

Steinert, Janina I. et al.

Working Paper

Do saving promotion interventions help alleviate poverty in Sub-Saharan Africa? A systematic review and meta-analysis

Discussion Papers, No. 226

Provided in Cooperation with:

Courant Research Centre 'Poverty, Equity and Growth in Developing and Transition Countries',
University of Göttingen

Suggested Citation: Steinert, Janina I. et al. (2017) : Do saving promotion interventions help alleviate poverty in Sub-Saharan Africa? A systematic review and meta-analysis, Discussion Papers, No. 226, Georg-August-Universität Göttingen, Courant Research Centre - Poverty, Equity and Growth (CRC-PEG), Göttingen

This Version is available at:

<https://hdl.handle.net/10419/157309>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

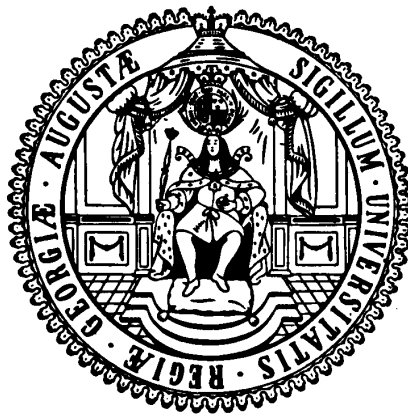
You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Courant Research Centre

‘Poverty, Equity and Growth in Developing and Transition Countries: Statistical Methods and Empirical Analysis’

Georg-August-Universität Göttingen
(founded in 1737)



Discussion Papers

No. 226

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

**Janina I. Steinert, Juliane Zenker, Ute Filipiak,
Ani Movsisyan, Lucie D. Cluver, Yulia Shenderovich**

April 2017

Platz der Göttinger Sieben 5 · 37073 Goettingen · Germany
Phone: +49-(0)551-3921660 · Fax: +49-(0)551-3914059

Email: crc-peg@uni-goettingen.de Web: <http://www.uni-goettingen.de/crc-peg>

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

Janina I Steinert, Juliane Zenker, Ute Filipiak,
Ani Movsisyan, Lucie D Cluver, Yulia Shenderovich

April 12, 2017

Abstract

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

1 Introduction

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

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

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

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

intervention types, and participant characteristics may influence outcomes beyond the explanatory power of a single study.

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

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

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

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

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

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

2 Theoretical Framework

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

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

2.1 Saving Barriers

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

Other interventions focus on educational or motivational elements to attenuate *demand constraints* that hinder individuals to build savings (see Dupas, Keats, Robinson, 2016; Karlan et al., 2014; Brune et al. 2011). For instance, interventions that emphasise financial literacy may counteract lack of trust in financial institutions and help increase knowledge of the procedures required to open a bank account. These hypothesise that financial knowledge is an antecedent to healthy financial decision-making and that increases in financial literacy will ultimately increase savings (Fernandes, Lynch & Netemeyer, 2014; Karlan et al., 2014; Karlan et al, 2010). Motivational components include visual representations of saving goals, drafting of detailed saving plans, and text- or mail-delivered savings reminders (Fiorill et al., 2014; Soman & Cheema, 2011) in order to increase the uptake of savings products as well as to increase savings.

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

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

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

2.2 Intermediate and Distal Outcomes: Savings and Poverty Alleviation

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

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

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

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

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

3 Data

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

3.1 Database Search

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

⁴ A protocol specifying search strategy and methods has been pre-published in the [Campbell Collaboration Library](#).

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

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

3.2 Data Extraction Process

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

3.3 Risk of Bias

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

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

⁷ For Eissa, Habyarimana & Jack (2014) and McConnell, Mullainathan & Zinman (2010) we could not retrieve information on the sample size for control and intervention group and for Cole et al. (2014) information on standard deviations/standard errors (as well as p-values for a possible t-test) were lacking.

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

4 Methods

4.1 Calculation of Effect Sizes

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

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

4.2 Meta-Analysis and Meta-Regression

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

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

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

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

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

where y_{ij} is 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.

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

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

where x_{1ij}, \dots, x_{pij} represent characteristics on study or effect size level. In the terminology of mixed effects models $\beta_1 x_{1ij}, \dots, \beta_p x_{pij}$ are often called “fixed effects” as β_1, \dots, β_p vary only as a function of known characteristics.

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

$$V^R(b) = (\sum_{j=1}^m X_j' W_j X_j)^{-1} (\sum_{j=1}^m X_j' W_j A_j e_j e_j' A_j' W_j X_j) (\sum_{j=1}^m X_j' W_j X_j)^{-1} \quad (3)$$

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

The null hypothesis for the effect of β_k ($H_0: \beta_k=0$) is then tested using the robust variance estimator V^R as:

$$t_k = \frac{\beta_k}{\sqrt{V_k^R}} \quad (4)$$

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

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

5 Results

5.1 Identified Studies

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

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

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

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

5.2 Pooled Effect Sizes

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

Intermediate Outcomes

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

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

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

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

Distal Outcomes

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

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

5.3 Meta-Regression: Heterogeneity in Effect Sizes

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

Intervention Design and Components

In our first set of regressions we investigate whether variations in outcomes can be explained by differences in programmatic characteristics (see Table 4). For this purpose, we recorded whether interventions featured supply-enhancing components, demand-enhancing components, or any form of behavioural constraints (see Appendix 3 for coding of components). Most programmes under investigation feature some kind of supply component, which is often combined with either a demand or behavioural component. Further, a few

studies in our sample feature stand-alone demand-enhancing interventions. We do not observe any stand-alone behavioural intervention in our sample (behavioural components are usually tied to a supply enhancing component).

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

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

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

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

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

Participant Characteristics

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

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

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

Study Design

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

5.4 Risk of Bias

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

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

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

6 Discussion

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

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

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

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

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

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

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

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

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

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

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

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

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

References

Included Studies¹⁶

Batista, C., Vicente, P.C. (2013). Introducing Mobile Money in Rural Mozambique: Evidence from a Field Experiment. Working Paper No 1301.

Beaman, L., Karlan, D., & Thuysbaert, B. (2014). Saving for a (not so) Rainy Day: A Randomized Evaluation of Savings Groups in Mali. Working Paper No. 1043, Economic Growth Center, Yale University.

Bureau of Applied Research in Anthropology (2013). Final impact evaluation of the Saving for Change program in Mali, 2009–2012. University of Arizona, and Innovations for Poverty Action. Evaluation Commissioned by Oxfam America and Freedom from Hunger.

Berg, G. & Zia, B. (2014). Harnessing emotional connections to improve financial decisions Using savings lotteries to promote financial inclusion in South Africa. In Lundberg, M. & Mulaj, F. (eds.), Enhancing financial capability and behavior in low- and middle-income countries. Washington, D.C.: International Bank for Reconstruction and Development / The World Bank.

Berry, J., Karlan, D., & Pradhan, M. (2015). The Impact of Financial Education for Youth in Ghana. Working Paper No. 21068, National Bureau of Economic Research.

Brune, L., Giné, X., Goldberg, J., & Yang, D. (2015). Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi. *Economic Development and Cultural Change*, 64 (2), 187-220

Brune, L., Giné, X., Goldberg, J., & Yang, D. (2015). Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi. Working Paper No. 20946, National Bureau of Economic Research.

Brune, L., Giné, X., Goldberg, J., & Yang, D. (2011). Commitments to save: a field experiment in rural Malawi/ Policy Research Working Paper Series No. 5748, The World Bank.

Buehren, N. (2011). Allocating Cash Savings and the Role of Information: Evidence from a Field Experiment in Uganda, Proceedings of the German Development Economics Conference, Berlin 2011, No. 16.

Annan, J., Bundervoet, T., Seban, J., & Costigan, J. (2013). A Randomized Impact Evaluation of Village Savings and Loans Associations and Family-Based Interventions in Burundi. USAID Final Report.

Bundervoet, T. (2012). Small Wonders? A Randomized Controlled Trial of Village Savings and Loans Associations in Burundi. Unpublished Manuscript.

Carter, M.R., Laajaj, R. & Yang, D. (2015). Savings and Subsidies, Separately and Together: Decomposing Effects of a Bundled Anti-Poverty Program. Unpublished Manuscript.

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

Cole, S., Zia, B., Abel, M., Crowley, L., Pauliac, C.S., & Postal, V. (2014). Evaluation of Old Mutual's On the Money program. Financial literacy in South Africa. In Lundberg, M. & Mulaj, F. (eds.), *Enhancing financial capability and behavior in low- and middle-income countries*. Washington, D.C.: International Bank for Reconstruction and Development / The World Bank.

Cole, S., Zia, B., Abel, M., Crowley, L., Pauliac, C.S., & Postal, V. (2013). Old Mutual Financial Education Impact Assessment (FEIA): Using a randomised controlled trial to determine the impact of financial education in Burial Societies and Microfinance groups. JPAL Final Report.

Coville, A., Di Maro, V., Zottel, S., & Dunsch, F. A. (2014). Nigeria's Nollywood nudge. An entertaining approach to saving. In Lundberg, M. & Mulaj, F. (eds.), *Enhancing financial capability and behavior in low- and middle-income countries*. Washington, D.C.: International Bank for Reconstruction and Development / The World Bank.

Dizon, F., Gong, E., & Jones, K. (2016). Does Financial Inclusion Exclude? The Effect of Access to Savings on Informal Risk-Sharing in Kenya. Job Market Paper.

Dizon, F., Gong, E., & Jones, K. (2015). Mental Accounting and Mobile Banking: Can labeling an M-PESA account increase savings. Unpublished Manuscript

Dupas, P., Karlan, D., Robinson, J., & Ubfal, D. (2016). Banking the Unbanked? Evidence from three countries. Unpublished Manuscript.

Dupas, P., Keats, A., & Robinson, J. (2016). The Effect of Savings Accounts on Interpersonal Financial Relationships: Evidence from a Field Experiment in Rural Kenya. NBER Working Paper No. 21339.

Dupas, P., Keats, A., & Robinson, J. (2013). Challenges in Banking the Rural Poor: Evidence from Kenya's Western Province. 3ie Grantee Final Report. Delhi: International Initiative for Impact Evaluation (3ie).

Dupas, P., & Robinson, J. (2013a). Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya. *American Economic Journal-Applied Economics*, 5(1), 163–192. <http://doi.org/10.1257/app.5.1.163>

Dupas, P., & Robinson, J. (2013b). Why Don't the Poor Save More? Evidence from Health Savings Experiments. *American Economic Review*, 103(4), 1138–1171. <http://dx.doi.org/10.1257/aer.103.4.1138>

Dupas, P., & Robinson, J. (2012). Why Don't the Poor Save More? Evidence from Health Savings Experiments. Working Paper No. 17255, National Bureau of Economic Research.

Eissa, N., Habyarimana, J., & Jack, W. (2014). Comic FX in Kenya Can cartoons improve the effectiveness of financial education? In Lundberg, M. & Mulaj, F. (eds.), *Enhancing financial capability and behavior in low- and middle-income countries*. Washington, D.C.: International Bank for Reconstruction and Development / The World Bank.

Flory, J.A. (2016). Banking the Poor: Evidence from a Savings Field Experiment in Malawi. Working Paper. Robert Day School of Economics and Finance, Claremont McKenna College.

Flory, J.A. (2014). Banking the Poor: Evidence from a Savings Field Experiment in Malawi. Selected Paper prepared for presentation at the Agricultural & Applied Economics Association's 2014 AAEA Annual Meeting, Minneapolis, Minnesota, July 27-29, 2014.

