The procedure has been suggested by Tipton (2015) for use in meta-regressions with less than 40 individual studies and potentially skewed covariates. The small sample adjustment corrects both the robust variance estimator itself as well as the degrees of freedom of the t-distribution used for determining significance levels and confidence intervals. By applying both of these corrections, Type I error rates can be substantially reduced as demonstrated in simulation studies (see Tipton, 2015; Fisher & Tipton, 2015).

Following Cochrane Collaboration conventions, meta-regressions were considered as inappropriate for outcome categories composed of less than ten individual studies (see Higgins, 2011).

6. Results

6.1. 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 stages of the search and screening process is provided in Fig. 1. We were unable to collect sufficient information on three studies which therefore had to be excluded from the meta-analysis.¹² The 24 trials included in the meta-analysis enrolled a total of **87,025** study participants.

¹¹ In one specification, the effect size moderator is a variable with three categories (female participants, male participants, both). The model therefore technically includes two dummy variables that are coded as 0 for male participants and 1 for female or both.

¹² For Eissa, Habyarimana, and Jack (2014) and McConnell (2012) 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.

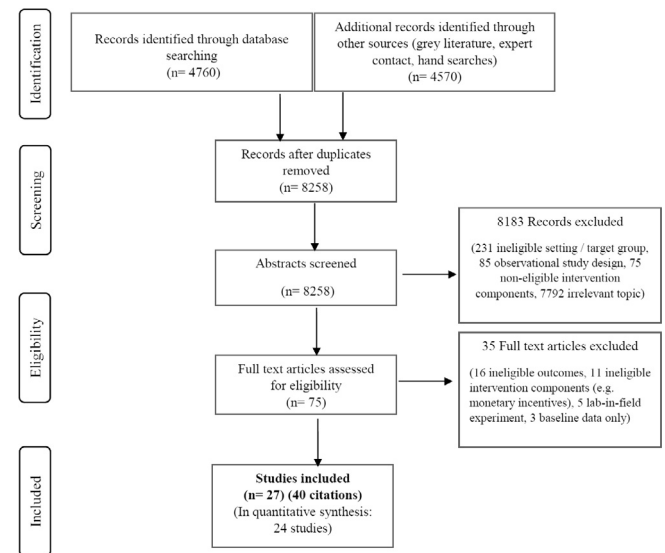


Fig. 1. Flow chart. *Note:* The flow chart depicts the flow of information that was processed throughout the different phases of the systematic review. The chart maps out the number of records identified, the records included and excluded, and the reasons for exclusions (see Moher, Liberati, Tetzlaff, Altman, & The PRISMA Group, 2009).

Characteristics of included studies are summarized in Table 1. Merely six out of the 27 studies were academic publications while the majority of records were grey literature outlets or working papers. This partly speaks to the topicality of saving promotion programs as some of the identified studies are so recent that they could not conceivably have made it through any publication process yet (records range from 2011 (one study) to 2016/17 (8 studies)). The 27 identified studies feature three broad programmatic foci: 1) 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, 2) delivery of financial education curricula around savings (14 studies), and 3) behavioral control schemes for promoting saving self-discipline (10 studies), either through imposing hard commitments (flexibility constraints or economic penalties) or soft psychological commitments (e.g. peer pressure). It is important to note that some of the studies featured combinations of the above components. There was substantial variation in program set-up, ranging from brief awareness raising campaigns (such as in Coville, Di Maro, Zottel, & Dunsch, 2014) to complex multi-component interventions (such as in Dizon, Gong, & Jones, 2016; 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 programs and others lasting for several months. Although the policy discourse on saving promotion is geared towards poverty reduction, only about half of the included studies do in fact look at more distal outcomes such as household expenditures and incomes. In particular, studies on pure financial literacy programs tend to disregard these outcomes. Fig. 2 depicts the geographic scope of included randomized studies on savings programs 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.

6.2. Pooled effect sizes

We report pooled effect sizes for each outcome category separately. Outcome categories are grouped into intermediate and

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 et al. (2013) (grey literature)	Burundi	Poor families with children	<ul style="list-style-type: none"> Savings group (VSLA) 	For savings group: 3 months training and 9 month cycle, in addition weekly discussion sessions (2 h/session)	<ul style="list-style-type: none"> N/A 	<ul style="list-style-type: none"> Expenditures/Consumption Poverty level Household assets 	cRCT	<ul style="list-style-type: none"> Intervention: 805 individuals Control: 743 individuals (across 77 self-help groups) 	12 months
Batista and Vicente (2013) (working paper)	Mozambique	Household heads of rural dwellers	<ul style="list-style-type: none"> Access to formal bank accounts Mobile banking scheme 	N/A	<ul style="list-style-type: none"> Adoption of mobile savings Financial literacy/knowledge Intention/willingness to save Trust in financial services Deposit amounts 	<ul style="list-style-type: none"> N/A 	cRCT	<ul style="list-style-type: none"> Community outreach & agent: 1020 individuals (51 Enumeration Areas) Information leaflet: 204 individuals Control: 1020 individuals 	2 months
Beaman, Karlan, and Thuysbaert (2014) (working paper)	Mali	Female household members	<ul style="list-style-type: none"> Savings group (VSLA) 	Introductory village meeting led by NGO agent, savings group meets on weekly basis for pre-determined cycle (varies in length)	<ul style="list-style-type: none"> Uptake of Savings Savings Consumption Smoothing 	<ul style="list-style-type: none"> Food security Business profits Health/health expenditures Investments in education Housing quality/assets Expenditures 	cRCT	<ul style="list-style-type: none"> Intervention: 209 village, 2508 women Control: 291 villages, 3492 women 	3 years
Berg and Zia (2014) (grey literature)	South Africa	Medium- to low-income households	<ul style="list-style-type: none"> Financial Literacy 	Screening of 26 episodes over a period of two months	<ul style="list-style-type: none"> Financial Knowledge Saved money in the past 6 months 	<ul style="list-style-type: none"> N/A 	iRCT	<ul style="list-style-type: none"> Intervention: 553 Control: 478 	4 months
Berry, Karlan, and Pradhan (2015) (working paper)	Ghana	School children in grades 5 & 7	<ul style="list-style-type: none"> Financial literacy Distribution of lock boxes 	<ul style="list-style-type: none"> Honest Money Box arm: 8 weekly one-hour sessions Aflatoun arm: approx. 24 hrs in total and continued school-based saving clubs 	<ul style="list-style-type: none"> Savings Savings behavior Savings attitudes Financial literacy 	<ul style="list-style-type: none"> N/A 	Multi-arm cRCT	<ul style="list-style-type: none"> Honest Money Box: 45 schools, 1800 students Aflatoun: 45, 1800 Control: 45, 1800 	9 months
Brune et al. (2015) (published)	Malawi	Smallholder cash crop farmers	<ul style="list-style-type: none"> Access to formal bank accounts Saving commitment schemes 	2 months	<ul style="list-style-type: none"> Deposits into savings accounts Savings balances Uptake of bank account Agricultural input 	<ul style="list-style-type: none"> Total expenditure last 30 days Profit from farming 	Multi-arm cRCT	<ul style="list-style-type: none"> Ordinary accounts: 1804 individuals Commitment accounts: 1763 individuals Control: 583 individuals 	1–1.5 years
Buehren (2011) (conference proceeding)	Uganda	Microfinance borrowers	<ul style="list-style-type: none"> Financial literacy Saving mobilisation through microfinance organisation 	6 months, weekly sessions	<ul style="list-style-type: none"> Savings 	<ul style="list-style-type: none"> N/A 	cRCT	<ul style="list-style-type: none"> Treatment arm: 809 individuals (270 Microfinance groups) Control: 628 (135 Microfinance groups) 	6 months