Flory, J.A. (2016). Formal Finance and Informal Safety Nets of the Poor: Evidence from a Savings Field Experiment. Unpublished Manuscript.

Jamison, J. C., Karlan, D. S., & Zinman, J. (2014). Financial Education and Access to Savings Accounts: Complements or Substitutes? Evidence from Ugandan Youth Clubs. Working Paper No. 20135, National Bureau of Economic Research.

Karlan, D., & **Linden**, L. L. (2014). Loose Knots: Strong versus Weak Commitments to Save for Education in Uganda. Working Paper No. 19863, National Bureau of Economic Research.

Karlan, D., Thuysbaert, B., Udry, C., Cupito, E., Naimpally, R., Salgado, E., & Savonitto, B. (2012). Impact Assessment of Savings Groups. 3ie Final Report.

Ksoll, A., Lilleør, H.B., Lønborg, J.H., & Rasmussen, O.D. (2016). Impact of Village Savings and Loan Associations: Evidence from a cluster randomized trial. *Journal of Development Economics*, 120(2016), 70–85. doi:10.1016/j.jdeveco.2015.12.003

Lee, Y.S., Johnson, L., Sherraden, M., Ansong, D., Chowa, G. & Osei-Akoto, I. (2015). Taking the Bank to the Youth: Impacts on Saving and Asset Building from the Ghana Youth Save Experiment. CSD Working Paper No. 15-43.

McConnell (2012). Between Intention and Action: An Experiment on Individual Savings. Unpublished Manuscript.

Sayinzoga, A., Bulte, E., & Lensink, R. (NA). Financial Literacy and Financial Behavior: Experimental Evidence from Rural Rwanda. Unpublished Manuscript.

Schaner, S. (2016). The Persistent Power of Behavioral Change: Long-Run Impacts of Temporary Savings Subsidies for the Poor. Working Paper. Dartmouth College: Hanover, NH.

Schaner S. (2015). The cost of convenience? Transaction costs, bargaining power, and savings account use in Kenya. Unpublished Manuscript.

Schaner S. (2015). Do Opposites Detract? Intrahousehold Preference Heterogeneity and Inefficient Strategic Savings. *American Economic Journal: Applied Economics* 25, 7(2), 135–174. <http://dx.doi.org/10.1257/app.20130271>

Shephard, D, Kaneza, Y., & Moclair, P. (*under review*) What Curriculum? Which Methods? A Cluster Randomized Controlled Trial of Social and Financial Education in Rwanda. Unpublished Manuscript.

Supanantaroeck, S., Lensink, R., & Hansen, N. (*in press*). The impact of social and financial education on savings attitudes and behavior among primary school children in Uganda. *Evaluation Review*.

Supanantaroek, S. (NA). Financial Literacy And Saving Attitudes Of Children: Empirical Evidence For Uganda. Aflatoun/PEDN Uganda Randomized Controlled Trial. Unpublished Manuscript.

Other

Ambec, S., & Treich, N. (2007). ROSCAs as financial agreements to cope with self-control problems. *Journal of Development Economics*, 82(1), 120–137.
doi:10.1016/j.jdeveco.2005.09.005

Anderson, M. L. (2008). Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association*, 103(484), 1481–1495.
<https://doi.org/10.1198/016214508000000841>

Ashraf, N., Gons, N., Karlan, D. S., & Yin, W. (2003). A review of commitment savings products in developing countries. New Haven: Innovation for Poverty Action.

Ashraf, N. (2009). Spousal control and intra-household decision making: An experimental study in the Philippines. *American Economic Review*, 99(4), 1245-1277.
doi: 10.1257/aer.99.4.1245

Ashraf, N., Karlan, D. S., & Yin, W. (2010). Female Empowerment: Impact of a Commitment Savings Product in the Philippines. *World Development*, 38(3), 333-344.
doi:10.1016/j.worlddev.2009.05.010

Ashraf, N., Karlan, D., & Yin, W. (2006). Tying Odysseus to the mast: Evidence from a commitment savings product in the Philippines. *The Quarterly Journal of Economics*, 635–672.

Banerjee, A. V., & Duflo, E. (2011). *Poor economics: A radical rethinking of the way to fight global poverty*. New York: Public Affairs.

Banerjee, A. V., & Duflo, E. (2007). The economic lives of the poor. *The Journal of Economic Perspectives: A Journal of the American Economic Association*, 21(1), 141-167.
doi: 10.1257/jep.21.1.141

Banerjee, A., Banerjee, R., Duflo, E., Glennerster, R., Khemani, S., Mullainathan, S. & Shotland, M. (2005). *The Impact of Information, Awareness and Participation on Learning Outcomes*. Mimeo, MIT.

Barnes, C., Gaile, G., & Kimbombo, R. (2001). Impact of three microfinance programs in Uganda. Retrieved from <http://erepository.uonbi.ac.ke/handle/11295/65429>. Accessed February 2, 2016.

Beaman, L., Karlan, D., & Thuysbaert, B. (2014). Saving for a (not so) Rainy Day: A Randomized Evaluation of Savings Groups in Mali (Working Paper No. 20600). National Bureau of Economic Research. doi: 10.3386/w20600

Benabou, R. & Tirole, J. (2004). Willpower and Personal Rule. *Journal of Political Economy*, 112(4), 848–86. doi: 10.1086/421167

- Berg, G. (2010). Evaluating the Impacts of Microsaving: The Case of Sewa Bank in India. *Journal of Economic Development*, 35(1). <http://ssrn.com/abstract=1346560>
- Borenstein, M., Hedges, L. V., Higgins, J. P. T., & Rothstein, H. R. (2009). *Introduction to Meta-Analysis*. Wiley and Sons.
- Brody, C., De Hoop, T., Vojtkova, M., Warnock, R., Dunbar, M., Murthy, P., & Dworkin, S. (2015). Economic Self-Help Group Programs for Improving Women's Empowerment: A Systematic Review. *Campbell Systematic Reviews* 2015:19.
- Brush, C.G. (1992). Research on Women Business Owners: Past Trends, a New Perspective and Future Directions. *Entrepreneurship Theory and Practice*, 5-30.
- Bryan, G., Karlan, D., & Nelson, S. (2010). Commitment devices. *Annual Review Economics*, 2(1), 671–698. doi: 10.1146/annurev.economics.102308.124324
- Churchill, C. (2002). "Trying to Understand the Demand for Microinsurance." *Journal of International Development* 14: 381-387.
- Cohen, J. (1988). *Statistical power analysis for the behavioral sciences* (2nd ed.). Hillsdale, NJ: Erlbaum.
- Collins, D. & Morduch, J. (2008). Banking low-income populations: Perspectives from South Africa. In Blank, R.M., & Barr, M.S. (Eds.), *Insufficient Funds: Savings, Assets, Credit and Banking Among Low-Income Households*. New York: Russell Sage.
- Collins, D., Morduch, J., Rutherford, S., & Ruthven, O. (2009). *Portfolios of the Poor: How the World's Poor Live on \$2 a Day*. Princeton, N.J.: Princeton University Press.
- Curley, J., Ssewamala, F., & Han, C.-K. (2010). Assets and educational outcomes: Child Development Accounts (CDAs) for orphaned children in Uganda. *Children and Youth Services Review*, 32(11), 1585–1590. <https://doi.org/10.1016/j.childyouth.2009.07.016>
- Dane, A.V., & Schneider, B.H. (1998). Program integrity in primary and early secondary prevention: Are implementation effects out of control? *Clinical Psychology Review*, 18, 23-45. <http://www.ncbi.nlm.nih.gov/pubmed/9455622>
- de Mel, S., Herath, D., McIntosh, C., & Woodruff, C. (2012). Linking Savings Accounts to Mobile Phones: Are Potential Users Interested? IGC Working Paper. Retrieved from <http://r4d.dfid.gov.uk/Output/200568/Default.aspx> (last accessed 23/08/2016).
- Demirguc-Kunt, A. & Klapper, L. (2012). Measuring financial inclusion: The global index database. Retrieved from <https://openknowledge.worldbank.org/handle/10986/6042>. Accessed March 16, 2016.
- D'Espallier, B., Guérin, I., & Mersland, R. (2011). Women and Repayment in Microfinance: A Global Analysis. *World Development*, 39(5), 758–772. <https://doi.org/10.1016/j.worlddev.2010.10.008>

- Duflo, E., Banerjee, A., Glennerster, R., & Kinnan, C. G. (2013). The Miracle of Microfinance? Evidence from a Randomized Evaluation (Working Paper No. 18950). National Bureau of Economic Research. doi: 10.1257/app.20130533
- Durlak, J.A., & DuPre, E.P. (2008). Implementation matters: A review of research on the influence of implementation on program outcomes and the factors affecting implementation. *American Journal of Community Psychology*, 41, 327-350. doi: 10.1007/s10464-008-9165-0.
- Duvendack, M., Palmer-Jones, R., Copestake, J.G., Hooper, L., Loke, Y., & Rao, N. (2011). What is the evidence of the impact of microfinance on the well-being of poor people? London: EPPI-Centre, Social Science Research Unit, Institute of Education, University of London.
- Fernandes, D., Lynch, J. G., & Netemeyer, R. G. (2014). Financial Literacy, Financial Education, and Downstream Financial Behaviours. *Management Science*, 60(8), 1861–1883. <http://doi.org/10.1287/mnsc.2013.1849>
- Fiorill, A., Potok, L., & Wright, J. (2014). Applying Behavioral Economics to Improve Microsavings Outcomes. Ideas 24. Retrieved from <http://www.ideas42.org/grameenmicrosavings/> (last accessed 18/11/2015).
- Fink, G., McConnell, M., & Vollmer, S. (2014). Testing for heterogeneous treatment effects in experimental data: false discovery risks and correction procedures. *Journal of Development Effectiveness*, 6(1), 44–57. <https://doi.org/10.1080/19439342.2013.875054>
- Fisher, Z., & Tipton, E. (2015). Robust variance meta-regression (Version 1.6) [Software]. Retrieved from <http://cran.r-project.org/web/packages/robmeta/robmeta.pdf> (last accessed 05/12/2016).
- Giné, X., Goldberg, J., Silverman, D., & Yang, D. (2012). Revising Commitments: Field Evidence on the Adjustment of Prior Choices. National Bureau of Economic Research. <http://www.nber.org/papers/w18065>
- Glazerman, S., Levy, D. M., & Myers, D. (2003). Nonexperimental Versus Experimental Estimates of Earnings Impacts. *The ANNALS of the American Academy of Political and Social Science*, 589(1), 63–93. <http://doi.org/10.1177/0002716203254879>
- Hedges, L. V., Tipton, E., & Johnson, M. C. (2010). Robust variance estimation in meta-regression with dependent effect size estimates. *Research Synthesis Methods*, 1(1), 39–65. doi: 10.1002/jrsm.5
- Higgins, J. P. T., Altman, D. G., Gøtzsche, P. C., Jüni, P., Moher, D., Oxman, A. D., ... Sterne, J. A. C. (2011). The Cochrane Collaboration's tool for assessing risk of bias in randomised trials. *BMJ*, 343, d5928. <https://doi.org/10.1136/bmj.d5928>
- Higgins, J.P.T. & Green, S. (Eds.) (2011). *Cochrane Handbook for Systematic Reviews of Interventions Version 5.1.0* [updated March 2011]. The Cochrane Collaboration, 2011. Available from www.cochrane-handbook.org.
- Hulme, D., Moore, K.; & Barrientos, A. (2015). Assessing the insurance role of microsavings. In Vos, R., Islam, N., & Koparanova, M. (Eds.), *Financing for Overcoming Economic Insecurity*. Bloomsbury Publishing.