Table 1 (continued)

Study	Country/ Setting	Participants	Intervention Type	Intervention Duration	Intermediate Outcomes	Distal Outcomes	Trial Design	Sample Size	Time to Follow-Up
Carter et al. (2015) (unpublished manuscript)	Mozambique	Farmers	<ul style="list-style-type: none"> Access to formal bank accounts Financial Literacy 	3 sessions (duration of each not specified)	<ul style="list-style-type: none"> Formal Savings 	<ul style="list-style-type: none"> Per capita consumption Total household assets Expenditures on education 	Multi-arm cRCT (but only focus on 1 arm as other arms include incentive scheme)	<ul style="list-style-type: none"> 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	<ul style="list-style-type: none"> Financial Literacy 	1 day (8 h)	<ul style="list-style-type: none"> Financial Literacy Savings Expenditure 	<ul style="list-style-type: none"> N/A 	cRCT	<ul style="list-style-type: none"> Intervention: 589 individuals Control: 661 individuals 	6 months
Coville et al. (2014) (grey literature)	Nigeria	Micro Entrepreneurs	<ul style="list-style-type: none"> Access to formal bank accounts Financial Literacy 	Once-off, 8–11 am film screening	<ul style="list-style-type: none"> Intentions to save Financial literacy 	<ul style="list-style-type: none"> N/A 	Multi-arm iRCT	<ul style="list-style-type: none"> Movie Screening: 327 Bank account: 287 Movie & Bank Account: 307 Control Arm: 309 Intervention: 304 women Control: 323 women 	4 months
Dizon et al. (2016) (unpublished manuscript)	Kenya	Vulnerable women (female sex workers, single/widowed women)	<ul style="list-style-type: none"> Access to formal bank accounts Weekly savings reminders Soft commitment through account labelling 	6 months	<ul style="list-style-type: none"> Takeup of mobile banking Consumption smoothing Savings 	<ul style="list-style-type: none"> N/A 	iRCT	<ul style="list-style-type: none"> Intervention: 304 women Control: 323 women 	8–12 months
Dupas, Karlan, Robinson and Ubfal (2017)	Uganda, Malawi, (Chile)	Household heads of unbanked rural households	<ul style="list-style-type: none"> Access to formal bank accounts 	Once-off	<ul style="list-style-type: none"> Uptake of savings product Savings Business investment 	<ul style="list-style-type: none"> Income Assets Expenditures Food expenditures Education expenditures Health expenditures Housing quality expenditures 	iRCT	<ul style="list-style-type: none"> Intervention: Uganda 1079, Malawi 1053 Control: Uganda 1081, Malawi 1054 	4, 8, and 20 months after treatment
Dupas et al. (2016) (working paper)	Kenya	Household heads around three market centers	<ul style="list-style-type: none"> Access to formal bank accounts 	Once-off home visit for delivery of bank vouchers	<ul style="list-style-type: none"> Uptake of savings product Usage of bank account Savings 	<ul style="list-style-type: none"> Food Security Expenditures 	Multi-arm cRCT	<ul style="list-style-type: none"> Intervention: 198 single-headed/404 dual-headed households Control: 283 households 	2 years
Dupas and Robinson (2013a) (published)	Kenya	Market vendors and taxi drivers	<ul style="list-style-type: none"> Access to formal bank accounts 	Not specified	<ul style="list-style-type: none"> Account usage Savings Business investment 	<ul style="list-style-type: none"> Business profit Expenditures 	iRCT	<ul style="list-style-type: none"> Treatment: 195 (130 female market vendors) Control: 197 (132 female market vendors) 	6 months

Table 1 (continued)

Study	Country/ Setting	Participants	Intervention Type	Intervention Duration	Intermediate Outcomes	Distal Outcomes	Trial Design	Sample Size	Time to Follow-Up
Dupas and Robinson (2013b) (published)	Kenya	Members of a ROSCA	<ul style="list-style-type: none"> • Distribution of saving devices (e.g. lock boxes) • Saving commitment schemes • Earmarking/peer pressure 	1 ROSCA meeting (and then ROSCA cycle)	<ul style="list-style-type: none"> • Take-up of savings technology 	<ul style="list-style-type: none"> • Resilience to health emergencies • Investment in preventative health products 	Multi-arm cRCT	<ul style="list-style-type: none"> • Safe Box: 354 individuals • Lock Box: 458 • Health Pot: 311 • Health Savings Accounts: 470 • Control: 320 	6 and 12 months, 3 years with a random subsample
Eissa et al. (2014) (not included in quantitative synthesis) (grey literature)	Kenya	High School students in last 2 years of school	<ul style="list-style-type: none"> • Financial Literacy 	<ul style="list-style-type: none"> • Weekly treatment materials for a period of 6 weeks 	<ul style="list-style-type: none"> • Savings • Financial literacy 	<ul style="list-style-type: none"> • N/A 	Multi-arm cRCT	<ul style="list-style-type: none"> • Comic & CD with financial education materials: 60 schools, 1140 students • Full financial literacy program: 54/1140 • Placebo (comics without financial education): 52/1140 • Control: 51/1140 	6 months
Flory (2016) (working paper)	Malawi	Households in central Malawi	<ul style="list-style-type: none"> • 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	<ul style="list-style-type: none"> • Awareness of financial services • Uptake of saving devices • Savings (only analyzed for subgroup of account opener) • Investment in agricultural business (land and fertilizer) 	<ul style="list-style-type: none"> • Crop Income • Food consumption (only analyzed for subgroup of account openers) 	cRCT	<ul style="list-style-type: none"> • Intervention: 56 clusters, 1003 households • Control: 56 clusters, 1003 households 	2 years
Jamison, Karlan, and Zinman (2014) (working paper)	Uganda	Members of Youth Clubs	<ul style="list-style-type: none"> • Access to formal bank accounts • Financial Literacy 	15-hour course delivered over 10 weeks	<ul style="list-style-type: none"> • Financial literacy (financial knowledge, awareness, and numeracy) • Savings 	<ul style="list-style-type: none"> • Income • School attendance • Expenditures • Nutrition 	Multi-arm cRCT	<ul style="list-style-type: none"> • Financial literacy: 60 clubs, 702 individuals • Bank Account: 60/702 • Both: 60/702 • Control: 60/702 	9–12 months
Karlan et al. (2012) (working paper)	Ghana, Malawi, Uganda	Low-income households	<ul style="list-style-type: none"> • Savings Group (VSLA) 	Cycle usually between 8–12 months	<ul style="list-style-type: none"> • Uptake of VSLA membership • Saving (total and weekly contributions) • Investment in agriculture 	<ul style="list-style-type: none"> • Business Profits • Household poverty (assets and consumption) • Food security • Education • Health 	cRCT	<ul style="list-style-type: none"> • Intervention: Ghana: 88 villages/ 2640 individuals, Malawi: 95/2265, Uganda: 98/2270 • Control: Ghana 87/ 2231, Malawi 95/ 2265, Uganda 98/ 2270 	Ghana: 2 years, Malawi & Uganda: 3 years

Table 1 (continued)

Study	Country/ Setting	Participants	Intervention Type	Intervention Duration	Intermediate Outcomes	Distal Outcomes	Trial Design	Sample Size	Time to Follow-Up
Karlan and Linden (2014) (working paper)	Uganda	Students grades 4–7	<ul style="list-style-type: none"> • Saving commitment schemes • Saving Account 	5 school terms, regular visits by intervention team	<ul style="list-style-type: none"> • Saving (both administrative data and self-reported) • Savings attitude • Uptake of savings product 	<ul style="list-style-type: none"> • Expenditure on education (school fees) • School attendance 	Multi-arm cRCT	<ul style="list-style-type: none"> • 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	<ul style="list-style-type: none"> • Savings Group (VSLA) 	Varies by cycle, typically 12 months	<ul style="list-style-type: none"> • Uptake of VSLA membership • Total Savings • Agricultural input 	<ul style="list-style-type: none"> • Food Security • Expenditures • Income/Pov-erty level • Housing Quality • Agricultural output 	cRCT	<ul style="list-style-type: none"> • Intervention arm: 23 villages, 568 households • Control arm: 23, 569 	2 years
Lee et al. (2015) (working paper)	Ghana	Low-income youth	<ul style="list-style-type: none"> • Marketing outreach for formal bank accounts 	3–7 visits to schools by bank staff over the course of a year	<ul style="list-style-type: none"> • Account opening • Savings 	<ul style="list-style-type: none"> • N/A 	Multi-arm cRCT	<ul style="list-style-type: none"> • 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) (unpublished manuscript)	Ghana	Market vendors	<ul style="list-style-type: none"> • Marketing of Savings (information and levels of convenience to open an account) , SMS reminders 	NA	<ul style="list-style-type: none"> • Account opening • Account usage • Intention of account usage 		iRCT	Unclear	1 month & 3 months (not clear)
Sayinzoga, Bulte, and Lensink (2016) (published)	Rwanda	Representatives of village banks	<ul style="list-style-type: none"> • Financial Literacy 	5 days (8 am to 5 pm)	<ul style="list-style-type: none"> • Savings • Financial literacy 	<ul style="list-style-type: none"> • N/A 	cRCT	<ul style="list-style-type: none"> • Intervention: 174 individuals • Control: 167 individuals 	15 months
Schaner (2015) (published)	Kenya	Low-income married couples	<ul style="list-style-type: none"> • Access to formal bank accounts 	One day for opening the account, interest rate running for six months	<ul style="list-style-type: none"> • Account usage • Savings 	<ul style="list-style-type: none"> • Income • Assets 	Multi-arm iRCT	<ul style="list-style-type: none"> • Intervention: 3372 • Control: 1302 	6 months, 3 years
Shephard, Kaneza and Moclair (under review)	Rwanda	Teachers & students	<ul style="list-style-type: none"> • Financial literacy 	Full Aflatoun curriculum	<ul style="list-style-type: none"> • General financial capability • Saving attitudes 	<ul style="list-style-type: none"> • N/A 	cRCT	<ul style="list-style-type: none"> • Intervention: 875 students, 125 teachers • Control: 875/125 	midline 3–4 months, endline 7 months
Supanantaroek, Lensink, and Hansen (2016) (grey literature)	Uganda	School children	<ul style="list-style-type: none"> • Financial literacy 	<ul style="list-style-type: none"> • 3 months, • 40 h in school 	<ul style="list-style-type: none"> • Savings • Saving attitudes 	<ul style="list-style-type: none"> • N/A 	cRCT	<ul style="list-style-type: none"> • Intervention: 22 schools, 936 students • Control: 22/810 	3 months

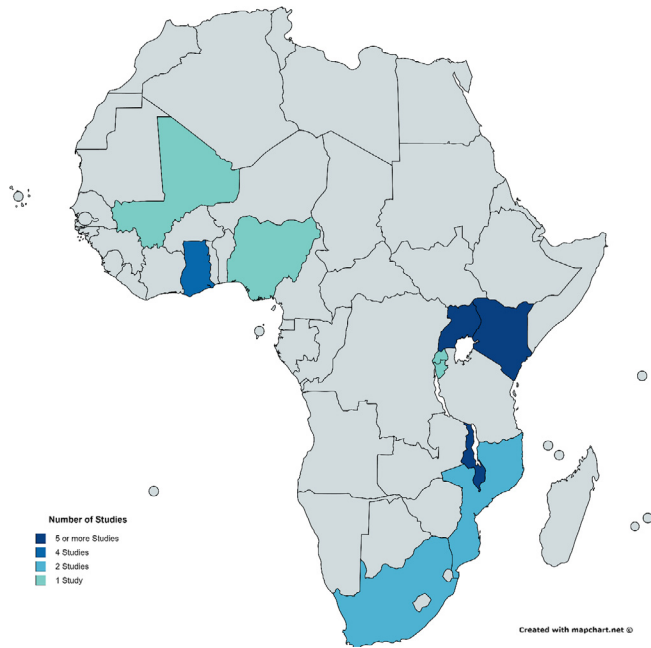


Fig. 2. Geographic distribution of RCTs on saving promotion interventions. *Note:* The figure shows the geographic Distribution of the 27 randomized controlled trials on saving promotion interventions in Sub-Saharan Africa that were identified in this systematic review.