- Jacoby, H. G., & Skoufias, E. (1997). Risk, Financial Markets, and Human Capital in a Developing Country. *The Review of Economic Studies*, 64(3), 311–335. <http://www.jstor.org/stable/2971716>
- Kaiser, T. & Menkhoff, L. (2016). Does Financial Education Impact Financial Behavior, and if So, When? DIW Discussion Papers 1562.
- Karlan, D., McConnell, M., Mullainathan, S., & Zinman, J. (2010). Getting to the top of mind: How reminders increase saving (Working Paper No. 16205). National Bureau of Economic Research. <http://www.nber.org/papers/w16205>
- Karlan, D., Ratan, A. L., & Zinman, J. (2014). Savings by and for the Poor: A Research Review and agenda. *Review of Income and Wealth*, 60(1), 36–78. doi: 10.1111/roiw.1210
- Klasen, S., Lechtenfeld, T., & Povel, F. (2015). A Feminization of Vulnerability? Female Headship, Poverty, and Vulnerability in Thailand and Vietnam. *World Development*, 71, 36–53.
- Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental analysis of neighborhood effects. *Econometrica* 75 (1), 83–119.
- Littell, J. H., Corcoran, J., & Pillai, V. (2008). *Systematic Reviews and Meta-Analysis*. Oxford University Press.
- McEwan, P. J. (2015). Improving Learning in Primary Schools of Developing Countries. A Meta-Analysis of Randomized Experiments. *Review of Educational Research*, 85(3), 353–394. <http://doi.org/10.3102/0034654314553127>
- Mendoza, R. U., & Thelen, N. (2008). Innovations to Make Markets More Inclusive for the Poor. *Development Policy Review*, 26(4), 427–458.
- Moher, D., Liberati, A., Tetzlaff, J., Altman, D.G., The PRISMA Group (2009). Preferred Reporting Items for Systematic Reviews and Meta-Analyses: The PRISMA Statement. *BMJ*, 339:b2535.
- O’Prey, L. & Shephard, D. (2014). Financial Education for Children and Youth: A Systematic Review and Meta-analysis. Aflatoun Working Paper 2014.1C.
- Pande, R., Cole, S., Sivasankaran, A., Bastian, G., Durlacher, K. (2012). Does poor people’s access to formal banking services raise their incomes? London: EPPI-Centre, Social Science Research Unit, Institute of Education, University of London.
- Prahalad, C. K., & Hammond, A. (2002). Serving the world’s poor profitably. *Harvard Business Review*, 89(9), 48–58. <https://hbr.org/2002/09/serving-the-worlds-poor-profitably>. Accesed 8 August, 2016.
- Prina, S. (2015). Banking the poor via savings accounts: Evidence from a field experiment. *Journal of Development Economics*, 115, 16–31. <http://doi.org/10.1016/j.jdeveco.2015.01.004>

Rutherford, S., & Arora, S. S. (Eds.). (2009). *The Poor and their Money: Microfinance from a Twenty-first Century Consumer's Perspective*. Bourton on Dunsmore, Rugby, Warwickshire England: Practical Action Publishing.

Rutherford, S. (2000). *The Poor and their Money*. New Dehli, Oxford University Press.

Saavedra, J. E. and Garcia, S. (2012) Impacts of Conditional Cash Transfer Programs on Educational Outcomes in Developing Countries. A Meta-analysis. RAND Labor and Population working paper.

Soman, D., & Cheema, A. (2011). Earmarking and Partitioning: Increasing Saving by Low Income Households. *Journal of Marketing Research*, 48, 14–22.

<http://doi.org/10.1509/jmkr.48.SPL.S14>

Sánchez-Meca, J., Marín-Martínez, F., & Chacón-Moscoso, S. (2003). Effect-size indices for dichotomized outcomes in meta-analysis. *Psychological Methods*, 8(4), 448–467.

<http://doi.org/10.1037/1082-989X.8.4.448>

Sen, A. (1993). Capability and Well-Being. In Nussbaum, M., & Sen, A. (Eds.), *The Quality of Life* (pp.30–53). New York: Oxford Clarendon Press

Shadish, W. R., & Ragsdale, K. (1996). Random versus nonrandom assignment in controlled experiments: do you get the same answer? *Journal of Consulting and Clinical Psychology*, 64(6), 1290–1305.

Scherr, F.C., Sugrue, T.E., & Ward, J.B. (1993). Financing the Small Firm Start-Up: Determinants of Debt Use. *The Journal of Small Business Finance*, 3(1), 17–36.

Ssewamala, F. M., Han, C.-K., Neilands, T. B., Ismayilova, L., & Sperber, E. (2010a). Effect of economic assets on sexual risk-taking intentions among orphaned adolescents in Uganda. *American Journal of Public Health*, 100(3), 483–488.

Ssewamala, F. M., Ismayilova, L., McKay, M., Sperber, E., Bannan, W., & Alicea, S. (2010b). Gender and the effects of an economic empowerment program on attitudes toward sexual risk-taking among AIDS-orphaned adolescent youth in Uganda. *The Journal of Adolescent Health: Official Publication of the Society for Adolescent Medicine*, 46(4), 372–378.

Ssewamala, F. M., & Ismayilova, L. (2009). Integrating Children's Savings Accounts in the Care and Support of Orphaned Adolescents in Rural Uganda. *The Social Service Review*, 83(3), 453–472.

Ssewamala, F. M., Karimli, L., Torsten, N., Wang, J. S.-H., Han, C.-K., Ilic, V., & Nabunya, P. (2016). Applying a Family-Level Economic Strengthening Intervention to Improve Education and Health-Related Outcomes of School-Going AIDS-Orphaned Children: Lessons from a Randomized Experiment in Southern Uganda. *Prevention Science*, 17(1), 134–143.

Stewart, R., van Rooyen, C., Dickson, K., Majoro, M., & de Wet, T. (2010). What is the impact of Microfinance on poor people?: a systematic review of evidence from sub-Saharan Africa. Technical Report. London: EPPI-Centre, Social Science Research Unit, University of London.

Stewart, R., van Rooyen, C., Korth, M., Chereni, A., Reelo Da Silva, N., & de Wet, T. (2012). Do micro-credit, micro-savings and micro-leasing serve as effective financial inclusion interventions enabling poor people, and especially women, to engage in meaningful economic opportunities in low- and middle-income countries? A systematic review of the evidence. London: EPPI-Centre, Social Science Research Unit, Institute of Education, University of London.

Strotz, R.H. (1956). Myopia and Inconsistency in Dynamic Utility Maximization. *Review of Economic Studies*, 23(3), 165-180.

Suri, T., & Jack, W. (2016). The long-run poverty and gender impacts of mobile money. *Science*, 354(6317), 1288–1292. <https://doi.org/10.1126/science.aah5309>

Tanner-Smith, E. E., & Tipton, E. (2014). Robust variance estimation with dependent effect sizes: practical considerations including a software tutorial in Stata and spss. *Research Synthesis Methods*, 5(1), 13–30. <https://doi.org/10.1002/jrsm.1091>

Tanner-Smith, E. E., Tipton, E., & Polanin, J. R. (2016). Handling Complex Meta-analytic Data Structures Using Robust Variance Estimates: a Tutorial in R. *Journal of Developmental and Life-Course Criminology*, 2(1), 85–112.

Thaler, R. H. (1990). Anomalies: Saving, Fungibility, and Mental Accounts. *The Journal of Economic Perspectives*, 4(1), 193–205. doi: 10.1257/jep.4.1.193

Tipton, E. (2015). Small simple adjustments for robust variance estimation with meta-regression. *Psychological Methods*, 20(3), 375–393. doi: 10.1037/met0000011

Tipton, E. (2013). Robust variance estimation in meta-regression with binary dependent effects. *Research Synthesis Methods*, 4(2), 169–187. doi: 10.1002/jrsm.1070

Tripney, J. S., & Hombrados, J. G. (2013). Technical and vocational education and training (TVET) for young people in low- and middle-income countries: a systematic review and meta-analysis. *Empirical Research in Vocational Education and Training*, 5(1), 3. <http://doi.org/10.1186/1877-6345-5-3>

World Bank (2016a). Poverty and Equity. Retrieved from <http://povertydata.worldbank.org/poverty/region> (last accessed 06/10/2016).

World Bank (2016b). *World Development Indicators 2016*. Washington, DC: World Bank.

Wright, G., & Mutesasira, L. (2001). The Relative Risks to Poor People's Savings. *Journal of Small Enterprise Development*, 12(3).

TABLES

Table 1. Summary of Included Studies

Study	Country/ Setting	Participants	Intervention Type	Intervention Duration	Intermediate Outcomes	Distal Outcomes	Trial Design	Sample Size	Time to Follow-Up
Annan, Bundervoet, Seban & Costigan 2013 (grey literature)	Burundi	Poor families with children	<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 & 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 EA) Information leaflet: 204 individuals Control: 1020 individuals 	2 months
Beaman, Karlan & 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 & 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 & 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"> <i>Honest Money Box</i> arm: 8 weekly one-hour sessions <i>Aflatoun</i> arm: approx. 24 hrs in total and continued school-based saving clubs 	<ul style="list-style-type: none"> Savings Savings behaviour 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, Giné, Goldberg & Yang 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 	6 months
Carter, Laajaj & Yang 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 <i>et al.</i> 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 hours)	<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 	4 months
Dizon, Gong & Jones 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 	N/A	iRCT	<ul style="list-style-type: none"> • Intervention: 304 women • Control: 323 women 	8-12 months
Dupas et al. 2016 (unpublished manuscript)	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, Keats & Robinson 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 & 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
Dupas & Robinson 2013b	Kenya	Members of a ROSCA	<ul style="list-style-type: none"> • Distribution of saving 	1 ROSCA meeting (and then	<ul style="list-style-type: none"> • Take-up of savings technology 	<ul style="list-style-type: none"> • Resilience to health emergencies 	Multi-arm cRCT	<ul style="list-style-type: none"> • Safe Box: 20 ROSCAS, 354 	6 and 12 months, 3

(published)			<ul style="list-style-type: none"> devices (e.g. lock boxes) Saving commitment schemes Earmarking/peer pressure 	ROSCA cycle)		<ul style="list-style-type: none"> Investment in preventative health products 		<ul style="list-style-type: none"> individuals Lock Box: 26,458 Health Pot: 23,311 Health Savings Accounts: 26,470 Control: 18,320 	years with a random subsample
<i>Eissa, Habyarimana & Jack 2014</i> (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 	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 analysed for subgroup of account opener) Investment in agricultural business (land and fertiliser) 	<ul style="list-style-type: none"> Crop Income Food consumption (only analysed 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 & 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	Ghana, Malawi,	Low-income households	<ul style="list-style-type: none"> Savings Group (VSLA) 	Cycle usually between 8-12	<ul style="list-style-type: none"> Uptake of VSLA membership Saving (total and 	<ul style="list-style-type: none"> Business Profits Household poverty (assets and 	cRCT	<ul style="list-style-type: none"> Intervention: Ghana: 88 villages/ 2640 individuals, 	Ghana: 2 years, Malawi &

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

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

Table 2. Pooled Effect Sizes for Intermediate Outcomes

	Savings	Saving Attitudes	Financial Literacy	Business Investment	Business Profits
	(1)	(2)	(3)	(4)	(5)
Hedges' G	0.077***	0.061[†]	0.12[†]	0.045[†]	0.041**
(SE)	(0.02)	(0.02)	(0.05)	(0.02)	(0.01)
95% CI	[0.03, 0.12]	[-0.02, 0.09]	[-0.01, 0.24]	[-0.00, 0.09]	[0.01, 0.07]
I²	69.6%	24.4%	85.9%	43.9%	20.8%
Tau²	0.004	0.001	0.017	0.002	0.000
N of studies	18	4	7	9	7
N of effect sizes	43	8	23	28	13