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. The pooled estimates should be interpreted with caution if heterogeneity between studies is high. Corresponding forest plots, presented in [Appendix 2](#), visualize individual effect sizes as well as grand pooled estimates (as indicated by the “diamond” in each graph) for studies in each outcome category.

6.2.1. Intermediate outcomes

[Table 2](#) reports pooled effect sizes for intermediate outcomes. Column (1) shows a positive and significant effect on *total savings* ($g_{pooled} = 0.077$, $p < .001$, 95% CI [0.03, 0.12]). Thus, the interventions considered in our analysis led to an overall increase in total savings that is significantly different from zero. Further, Column (2) shows that the pooled effect size for *pro-savings attitudes* is positive and borderline significant ($g_{pooled} = 0.061$, $p < .1$, 95% CI [−0.02, 0.09]). This points to a trend towards improvements in financial attitudes across four studies reporting on this outcome. Similarly, findings show a trend towards increases in *financial literacy* levels, but heterogeneity levels are high ($g_{pooled} = 0.12$, $p < .1$, 95% CI [−0.01, 0.24]) (see Column (3)). Column (4) depicts that *business investments* are positively related to savings interventions

($g_{pooled} = 0.045$, $p < .1$, 95% CI [−0.00, 0.09]). Although the effect size for investment is small, we see significant downstream impacts on *business returns and profits* ($g_{pooled} = 0.044$, $p < .01$, 95% CI [0.02, 0.07]) as reported in Column (5).

While all pooled effect sizes are positive and (borderline) significant, heterogeneity between individual effect sizes is not negligible for some outcomes: I^2 statistics for savings and business investments range from high to moderate with 69.6% and 43.9%, respectively. Further, an I^2 statistic of 86% likely points to substantial variations in effects on financial literacy across included studies, possibly because some interventions particularly target financial literacy while others do not. Moreover, validated scales and procedures for quantifying levels of financial literacy are lacking. The wide range of effect sizes might therefore also reflect a differing quality of the various applied measures.

6.2.2. Poverty-related distal outcomes

Program impact on distal, poverty-related outcomes is summarized in [Table 3](#). We find significant increases in households' *expenditures and incomes* ($g_{pooled} = 0.066$, $p < .01$, 95% CI [0.02, 0.12], Column (1)). With regards to the wider aspects of household poverty and wellbeing, our results further point to significant increases in *food security* ($g_{pooled} = 0.052$, $p < .05$, 95% CI [0.01, 0.10]) as reported in Column (2). In contrast, we do not find significant impacts across interventions on *asset ownership and housing quality* ($g_{pooled} = 0.038$, $p > .1$, 95% CI [−0.01, 0.09], see Column (3)). It is conceivable that these two 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 [Suri & Jack, 2016](#)). In a similar vein, we do not find indication of program effectiveness with regards to health and education. Columns (4)–(5) show that *educational investment* ($g_{pooled} = 0.009$, $p > .1$, 95% CI [−0.03, 0.05]) and school enrolment (log odds = 0.059, $p > .1$, 95% CI [−0.18, 0.3]) do not reach significance across included studies. Likewise, savings interventions fail to materialize any downstream impacts on *general health status or health investments* ($g_{pooled} = 0.010$, $p > .1$, 95% CI [−0.01, 0.03]) as reported in Column (6). However, pooled estimates rely on few studies and results should therefore be interpreted with caution.

Heterogeneity in the remaining outcome categories ranged from high to moderate (with I^2 values from 38.5% to 65.9%). A likely explanation may be that the studies in our sample are, despite the effort a relatively narrow sample, quite diverse. It is possible that factors such as intervention design, socio-economic characteristics of participants, or social and cultural context explain the variation in effect size estimates across studies. Particularly, variation is most pronounced in expenditure or income and asset/housing outcome categories. This may be indicative of some noise in measuring these outcome concepts in poor populations that could stem from seasonal fluctuations, potential recall bias, or reporting bias due to fear from taxation (see [Sahn & Stifel, 2000](#)).

Table 2
Pooled effect sizes for intermediate outcomes.

	Total Savings (1)	Saving Attitudes (2)	Financial Literacy (3)	Business Investment (4)	Business Profits (5)
Hedges' G (SE)	0.077 ^{***} (0.02)	0.061 [†] (0.02)	0.12 [†] (0.05)	0.045 [†] (0.02)	0.044 [†] (0.01)
95% CI	[0.03, 0.12]	[−0.02, 0.09]	[−0.01, 0.24]	[−0.00, 0.09]	[0.02, 0.07]
I^2	69.6%	24.4%	85.9%	43.9%	20.8%
τ^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	14

Notes: [†] $p < .1$, ^{*} $p < .05$, ^{**} $p < .01$, ^{***} $p < .001$. Note that some studies are multi-arm trials and therefore contribute effect sizes to two or more intervention type categories.

Table 3

Pooled effect sizes for distal outcomes.

	Expenditures/Income (1)	Food Security (2)	Assets/Housing (3)	Education Investment (4)	School Enrollment (binary) (5)	Health/Health Investment (6)
Hedges' G (SE)	0.066** (0.02)	0.052* (0.02)	0.038 (0.02)	0.009 (0.01)	0.059 (0.05)	0.010 (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	22	17	11	17

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$, **** $p < .001$. Note that some studies are multi-arm trials and therefore contribute effect sizes to two or more intervention type categories. Pooled effect sizes for Enrollment (binary variable) are log odds.

Table 4

Meta-regression: intervention components.