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

Table 3. Pooled Effect Sizes for Distal Outcomes

	Expenditures/ Income	Food Security	Assets/ Housing	Education Investment	Enrolment (binary)	Health / Health Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Hedges' G	0.066**	0.052*	0.038	0.009	0.059	0.010
(SE)	(0.02)	(0.02)	(0.02)	(0.01)	(0.05)	(0.01)
95% CI	[0.02, 0.12]	[0.01, 0.10]	[-0.01, 0.09]	[-0.03, 0.05]	[-0.18, 0.3]	[-0.01, 0.03]
I²	61.7%	38.5%	65.9%	41.9%	39.7%	2.7%
Tau²	0.003	0.001	0.003	0.000	0.005	0.000
N of studies	11	8	9	6	3	5
N of effect sizes	38	18	23	17	11	17

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

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

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

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

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

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

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

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

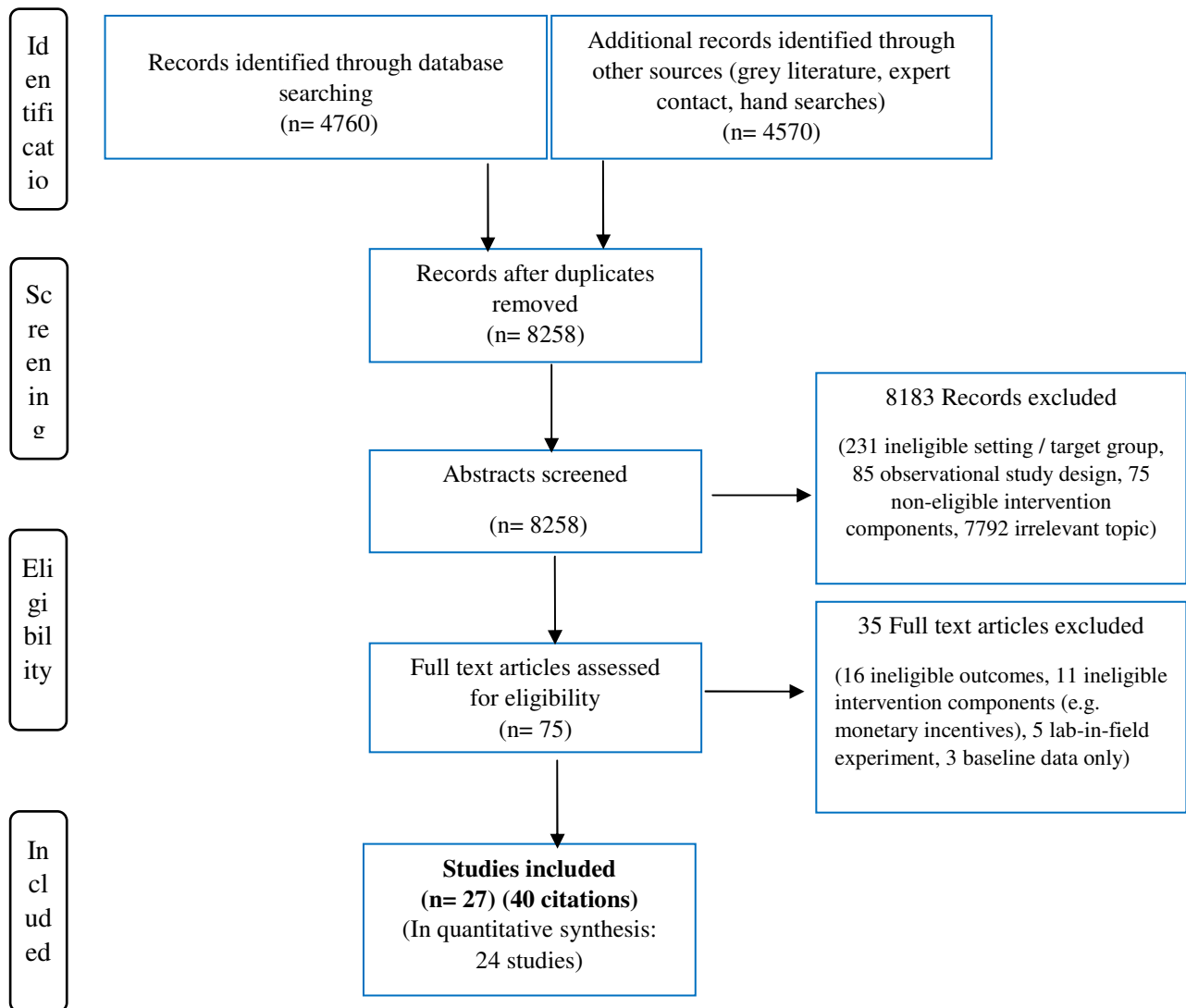
Table 5. ctd.

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

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

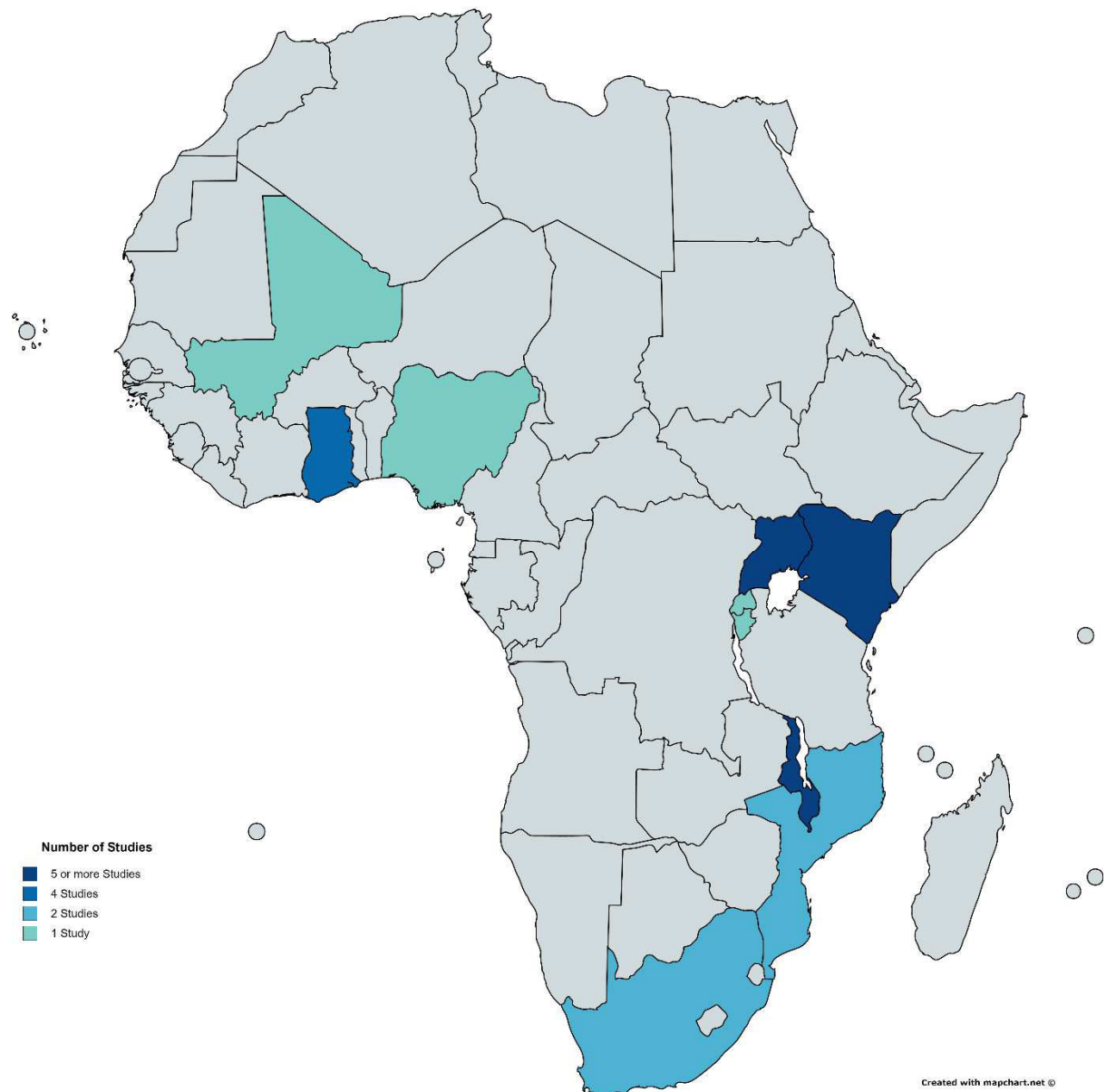
FIGURES

Figure 1. Flow Chart



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

Figure 2. Geographic Distribution of RCTs on Saving Promotion Interventions



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

Figure 3. Risk of Bias of Included Studies

	Random sequence generation (selection bias)	Allocation concealment (selection bias)	Blinding of participants and personnel (performance bias)	Blinding of outcome assessment (detection bias)	Incomplete outcome data (attrition bias)	Selective reporting (reporting bias)	Implementation fidelity	Baseline differences	Spillover and contamination
Annan et al. 2013	+	+	-	+	+	?	?	?	+
Batista & Vicente 2013	?	?	-	+	-	?	?	-	-
Beaman, Karlan & Thuysbaert 2014	+	?	-	+	+	+	+	+	+
Berg & Zia 2014	+	+	+	+	+	?	?	+	-
Berry, Karlan & Pradhan 2015	+	+	-	+	+	+	+	+	+
Brune et al. 2015	+	+	-	+	+	+	?	+	+
Buehren 2011	+	+	-	?	-	?	?	-	-
Carter, Laajaj & Yang 2015	+	?	-	?	+	+	?	-	+
Cole et al. 2014	+	+	-	+	-	?	?	+	+
Coville et al. 2014	+	+	+	+	-	?	+	+	-
Dizon, Gong & Jones 2016	+	+	+	+	+	+	?	+	-
Dupas, Karlan, Robinson & Ubfal 2016	+	+	+	+	+	+	+	+	+
Dupas, Keats & Robinson 2016	+	+	-	+	+	+	+	+	+
Dupas & Robinson 2013a	+	+	-	+	-	+	+	+	+
Dupas & Robinson 2013b	+	+	+	+	+	+	?	+	+
Eissa, Habyarimana & Jack 2014	?	?	+	+	-	?	?	?	+
Flory 2014	+	+	-	+	+	-	?	+	+
Jamison, Karlan & Zinman 2014	+	+	-	+	+	+	-	+	+
Karlan & Linden 2014	+	+	+	+	+	+	+	+	-
Karlan et al. 2012	+	+	+	+	+	?	?	+	-
Ksoll et al. 2016	+	+	-	+	+	+	?	+	-
Lee et al. 2015	?	?	+	+	?	?	?	-	+
McConnell 2012	?	?	-	+	-	?	?	+	-
Sayinzoga, Bulte & Lensink NA	?	?	-	?	+	?	?	+	+
Schaner 2013	+	+	-	+	+	-	-	+	+
Shephard, Kaneza & Moclair (under review)	+	+	-	+	+	+	+	+	+
Supanantaroeek (in press)	-	-	-	+	?	+	?	?	+

APPENDIX 1. Search Strategy

List of databases searched:

1. Cochrane Collaboration Library
2. Campbell Collaboration Library
3. EPPI-Centre Library
4. 3ie Database for Systematic Reviews
5. DFID Database for Systematic Reviews
6. PsycINFO
7. International Bibliography of Social Science (IBSS)
8. SCOPUS
9. Web of Sciences
10. Applied Social Science Index and Abstracts (ASSIA)
11. Conference Proceedings Citation Index - Social Science & Humanities
12. JOLIS (database of 14 World Bank and International Monetary Fund libraries)
13. ECONLIT
14. IDEAS/ RePEc
15. Business Source Premier
16. 3ie Impact Evaluation repository (indexed list of impact evaluations)
17. 3ie RIDIE (list of ongoing/registered impact evaluations)
18. Abdul Latif Jameel Poverty Action Lab (J-PAL)
19. Innovations for Poverty Action (IPA)
20. Eldis
21. USAID Development Experience Clearing House:
22. World Bank Impact Evaluation Working Paper Series
23. Research4Development (DFID)
24. African Development Bank Evaluation Reports:
25. <http://www.afdb.org/en/documents/evaluation-reports/>
26. Agence Française de Développement: Impact Evaluations
27. Asian Development Bank Evaluation Resources
28. Inter-American Development Bank Evaluations

Search String:

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

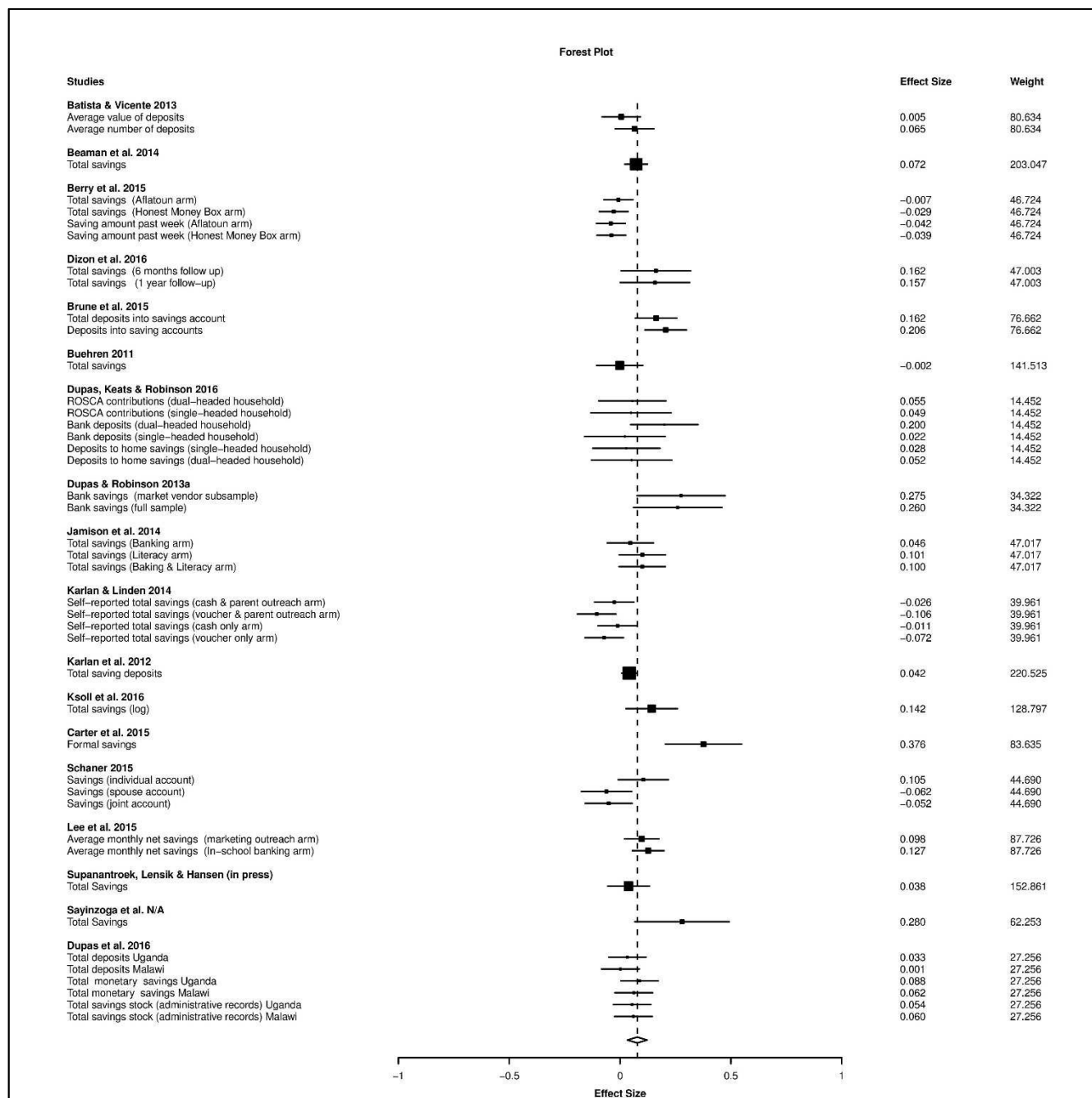
Search number	Search
#1 [Program]	(AB,TI (saving* OR microsaving* OR ROSCA* OR stokvel* OR "savings group*" OR (rotating saving credit association) OR VSLA OR (village saving* loan association) OR ASCA OR "village bank*" OR (financial NEAR/2 (literacy OR literate)) OR Aflatoun OR YouthSafe OR (financial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

	"cash box" OR "saving box" OR "safe deposit box" OR "safe-deposit box")) OR (SU(financial inclusion OR banking OR financial literacy OR saving*))
#2 [Study Design]	(AB, TI ("randomized control* trial" OR "randomised control* trial" OR randomised OR randomized OR RCT OR randomly OR trial OR experiment* OR "control group" OR "comparison group")) OR (SU(randomised control study OR randomised control trial OR randomised controlled trial OR randomized control trial OR randomized controlled trial OR experiment design OR experimental design OR randomized experiment))
#3 [Country]	ALL FIELDS (Africa OR Sub-Sahara* Africa* OR "Sub Sahara Africa" OR Sub-Sahara* OR "Sub Sahara" OR Angola OR Benin OR Botswana OR "Burkina Faso" OR "Burkina Fasso" OR "Upper Volta" OR Burundi OR Urundi OR Cameroon OR Cameroons OR Cameron OR Camerons OR "Cape Verde" OR "Central African Republic" OR Comoros OR "Comoro Islands" OR Comores OR Mayotte OR Congo OR Zaire OR "Cote d'Ivoire" OR "Ivory Coast" OR Djibouti OR "French Somaliland" OR Ethiopia OR Gabon OR "Gabonese Republic" OR Gambia OR Ghana OR "Gold Coast" OR Guinea OR Guam OR Guiana OR Jamaica OR Kenya OR Lesotho OR Basutoland OR Liberia OR Madagascar OR "Malagasy Republic" OR Mali OR Mozambique OR Namibia OR Niger OR Nigeria OR Rwanda OR Ruanda OR "Sao Tome" OR Senegal OR "Sierra Leone" OR Somalia OR Sudan OR "South Sudan" OR Swaziland OR Tanzania OR Togo OR "Togolese Republic" OR Tonga OR Uganda OR Zambia OR Sambia OR Zimbabwe OR Rhodesia OR "developing country" OR "developing countries" OR "developing nation" OR "developing nations" OR "developing population" OR "developing populations" OR "developing world" OR "less developed country" OR "less developed countries" OR "less developed nation" OR "less developed nations" OR "less developed population" OR "less developed populations" OR "less developed world" OR "lesser developed country" OR "lesser developed countries" OR "lesser developed nation" OR "lesser developed nations" OR "lesser developed population" OR "lesser developed populations" OR "lesser developed world" OR "under developed country" OR "under developed countries" OR "under developed nation" OR "under developed nations" OR "under developed population" OR "under developed populations" OR "under developed world" OR "underdeveloped country" OR "underdeveloped countries" OR "underdeveloped nation" OR "underdeveloped nations" OR "underdeveloped population" OR "underdeveloped populations" OR "underdeveloped world" OR "middle income country" OR "middle income countries" OR "middle income nation" OR "middle income nations" OR "middle income population" OR "middle income populations" OR "low income country" OR "low income countries" OR "low income nation" OR "low income nations" OR "low income population" OR "low income populations" OR "lower income country" OR "lower income countries" OR "lower income nation" OR "lower income nations" OR "lower income population" OR "lower income populations" OR "underserved country" OR "underserved countries" OR "underserved nation" OR "underserved nations" OR "underserved population" OR "underserved populations" OR "underserved world" OR "under served country" OR "under served countries" OR "under served nation" OR "under served nations" OR "under served population" OR "under served populations" OR "under served world" OR "deprived country" OR "deprived countries" OR "deprived nation" OR "deprived nations" OR "deprived population" OR

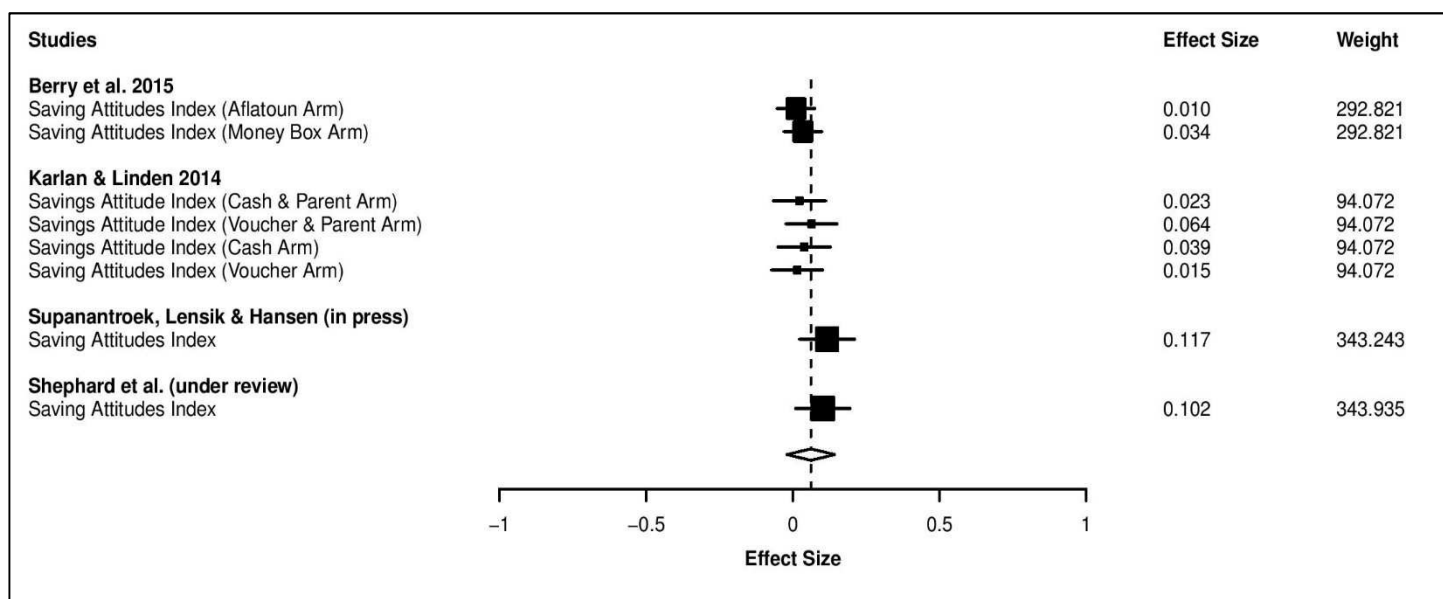
	"deprived populations" OR "deprived world" OR "poor country" OR "poor countries" OR "poor nation" OR "poor nations" OR "poor population" OR "poor populations" OR "poor world" OR "poorer country" OR "poorer countries" OR "poorer nation" OR "poorer nations" OR "poorer population" OR "poorer populations" OR "poorer world" OR "developing economy" OR "developing economies" OR "less developed economy" OR "less developed economies" OR "lesser developed economy" OR "lesser developed economies" OR "under developed economy" OR "under developed economies" OR "underdeveloped economy" OR "underdeveloped economies" OR "middle income economy" OR "middle income economies" OR "low income economy" OR "low income economies" OR "lower income economy" OR "lower income economies" OR "low gdp" OR "low gnp" OR "low gross domestic" OR "low gross national" OR "lower gdp" OR "lower gnp" OR "lower gross domestic" OR "lower gross national" OR lmic OR lmics OR "third world" OR "lami country" OR "lami countries" OR "transitional country" OR "transitional countries")
#4	Search #1 AND #2 AND #3