	Savings			Consumption			Investment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Informal Supply-enhancing Component	−0.07* (0.03)			−0.02 (0.02)			−0.01 (0.02)		
Demand-enhancing Component		0.02 (0.04)			0.04 (0.03)			0.00 (0.03)	
Behavioral Constraint			−0.03 (0.03)			−0.02 (0.02)			0.00 (0.02)
Const. (Formal supply)	0.09** (0.02)			0.08*** (0.02)			0.04* (0.01)		
Const. (Any Supply)		0.05* (0.02)	0.08** (0.02)		0.06*** (0.01)	0.08** (0.02)		0.04* (0.01)	0.04* (0.01)
Sample		Any Supply			Any Supply			Any 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, 68.6	63.0, 65.0	63.0, 64.2	63.0, 65.2	34.0, 35.5	34.0, 37.6	34.0, 37.7

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$, **** $p < .001$. Standard errors from robust variance estimation in parentheses. Intervention channels defined as follows: (i) *formal supply* as access to institutionalized banking, (ii) *informal supply-enhancing component* as savings groups or lockboxes etc., (iii) *demand-enhancing component* as literacy or motivational scripts/outreach, (iv) *behavioral constraint* as priming on mental accounting, and formal commitment devices or peer pressure and regulatory frameworks in groups.

6.3. Meta-regression: heterogeneity in effect sizes

A meta-regression can give an indication of how participant, program, and design characteristics may affect outcomes. It is also an attempt to explain high levels of heterogeneity in effect sizes. Given the limited number of individual studies per outcome, we pooled effect sizes into broader categories to meet the requirement of 10 individual studies per regression (Higgins, 2011). In line with our theoretical framework, we generate three broad categories of outcomes, namely (i) savings, (ii) consumption proxies, and (iii) future-oriented investments. Our savings category hereby includes 'cash' and 'kind', ranging from account deposits, cash savings, and total savings, to livestock and household asset holdings. Second, consumption comprises food and household expenditures, any income, profits from agricultural activity and small-scale business, as well as actual food insecurity, and frequency of meals. Lastly, for the investment category, we combine human capital investment (i.e. expenditures in health and education,) as well as investments in income-generating activities, business, agricultural inputs or fertilizer. We also include returns on investments by adding broader measures of actual health and educational attainment. Results from meta-regressions are summarized in Tables 4–5.

6.3.1. 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 behavioral constraints (see Appendix 3 for coding of components). Most programs under

investigation feature some sort of supply component (either formal or informal), for some combined with either a demand or behavioral component. Further, seven studies feature treatment arms that comprise single demand-enhancing components. We do not observe any stand-alone behavioral intervention in our sample (behavioral components are usually tied to supply programs).

Based on their prevalence, supply-based programs were used as the base category in all regressions. Intercepts therefore determine the magnitude and significance level of pooled effect sizes for these programs. We find that supply-based programs show consistently positive and significant effect sizes for all three outcome categories (see constants in Columns (1)–(9), ranging from $\beta = 0.04$, $p < .05$ to $\beta = 0.09$, $p < .01$). Among these, programs with formal supply components (i.e. increasing access to bank or mobile money accounts) appear more effective in increasing savings as compared to programs with informal supply components (i.e. initiation of group-based saving schemes or supply of money boxes): Actual savings amounts are significantly lower for programs with an informal supply provision ($\beta = -0.07$, $p < .05$), as reported in Column (1). The difference in savings outcomes between formal and informal supply components deserves further attention. First, it could reflect possible loss rates in savings that may be higher in the context of informal compared to formal savings infrastructure (due to lower levels of security) (Wright & Mutesasira, 2001). Further, some measurement differences are noteworthy: Studies on informal program components (such as promotion of savings groups) tend to rely on self-reported savings measures. In contrast, studies on formal program components usually collect data from administrative records of the bank. Measurement error is therefore likely higher for self-reported savings. In this vein, Dupas and colleagues (2017) as well

Table 5

Meta-regression: study design, participant characteristics and bias.

	Savings						Consumption					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Duration	–0.04 (0.02)						–0.05** (0.01)					
Female		–0.08* (0.03)						–0.06† (0.02)				
Mixed		–0.07† (0.03)						–0.01 (0.03)				
Youth			–0.05 (0.04)						–0.03* (0.01)			
Follow Up Time				–0.01 (0.01)						–0.02* (0.01)		
Risk of Bias					0.00 (0.01)						0.01 (0.01)	
Publication Status						0.07 (0.05)						–0.01 (0.02)
Const.	0.12** (0.03)			0.08† (0.04)	0.07* (0.03)	0.05* (0.02)	0.10** (0.02)			0.14** (0.03)	0.15* (0.04)	0.07** (0.01)
Const. (Male)		0.12* (0.03)						0.09* (0.02)				
Const. (Adults)			0.08*** (0.02)						0.08*** (0.01)			
Sample N (Studies)			Full 20						Full 12			
N (Effect Sizes)			66						70			
I ² (original, resid.)	66.3, 63.9	66.3, 63.8	66.3, 66.9	66.3, 67.6	66.3, 67.2	66.3, 64.1	62.5, 63.0	62.5, 61.0	62.5, 65.2	62.5, 59.1	62.5, 62.5	62.5, 65.4
	Investment											
	(13)	(14)	(15)	(16)	(17)	(18)						
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)							
Publication Status						0.06 (0.04)						
Const.	0.05 (0.04)			0.08* (0.03)	0.04 (0.03)	0.02† (0.01)						
Const. (Male)		0.09* (0.02)										
Const. (Adults)			0.04* (0.01)									
Sample N (Studies)			Full 13									
N (Effect Sizes)			62									
I ² (original, resid.)	33.7, 37.3	33.7, 13.9	33.7, 36.6	33.7, 30.7	33.7, 38.6	33.7, 14.2						

Notes: †p < .1, *p < .05, **p < .01, ***p < .001. 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 programs. Savings groups programs 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 program 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.

as Jamison and colleagues (2014) compare self-reported and administrative data on savings balances and find that the former yields overstated estimates of savings balances. Assuming that this finding extrapolates to other studies, our regression coefficient might indeed underestimate differences in effectiveness between formal and informal saving promotion due to social desirability

bias in self-reported measures. However, when we examine the investment and consumption categories, coefficients are close to zero and turn non-significant, as evident from Columns (4) and (7), thus suggesting that formal and informal programs do not differ in their effectiveness. One possible explanation for this might be that many informal savings groups additionally feature rotating

group loans. These could then both encourage investment and temporarily boost consumption.

For completeness, we also use demand promotion instead of supply promotion as base category (not included in table) to see whether demand components are equally effective or not. In fact, constants remain similar but turn non-significant for savings ($\beta = 0.07$, $p = .28$), and drop close to zero and insignificant for consumption ($\beta = 0.00$, $p = .83$) and investment ($\beta = 0.01$, $p = .67$). Thus, demand-based programs, namely financial literacy and financial education, are not associated with significant changes in any of the three pooled outcomes, and particularly appear ineffective in changing the more distal outcomes. This corroborates with previous meta-analyses of financial literacy programs that have found none or little evidence of program effectiveness (Fernandes et al., 2014), and particularly low effect sizes for interventions implemented in developing countries (see Kaiser, 2016). Further, we find no 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 programs (see Table 4, Columns (2), (5), (8)).¹³ We may however caution that the demand-based programs included in here were quite heterogeneous and ranged from subtle endorsement of certain financial behaviors via TV soaps or movie screenings to full-fledged financial education curricula. It is therefore possible that some of these programs are more promising, but we did not have sufficient data to examine this.

Lastly, we find no support for the hypothesis that “tying one’s hands” through *external or psychological behavioral constraints* (such as purpose-labeled accounts, peer pressure, and institutionalized withdrawal restrictions) can increase effectiveness of supply-based programs. Coefficients are virtually zero across all three outcome categories (see Columns (3), (6), and (9)).

Finally, we compare once-off/one-day programs with longer programs featuring several weekly meetings or complex curricula (see Table 5). We find no evidence that longer program duration yields higher effect sizes (see Columns (1), (7), (13)). In contrast, program length is *negatively* associated with the effectiveness of consumption outcomes ($\beta = -0.05$, $p < .01$, Column (7)). This finding feeds back into the above discussion of programmatic components: While longer programs tend to target demand aspects (e.g. financial education curricula), we may still expect to see higher effects from a once-off program with a strong supply component. It would be interesting to examine the impact of program intensity and duration solely for the sub-group of literacy programs, but unfortunately, we did not have a sufficiently large number of studies to proceed with such a post-hoc analysis.

6.3.2. Participant characteristics

In the next set of meta-regressions (see Table 5, Columns (2), (8), (14)), we seek to elucidate whether program effectiveness varies with participant characteristics. Our analyses reveal relatively larger program effects for male participants across all three outcome categories as well as some substantial decreases in heterogeneity statistics (for investments I^2 values decrease from 33.7% to 13.9%).¹⁴ We can only speculate about the underlying mechanisms of this difference in effectiveness by gender. One interpretation in line with previous research could be that women are more risk-averse when it comes to portfolio investments such as the purchase of business assets (see Coleman, 2000; Brush, 1992). More

importantly, our findings relate to a body of literature that sees women at disadvantage with regards to intra-household financial decision-making and distribution of resources (Ghosh & Vinod, 2017; Ashraf, 2009; De Mel, McKenzie, & Woodruff, 2009; Schaner, 2015). That is, female program recipients may face resistance from male household heads when seeking to implement certain savings practices (as endorsed by the intervention) in their households. Yet, 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 (Brune et al., 2015; Carter et al., 2015; Ksoll, Lilleør, Lønborg, & Rasmussen, 2016). 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), (9), (15)). This might partly be explained by the fact that some program types, such as those which target access to bank accounts, do often constrain eligibility for younger populations. In addition, children and adolescents have likely less monetary resources than adults – which limits their ability to save. 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.

6.3.3. 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 program impact ($\beta = -0.02$, $p < .05$, see Table 5, Column (10)). For instance, we run post-hoc sub-group analyses (not included in tables) that reveal that the pooled effect size for household incomes and expenditures lies at $g_{pooled} = 0.12$ (95% CI [0.05, 0.19]) after 6 months of program delivery and is diminished to effectively zero after more than two years ($g_{pooled} = 0.02$, 95% CI [-0.01, 0.04]). Coefficients for savings (see Table 5, Column (4)) and for investments (see Table 5, Column (16)) are negative as well but insignificant.

6.3.4. Risk of bias

The quality of included studies ranges from moderate to high as detailed in Fig. 3 (as well as Appendix 4). Three points are noteworthy. First, blinding of participants is notoriously difficult in non-medical trials and could thus not be 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 program implementation and fidelity may partly explain variations in outcomes (see Dane & Schneider, 1998; Durlak & DuPre, 2008). Third, none of the 27 included studies used any corrections for multiple hypotheses testing (such as family wise error rate or false discovery rate adjustments) (see Anderson, 2008; Fink, McConnell, & Vollmer, 2014; 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), (11), (17)). Risk of bias did not explain heterogeneity in treatment effects for any of the three out-

¹³ Note that confidence intervals are quite large and it is therefore conceivable that low statistical power makes it impossible to detect some small but true differences between pooled effect sizes.

¹⁴ Note that, in contrast, Dupas and Robinson (2013a) find higher program effectiveness for (largely) female market vendors than for male bicycle taxi drivers. However, differences in outcome could be defined by occupation rather than gender.