APPENDIX 2. Forest Plots

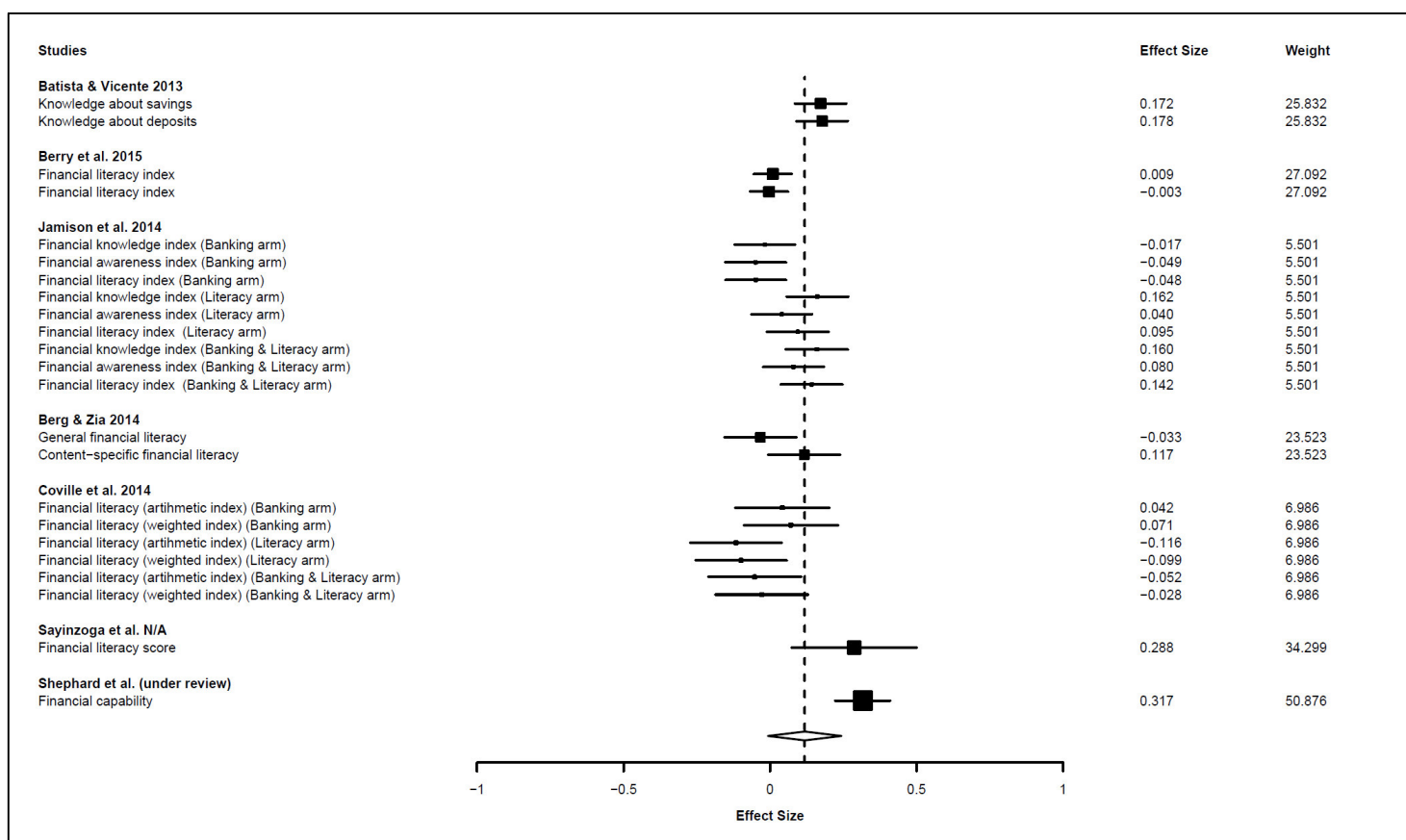
1. Forest Plot for Total Savings



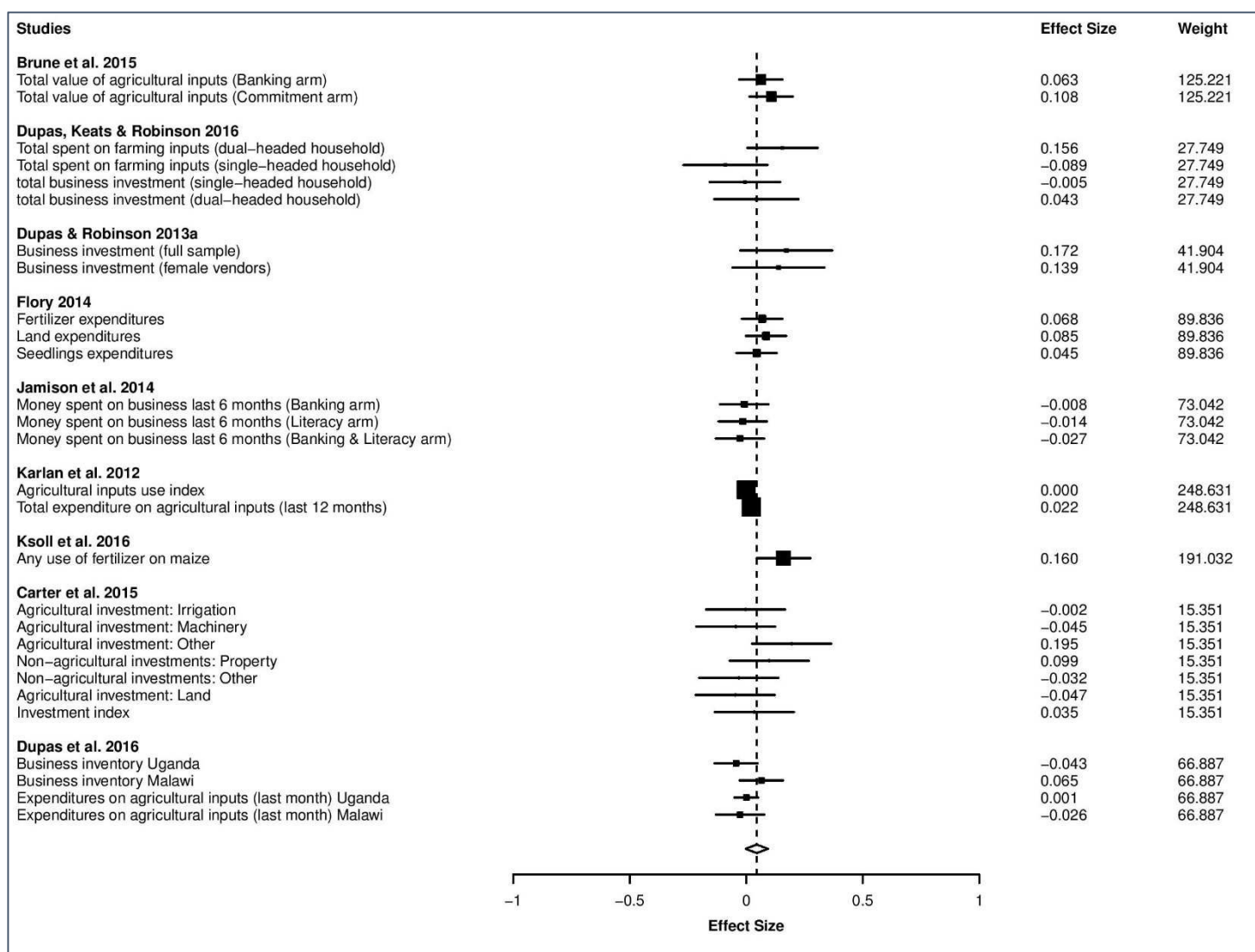
2. Forest Plot Savings Attitudes



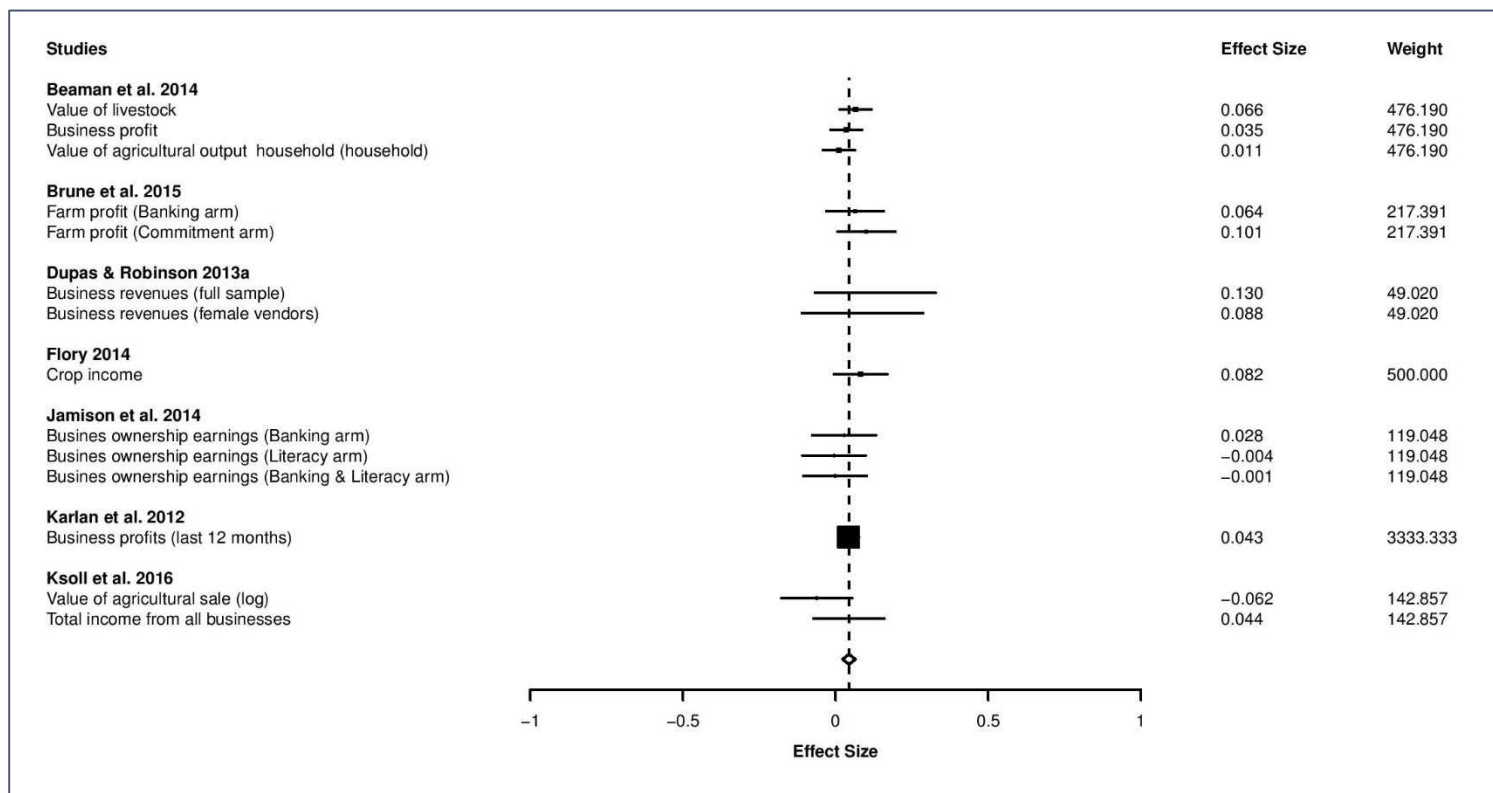
3. Financial Literacy



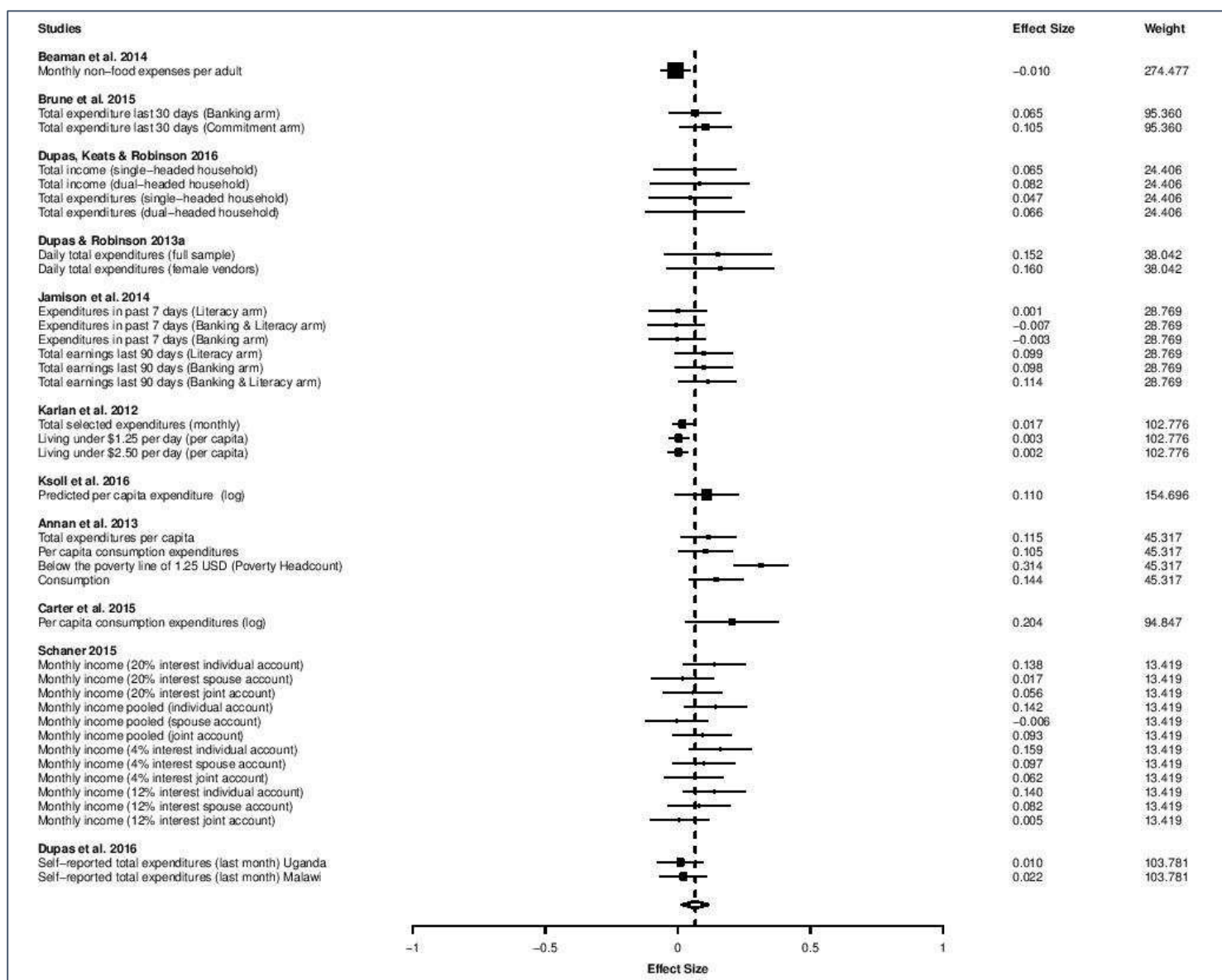
4. Forest Plot Business Investment



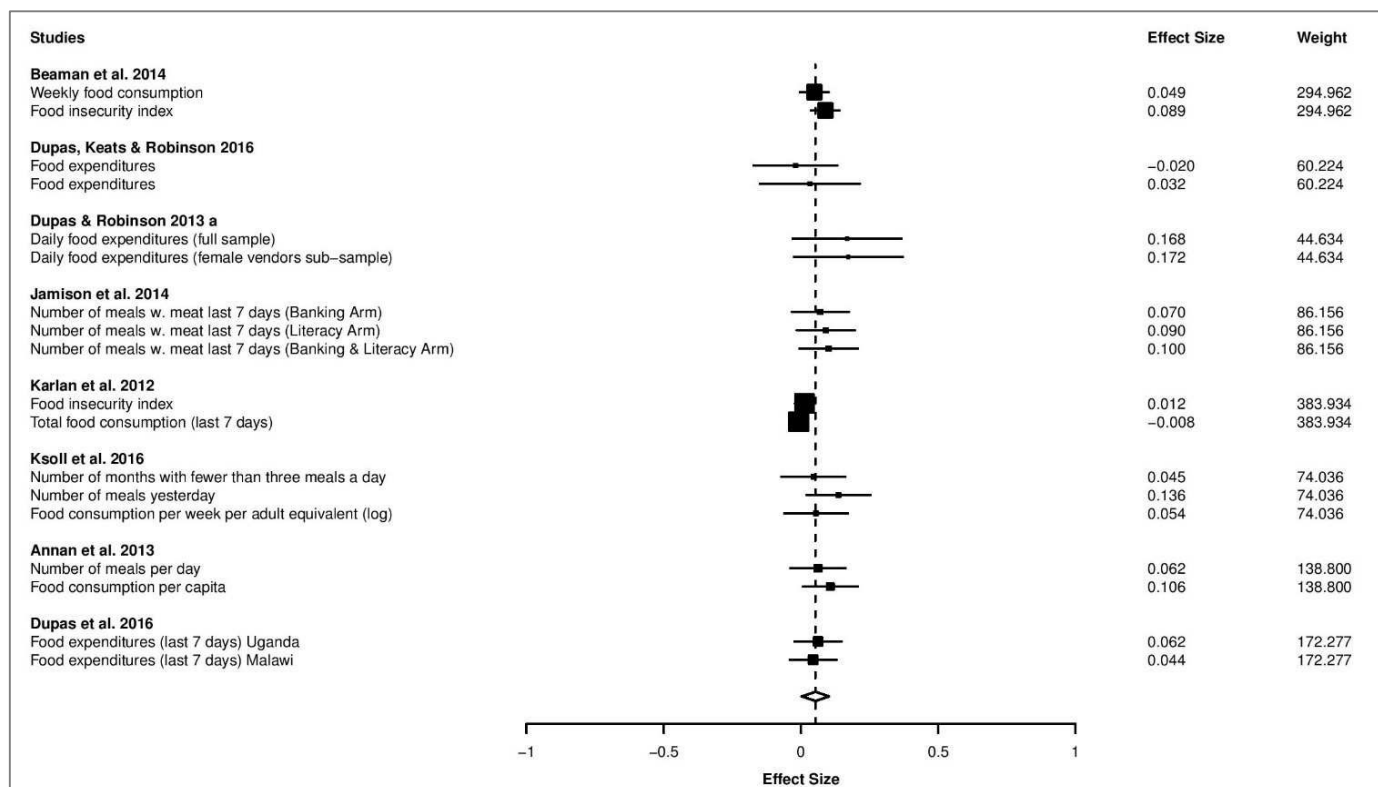
5. Forest Plot for Business Profit



6. Forest Plot for Poverty (Income/Expenditures)

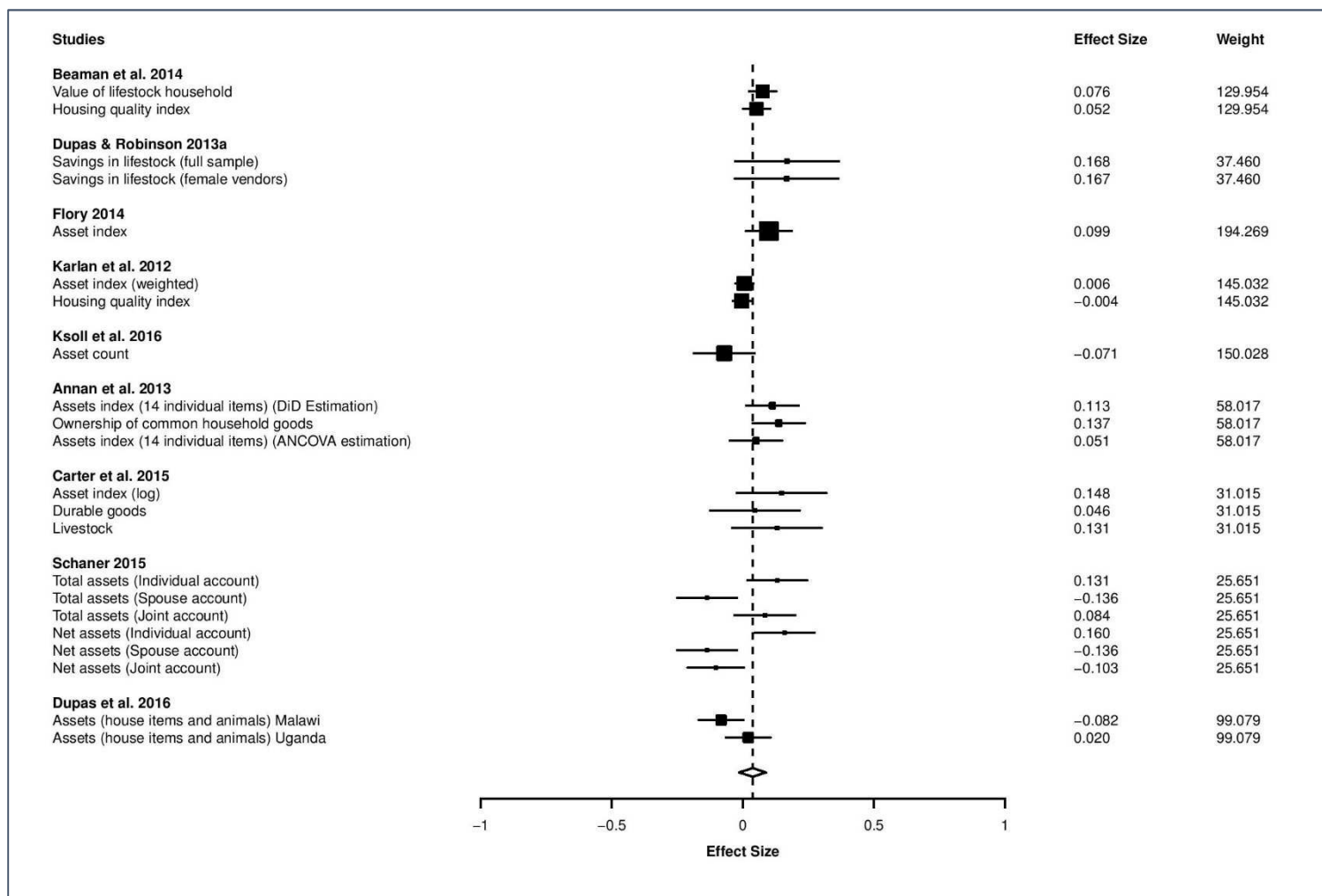


7. Food Security

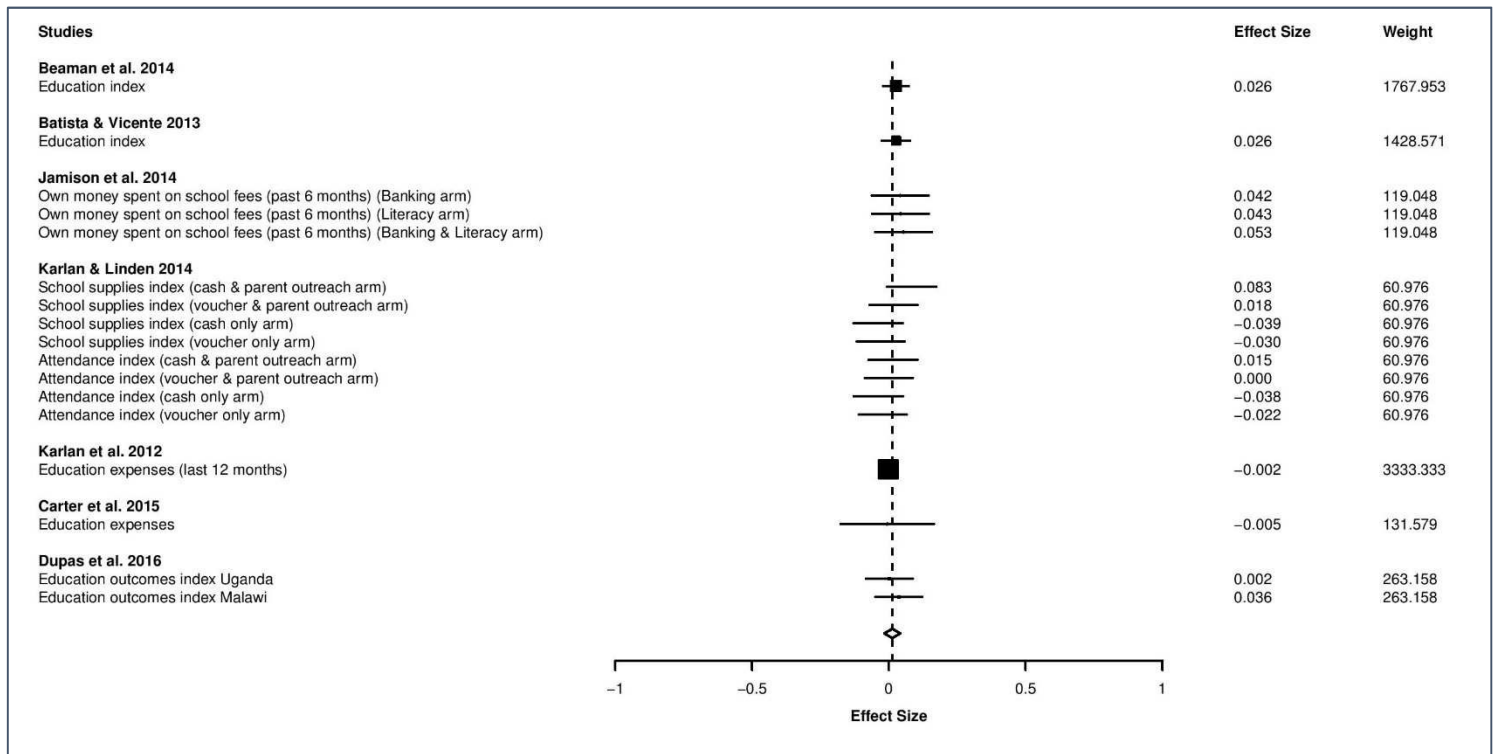


Note: A number of studies measured food insecurity and reduction thereof. Effect sizes were therefore reverse-coded.

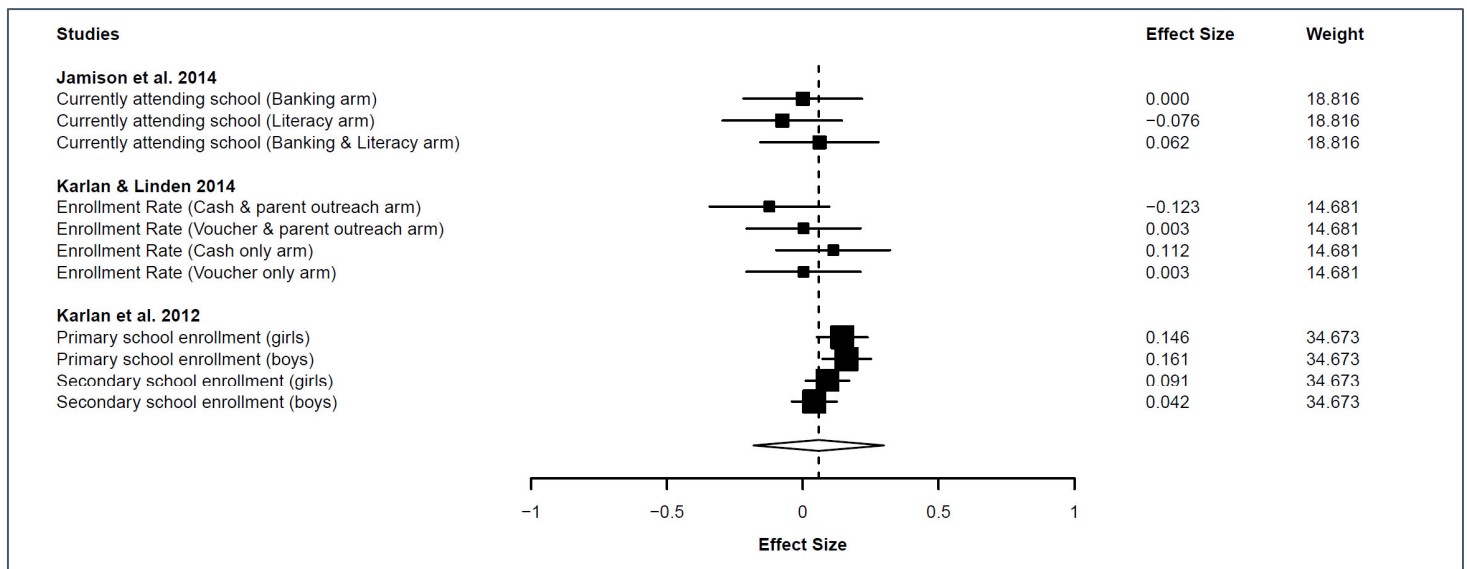
8. Forest Plot for Household Assets



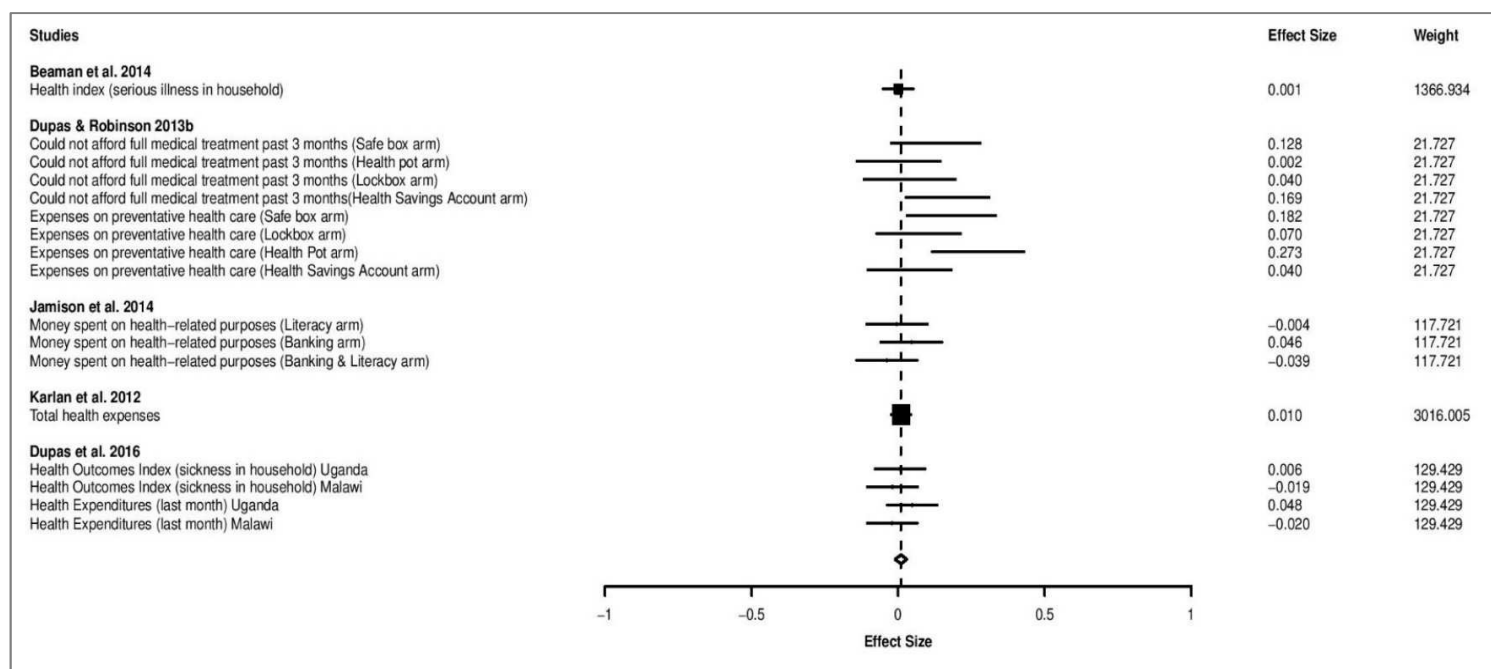
9. Forest Plot Education



10. Forest Plot School Enrolment



11. Forest Plot General Health and Health Investment



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

APPENDIX 3. Coding of Intervention Components

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

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

APPENDIX 4. Risk of Bias Assessment

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

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

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

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

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

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

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

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

APPENDIX 5. Robustness Test: Wider Definition of Outcome Categories

Meta-Regression: Intervention Components (Wider Definition)

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

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

Meta-Regression: Study Design, Participant Characteristics and Bias

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

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

**Meta-Regression: Study Design,
Participant Characteristics and Bias (ctd.)**

	Investment				
	(11)	(12)	(13)	(14)	(15)
Duration	-0.01 (0.02)				
Female		-0.07* (0.02)			
Mixed		-0.06* (0.02)			
Youth			-0.04 (0.02)		
Time to Follow Up				-0.02 (0.01)	
Risk of Bias					0.00 (0.00)
Const.	0.05 (0.04)			0.08 [†] (0.03)	0.04 (0.02)
Const. (Male)		0.09* (0.02)			
Const. (Adults)			0.04* (0.01)		
Sample			Full		
N (Studies)			13		
N (Effect Sizes)			62		
I ² (original vs. resid.)	33.7, 37.3	33.7, 13.9	33.7, 36.6	33.7, 30.7	33.7, 38.6

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