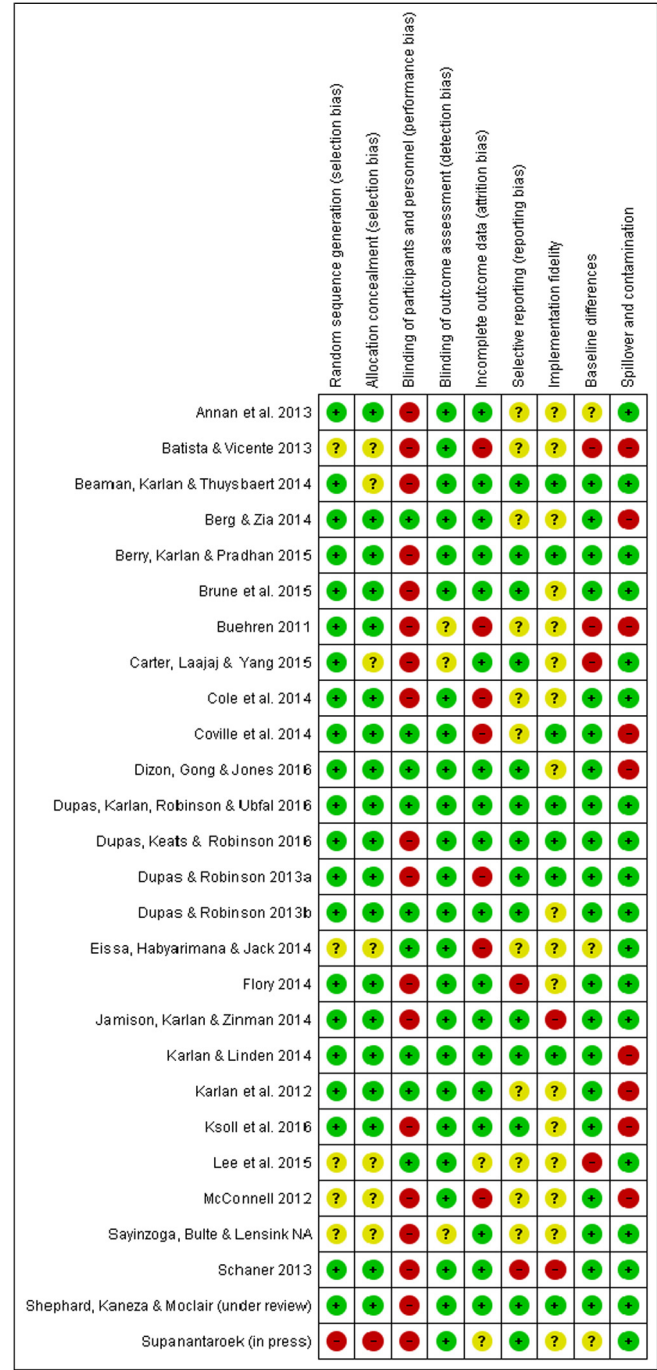


Fig. 3. Risk of bias of included studies. *Notes:* The Cochrane Collaboration defines the risk of bias categories as follows: (1) Random sequence generation: blind/external randomization to prevent selection bias, (2) Allocation concealment: those enrolling participants into study ignorant of upcoming assignment, (3) Participant blinding to prevent performance bias, (4) Outcome assessor blinding to prevent detection bias, (5) Incomplete data if systematic attrition and no sensitivity analyses, (6) Selective reporting if analysis neglects a priori specified outcomes. Three auxiliary categories were added to assess (7) Possibility of null effects due to delivery/implementation failure, (8) Imbalance at baseline or absence of baseline checks, and (9) Unintended spillover to control.

comes, thus suggesting that study quality was not a significant factor in explaining heterogeneity in effect sizes.

6.3.5. Publication bias

Finally, potential publication bias was assessed for the studies included in this meta-analysis. Visual inspection of the funnel plots

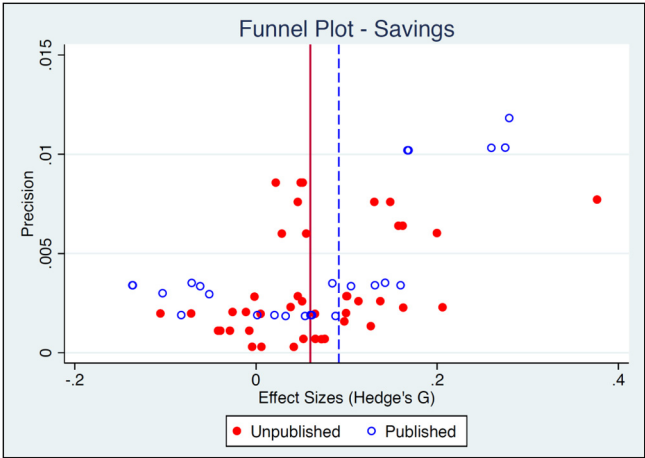


Fig. 4. Funnel plot: savings. *Note:* Standardized Hedges' g effect sizes displayed on x axis and level of precision as indicated by the variance of Hedges' g on y axis.

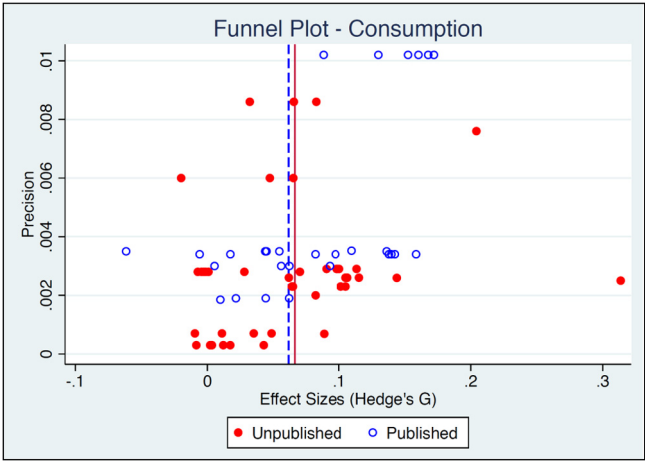


Fig. 5. Funnel plot: consumption.

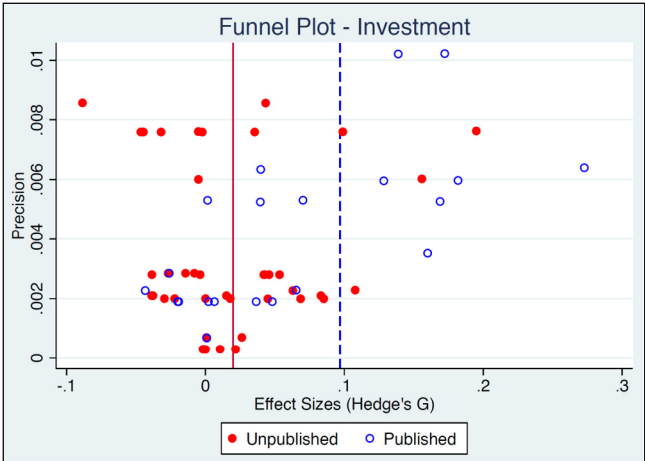


Fig. 6. Funnel plot: investments.

displayed in Figs. 4–6 points to, on average, higher effect sizes for published studies both for aggregated savings (Fig. 4) and aggregated investments (Fig. 6). Effect sizes for consumption-based outcomes do not vary by publication status (Fig. 5). While differences in effect sizes between published and unpublished studies failed to

reach statistical significance in our meta-regressions (see Table 5, Columns (6), (12), (18)), we note that the estimated coefficients for savings and investment are relatively large and may only be insignificant due to a lack of statistical power. We therefore cannot fully rule out the existence of publication bias. The eligibility framework applied in this meta-analysis therefore helps avoid overestimating the impact of saving promotion programs in Sub-Saharan Africa.

7. Discussion

This study sets out to examine the poverty-alleviating potential of saving promotion programs in Sub-Saharan Africa. Financial inclusion and saving promotion are increasingly researched in the region, as expressed by 27 studies included in this review and more studies still underway. Our findings suggest that savings interventions do indeed show significant impacts such as on increases in total savings amounts and promotion of small-scale family businesses. Yet, it is important to move beyond these intermediate outcomes in order to assess whether saving promotion programs can fulfil the promises they make with regards to poverty alleviation. This is not only important from a policy perspective, but can also help us rule out any potential harmful effects. For instance, it is conceivable that higher savings are in fact realized through cuts in food consumption or other human capital expenses rather than through more deliberate budgeting and reduced temptation spending. If this was the case, we would expect to see negative effects on poverty-related outcomes. However, we find no indication of harm for any aspect of household economic welfare. In contrast, we observe positive and significant impact on some poverty-related outcomes, including increases in household expenditures, incomes, and improved food security. This is particularly remarkable when considering that saving promotion does not necessitate infusion of large external capital.

The findings from our meta-regression point to two important policy implications. First, we found that programs with supply-enhancing components appear most promising across all observed outcomes. Linking this to our theoretical framework, our findings thus suggest that undersaving in Sub-Saharan Africa may primarily stem from barriers in supply and absence of institutionalized structures to facilitate saving. The results therefore provide strong empirical grounds to iron out the misconception that poor people are “too poor” (or worse: too unsophisticated) to save. The aggregated evidence from 24 meta-analyzed trials points to a gap between the demand for savings and the actual provision of reliable, safe, and easily accessible institutionalized saving devices. Indeed, the large majority of low-income households in Sub-Saharan Africa remains alienated from the formal financial sector, and in the least developed countries of the continent formal banking reaches still less than 20% of the general population. In view of this gulf, mobile banking technology is currently developing as a promising savings tool. Accordingly, Suri and Jack (2016) estimate that “M-PESA”, the Kenyan mobile money scheme, has helped to lift 2% of the Kenyan population out of poverty. Mobile banking is particularly attractive for geographically remote areas as it can reduce dependence on the brick-and-mortar presence of bank branches (Munyegera & Matsumoto, 2016). While physical bank branches are scarce, mobile phone coverage is high across Sub-Saharan Africa and mobile money may thus have the potential to considerably boost supply in the coming years. In addition, savings groups that feature access to group loans might effectively increase investment rates in contexts where individual credit constraints are high.

Second, our findings suggest that saving promotion programs do not seem to generate the intended economic impacts for female recipients. Impact might partly fail to materialize due to their

constrained intra-household bargaining power vis-à-vis male spouses or partners. To date, women tend to hold low hierarchical status in many Sub-Saharan African societies (Chowa, 2006; Kim et al., 2007) and may therefore face resistance when seeking involvement in household financial decision making and distribution of resources. Some studies have even highlighted the potential of harm, arguing that economic empowerment of women may provoke conflict with male partners and thus exacerbate the risk of partner-perpetrated violence (Jewkes, 2002; Kim et al., 2007). It may therefore be desirable for future saving promotion programs to mobilize not only women but also male household heads, thus seeking to ensure intra-household consensus on the endorsed changes in financial management and budgeting. Indeed, a recent multi-arm randomized controlled trial from Indonesia found that impacts of a financial literacy program on a range of financial behaviors were significantly increased when the entire family was targeted rather than only the male migrant worker or the remaining family members (Doi, McKenzie, & Zia, 2014). It may also be worthwhile to combine economic strengthening curricula with additional components focused on gender awareness training or sexual risk taking (such as in Annan, Bundervoet, Seban, & Costigan, 2013; Kim et al., 2007) in order to further strengthen the role of vulnerable women.

Our analysis also points to avenues for future research. First, while our meta-analysis highlights causal impacts on both financial behavior and aspects of household poverty, it can not provide empirical insights on the causal mechanisms at play. Future research will need to open this ‘black box’ and examine how different components of a program can translate into poverty-relevant outcomes. For instance, future studies might benefit from drawing on mixed-methods designs to elicit the role of context or on qualitative in-depth interviews with program recipients to explore some narratives of change. Related to this, it is also essential to scrutinize why hypothesized trickle-down effects on education and health have failed to materialize.

Second, only a small number of assessed programs have featured institutionalized commitment devices such as account withdrawal restrictions or prescribed earmarking of specific expenses (e.g. on health or scholastic materials) (see Dupas & Robinson, 2013b; Karlan & Linden, 2014; Brune et al., 2015). Evidence from the Philippines (Ashraf et al., 2006) suggests that administrative behavioral constraints, if combined with access to banking, may help consolidate effects on savings (and potentially on other outcomes). More research is needed in order to establish whether the same holds true for Sub-Saharan Africa. Third, highest impact in our sample was achieved for programs targeted at male entrepreneurs or farmers. More evidence is therefore needed on the question of whether savings programs can reach those most in need and maintain their effectiveness even for the ultra-poor (Halder & Mosely, 2004). Lastly, the average time to follow-up across the 27 included trials was 16 months. Future trials should therefore aim for longer follow-up periods in order to generate additional empirical insights on whether program effects are sustainable over time.

On a cautionary note, while our findings are encouraging, 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 offered savings tools is low^{15,16} or that program impact, overall, is too small to substantially lift individuals out of poverty. More importantly, our

¹⁵ 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 et al., 2017).

¹⁶ Given that included studies applied intent-to-treat analyses it is likely that they offer realistic estimates of take up in the “real world”.

results suggest that program 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 program roll-out, not even by those who were registered as high-frequent users in the first couple of months. This finding requires further analytical investigation but, from a policy perspective, it may call for continued support to those who take up savings and for follow-up outreach one to two years after a program's launch. Future research will also need to include cost effectiveness analyses that weigh overall program costs against material as well as psychosocial benefits for target populations.

This being said, 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 enrollment (Saavedra & Garcia, 2012). Likewise, McEwan (2015) examines a range of school-based interventions in developing countries and finds that monetary grants and school-based deworming programs have effect sizes close to zero. The review reveals the largest effect size for technology and computer training which still does not exceed a standardized mean difference of 0.15. Lastly, a meta-analysis on technical and vocational training for youth in low- and middle-income countries finds a mean effect size of 0.13 on income which is, again, quite similar to what we find (Tripney & Hombrados, 2013).

Findings from this analysis may caution against predictions of a savings-driven '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 demonstrated that the poor in Sub-Saharan Africa are indeed able and willing to save their money, but face constraints that can be overcome. The 2030 Sustainable Development Agenda pledges to "strengthen the capacity of domestic financial institutions to encourage and expand access to banking, insurance and financial services for all". Our research supports this policy imperative through empirical evidence on how an expansion of the (semi-)formal financial sector to the world's poor and a better adaptation of services to their specific financial needs can help achieve development for all.

Conflict of interest statement

JIS, YS, and LDC are involved in an ongoing trial on financial literacy and household budgeting training. However, all eligible trials were evaluated according to predefined and objective criteria, consistent with the review protocol. No funding was received specific to this study. There is no other potential competing interests.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at <https://doi.org/10.1016/j.worlddev.2017.11.018>.

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